The Challenges of Universal Health Insurance in Developing Countries: Experimental Evidence from Indonesia's National Health Insurance*

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Abstract

To investigate barriers to universal health insurance in developing countries, we designed a randomized experiment involving about 6,000 households in Indonesia who are subject to a government health insurance program with a weakly enforced mandate. Time-limited subsidies increased enrollment and attracted lower-cost enrollees, in part by reducing the strategic timing of enrollment to correspond with health needs. Registration assistance also increased enrollment, but increased *attempted* enrollment much more, as over half of households who attempted to enroll did not successfully do so. These findings underscore how weak administrative capacity can create important challenges in developing countries for achieving widespread coverage.

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I. INTRODUCTION

As developing countries emerge from extreme poverty and enter middle-income status, many aim to expand government-run social safety nets (Chetty and Looney 2006). An important part of this process is the creation of universal health insurance programs, which have expanded to many lower- and middle-income countries over recent decades (Lagomarsino et al. 2012). In expanding health insurance, however, emerging countries may face particularly vexing versions of the challenges faced by developed countries, because of the large informal sector operating outside the tax net (Jensen 2019). Some countries, such as Thailand, have created a single-payer-like system funded entirely out of tax revenues and supplemented by small co-pays; this has been shown to improve healthcare utilization and outcomes, but faces funding challenges (Gruber, Hendren, and Townsend 2014). Other countries, such as Ghana, Kenya, the Philippines and Vietnam – as well as Indonesia, which is the focus of our study – have sought to create a contributory system with an individual mandate to reduce the financial burden on the government. In these systems, the very poor are subsidized by tax revenues, but everyone else is required to pay a premium, collected through a payroll tax for formal sector workers and collected directly from individuals for everyone else.

The challenge with contributory systems is that enforcing the insurance mandate for those who directly pay premium is difficult. While the political and administrative challenges of enforcing mandates are not unique to developing countries – for example, the 2010 Obamacare mandate did not achieve universal coverage in the United States (Berchick et al. 2018) and the U.S. tax penalties for failing to comply with the mandate were eliminated starting in 2019 – they are particularly hard for developing countries, again because the majority of their citizens are outside the tax net. This means that the types of penalties for non-compliance used initially under Obamacare – fines collected through the personal income tax system – are not even an option. Since developing countries have, perhaps rightly, shown little appetite for enforcing the few possible remaining sanctions on this population (e.g., denying delinquent households the ability to enroll their children in school), what they are left with is a toothless mandate.

A toothless mandate can create two related challenges for governments that are trying to increase coverage: low program enrollment and adverse selection, where the least healthy are most likely to enroll, raising program costs per enrollee above the population average (Akerlof 1970;

Einav and Finkelstein 2011). Indonesia, like other countries, has experienced both problems: Although mandatory, universal health insurance was launched in 2014, one year after its introduction, the contributory portion of the program, known as *JKN Mandiri*, had enrolled only 20 percent of the targeted population; moreover, because enrollees were much less healthy than the typical targeted population, claims exceeded premiums by a ratio of 6.45 to 1.[1]

These facts motivate the question of whether and how developing country governments can design supplemental policies to mitigate these challenges—to boost national health insurance enrollment, while also reining in the financial costs to the tax-funded government budget—in contexts with mandatory, but weakly enforced, contributory health insurance programs. The aim is not necessarily for the government to break even—some subsidies may be needed in order to make sure that there is enough social protection against health shocks—but to limit government spending while insuring as many people as possible. With this perspective in mind, in 2015 and in cooperation with the Indonesian government, we designed a large-scale, multi-arm experiment—involving almost 6,000 households.

We designed three interventions that simple economic theory suggested could increase enrollment and reduce adverse selection in this nationally mandated insurance program. First, we examined the role of large, temporary subsidies. We randomized households to receive subsidies of either 50 percent ("half subsidy") or 100 percent ("full subsidy") for the first year of enrollment. To be eligible for the subsidy, households had to enroll within two weeks after they were offered it, akin to governments offering a large, time-limited registration incentive. Second, we examined the role of transaction costs by randomly offering some households at-home assistance with the online registration system, so that they did not have to travel to a far-off insurance office to enroll. Third, we examined information constraints by randomly advertising three different types of basic insurance information: the financial costs of a health episode and how they relate to insurance prices, the presence of a two-week waiting period from enrollment to coverage (so that one could not wait to get sick and immediately sign up), and the fact that insurance coverage is legally mandatory.

[1] Enrollment rates are from authors' calculation based on official membership numbers and the national sample survey, SUSENAS 2015 (BPS 2015). Claims-to-premium ratios are from LPEM-UI (2015).

To assess the impacts of these interventions, we primarily analyze government administrative data. These data contain detailed information on all program enrollees from the study group for 20 months after the intervention, including monthly data on registration, premiums paid, and the amount and nature of any insurance claims made. These extensive, high-frequency administrative data allow us to examine the dynamic responses to the interventions both during and after the subsidy period. We supplement these data with a short baseline assessment survey in which we collected data on demographics and self-reported health; this allows us to measure pre-intervention "health status" for all study participants, regardless of whether they subsequently enrolled in the insurance program.

We use these data to study several key outcomes. First, we examine both *attempted enrollment*, which we define as the household starting the initial registration process, and *actual enrollment* (hereafter "enrollment"), which we define as successfully completing the initial registration process. Second, since the decision to stay enrolled is a dynamic one in which households need to pay a monthly premium, we also examine the impact of the intervention on the time path of *insurance coverage*, which we define as having paid the premium for a given month. Third, we study the patterns of insurance claims among those who have insurance coverage.

We have three main findings. First, there are both monetary and non-monetary barriers to enrollment. Both the time-limited, temporary subsidies and the registration assistance increased enrollment. The full subsidy increased enrollment by 18.6 percentage points off of a control group mean of 8 percent. Moreover, even after the subsidy ended, insurance coverage in the full-subsidy group remained about twice as high as in the no-subsidy group, consistent with the idea of health insurance as an "experience good" (Cai et al. 2020; Dupas 2014; Delavallade 2017). Reducing hassles by assisting with internet-based registration also increased enrollment, by a statistically significant 3.5 percentage points (45 percent), but none of the information treatments affected enrollment. This suggests that lack of information may not be a key barrier and, relatedly, and that while information and nudge campaigns are often an attractive policy option given their low cost (Thaler and Sunstein 2009), they may be of limited effectiveness in this context.

Second, weak administrative state capacity is a central impediment to achieving universal coverage. The hassles of having to enroll at a government office appear to be a substantial barrier: nearly as many people attempted to enroll in the assisted internet-based registration with no subsidy as did in the full-subsidy group with status quo office-based registration. This suggests that the hassle costs involved with in-person enrollment are in some sense as costly as a full year's worth of premiums.

But an even more important finding is that many more people *attempted* to enroll than were actually able to do so. For example, when offered both a full subsidy and assisted registration, nearly 60 percent of households tried to enroll, but less than half of these households were successfully able to do so. The large wedge between attempted and actual enrollment was due to technical and administrative challenges with the government's online enrollment system. While also reminiscent of the issues with Healthcare.gov in the United States, this particular challenge stemmed from a problem common in many developing countries: Indonesia's underlying state civil registry, i.e., the data on who is in each family, is often inaccurate (Sumner and Kusumaningrum 2014). Because each family members must be enrolled at once (to help mitigate adverse selection), inaccuracies in family definitions in the civil registry meant that people would have to visit an office to fix these errors before they could return to the website and sign up correctly. Since imperfect civil registries are common throughout the developing world (see e.g. Mikkelsen et al. 2015; Muralidharan et al. 2020), these types of challenges are likely to be encountered in other contexts as well. Households could overcome these issues by showing up in person to a district office and having an official override the system in some way, but this was a significant additional hassle cost that appears to have discouraged many households.

Third, we find evidence of strategic timing in health insurance coverage that also in part reflects the consequences of Indonesia's administrative structure. In Indonesia, individuals can enroll in health insurance at any point during the year. This creates incentives to delay coverage until one gets sick and - despite penalties if someone drops coverage and then tries to reactive it – to drop coverage once one recovers. Such strategic "wait till you need it" enrollment timing became evident when we compared outcomes for the no-subsidy group to outcomes for the group to whom we offered time-limited subsidies. Relative to enrollees in the no-subsidy group, those enrolling in insurance in the full-subsidy treatment reported better health at baseline and had fewer claims (and notably, fewer claims for chronic conditions) during their first year of enrollment than typical new enrollees in the no-subsidy group. These cost differences in part reflect strategic timing decisions by the no-subsidy group, rather than just fixed health differences alone: The no-subsidy enrollees submitted more claims than did full-subsidy enrollees in the first three months after enrollment, and many enrollees in the no-subsidy group subsequently dropped coverage – i.e., stopped paying premiums – after a few months.

The fact that the subsidies brought in healthier people who were less likely to drop coverage soon after an initial claim has potential implications for the longer-term ability of the program to increase coverage. On net, once the subsidies had ended, the government was able to cover substantially more people at no higher total cost, at least up to the first twenty months since offer date, because the time-limited subsidies brought in healthier enrollees with fewer claims.

A natural policy tool to limit such strategic timing is to allow individuals a limited window each year to enroll. Such "open enrollment" periods are the norm in the United States, but are absent in Indonesia and in several other developing countries (e.g. Ghana, Kenya, the Philippines, and Vietnam) that also have a public health insurance system in which the non-poor informal sector may pay premiums to enroll. One potential reason developing countries may not use limited enrollment windows is that lumpy incomes and credit constraints may hinder households from making the first payment if the timing of that payment is not aligned with the timing of their income. In fact, take-up of various products in developing countries (e.g. fertilizers, scholarships) has been shown to be dramatically higher if households are given the opportunity to make the first payment right after they receive income (e.g. see, for example Duflo, Kremer, and Robinson, 2011, for an example from agriculture). Since the precise timing of when households receive incomes differ across the population, a fixed, limited enrollment window might substantially discourage enrollment for many households. In fact, in Indonesia, the Social Agency's understanding of Indonesian law was that they were legally mandated to allow enrollment throughout the year in order to allow for more flexible access. The inability to offer limited enrollment windows – perhaps for this reason, or because of the practical challenges associated with doing so– reveals another important challenge for developing countries in trying to administer a universal contributory health insurance program.

Our study builds on the large literature on participation in public health insurance systems – and in social insurance programs.[2] Existing evidence from both developed and developing countries points to increased participation in social insurance programs, including health insurance, from monetary subsidies (e.g., Thornton et al. 2010; Asuming 2013; Fischer et al. 2018; Finkelstein, Hendren, and Shepard 2019), reductions in transaction costs (e.g., Alatas et al. 2016; Bettinger et al. 2012; Dupas et al. 2016), and information (e.g., Gupta 2017; Bhargava and Manoli 2015). We examine the impact of all three of these commonly conjectured participation barriers in the context of a large-scale government insurance program.

Our rich administrative data allow us to probe more deeply than is typically feasible in developing countries to identify key challenges to achieving universal coverage in the context of weak state capacity. For example, our ability to measure both enrollment attempts and actual enrollment successes highlights the key obstacles that imperfect civil registries can impose. In the same vein, our high-frequency data on the dynamics of premium payments and claims allow us to identify the strategic timing of insurance coverage that can occur when the state is unable to limit enrollment to a short period of time each year;[3] Diamond et al. (2019) explore similar phenomena in the individual health insurance market in California subject to the rules of the U.S. Affordable Care Act. Cabral (2017) demonstrates how adverse selection can be generated not only through the strategic timing of coverage (as we and Diamond et al. (2019)) focus on), but also through the strategic timing of treatment decisions.

The remainder of the paper is organized as follows. Section II describes the setting. Section III presents the experimental design and data. Section IV presents the impacts of the intervention on enrollment, coverage patterns over time, and attempted enrollment. Section V illustrates the strategic coverage timing under the status quo and how time-limited subsidies reduce such behavior. The last section concludes.

II. SETTING

[2] It is particularly closely related to Thornton et al. (2010), who examine the impact on enrollment and health care utilization of randomly offered subsidies for contributory health insurance for informal workers in Nicaragua. They find impacts of subsidies on enrollment and health care utilization.

[3] Our use of administrative data allows us to go further than Asuming et al. (2019), who use survey data to assess the impact of one-year subsidies on enrollment and health behaviors during and after the subsidy in Ghana. Our high-frequency administrative data allow us to unpack the dynamics of selection and show how differential retention affects our understanding of these health insurance markets.

A. The Indonesian Health Care System

The Indonesian health care system consist of a mix of public and private providers. The public sector provides all levels of care, including large tertiary care hospitals in major cities, smaller secondary care hospitals in virtually every district of Indonesia, and a vast network of clinics at the sub-district and village levels. Private hospitals and private clinics operate alongside these public providers, particularly in urban areas. Publicly-employed physicians and midwives are also allowed to operate private practices after hours, and many do so, even outside of urban areas. Nationwide, about 55 percent of inpatient visits are in public hospitals and clinics, while 45 percent are in private hospitals and clinics; about 40 percent of outpatient visits take place in public clinics, and 60 percent in private (Appendix Table 1).[4]

At the time of our intervention, the main provider of health insurance was the government, through several different programs (JKN Mandiri, which we study here; the branches of JKN that provide insurance for formal sector workers and government employees, and the government-run schemes (JKN and otherwise) that provide free coverage for the poor from national or district governments); only about 1 percent of the population had private insurance. In 2014, about half of the Indonesian population lacked formal health insurance.[5] Those without insurance faced substantial risk of out-of-pocket health care expenditures. For uninsured, non-poor informal sector households – the sample we will focus the study on – we estimated average annual out-of-pocket health expenditures of Rp. 894,024, or about 2.3 percent of total non-health household consumption. But this average masks considerable variance; for example, while the median household spent only 0.4 percent of total non-health consumption on health expenditures, the 95th percentile household spent 9.1 percent, and the 99th percentile household spent 36.1 percent. By way of comparison, King et al.'s (2009) study of the uninsured in Mexico defined "catastrophic" health expenditures as those that exceed 30 percent of annual household spending, after subtracting a subsistence food allocation. This suggests that there is substantial risk to be insured.

B. Mandatory, contributory public health insurance: the *JKN Mandiri* program

[4] All statistics in this sub-section are authors' calculation from the nationally representative 2015 Indonesian National Household Survey (SUSENAS).

[5] Since the time period that we started our study, there has been an increase in coverage, and as of 2020, approximately 20 percent are still uncovered (https://bpjs-kesehatan.go.id/bpjs/). For the population that we focus on—the informal, non-poor who do not qualify for free insurance paid for by the government—over 60 percent remain uninsured.

In January 2014, the Government of Indonesia launched *Jaminan Kesehatan Nasional* (JKN), a national, mandatory, public health insurance program designed to provide universal coverage by 2019. It consists of three different sub-programs based on income and employment status. The program we study - *JKN Mandiri* - covers non-poor informal workers and their households, which represent 30 percent of the population, and is supposed to be funded by their premium contributions.[6]

Public insurance covers essentially all health care costs incurred at all public and affiliated private clinics and hospitals with no co-pays, although certain specific procedures (e.g., cosmetic surgery, infertility treatments, orthodontics, etc.) are excluded. Primary care clinics are reimbursed under capitation based on the total number of practitioners, the ratio of practitioners to beneficiaries, and operating hours. Hospitals are reimbursed by case following a tariff system called INA-CBG (Indonesia Case Base Groups), in which amounts are determined jointly by primary diagnosis and severity of the case. Having JKN insurance changes who pays for care, but does not change access to care, other than requiring referrals from primary care before accessing secondary or tertiary care services.

To receive coverage under *JKN Mandiri*, households must complete an initial registration process and then subsequently pay their monthly premiums. To enroll, households must register either in person at the *Badan Penyelenggara Jaminan Sosial – Kesehatan* (Social Security Administration for Health, or BPJS) office or through the social security administration website; the latter option, however, is often unavailable to households since they may lack internet access or find the on-line process confusing. They must also register the entire family, as listed on the household's *Kartu Keluarga* (family card), which is maintained in the civil registry by another ministry (Ministry of Home Affairs); registering a single individual within the family is not allowed. Registration requires each individual's national ID number (*Nomor Induk Kependudukan*, or NIK), and family card number.

[6] About 40 percent of households that are classified as poor and near poor receive insurance funded completely out of general government revenues. The program for formal workers is funded jointly by employee and employer contributions that are withheld by the tax system.

Those registering in person in a BPJS office must bring a photocopy of their national identity card and government-issued family card, as well as a 3cm by 4cm photo of each individual being registered. Meanwhile, those registering online should input their family card number for the system to automatically retrieves their national identity numbers and addresses from the national civil registry system. Households registering online must also provide an active telephone number and must have a photo that can be uploaded or scanned to the system for each household member. Registrants select a primary care location at the time of registration.

To maintain coverage, the household must then pay premiums each month. The premium can be paid at any social security administration office, ATM, or select convenience stores, or mobile banking. Paying the premium by the 10th of a given month ensures coverage for that calendar month. If no payment is made, coverage is deactivated after a one-month grace period.

The per-person monthly premium for basic coverage (known as Class III) is Rp. 25,500 (~$2).[7] This means that premiums for a family of four (Rp. 100,000) are roughly similar to the average household out-of-pocket health expenditures which, we noted above, are Rp. 894,024 per year, or about Rp. 75,000 per month. However, as we will see below, due to some combination of adverse selection and moral hazard, the claims for those actually enrolled are substantially higher than premiums paid. This high level of claims relative to out-of-pocket expenditures among the uninsured is consistent with the insurance product being potentially valuable and useful.

Institutional features to reduce selection

While insurance enrollment is legally mandatory, the mandate is hard to enforce in practice. There are no penalties assessed on households that do not enroll. However, as one method to combat potential adverse selection, the government requires households who enroll to register all nuclear family members (e.g., father, mother, and children), as listed on their official *Kartu Keluarga* (family card). In addition, if a household stops paying premiums and drops coverage,

[7] There are three different classes of coverage that cover the same medical procedures, but offer different types of accommodations should an inpatient procedure be required. The per-person monthly premium during the period of the study was Rp., 42,500 (~$3) for class II (3-5 beds per room) and Rp., 59,500 (~$4.5) for class I (2-3 beds per room). Class III (more than 5 beds) is the most common insurance among our population of interest, with 72 percent of households in the control group enrolling in Class III insurance.

there are financial penalties if they subsequently try to re-enroll. The amount of these penalties are increasing with the amount of initial claims they incur after re-enrolling.[8]

Despite these features, a key opening for selection is that households may register for *JKN Mandiri* at any time of the year. After the program's introduction in 2014, the government became concerned that this might lead individuals to wait to enroll in JKN until they had a health emergency. However, existing constitutional rulings precluded it from limiting enrollment to a fixed window of time within the year, as all social benefit programs must be open to enrollment by all citizens at all times. Lacking the ability to limit enrollment to a short period of time within the year, in September 2015 the government instead introduced a two-week waiting period after enrollment before households can submit an insurance claim.

III. EXPERIMENTAL DESIGN AND DATA

A. Study Setting and Sample

We carried out this project in two large Indonesian cities: Kota Medan in North Sumatra and Kota Bandung in West Java. These are the fourth and third largest urban areas in Indonesia, respectively, each with a population of approximately 2.2-2.5 million people. We focused on an urban setting to abstract from supply-side issues that are likely to depress demand in rural areas. We chose Medan and Bandung because a significant fraction of their population was uninsured. Moreover, selecting cities both on and off Java helps ensure representativeness of Indonesia's heterogeneity in culture and institutions (Dearden and Ravallion 1988).

Working with the government, we implemented the interventions in two sub-districts in Medan and eight sub-districts in Bandung. Using the 2010 Census, we selected sub-districts from among those with the highest concentration of non-poor informal workers; within those sub-districts, we randomly selected neighborhoods for the study.[9] To identify JKN-eligible households

[8] Specifically, for coverage to reactivate at a later date, the household must pay their premiums in arrears, up to a maximum of 6 months. If no inpatient claims are submitted within 45 days from re-activation, there are no additional fees beyond these premiums-in-arrears. Otherwise, the household has to pay an additional penalty equal to 2.5 percent of the inpatient claims times the number of inactive months, up to a maximum of 12 months or Rp. 30 million.

[9] We excluded sub-districts with universities, large factories, or malls to avoid areas with a high concentration of temporary residents. We then randomly selected twelve *kelurahan* (urban municipal units) in the two sub-districts in

within the sampled areas, we targeted uninsured, informal workers by administering a rapid eligibility survey to all listed households. We excluded households that already had at least one member covered by health insurance and those that were officially below the poverty line (and thus qualified for free insurance). Of the 52,584 listed households, 7,629 (14.5 percent) satisfied the target population criteria.

When we matched our baseline survey data with the government's administrative data, we discovered that some households were already covered by health insurance, even if they reported that they were not. This was mostly an issue for the city of Medan, where the local government had recently expanded the set of poor households who qualified for free insurance, but had not yet communicated this to the newly insured. Since households with at least one insured member were not eligible for the study (and this was pre-determined), we excluded those already enrolled. Our final sample was 5,996 households, about three-quarters of whom were in Bandung.

We implemented the intervention in Medan in February 2015 and in Bandung in November and December 2015. Because of the introduction of the two-week waiting period from enrollment to coverage in September 2015, the households in the Bandung sample were subject to the waiting period, whereas households in the Medan sample were not. Otherwise, the health insurance program was identical in the two cities.

B. Experimental design

Upon identifying an eligible household, we administered a short baseline survey, at the end of which the household was randomly assigned to three fully-crossed treatment arms affecting the insurance price, the hassle cost of registration, and the information available. Figure 1 summarizes the experimental design for the cities of Medan and Bandung separately.[10]

Medan (out of 16 possible *kelurahan*) and four *kelurahan* in each sub-district in Bandung (out of 41 possible *kelurahan*). Within each *kelurahan*, we randomly selected the neighborhoods (*rukun warga*, also known as RW) to enumerate.

[10] The number of households differs in each treatment for two reasons. First, while in Medan we maximized power to detect differences in enrollment, in Bandung we maximized power to detect differences in claims conditional on take-up. Since we expected greater take-up with a larger subsidy, we randomized more households into groups with smaller subsidy amounts. Second, a coding error meant that while the overall treatment probabilities were as assigned, some combinations of treatments were more likely to be randomly assigned to households than others (this coding error was corrected partway through the Bandung experiment). We include in the analysis a dummy for whether the old or new randomization was used, and reweight observations to obtain the intended cross-randomization weights so that each main treatment group has the same mix of each crossed additional treatment.

1. Time-Limited, Temporary Subsidies

Households were randomly selected to be in one of three groups: a control group, a full-subsidy group covering the premiums for all family members for one year, and a half-subsidy group covering half of a family's premiums for a year. Importantly, the subsidy offer was explicitly time-limited: it was only available for up to two weeks after the offer was made. Thus while the state does not have the capacity to enforce a limited annual window for enrollment, our time limited subsidy was designed to approximate the idea of there being a limited time period under which enrollment conditions are more favorable.[11] For participants whom we enrolled within two weeks of the offer, subsidies were administered for twelve months.

For logistical reasons, we could not pay half of each person's premium. Instead, we implemented the half subsidy through a "buy-one-get-one-free" scheme in which we paid the full premiums for half the family members for one year, and the household was then required to pay for the other half.[12] Households chose which family members were subsidized. In theory, the government regulated that all immediate household members be registered, so subsidizing half of the household members was roughly equivalent to providing a 50 percent discount. The subsidy receipt for the subsidized members was conditional on payment for the non-subsidized members for the first month, but unconditional thereafter in practice. Households in the full-subsidy group were not required to make any payments during the subsidy period.

2. Assisted Registration

Registering for *JKN Mandiri* usually requires traveling to the social security administration office in the district capital. To reduce the one-time hassle costs of registration, we randomly offered half of the study households the possibility of completing the registration process online at home with the assistance of the study enumerator. The enumerators had internet-enabled laptops that they used to access the official social security website. They then assisted the household with gathering the correct documentation, taking pictures, and filling in the forms on the website. Upon successful registration, the enumerators provided information to the household on payment procedures. If the

[11] In each city, we additionally randomized a separate sub-treatment within the subsidy design. The households in these sub-treatments are included in all our main analysis, but they are described in more detail and results presented in Appendix A.

[12] If a family had an odd number of members, we randomly assigned the household to receive a subsidy for $(y+1)/2$ or $(y-1)/2$ members with equal probability. If there was only one member, the member received a full subsidy.

household wanted to think more about their options, needed time to assemble the documentation, or had technical registration problems, the enumerators returned within a few days to try to assist with the enrollment process again.

3. Information

All study households received basic information, such as what the insurance covered, the premiums, and the procedure for registration. For randomly selected households in each city, we provided additional types of general, one-time information to test whether various forms of knowledge constrained enrollment.

In Medan, we randomly assigned a group of households to receive additional information on the financial costs of a health episode ("extra information treatment"). Using a script and an accompanying booklet, we detailed the average out-of-pocket expenditures for Indonesia's most common chronic health conditions, as well as the cost of having a heart attack.

In Bandung, all households received basic insurance information, as well as a discussion of the out-of-pocket expenditures associated with accessing care. However, based on discussions with the government, we then randomly assigned households to the following two treatments: 1) a "waiting period" treatment, in which we informed households about the new two-week waiting period between enrollment and the start of coverage, and 2) a "mandate information" treatment, in which we reminded households that enrollment is mandatory, and that there was a possibility that the government would soon introduce regulations requiring proof of insurance to be able to renew government documents, such as a passport or driver's license.

C. Data

We compiled two new datasets for this project: a baseline survey and government administrative data. Figure 2 shows the time period for our data relative to the experimental interventions, separately for each city.

We conducted a short baseline survey in conjunction with an independent and established survey firm (SurveyMeter). We administered the baseline survey immediately following the listing questionnaire to determine eligibility for the study (e.g. informal worker without health insurance). The baseline survey collected information on the demographic characteristics of family members, self-reported health and previous health care utilization, and existing knowledge of the program.[13] Self-reported health was measured on a four-point scale from 1 (unhealthy) to 4 (very healthy); we analyze average self-reported health across household members. The survey was identical in Bandung and Medan, with the one exception being that we added questions on income and employment in Bandung.

All of our outcomes are measured using detailed, high-frequency, high-quality government administrative data from February 2015 to August 2018. We track all of our participants for either 20 months since the date of offer or date of enrollment, depending on the analysis. We matched the study participants to the administrative data using individuals' unique national identification number (*Nomor Induk Kependudukan* or NIK).[14]

We define *enrollment* to be the household's successful completion of the registration process for the national insurance program. Since a household may enroll but not actually pay any premiums, we then define *coverage* in a given month to mean that the enrolled household's premiums were paid that month. We use the administrative data on registration date to measure enrollment. We use the administrative premium payment data, which report the date and value of each payment, to measure coverage in each month.

Since a household may also attempt to enroll but not succeed, we also measure *attempted enrollment.* By construction, this is measured differently depending on the treatment arm. For households in the assisted registration arm, we define them as attempting to enroll if the enumerator records that the household accepted the enumerator's offer of assistance with enrollment and that the enumerator began the internet-based process of helping them. For households assigned to the status quo registration procedures, if they are in the subsidy treatments,

[13] To minimize priming, the questions related to knowledge of the program were asked after the information on health status. The consent form only mentioned SurveyMeter and Indonesia's National Development Planning Agency (*Bappenas*), the other partner in the study, but not the social security administration or JKN.
[14] To ensure that we identify the correct individuals, we exclude matches when the year of birth reported in the baseline and that reported in the administrative database differ by more one year. When the same NIK links to two different membership numbers, we consider both observations as a match. When two different NIKs link to the same membership number, we exclude the observation. When enrollment date or membership type changes in subsequent extracts, we retain the information as reported in the first extract in which the individual appears. About 23 percent of the individuals surveyed did not have a NIK at baseline and cannot be matched to the administrative data. We show in Column 1 of Appendix Table 2 that the probability that a household reports the NIK of at least one of its members is not differential across treatment. Given that a NIK is a requirement of enrollment, those without a NIK are likely not enrolled in JKN, and we treat them as such.

we define them as attempting to enroll if they showed up to the social security office to enroll. Households in the subsidy treatments had to contact the study assistant at the social security office in order to redeem their voucher, enabling us to record their attempt. For households in the no-subsidy group, we set attempted enrollment equal to actual enrollment, a choice justified by the fact that the failure rate of enrollment attempts for households assigned to the status quo registration in the subsidy treatments was negligible (as social security officials within the office could manually fix family card issues within the system).

Finally, to measure insured health care utilization, we analyze data on all claims that are covered by the JKN insurance in both hospitals and clinics. The hospital claim data report start and end date, diagnosis, reimbursement value, and facility where the claim was made.[15] The clinic claims data report similar information to the hospital claims data, except that – due to capitation – claim values are not available. We also use diagnoses to code whether each claim is for a chronic versus emergent condition.[16]

D. Empirical Specification:

We estimate the following equation for a variety of outcome variables y_i:

$$y_i = \beta_0 + \beta_1 HALF\ SUBSIDY_i + \beta_2 FULL\ SUBSIDY_i + \beta_3 ASSISTANCE_i + INFO_i{}'\beta_4 + X_i{}'\delta + \varepsilon_i \qquad (1)$$

where $HALF\ SUBSIDY_i$, $FULL\ SUBSIDY_i$ and $ASSISTANCE_i$ are dummy variables equal to 1 if household i was randomly assigned to the respective treatment, and $INFO_i$ is a vector of dummies equal to 1 if household i was randomly assigned to a particular information intervention. X_i is a matrix of household-level controls that includes dummy variables for the assignment to the other sub-treatments (see footnote 11), a dummy for the randomization procedure (see footnote 10) and a dummy variable for city of residence. Regressions are weighted to reflect the desired cross-

[15] A claim corresponds to an outpatient or inpatient event. Each event is associated with a series of diagnoses. The hospital is reimbursed for the amount that corresponds to the primary diagnosis according to the INA-CBG tariff. All exams and treatment needed for an event get reimbursed under the same claim.

[16] We build our chronic condition classification from the Chronic Condition Indicator for the International Classification of Diseases from the Healthcare Cost and Utilization Project. This database provides information on whether diagnoses included in the ICD-10-CM: 2018 can be classified as chronic conditions. We link conditions in the ICD-10-CM: 2018 to conditions in the ICD-10: 2008 – the classification system followed by BPJS – using the first three digits of the diagnosis code. This is the lowest classification that straightforwardly corresponds across the two systems. We consider a diagnosis as chronic if it belongs to a three-digit code group with more than 75 percent chronic diagnoses.

treatment randomization design (see footnote 10). Given the household-level randomization, we report robust standard errors. Our exhibits often report results for sub-sets of treatments, but the full set of indicator variables is always included.

To assess balance across treatment arms, we estimate equation (1) using various household characteristics measured in the baseline survey. Appendix Table 2 shows the results. Only 6 out of the 54 coefficients are significantly different from zero at the 10 percent level, in line with what we would expect by chance.

IV. IMPACTS ON ENROLLMENT, COVERAGE, and ENROLLMENT ATTEMPTS

A. Enrollment

Table 1 presents the impact of the interventions on enrollment – i.e., successfully completing the registration process. Subsidies substantially increased the probability of enrollment during the 12 months after the offer date (while the subsidies were still active), while assisted registration had a positive but much smaller impact (Panel A column 1). Only about 8 percent of the no-subsidy, status-quo registration group enrolled within the 12-month period. Relative to this, offering the full subsidy increased enrollment by 18.6 percentage points. Offering the half subsidy increased enrollment by 10 percentage points. By contrast, the assisted registration treatment only increased enrollment by 3.5 percentage points.

Because the subsidy offer and the offer of registration assistance were time-limited, it is possible that they shifted forward in time an enrollment decision that would have occurred anyway (so-called "harvesting"). This dynamic response seems particularly plausible given that both the offer of registration assistance and the subsidy offers were time-limited. To examine this, we separately analyze the impact on enrollment within the first 8 weeks of the offer, and after 8 weeks but within the first year (the subsidy offers were only valid for up to two weeks, but we look before and after 8 weeks to allow for some margin of error in terms of data lags). The results indicate a small, but statistically significant harvesting effect (column 3), which accounts for slightly more than 10 percent of the total additional enrollment effect from the first eight weeks (column 2).

Panels B and C report the same analyses for the information treatments. We report the results separately by city because we tested different information treatments in different cities, providing detailed information on heart attack costs in Medan (Panel B) and about the nature of insurance (i.e., that enrollment is mandatory and that households must enroll at least two weeks in advance of a health claim) in Bandung (Panel C). We find no statistically significant effect of any of these information treatments. We can rule out effect sizes respectively bigger than 8.5 percentage points (information on heart attack costs), 2.5 percentage points (information on mandates), and 3.2 percentage points (information on waiting period).

B. Subsequent Coverage

After the initial decision to enroll examined in Table 1, households must decide whether to continue to pay their monthly premiums to remain covered at any given point in time. Figure 3 shows household coverage patterns over time for different subsidy groups, shown by month since offer (Panel A) and, for those who ever enroll, by month since enrollment (Panel B).[17] Coverage is defined as the premium having been paid in full for all its members that month. Payment may be made either independently by the household or by the study. Thus all households in the full-subsidy group who successfully enroll are covered for twelve months but not thereafter; households in the no-subsidy and half-subsidy group need to remit payments each month to remain covered.

In the no-subsidy group, coverage slowly increased over time from 0.61 percent in the first month of the experiment to 6.66 percent almost two years later (Panel A). However, among those who enrolled, many in the no-subsidy group quickly dropped coverage; one-quarter of enrolled households in the no-subsidy group had stopped paying their premiums three months post-enrollment, and nearly half had stopped paying their premiums a year post-enrollment (Panel B). The steady increase in coverage for the no-subsidy group in Panel A implies that the rate of new enrollment was large enough so that net coverage rates continued to increase despite the dropout effect.[18]

These patterns look very different for the full-subsidy group. About 25 percent of those offered the full subsidy enrolled in the first two months after the offer, statistically significantly

[17] Appendix Table 4 reports the magnitudes and statistical significance of the patterns observed, while Appendix Table 5 provides the underlying regression estimates for the *p*-values reported in the table.
[18] The steady increase in enrollment of the no-subsidy group throughout the study period is in line with the number of enrollees going from approximately 10 million in January 2015 to more than 15 million in January 2016.

higher than the no subsidy group (Panel A). Their coverage mechanically remained constant during the first year, when the subsidies were active,[19] and fell substantially once the subsidy ended; by month 20, coverage in the full subsidy-group was about 60 percent of the first year level, with most of this decline occurring in the first few months after the subsidy ended (Panel A).

Despite the large declines after the subsidy ended, coverage in the full subsidy group remained higher than the no-subsidy group. For example, the full subsidy group was 4.6 percentage points (86 percent; p-value < 0.001) more likely than the no-subsidy group to have coverage at month 15, and 3.9 percentage points (58 percent; p-value = 0.001) more likely than the no-subsidy group to have coverage at month 20 (see Appendix Table 4). This persistence in elevated coverage even after the subsidies expired cannot be explained by inertia or switching costs – which have been well-documented in other health insurance contexts (e.g. Handel 2013, Polyakova 2016); in our context, staying enrolled required an active choice to pay premiums each month at an office, ATM, or convenience store. There is a financial penalty for dropping and re-enrolling (see footnote 8 above), which may explain part of this persistence. The persistence is, however, consistent with health insurance as an "experience good", for which initial exposure increases household demand. For example, individuals may not have understood the benefits of insurance until they experienced it; indeed, we show in Section 5 below that retention is higher among those households who had a claim.

As one might expect, results for the half-subsidy group are somewhere between the no-subsidy and full-subsidy results. Their coverage rate in the first year was higher than the no-subsidy group, but far below the full-subsidy group. They also experienced a drop in coverage when the subsidy ended, and while their coverage level was roughly flat in the second year, the no-subsidy group slowly caught up to them. By the 20-month mark, their coverage rates appear similar.

Figure 4 shows coverage patterns over time separately for the assisted-registration group compared to the status quo group. Relative to the status-quo group, the assisted-registration group saw a slight increase in coverage initially, but coverage rates soon converged. After subsidies ended, there is some evidence of a larger coverage decline for those in the assisted-registration

[19] The slight increase in coverage shown in Figure 3 Panel A for the full-subsidy group during months 4-12 comes from the fact that a small number of households in this group enrolled after the subsidy period was over.

group, which may indicate that some of the households brought into the insurance system by reducing hassles were particularly sensitive to the hassles of paying each month.

C. Attempted Enrollment

The enrollment measures in Table 1 and the coverage measures in Figure 3 and 4 mask the fact that many more households, particularly those in the assisted registration treatment, attempted to enroll than were actually successful. Table 2 sheds light on this by examining the impact of the interventions on both *attempted* enrollment in the first eight weeks after the intervention and *actual* enrollment. Panel A shows that, averaged across all subsidy treatments, assisted registration led to a 23.7 percentage point increase in attempted enrollment during the first eight weeks (column 1), but only a 4.3 percentage point increase in successful enrollment during that period (column 2). In other words, less than one-fifth of the households induced by the registration assistance to attempt enrollment were actually successful in doing so.

Panel B explores the fully interacted effects of subsidies and registration assistance. It estimates an enhanced version of equation (1) that also includes a full set of interactions between the (cross-randomized) subsidy treatments and the assisted registration treatment. Even with a full subsidy and assisted registration, enrollment during the first eight weeks only reached 26 percent, compared to 2 percent in the no-intervention status quo (column 2). However, 58 percent of households *tried* to enroll when offered both free insurance for the year and assistance with registration (column 1). This points to important challenges to successful enrollment.

To explore the sources of these challenges in more depth, Table 3 reports the reasons for failing to enroll among households that attempted to enroll in the registration assistance intervention. These data are from the enumerators' recording of the reason for failed enrollment.[20] Over 80 percent of failed enrollment attempts were due to an issue with the Family Card, the official identification document. As mentioned in Section III, the Family Card was required for

[20] Specifically, enumerators were given a structured choice of 7 possible answers, or other. We grouped two possibilities (internet connection issue and BPJS registration website issue) into "technical issues", and four possibilities related to family card issues (supporting documents issue, family card is not registered in the online system, family already has insurance according to the online system, and family card does not match the family members listed in the online system). We do not have data on the reasons for enrollment failure in the status quo arms, but as can be seen in Table 2 Panel A, less than 1 percentage point of people who tried to enroll in the subsidy interventions with status quo registration failed to do so, compared to 20 percentage points in the registration assistance arms.

registration because households were required to enroll all nuclear family members (defined as those listed on the Family Card) as a way to combat adverse selection. The Family Card information was supposed to be pulled automatically from the digital records held by the Home Affairs Ministry, but this turned out to be problematic if the family composition had changed, but the card had not been updated. In practice, updating the card is challenging – it cannot be updated online, and instead requires at least one trip to a Home Affairs-linked administrative office, and can often incur delays and other additional costs. During in-person enrollment, social security administration officials could use discretion to overrule the system for certain causes (e.g., if households had documentation that the Home Affairs record was inaccurate), but the lack of flexibility in the online system made web enrollment nearly impossible for many.

V. Strategic Coverage Timing and the Impact of Time-Limited Subsidies

As discussed, the Indonesian government instituted certain features to try to combat adverse selection – such as requiring all nuclear family members to enroll and imposing financial penalties for those who re-enroll after dropping coverage – but was not allowed to impose a limited annual enrollment period. This opened the door for strategic timing of enrollment when a health emergency occurs. Since the prohibition on a limited annual enrollment period precluded our studying its potential impact directly, we approximated it in spirit by making our subsidy offer explicitly time-limited, only available for up to two weeks after the offer was made. Our results suggest that this time-limited subsidy was able to reduce strategic coverage timing. Indeed, because the time-limited subsidies brought in healthier enrollees with fewer claims, they allowed the government to cover almost double the number of households at no higher total cost, at least with the 20-month period we examined.

A. Impacts on Selection

Table 4 shows that the subsidies brought in healthier, lower cost enrollees, as standard models would predict (e.g. Akerlof 1970). The analysis is limited to households who enrolled and had coverage for at least one month during the first year (as measured in column 1 of Appendix Table 4), and shows the means for each group.

Column 1 shows that the marginal household who received coverage in response to the subsidies had a higher level of self-reported health at baseline than enrollees in the no-subsidy group. Self-reported health score is a Likert score ranging from 1-4, with 4 as the highest option. The average self-reported health of those enrolling with either the full or half subsidy is about 4-4.5 percent higher than those enrolling with no subsidy (these differences are statistically significant at the 5 percent level). The effects of assisted registration were smaller, but in the same direction of bringing in healthier enrollees, and statistically significant at the 10 percent level.

The remaining columns examine enrollee claims for the 12 months after the enrollment date. We focus on four main indicators: whether the household had any claim, the number of claims (overall and for chronic visits), the total value of claims, and the number of days to first claim. The latter is a way to proxy for the value of claims with greater precision (Aron-Dine et al. 2015).

Those who enrolled under the subsidies were lower-cost.[21] For example, in the no-subsidy group, 62 percent had any claim compared to 48 percent in the full-subsidy group (column 2; *p*-value = 0.040). Column 3 shows that those enrolled in the full-subsidy group were also less likely to have a claim for a chronic, ongoing condition (17 percentage points) than those enrolled in the no subsidy group (27 percentage points; *p*-value of difference = 0.082). Compared to the no-subsidy group, the full-subsidy group also had 40 percent lower average claims (column 6 of Table 4; *p*-value of difference = 0.095) and waited 32 percent longer before submitting their first claim (column 7; *p*-value of difference = 0.006).[22] Results for the half-subsidy group and the assisted registration group are mostly qualitatively similar to the full-subsidy group, but smaller in magnitude and never statistically significantly different from the no-subsidy group.

Figure 5 shows that these cost differences in part reflect strategic timing decisions by the no-subsidy group, rather than only fixed health differences across enrollees in different intervention groups. It plots the number of claims by month since enrollment, separately by subsidy treatment groups; as with Table 4, the analysis is limited to households who enrolled and had coverage for at least one month during the first year. Those who enrolled without the subsidy submitted more claims in the first few months upon enrollment than those who enrolled in the full-subsidy group, but over time this difference became less stark and, by the end of the period, the groups display similar patterns in number of claims. Enrollees in the half-subsidy group also

[21] While these two measures capture different objects – namely, health and healthcare usage – perhaps not surprisingly, enrollees with better self-reported health indeed tend to have fewer claims (see Appendix Table 6).
[22] Appendix Table 7 reports the underlying regression estimates for the *p*-values reported in Table 4.

submitted more claims than households in the full-subsidy group, and even submitted claims for a higher value than the no-subsidy group in a handful of months.

In addition to the full subsidy group having fewer claims than the no subsidy group, Figure 6 shows that, in the first twelve months since enrollment, those who enroll and have claims in the fully subsidy group are much less likely to have "large claims" (which are suggestive of a substantial health emergency) than those who enroll and have claims in the no subsidy group. Indeed, the probability distribution function of the value of inpatient claims submitted within twelve months since enrollment is markedly left-shifted for the full-subsidy group relative to the no-subsidy group. Again, the same is true – although less pronounced – in comparing the half-subsidy and no-subsidy groups. The differences across groups are statistically significant according to a Kolmogorov-Smirnov test for equality of distribution functions ($p = 0.012$ for the test of equality between the distribution of the half-subsidy and no-subsidy groups and $p = 0.001$ for the test of equality between the distribution of the full-subsidy and no-subsidy groups). In short, when they use the health care system, those whose coverage was heavily subsidized have less expensive health incidents.

Combined with the payment patterns from Figure 3 Panel B, these results suggest that no-subsidy households may have had large claims once they enrolled, but then stopped paying premiums (i.e., dropped coverage). In contrast, the subsidy groups brought in healthier people, who kept paying premiums longer in the first year while the subsidies were active (see Figure 3), and had smaller claims throughout the year (Figure 5).

We also explored how the subsidies affected selection in terms of who retained coverage in the period after all subsidies are over, which is important for understanding the long-run cost implications of temporary subsidies. For each treatment, we divide those who enrolled in the first year into "dropouts" – those who did not still have coverage in month 15 – and "stayers" – those who did. The results in Table 5 indicate that in the full-subsidy group, those who retained coverage had *higher* baseline self-reported health than those who did not (column 1; p-value = 0.072), but also were more likely to have had claims (column 2; p-value = 0.005) and to have had more visits (column 4; p-value = 0.002), particularly for chronic conditions (column 5 p-value = 0.14). The half-subsidy group showed a similar pattern of claims,[23] while the patterns are more ambiguous for the no-subsidy group. The fact that stayers are more likely to have had claims is consistent with an "experience effect," whereby having had experience utilizing the insurance can increase future demand. These patterns also highlight the potential – emphasized theoretically and empirically by Diamond et al. (2019) – for strategic drop-outs to generate ex-post adverse selection, even among consumers who may be ex-ante identical in their expected health care utilization. However, on net, the overall selection effect from the full subsidy compared to the no-subsidy still dominates its differential retention effect, so that on net, the stayers in the full subsidy group are healthier than the stayers from the no subsidy group, both in terms of self-reported baseline health and in terms of claims during the first year.

B. Implications for the government's budget

Table 6 shows the effects of the subsidies on the net revenue for the government. Government net revenues is the difference between premiums received (net of subsidies) and claim expenditures. It reflects the amount that would need to be covered from the general government budget. The outcomes we have previously studied – enrollment, coverage retention, and claims patterns – all affect government net revenue. We show the total cost to the government per capita (i.e. per household in the population).

Panel A shows the results during the time period the subsidy was in effect. As we have seen, full-subsidy households have higher coverage during the subsidy period. In fact, column 1 shows that the mean number of household-months within insurance coverage in the full-subsidy group is almost *nine times* that of the no-subsidy group during the subsidy period. For the full subsidy group, revenue (i.e. premiums received net of subsidies) per-household month is of course lower than for the no-subsidy group (column 2),[24] but claims per household month are not statistically different, even despite the larger number of household-months covered (column 3). The last two columns report net revenue per household-month with (column 4) and without

[23]Appendix Table 8 presents the equivalent results broken down by the assisted registration treatment, and finds a similar pattern: stayers were more likely to have had claims.

[24] Revenues should be mechanically zero for the full-subsidy group while the subsidy is in effect, but are not literally zero since a few households in this group enrolled after the time period the subsidy offer was in effect and therefore had to pay premiums.

accounting for capitation payments (column 5).[25] Accounting for capitation payments, net revenue per household-month are about negative Rp. 3,000 in the no subsidy group, compared to about negative Rp. 12,000 per household in the full subsidy group (although these differences are not statistically indistinguishable; p-value of 0.25). This implies that while the subsidies are in effect, the government covers nine times more household-months at four times cost.

The financial implications for the government improve once the subsidies are withdrawn (Panel B). We observe about twice as many household-months covered in the full subsidy group as compared to no subsidy (column 1; p-value < 0.001), but now they are also paying premiums. Putting this together, net revenues to the government are almost identical between the full and no subsidy group (column 5) – it costs the government about Rp. 5,000 per household in both the no-subsidy and the full subsidy arm. In short, the full subsidy was able to substantially expand coverage in both the subsidy year and the year after, at no higher cost to the government.

V. CONCLUSION

As incomes have risen in emerging economies, there has been a growing move to increase coverage of health insurance programs through mandated, national enrollment. However, many countries are running into key challenges in expanding coverage, particularly among informal workers, who often comprise a large share of their populations.

In this paper, we show that there are both monetary and non-monetary barriers to enrollment. Time-limited, temporary subsidies increased enrollment. In fact, households that received the subsidies were even more likely to stay enrolled after the subsidies expired, consistent with an experience effect. Reducing hassles by providing registration assistance to households also increases enrollment.

These enrollment gains, however, were muted by a weak administrative state capacity. In particular, the hassles of status quo (i.e. without assistance) enrollment at a government office

[25] Capitation payments depend on the number of enrollees that declare the facility as their primary provider, the total number of practitioners, the ratio of practitioners to beneficiaries, and operating hours, and range between Rp. 3,000-6,000 per enrollee for *puskesmas* and Rp. 8,000-10,000 for clinics. Given that approximately 80 percent of *JKN Mandiri* enrollees declare *puskesmas* and 20 percent declare clinics as their primary health facility, for these calculations we assume capitation payments to be Rp. 6,800 per enrollee per month. Capitation payments are only paid to healthcare facilities in months in which the household paid the premium.

appear to be a substantial barrier: nearly as many people attempted to enroll in the assisted internet-based registration with no subsidy as did in the full-subsidy group with status quo office-based registration. Even more importantly, many more people *attempted* to enroll when assistance was provided than were actually able to do so. For example, when offered both a full subsidy and assisted internet registration, nearly 60 percent of households tried to enroll, but less than half of these households were successful. The primary reasons were challenges with the government's online enrollment system arising from inaccuracies in the state's underlying civil registries of families, a problem that is unfortunately common throughout the developing world.

The state's inability to limit enrollment to a fixed period of time also contributed to low enrollment by encouraging households to strategically time their coverage. Households who enrolled in the control group tended to have a large insurance claim in the first three months and also had high dropout rates, relative to those who enrolled when we offered time-limited subsidies. The state's limited capacity here is directly due to current law, which requires that individuals be allowed to enroll in social insurance programs throughout the year. However, even absent such legal constraints, the state might well face challenges in trying to offer a limited enrollment period. Challenges could come from the demand side, since households in developing countries often have lumpy incomes that may not correspond to prescribed enrollment periods. There could also be substantial administrative challenges in implementing these limited enrollment periods. For example, when there is a limited time period for enrollment in other settings, such as the annual enrollment period for the health insurance exchanges under the Affordable Care Act in the United States, exceptions are always allowed for certain "qualifying events" – such as a change in marital status, the birth of a child, or a relevant employment change; such life-events may be harder for the state to verify in developing countries, or would impose an additional administrative burden on citizens since getting these types of documents can be challenging. Of course, even when states can limit enrollment to once a year, individuals may still engage in some strategic coverage dropping after time-limited health care needs are met (Diamond et al. 2019).

The results suggest several key dimensions through which governments in emerging economies can improve health insurance coverage. First, time-limited subsidies can be used to entice the healthy to try the insurance; our results indicate that some of those who try it will remain. Second, governments can make complementary investments in reducing the hassles associated with registration *and* in the underlying state registry systems needed to make them work efficiently. Neither of these steps is likely sufficient for achieving universal coverage; our most intensive efforts only yielded 60 percent attempted enrollment, and other barriers certainly exist including the possibility of optimization frictions if households, for example, misperceive the value of insurance or the quality of health care. But collectively, such steps may allow governments to cover substantially more households and at lower costs per covered household.

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Figure 1: Experimental Design

Panel A: Medan

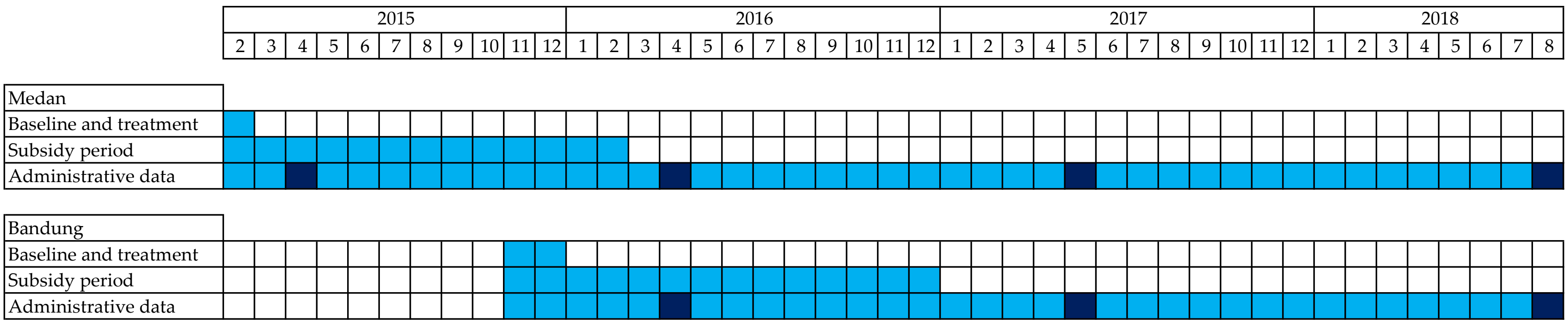
	Status quo		Assisted registration		Subsidy treatment totals
	Standard information	Extra information	Standard information	Extra information	
No subsidy	37	63	134	237	471
Half subsidy	171	215	26	66	478
Full subsidy	176	54	170	97	497
Registration treatment totals	716		730		1446

Panel B: Bandung

		Status quo		Assisted registration		Subsidy treatment totals
		Standard information	Mandate information	Standard information	Mandate information	
No subsidy	Standard information	236	307	241	274	2236
	Waiting period information	232	300	297	349	
Half subsidy	Standard information	160	153	77	82	918
	Waiting period information	141	114	100	91	
Full subsidy	Standard information	85	40	62	54	478
	Waiting period information	63	51	70	53	
Bonus subsidy	Standard information	114	86	170	111	918
	Waiting period information	101	86	131	119	
Registration treatment totals		2269		2281		4550

Note: This figure shows the randomization into each treatment arm, by city. Each cell reports the number of households allocated to the specific treatment cell. For more information on the bonus subsidy treatment, see Appendix A.

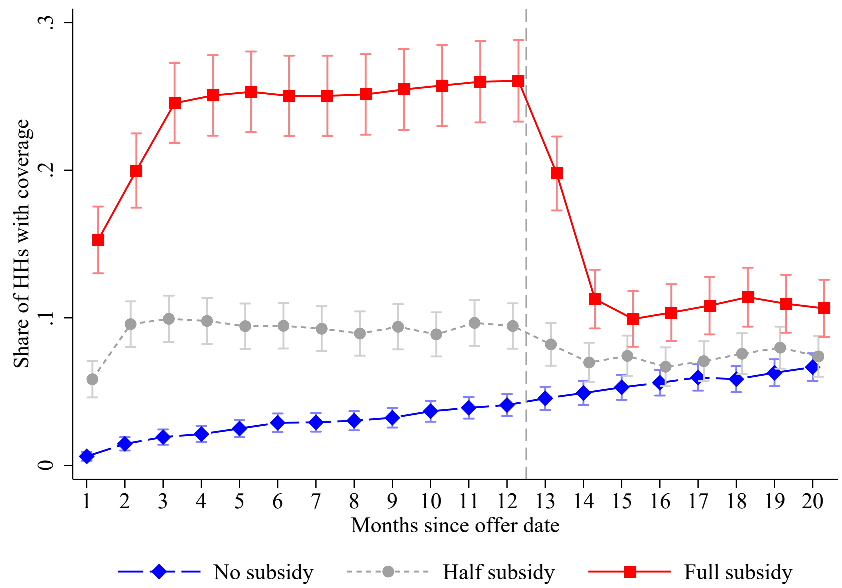
Figure 2: Timeline



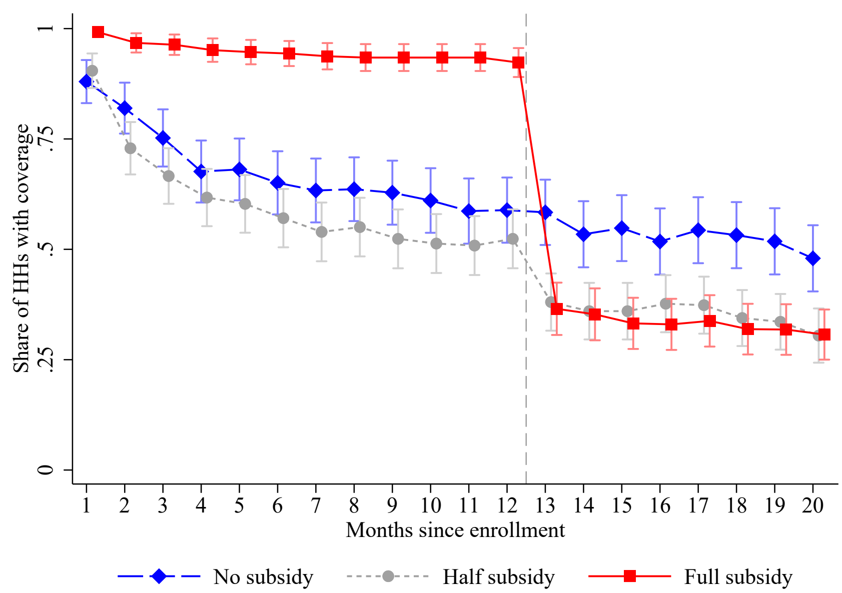
Note: This figure provides a timeline by city. The baseline and treatment offer occurred in the same household visit, in February 2015 in Medan and November/December 2015 in Bandung. Subsidies were disbursed over the course of the following twelve months after offer. We accessed the administrative data in April 2015, April 2016, May 2017, and August 2018.

Figure 3: Insurance Coverage by Subsidy Treatment

Panel A: By month since offer



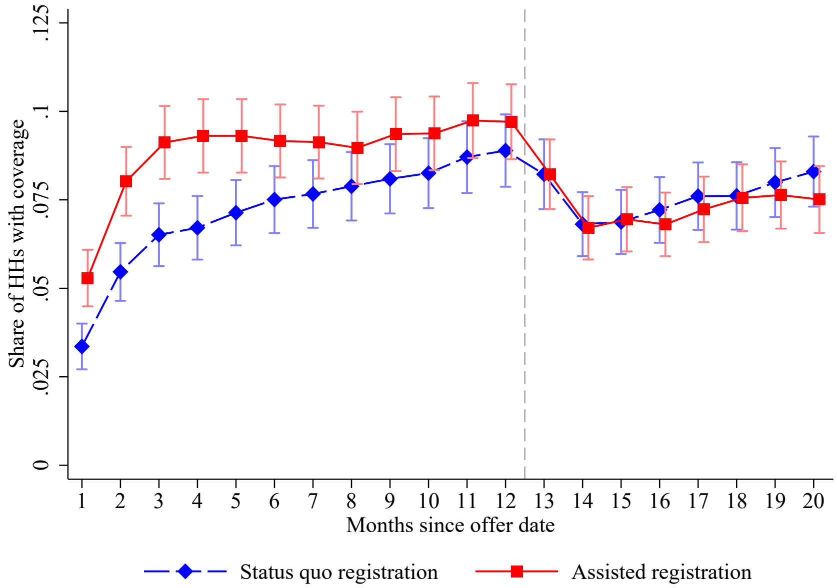
Panel B: By month since enrollment



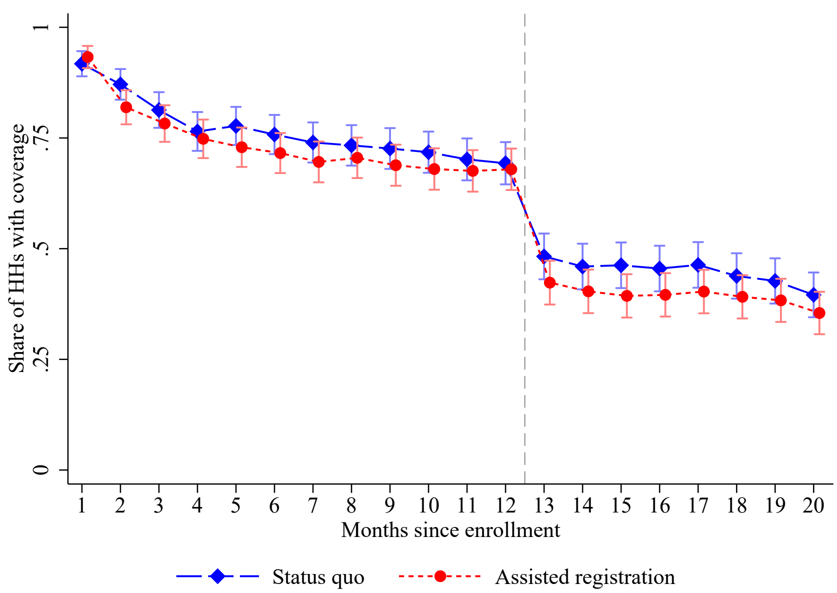
Note: This figure shows mean insurance coverage for households assigned to different subsidy treatments, by month since the offer (panel A) and by month since the enrollment (panel B), with 95% confidence intervals for the mean. Means are weighted to reflect the intended randomization. Coverage for a household is defined as the premium having been paid in full for all its members that month. The sample size is in Panel A is 5996 households. In panel B, the sample is restricted to households who enrolled within a year since offer date and had coverage for at least one month over the same time period. The sample size is 749 households.

Figure 4: Insurance Coverage by Assisted Registration

Panel A: By month since offer

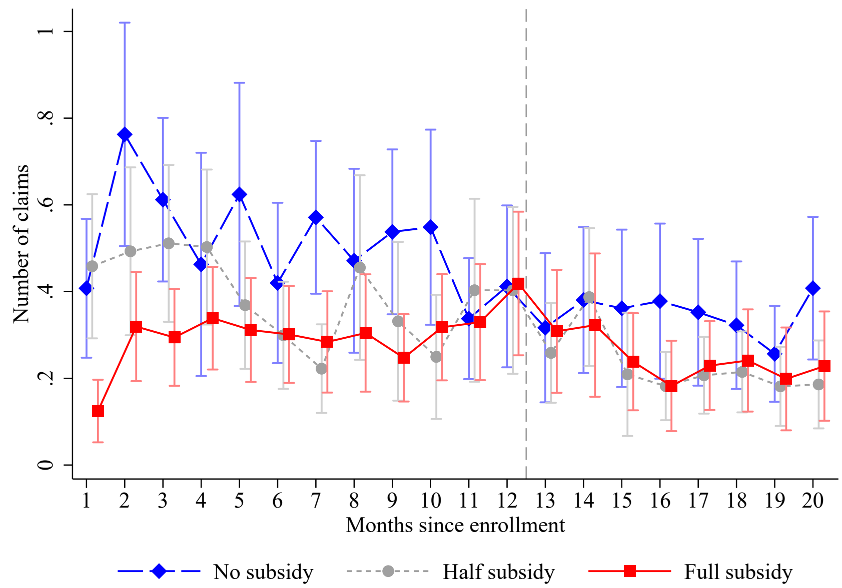


Panel B: By month since enrollment



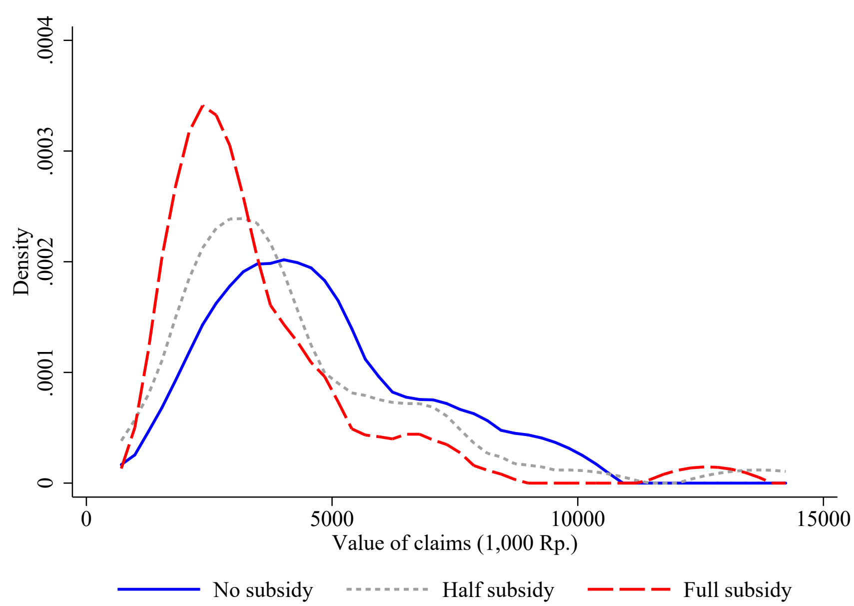
Note: This figure shows mean insurance coverage for households assigned to different registration treatments by month since the offer (panel A) and by month since the enrollment (panel B), with 95% confidence intervals for the mean. Means are weighted to reflect the intended randomization. Coverage for a household is defined as the premium having been paid in full for all its members that month. The sample size is in Panel A is 5996 households. In panel B, the sample is restricted to households who enrolled within a year since offer date and had coverage for at least one month over the same time period. The sample size is 749 households.

Figure 5: Number of Claims, by Month since Enrollment and by Subsidy Treatment



Note: The figure shows the mean number of claims in each month since enrollment with 95% confidence intervals for the mean. Means are weighted to reflect the intended randomization. The sample is restricted to households who enrolled within one year since offer and had coverage for at least one month over the same time period. The sample size is 749 households. Appendix Table 10 presents the regression form analog.

Figure 6: Distribution of Inpatient Claims in Year 1 since Enrollment, by Subsidy Treatment



Note: This figure shows the probability distribution function of the value of inpatient claims submitted within the first twelve months since enrollment by subsidy treatment. The unit of observation is a single claim. The sample is restricted to 749 households who enrolled within one year since offer and had coverage for at least one month over the same time period, which the same sample we use in Figure 5 and Table 4. The sample size is 3827 inpatient claims.

Table 1: Effect of Temporary Subsidies and Assisted Registration on Year 1 Enrollment

		Decomposition	
	Enrolled within 1 year	Enrolled within 8 weeks of offer date	Enrolled after 8 weeks, but within 1 year of offer date
	(1)	(2)	(3)
Panel A: Subsidy and assisted registration treatments			
Full subsidy	0.186***	0.209***	-0.023**
	(0.020)	(0.018)	(0.010)
Half subsidy	0.100***	0.114***	-0.014*
	(0.014)	(0.013)	(0.008)
Assisted registration	0.035***	0.043***	-0.008
	(0.011)	(0.009)	(0.006)
Observations	5996	5996	5996
No subsidy, status quo registration mean	0.078	0.018	0.060
P-value of test of hypothesis			
Half subsidy = full subsidy	0.000	0.000	0.441
Assisted registration = full subsidy	0.000	0.000	0.265
Panel B: Information treatments, Medan			
Information on cost of treatment for heart attack	0.029	0.034	-0.005
	(0.029)	(0.025)	(0.016)
Observations	1446	1446	1446
No information mean	0.190	0.131	0.059
Panel C: Information treatments, Bandung			
Information on possible mandate penalties	0.004	-0.001	0.004
	(0.011)	(0.009)	(0.007)
Information on two weeks waiting period	0.011	0.009	0.002
	(0.011)	(0.009)	(0.007)
Observations	4550	4550	4550
No information mean	0.123	0.078	0.045

Note: This table shows the effect of subsidies, assisted registration, and the information treatments on enrollment in year 1 since offer. We regress each outcome on indicator variables for assignment to all treatment arms, an indicator variable for the randomization procedure used and an indicator variable for the study location (equation (1)). The omitted category is no subsidy for the subsidy treatments, status quo registration for the assisted registration treatment, and no information for all information treatments. The p-values reported in panel A are from a test of the difference between the half subsidy and full subsidy treatments ($\beta_1 = \beta_2$) and assisted registration and full subsidy treatments ($\beta_1 = \beta_3$). All regressions are estimated by OLS and weighted to reflect the intended cross-randomization. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Effect of Temporary Subsidies and Assisted Registration on Attempted Enrollment

	Attempted to enroll within 8 weeks of offer date	Enrolled within 8 weeks of offer date
	(1)	(2)
Panel A: Main effect		
Assisted registration	0.237***	0.043***
	(0.011)	(0.009)
Status quo registration mean	0.079	0.077
Panel B: Interacted specification		
Full subsidy and assisted registration	0.558***	0.239***
	(0.026)	(0.024)
Full subsidy and status quo registration	0.191***	0.200***
	(0.025)	(0.024)
Half subsidy and assisted registration	0.418***	0.162***
	(0.029)	(0.023)
Half subsidy and status quo registration	0.099***	0.087***
	(0.015)	(0.013)
No subsidy and assisted registration	0.179***	0.023***
	(0.012)	(0.007)
No subsidy, status quo registration mean	0.018	0.018

Note: This table shows the effect of subsidies and assisted registration on attempted and actual enrollment within eight weeks since offer date. The sample size if 5996 households. In panel A, we regress each outcome on indicator variables for assignment to all treatment arms, an indicator variable for the randomization procedure used and an indicator variable for the study location (equation (1)). The omitted category is status quo registration. Panel B reports estimates from a variant of equation (1) that includes the fully interacted effects of subsidies and registration assistance. The omitted category is no subsidy and status quo registration treatment. All regressions are estimated by OLS and weighted to reflect the intended randomization. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Reasons for Failing to Enroll

	N	%
	(1)	(2)
Panel A: All issues		
Family card issues	468	83.6
Other issues	71	12.7
Technical reasons (internet, website)	21	3.8
Panel B: Breakdown of family card issues		
Supporting document issue	286	61.1
Family card does not match the family members listed in the online system	91	19.4
Family already has insurance according to the online system	80	17.1
Family card not registered in the online system	11	2.4

Note: The sample includes households assigned to assisted registration treatment that attempted to enroll within six weeks from offer date but failed to complete the registration process. Data is from enumerators' recording at the end of the baseline survey.

Table 4: Self-Reported Health and Claims in 12 Months since Enrollment, by Temporary Subsidies and Assisted Registration

	Self-reported health	Had a claim		Total # of claims		Claims	
		Of any type	Chronic	Of any type	Chronic	Value of claims	Days to first claim
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Full subsidy	3.237	0.480	0.171	3.589	0.184	0.986	232.425
	[0.452]	[0.501]	[0.377]	[6.805]	[0.429]	[2.620]	[141.273]
Half subsidy	3.244	0.511	0.231	4.698	0.292	1.879	213.850
	[0.541]	[0.501]	[0.422]	[10.326]	[0.624]	[4.712]	[150.106]
No subsidy	3.099	0.622	0.272	6.167	0.339	1.637	176.272
	[0.538]	[0.486]	[0.446]	[9.712]	[0.600]	[4.064]	[154.913]
Assisted registration	3.217	0.525	0.226	4.254	0.259	1.366	214.823
	[0.524]	[0.500]	[0.419]	[7.419]	[0.512]	[3.676]	[147.576]
Status quo registration	3.149	0.555	0.214	5.176	0.267	1.519	200.836
	[0.505]	[0.498]	[0.411]	[10.136]	[0.583]	[3.853]	[150.794]
	P-value of test of hypothesis						
Full subsidy = no subsidy	0.016	0.040	0.082	0.025	0.036	0.095	0.006
Half subsidy = no subsidy	0.014	0.155	0.639	0.362	0.676	0.625	0.107
Full subsidy = half subsidy	0.888	0.540	0.138	0.164	0.053	0.046	0.278
Assisted registration = status quo	0.083	0.451	0.786	0.117	0.815	0.576	0.269

Note: This table shows mean self-reported health and mean claims submitted in months 1 to 12 since one's enrollment date by temporary subsidies and assisted registration. Means are weighted to reflect the intended randomization. Standard deviations are in brackets. The sample is restricted to households who enrolled within a year since offer and had coverage for at least one month over the same time period. The sample size is 749 households. In Column (1), In Column (1), the outcome is the average self-reported health of all household members, where the self-reported health score is a Likert score ranging from 1-4, with 4 as the highest option (better self-reported health). The value of claims in Column (6) is winsorized at the 99% level and only refers to hospital claims. The value of claims in Column (6) is in thousands Rp. We regress each outcome on indicator variables for assignment to all treatment arms, an indicator variable for the randomization procedure used and an indicator variable for study location (equation (1)). All regressions are estimated by OLS and weighted to reflect the intended randomization. The p-values reported are from a test of the difference between the no subsidy and full subsidy treatments ($\beta_2 = 0$), between the no subsidy and half subsidy treatments ($\beta_3 = 0$), between the half subsidy and the full subsidy treatment ($\beta_2 = \beta_3$), and between the status quo and assisted registration treatments ($\beta_4 = 0$). All regressions are estimated by OLS and weighted to reflect the intended randomization. Appendix Table 7 provides the regression estimates behind the numbers reported in this table.

Table 5: Year 1 Claims by Retention in Year 2, by Temporary Subsidies

	Self-reported health	Had a claim		Total # of claims		Claims	
		Of any type	Chronic	Of any type	Chronic	Value of claims	Days to first claim
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full subsidy							
Dropouts	3.198	0.414	0.154	2.552	0.154	0.755	251.238
	[0.441]	[0.494]	[0.362]	[4.905]	[0.362]	[2.333]	[134.730]
Stayers	3.313	0.610	0.205	5.653	0.246	1.446	194.983
	[0.467]	[0.490]	[0.406]	[9.213]	[0.535]	[3.077]	[147.208]
P-value of test of hypothesis							
Dropouts = stayers	0.072	0.005	0.340	0.002	0.140	0.073	0.005
Panel B: Half subsidy							
Dropouts	3.248	0.413	0.155	2.773	0.192	1.600	240.240
	[0.548]	[0.494]	[0.364]	[5.618]	[0.481]	[4.225]	[147.498]
Stayers	3.238	0.674	0.357	7.922	0.459	2.345	169.670
	[0.531]	[0.471]	[0.482]	[14.737]	[0.784]	[5.423]	[144.754]
P-value of test of hypothesis							
Dropouts = stayers	0.916	0.004	0.006	0.001	0.012	0.370	0.009
Panel C: No subsidy							
Dropouts	3.169	0.530	0.234	3.632	0.327	1.840	197.490
	[0.507]	[0.503]	[0.426]	[5.470]	[0.643]	[4.231]	[157.035]
Stayers	3.055	0.680	0.296	7.788	0.346	1.507	162.699
	[0.555]	[0.468]	[0.458]	[11.376]	[0.574]	[3.968]	[152.741]
P-value of test of hypothesis							
Dropouts = stayers	0.192	0.075	0.450	0.004	0.877	0.612	0.189
Panel D: Stayers across subsidy groups							
P-value of test of hypothesis							
Full subsidy = no subsidy	0.001	0.346	0.159	0.139	0.207	0.905	0.162
Half subsidy = no subsidy	0.026	0.937	0.418	0.941	0.269	0.239	0.778

Note: This table shows mean self-reported health and claims in the first year since enrollment, separately by temporary subsidies and by whether households kept or dropped coverage at month 15 since offer date. Means are weighted to reflect the intended randomization. Standard deviations are in brackets. The sample is restricted to households who enrolled within a year since offer and paid for at least one month over the same time period. The sample size is 749 households. In Column (1), In Column (1), the outcome is the average self-reported health of all household members, where the self-reported health score is a Likert score ranging from 1-4, with 4 as the highest option (better self-reported health). The value of claims in Column (6) is winsorized at the 99% level and only refers to hospital claims. The value of claims in Column (6) is in thousands Rp. The p-values in panels A, B, and C are from a specification where the outcome is regressed on an indicator variable for whether the household has coverage in month 15 and the sample is restricted to households assigned to the subsidy treatment specified. The p-values in panel D are from a specification where the outcome is regressed on indicator variables for subsidy treatment assignment and the sample is restricted to households with coverage in month 15. All regressions are estimated by OLS and weighted to reflect the intended randomization. Standard errors are robust. The coverage rates of these two groups are shown in Appendix Table 4.

Table 6: Expenditures and Revenues, by Temporary Subsidies

	Mean number of household coverage months	Per household-month (i.e. total cost to the government in 1,000 Rp)			
		Revenues	Claims expenditures	Net revenues	Net revenues including capitation
	(1)	(2)	(3)	(4)	(5)
Panel A: Months 1 to 12 since offer date					
Full subsidy	2.887	1.056	7.459	-6.403	-11.938
	[4.899]	[10.123]	[157.686]	[157.884]	[159.105]
Half subsidy	1.096	4.210	17.687	-13.477	-15.185
	[3.013]	[18.013]	[477.729]	[475.367]	[475.804]
No subsidy	0.323	2.214	4.927	-2.712	-3.167
	[1.535]	[14.270]	[214.599]	[212.905]	[213.125]
Observations	5996	71952	71952	71952	71952
P-value of test of hypothesis					
Full subsidy = no subsidy	0.000	0.003	0.906	0.890	0.254
Half subsidy = no subsidy	0.000	0.000	0.033	0.073	0.042
Half subsidy = full subsidy	0.000	0.000	0.114	0.252	0.543
Panel B: Months 13 to 20 since offer date					
Full subsidy	0.951	8.548	11.104	-2.556	-5.057
	[2.317]	[29.350]	[239.891]	[238.591]	[238.819]
Half subsidy	0.593	6.280	13.823	-7.543	-8.935
	[1.911]	[25.460]	[321.711]	[319.543]	[320.074]
No subsidy	0.451	3.966	8.594	-4.628	-5.565
	[1.680]	[19.484]	[341.563]	[340.097]	[340.430]
Observations	5996	47968	47968	47968	47968
P-value of test of hypothesis					
Full subsidy = no subsidy	0.000	0.000	0.141	0.657	0.411
Half subsidy = no subsidy	0.034	0.003	0.181	0.332	0.292
Half subsidy = full subsidy	0.000	0.042	0.828	0.504	0.653

Note: This table shows mean revenues and expenditures by temporary subsidies, for months 1 to 12 from offer (Panel A) and for months 13 to 20 from offer (Panel B). Means are weighted to reflect the intended randomization. Standard deviations are in brackets. Column (1) reports mean number of months with insurance coverage. Observations are at the household level. Columns (2) to (5) show mean revenues (premiums paid by enrollees), expenditures (total value of claims), net revenues, and net revenues including capitation payments, in thousands Rp. for all household-months. Observations are at the household-month level. The value of claims in Column (3) is winsorized at the 99% level and only refers to hospital claims. The p-values are from regressions of each outcome on indicator variables for assignment to all treatment arms, an indicator variable for the randomization procedure used and an indicator variable for study location (equation (1)). In Column (1) standard errors are robust, while in Columns (2) to (5) standard errors are clustered at the household level. The p-values reported are from a test of the difference between the no subsidy and full subsidy treatments ($\beta_2 = 0$), between the no subsidy and half subsidy treatments ($\beta_3 = 0$) and between the half subsidy and full subsidy treatments ($\beta_1 = \beta_2$). All regressions are estimated by OLS and weighted to reflect the intended randomization. Appendix Table 10 provides the regression estimates behind the numbers reported in this table.