Subsidies and the Dynamics of Selection: Experimental Evidence from Indonesia's National Health Insurance[*]

Abhijit Banerjee, MIT
Amy Finkelstein, MIT
Rema Hanna, Harvard University
Benjamin Olken, MIT
Arianna Ornaghi, University of Warwick
Sudarno Sumarto, TNP2K and SMERU

April 2020

Abstract

How can developing countries increase health insurance? We experimentally assessed three approaches that simple theory suggests could increase coverage and potentially reduce adverse selection: temporary price subsidies, registration assistance, and information. Temporary subsidies attracted lower-cost enrollees, in part by reducing strategic coverage timing. While subsidies were active, coverage increased more than eightfold, at no higher unit cost to the government; after subsidies ended, coverage remained twice as high, again at no higher cost. However, subsidies are not sufficient to achieve universal coverage: the most intensive intervention – a full one-year subsidy combined with registration assistance – resulted in only 30 percent enrollment.

[*] We thank our partners at BPJS Kesehatan, Bappenas, TNP2K, and KSP for their support and assistance. In particular, we wish to thank from BPJS Kesehatan Fachmi Idris, Mundiharno, Tono Rustiano, Dwi Martiningsih, Andi Afdal, Citra Jaya, Togar Siallagan, Tati Haryati Denawati, Atmiroseva, Muh. Syahrul, Golda Kurniawati, Jaffarus Sodiq, Norrista Ulil, and the many staff at regional BPJS offices who provided assistance; Maliki and Vivi Yulaswati from Bappenas, Jurist Tan from KSP, and Bambang Widianto and Prastuti (Becky) Soewondo from TNP2K. We thank the outstanding JPAL SEA team members for their work on this study, in particular Ignasius Hasim, Masyhur Hilmy, Amri Ilmma, Ivan Mahardika, Lina Marliani, Patrya Pratama, Hector Salazar Salame, Reksa Samudra, Nurul Wakhidah, and Poppy Widyasari. Yuanita Christayanie provided excellent research assistance. We thank SurveyMeter for outstanding data collection and fieldwork, especially Bondan Sikoki and Nasirudin. Funding from the Australian Department of Foreign Affairs and Trade and KOICA is gratefully acknowledged. The views expressed here are those of the authors, and do not necessarily reflect those of the many individuals or organizations acknowledged here.

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 the past decade (Lagomarsino et al. 2012). In expanding health insurance, however, emerging countries may face particularly vexing versions of the challenges faced by many developed countries, because of the large informal sector operating outside the tax net (Jensen 2019). Some countries, such as Thailand, have sought a single-payer-like system funded entirely out of tax revenues and supplemented by small co-pays; this has been shown to improve health, but faces funding challenges (see 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, however, is that enforcing the insurance mandate for those who directly pay premiums 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 2018) – 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 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 kids 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, the contributory portion of

the program, known as *JKN Mandiri*, had enrolled only 20 percent of the targeted population a year after its introduction, and, because enrollees were much less healthy than the typical targeted population, claims exceeded premiums by a ratio of 6.45 to 1.[1]

In the presence of such adverse selection, it is possible that temporary monetary subsidies, or other one-time reductions in non-monetary barriers to enrollment, can attract healthier individuals, allowing the government to provide insurance to a larger number of people at lower government cost per enrollee – and potentially even similar total government cost. Temporary subsidies may be particularly effective if individuals are otherwise prone to time their coverage in anticipation of health expenditures; the presence of a limited-time subsidy may induce healthy individuals to enroll today, rather than wait until they are sick. The ultimate longer-run impact of such a policy, however, depends not just on what happens when the subsidy is active, but also on which types of individuals choose to retain coverage once the subsidy period is over.

We therefore experimentally test whether providing one-time monetary and non-monetary inducements to enroll could increase enrollment and reduce adverse selection in national contributory insurance programs, when the mandate is imperfectly enforced. We do so in the context of a large-scale, multi-arm experiment – involving almost 6,000 households – designed in conjunction with the Indonesian government and implemented in 2015, a year after the mandatory universal national health insurance program was launched. Our main treatments examine the role of large, temporary monetary subsidies: we randomized households to receive subsidies of either 50 percent ("half subsidy") or 100 percent ("full subsidy") for their first year of enrollment. To be eligible for the subsidy, households had to enroll within two weeks of the subsidy offer, akin to governments offering a large, time-limited registration incentive. In addition, we tested the impact of reducing the cost of non-monetary barriers to enrollment in two ways. First, we tried to reduce the transaction costs of enrolling by randomly offering some households a one-time opportunity for at-home assistance with the online registration system; this would allow them to register without traveling to a far-off insurance office to enroll. Second, we tried to reduce potential information barriers about the insurance program by randomly providing households with different types of basic insurance information; specifically, we provided information about the financial

[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).

costs of a health episode and how they relate to insurance prices, the two-week waiting period from enrollment to coverage (so that one could not wait to get sick to sign up), and the fact that insurance coverage is legally mandatory.

To assess the impacts of these interventions, we focus primarily on administrative data from the government. These data contain detailed information on program enrollees for up to 32 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 first use these data to examine the impact of the interventions on *enrollment*, which we define as completing the initial registration process. 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 interventions on the time-path of *insurance coverage*, which we define as having paid the premium for a given month and thus having insurance coverage in that month. Finally, we examine questions relating both to adverse selection and to the ultimate government costs under the various policy treatments by examining the types of households that enroll and that retain or drop coverage after the subsidy ends, as well as costs per household insured and total costs to the government, inclusive of the monetary subsidies. We supplement these data with a short baseline assessment survey in which we collected data on demographics and self-reported health, which allows us to measure pre-intervention "health status" for all study participants, regardless of whether they subsequently enrolled in the insurance program.

We find that temporary monetary subsidies can be effective at increasing enrollment and reducing adverse selection, and that these effects persist even after the subsidies expire. We focus our discussion on the impact of the full subsidy; results for the half-subsidy treatment are generally somewhere in between the full- and no-subsidy treatments, but often we lack precision to test whether they are proportionally different. Those offered the one-year full subsidy were 20.9 percentage points more likely to enroll than were those in the no-subsidy group during the active subsidy period, leading to an *eight-fold* increase in the total number of household-months covered by insurance during the first year. Even in the year after the subsidy ended, insurance coverage in the full-subsidy group still 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).

The full subsidy brought in substantially lower-cost enrollees because it induced healthier individuals to enroll, rather than wait until they were sick to sign up. Relative to enrollees in the no-subsidy group, those 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. These cost differences in part reflect strategic timing decisions by the no-subsidy group, rather than 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. Such strategic "wait till you need it" enrollment timing was not an option for full-subsidy enrollees because the subsidy offer was time-limited.

As a result of these differences in selection, the net cost to the government per covered household – i.e., the difference between revenues from premiums and payments to providers, and hence the amount that would need to be covered from the general government budget – was similar in the no-subsidy control group and the temporary full-subsidy group. Remarkably, this was true *even* in the first year, when the subsidy was active and hence when the full-subsidy group brought in no revenue, because the enrollees induced to enroll were so much healthier – and had so many fewer claims – than in the no-subsidy group. It was also true in the year after the subsidies ended; while comparatively healthier individuals were more likely to drop coverage once the subsidy period ended, the population from the full-subsidy group that remained covered was healthier than the comparable population from the no-subsidy group. The estimates therefore suggest that the temporary full subsidy was able to substantially expand coverage in both the subsidy year and the year after, at no higher unit cost to the government. In fact, the estimates suggest that the total cost to the government in the year after the subsidy was no higher in the full-subsidy group, despite substantially higher coverage rates.

In terms of the non-monetary inducements, we found that the information treatments had no observable impact on enrollment, while assistance with registration boosted enrollment, but did so with no detectable impact on selection. Offering assistance via the option of internet-based registration at home increased enrollment by 3.5 percentage points (41 percent); there was no

evidence, however, that assisted registration selected in healthier members. Interestingly, many more people attempted to enroll than were actually successful; households' efforts were substantially muted by technical and administrative challenges with the government's online enrollment system. While reminiscent of the issues with Healthcare.gov in the United States, this particular challenge stemmed from a problem common to 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), and since whole families must be enrolled at once (to help mitigate adverse selection), these problems in the civil registry meant that people needed to visit an office to fix errors and sign up correctly. This means that, on net, even our most generous treatment – offering *both* a full one-year premium subsidy and assisted internet registration – resulted in only about a 30 percent enrollment rate, substantially more than the no-intervention 8 percent rate, but a far cry from universal coverage. Since imperfect civil registries are common throughout the developing world (Mikkelsen et al. 2015), these types of challenges are likely to be encountered in other contexts as well.

Taken together, we show that large, *temporary* subsidies can be effective in settings with the potential for adverse selection. A common concern with offering a "free" trial period is that individuals may become used to receiving insurance without paying, thus decreasing payments in the long run. We find the opposite: temporary registration incentives, featuring limited periods of free coverage before requiring premiums to be paid, actually increase coverage and premiums paid in the subsequent year, while also reducing adverse selection. The fact that these temporary subsidies lead to higher enrollment among the healthy even after the subsidies are withdrawn may be because many households in developing countries lack experience with insurance (Acharya et al. 2012; Cai et al. 2020), suggesting an important role for registration drives featuring temporary "free subsidy" periods to give people experience with insurance as part of campaigns to increase enrollment among the healthy.

Our study builds on the literature on participation in public health insurance systems – and in social insurance programs, more broadly – not only by testing the impact of these individual policy tools on enrollment against one another, but also on understanding how these various tools

affect enrollment and selection both initially and over time.[2] Existing evidence 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 contribute to this literature in several ways. First, we examine the impact of all three of these commonly conjectured participation barriers in the context of a large-scale government insurance program. Second, our high-quality, extensive health insurance claims data – rare for developing countries – allow us to study not only how these interventions affect participation, but also how they affect adverse selection and government budgets. Third, the high-frequency administrative data allow us to study whether these different types of interventions have persistent results over time, as different types of individuals make dynamic, and possibly strategic, decisions over insurance coverage each month.[3]

The remainder of the paper is organized as follows. Section II presents the setting, the experimental design, and the data used in the analysis. Section III presents the enrollment effects of the intervention as well as its impacts on coverage over time. Section IV presents the selection effects and discusses their implications for government costs. The last section concludes.

II. SETTING, EXPERIMENTAL DESIGN AND DATA

A. Setting: the *JKN Mandiri* program

[2] This study, in particular, is related to Thornton et al. (2010), who examine the impact of whether informal workers, recruited through a health insurance registration booth in the market, are randomized to receive a subsidy for contributory insurance through Nicaragua's social security system offices or through a microfinance organization, which could potentially have been more convenient for the informal workers. Their study finds impacts of subsidies on enrollment, and therefore on utilization, but does not study how the treatments affect the degree to which the market is adversely or advantageously selected, as we do here.

[3] In the developing world, little is known about the longer-run impacts of improving health insurance take-up, as well as selection, through interventions. One notable exception is Asuming et al. (2019), who use survey data to assess the impact of one-time subsidies on enrollment and subsequent health behaviors in Ghana, three years post-intervention. 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. The only related paper that we know that explores these issues in health does so in a developed country setting, studying California's Affordable Care Act (Diamond et al. 2019). Cai et al. (2020) also explore dynamics in weather insurance markets, focusing on the effect of having experienced a payout on subsequent insurance demand.

In January 2014, the Government of Indonesia launched *Jaminan Kesehatan Nasional* (JKN), a national, contributory health insurance program aimed at providing universal coverage by 2019. JKN comprises different sub-programs based on income and employment status. Non-poor informal workers, which represent 30 percent of the country, are covered through a sub-program called *JKN Mandiri*. Under *JKN Mandiri*, households must complete an initial registration process and then subsequently pay their monthly premiums.[4] While insurance enrollment is legally mandatory, the mandate is hard to enforce in practice, and there are currently no penalties assessed on households who do not enroll.

Households may register for *JKN Mandiri* at any time of the year, 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. Households are required to register all nuclear family members (e.g., father, mother, and children), as listed on their official Family Card (*Kartu Keluarga*), which is maintained in the civil registry by another ministry (Department of Home Affairs).

The per-person monthly premium for basic coverage (known as Class III) is IDR 25,500 (~$2), corresponding to 3.5 percent of average monthly total expenditures for eligible households.[5] The premium that a household would pay to have JKN coverage for a year is lower than the reported yearly out-of-pocket health expenditures for 12 percent of all non-poor informal households without health insurance. For households who had an inpatient episode in the last year, by contrast, the median "savings" from having had health insurance – i.e., the difference between out-of-pocket expenditures for those with no insurance and the premiums that would have been charged – are large (IDR 231,341 per month).[6] This suggests that the program functions as true

[4] Those below the poverty line (about the bottom 40 percent) receive fully subsidized insurance. Formal workers are covered jointly by employees and employers, and their contributions are withheld by the tax system.
[5] There are three different classes 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 IDR 42,500 (~$3) for class II (3-5 beds per room) and IDR 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.
[6] For each household, we compute what would have been the yearly JKN premium based on household size and compare with the yearly out-of-pocket expenditures reported in the survey using SUSENAS 2015 data (BPS 2015).

insurance, with premiums from those who use relatively little care covering the costs of those with large claims.

The premium can be paid at any social security administration office, ATM, or equipped convenience store. 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. For coverage to reactivate at a later date, the household must pay arrears, which are capped at a maximum of 6 months.[7] After the program's introduction, the government became concerned that individuals might only enroll in JKN when they had a health emergency. To limit this, in September 2015, the government introduced a two-week waiting period after enrollment, only after which households could submit an insurance claim.

An active membership provides coverage for healthcare costs incurred at public or affiliated 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.

B. Sample

We carried out this project in two large Indonesian cities: Kota Medan in North Sumatra and Kota Bandung in West Java. 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.[8] Moreover, selecting cities both on and off Java helps ensure representativeness of Indonesia's heterogeneity in culture and institutions (Dearden and Ravallion 1988).

[7] If no inpatient claims are submitted within 45 days from re-activation, there are no additional fees. Otherwise, the household has to pay a penalty equal to 2.5 percent of the treatment cost times the number of inactive months, up to a maximum of 12 months or IDR 30 million.
[8] Other large cities, such as Jakarta, Surabaya and Makassar, introduced free local health insurance programs covering a large fraction of the population. Neither Bandung nor Medan had local programs of this type during the study period.

Working with the government, we implemented the interventions in two sub-districts in Medan in February 2015 and in eight sub-districts in Bandung in November and December 2015. 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 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 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, we excluded those already enrolled, resulting in a sample of 5,996 households.

C. Experimental design

Upon identifying an eligible household, we administered a short baseline survey (see below for details), 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 (see Figure 1).

1. Temporary Subsidy treatments

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

[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 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.

covering half of a family's premiums for a year.[10] After the offer, the subsidy was valid for up to two weeks in Bandung and two weeks in Medan; to be conservative and ensure sure we capture all households who enroll during the subsidy period even accounting for data lags, our definition of households enrolled during the subsidy period includes all households that enrolled within eight weeks of the offer date.

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.[11] 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 Internet Registration treatment

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 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 all of the forms on the website. Upon successful registration, the enumerators provided information on payment procedures. If households wanted to think more about their options, wanted to enroll but needed time to assemble

[10] In Medan, households with a positive subsidy offer were randomized to receive a one-week deadline, a two-week deadline, or the ability to choose either a one- or two-week deadline to enroll using the subsidy. In Bandung, we additionally offered a fourth subsidy sub-treatment in which households that enrolled but did not submit an inpatient claim within a 12-month period were reimbursed 50 percent of the premiums that they had paid. Since these sub-treatments only took place in one of the two cities, we exclude them from the main analysis, but we discuss these findings below and show the results in the accompanying appendix.
[11] 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.

the documentation, or had technical registration problems, the enumerators returned within a few days to continue the enrollment process.

3. Information treatments

All study households received basic information about the insurance service coverage, the premiums, the procedure for registration, etc. 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 penalties" 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 passport, driver's license, etc.

D. Randomization design and timing

Figure 1 shows the experimental design for the cities of Medan and Bandung separately.[12] Figure 2 provides the experimental timeline. The study occurred in February 2015 in Medan and in

[12] 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.

November and December 2015 in Bandung. Subsidies were administered for twelve months after the offer for those who enrolled within two weeks of the offer.

E. Data and Variable Definitions

We compiled two new datasets for this project. First, 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. 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.

Second, we use uniquely detailed, high-frequency government administrative data from February 2015 to August 2018 to measure enrollment outcomes, coverage, and health care utilization.[14] Importantly, we track all participants for 32 months after the baseline survey, which allows us to examine dynamic, longer-run behavior. We matched the study participants to the administrative data using individuals' unique national identification number (*Nomor Induk Kependudukan* or NIK).[15]

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 also define *coverage* in a given month to mean that the enrolled household's

---

[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] The administrative data quality is good and has been improving over time, but some inconsistencies still arise. 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.

[15] 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 1 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.

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.

To measure health care utilization, we analyze extensive, high-quality administrative data on all claims that are covered by JKN in both hospitals and clinics. The hospital claim data report start and end date, diagnosis, reimbursement value, and facility where the claim was made.[16] We are able to distinguish between outpatient and inpatient hospital claims. In contrast, all clinic claims are for outpatient procedures. The clinic claims data report similar information to the hospital claims data, except that – due to capitation – claim values are not available. In addition to overall claims, we report two other types of information. First, we also examine the number of days until the first claim is submitted, which can provide greater precision than the value of claims, which tend to have a large right tail (Aron-Dine et al. 2015). Second, we use the diagnoses to code whether the claim was for a chronic versus emergent condition.[17]

F. Balance

Appendix Table 1 provides a check on the randomization by regressing various household characteristics measured in the baseline survey on treatment dummies. 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.

III. IMPACTS ON ENROLLMENT AND ON SUBSEQUENT COVERAGE

A. Enrollment

[16] 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.

[17] 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.

To examine the impacts of the various treatments on enrollment – i.e., successfully completing the registration process – we estimate the following regression:

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

where $HALF\ SUBSIDY_i$, $FULL\ SUBSIDY_i$ and $INTERNET_i$ 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 treatments (see footnote 10), a dummy for the randomization procedure (see footnote 12) and a dummy variable for city of residence. Regressions are weighted to reflect the desired cross-treatment randomization design (see footnote 12). Given the household-level randomization, we report robust standard errors.[18]

Table 1 presents the coefficients for $HALF\ SUBSIDY_i$, $FULL\ SUBSIDY_i$, and $INTERNET_i$ from equation (1), as well as the *p*-values from a test that the half and the full subsidy have the same treatment effect (i.e., $\beta_1 = \beta_2$) and from a test that the full subsidy and the assisted internet registration have the same effect (i.e., $\beta_1 = \beta_3$). We focus first on Panel A.

Column 1 shows that the monetary subsidies substantially increased the probability of enrollment during the following 12 months after the offer date (while the subsidies were still active), while assisted registration had a positive but much smaller impact. Only about 9 percent of the no-subsidy group enrolled within the 12-month period. Relative to this, offering the full subsidy increased enrollment by 18.6 percentage points. This means that enrollment in the full-subsidy group tripled compared to the no-subsidy group; in fact, as we will show in more detail in Section IV.C below, since subsidized households stay enrolled longer, the number of covered household-months increased more than eightfold. Offering the half subsidy increased enrollment by 10 percentage points. By contrast, the assisted internet registration treatment only increased enrollment by 3.5 percentage points.[19]

[18] Note that to facilitate comparisons, we separate out interventions reported in tables. Nevertheless, the full set of indicator variables is always included.
[19] Appendix Tables 2a and 2b replicate Table 1, but disaggregate the data by city. Overall, subsidies had similar effects on actual enrollment in the two cities.

The enrollment measure by itself masks the fact that many more households, particularly those in the assisted internet registration treatment, attempted to enroll than were actually successful. To shed light on this, in Column 2, we examine whether households initiated the enrollment process, regardless of whether they successfully enrolled.[20] Assisted internet registration led to a 23.8 percentage point increase in attempted enrollment during the first eight weeks (column 2), but only a 4.3 percentage point increase in successful enrollment during that period (column 3). This indicates that less than one-fifth of the households induced by the registration assistance to attempt enrollment were actually successful in doing so. The most common reason for unsuccessful enrollment was an inaccurate Family Card, the official identification document (see Appendix Table 3). In order to combat adverse selection, the government required that households enroll all nuclear family members as listed in this document, which was pulled automatically from the digital records held by the Home Affairs Ministry. This turned out to be problematic if the family composition had changed, but the document was not 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 use discretion to overrule the system for cause (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.

The evidence in Column 3 of impacts on enrollment within eight weeks of the offer date (i.e., when the subsidy offer was valid plus some margin for error)[21] also raises the question of whether the interventions merely 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.

[20] For households assigned to the assisted internet registration treatment, we set attempted enrollment equal to 1 if they stated that they wanted to enroll during the visits. For households assigned to follow the status quo registration procedures, we recorded whether they showed up to the office, regardless of whether they were successful in enrolling. Since only households with a voucher had to contact the study assistant at the social security office, we do not know whether households assigned to the no-subsidy group attempted to enroll if they were not ultimately successful in enrolling. For these households, attempted enrollment is set equal to actual enrollment, a choice justified by the fact that the failure rate for households assigned to the status quo registration in the subsidy treatments was negligible.

[21] For all groups (including the control group), the offer date is that of the baseline survey. For subsidy group households, we consider households who have a signup date in the administrative data within eight weeks from the offer date as having enrolled using the subsidy to allow for potential delays in the data.

Therefore, Column 4 shows the probability of enrolling after the subsidy offer expired but throughout the subsidy period – specifically, after eight weeks post-offer but within one year of the offer date. The results indicate that the subsidy interventions reduced the probability of enrolling in this period, but the decline is significantly smaller than the increase due to the subsidies in the initial period (shown in Column 3). Thus, the harvesting effect is relatively small, accounting for no more than about 10 percent of the total additional enrollment that we observed in the first eight weeks.

Taken together, the results in Panel A show substantial enrollment impacts from the monetary subsidies and, to a lesser extent, from the one-time assistance, but they also indicate that even with a *full subsidy* for a year, most people do not enroll. One explanation is that the hassle costs of the initial enrollment discussed above are large enough to provide a barrier, even when the insurance has no monetary costs. To investigate this, Panel B of Table 1 shows estimates from an enhanced version of equation (1) that also includes a full set of interactions between the (cross-randomized) subsidy treatments and the assisted intervention treatment. Column 1 shows that even with a full subsidy and assisted internet registration, enrollment only reached 30 percent, compared to 8 percent in the no-intervention status quo. Column 2 shows that less than 60 percent of households even *tried* to enroll when offered both free insurance for the year and assistance with registration. This suggests that while hassle costs provide a significant barrier – even when the insurance is free – they do not fully explain why people do not enroll.

Informational constraints, which we examine in Table 2, could be another barrier why households do not value the insurance. 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 A) and about the nature of insurance (i.e., that enrollment is mandatory and that households must enroll in advance of a health shock) in Bandung (Panel B). 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).[22]

[22] In Medan, we also experimentally tested whether individuals would want the offer, but procrastinate on it. Specifically, households with a positive subsidy offer were cross-randomized into different deadlines: one-week, two-week, or the possibility to choose between a one- and a two-week deadline to enroll using the subsidy. As shown in Appendix Table 4, this treatment also had effects that were indistinguishable from zero.

B. Coverage Dynamics

Insurance coverage is not a one-time decision. After the initial decision to enroll that we examined in Table 1, households must decide whether to continue to pay their monthly premiums to remain covered at any given point in time. It is also possible that health insurance is an "experience good" for which initial exposure increases household demand; in that case, temporary subsidies can increase long-run enrollment.

We therefore turn to the administrative data on premium payments to examine these monthly payment decisions. Figure 3 plots coverage by month since the offer date, by subsidy group. Coverage for a household 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.

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. However, many enrollees quickly dropped coverage; one-quarter of enrolled control group households had stopped paying their premiums three months post-enrollment, and nearly half of the enrollees in the no-subsidy group had stopped paying their premiums a year post-enrollment (Appendix Figure 1). The steady increase in coverage for the no-subsidy group in Figure 3 implies that the rate of new enrollment was large enough so that net coverage rates continued to increase despite the dropout effect.[23]

Interestingly, the different subsidy groups exhibited quite different levels and patterns of coverage, both before and after the subsidies expired. In the full-subsidy group, roughly 25 percent of those offered the full subsidy enrolled in the first two months after the offer, and their coverage mechanically remained constant during the first year, when the subsidies were active.[24] While the full-subsidy group also had a high dropout rate after the subsidy ended (at about month 13-14), their coverage levels continued to remain higher than the no-subsidy group, even at 20 months

[23] 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.

[24] The slight increase in coverage shown in Figure 3 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.

after the offer date.[25] The fact that those brought in with the temporary full subsidy stayed enrolled in the second year suggests a strong "experience effect," i.e., that these individuals may not have understood the benefits of insurance until they experienced it. This implies that temporary subsidies can help boost enrollment past their expiration date, and may be an important tool in boosting insurance coverage in low-enrollment settings.

As one may expect from theory, 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.

Table 3 summarizes the coverage patterns in Figure 3.[26] In Column 1, we report the percentage of households that enrolled and had coverage for at least one month in the first year after the offer. Columns 2 and 3 decompose those with coverage in Column 1 into those who no longer had coverage by month 15 ("the dropouts") and those who did ("the stayers"); Column 4 provides the p-value of the difference in the dropout vs. stayer shares. Column 5 reports the percentage who had coverage in month 15, after the subsidies ended, relative to all households in the sample; note that the interpretation in this column differs from Column 3 since we do not condition on the household having enrolled within 1 year of the offer date. Column 6 reports p-values for tests of whether coverage rates were the same during the subsidy period (Column 1) and at 15 months (Column 5). Finally, Columns 7 and 8 report the same information for month 20 since offer date. In the final 3 rows of the table, we provide the p-values for tests of whether the full- and half-subsidy coverage rates each differ from the no-subsidy coverage rates ($\beta_1 = 0$ and $\beta_2 = 0$), as well as whether the assisted-registration coverage rates differs from the status quo

[25] Appendix Figure 1 shows the coverage rate for the sample of those who enrolled in the first year, by month of enrollment. One can see a continuous decline in payments for those in the no- and half-subsidy groups. In contrast, there is a sharp decline for those in the full-subsidy group at month 13, the exact time when households had to start paying premiums.

[26] We report means in each treatment group in this and subsequent tables to facilitate comparisons both across time and across treatment groups. The means for each cell are calculated using the weights described in footnote 12, so that each treatment group shown has the same (weighted) combination of sub-treatments; that is, half subsidy has the same weighted mix of status quo vs. assisted internet registration as full subsidy, and so on.

registration ($\beta_3 = 0$). Appendix Table 5 provides the underlying regression estimates for the *p*-values reported in this table.

Table 3 quantifies the magnitude of several important patterns observed in Figure 3. First, the full-subsidy group retained substantially higher coverage than the no-subsidy group, even after the subsidies were over. Those offered the full subsidy were 4.6 percentage points (86 percent; *p*-value < 0.001) more likely than the no-subsidy group to have coverage at month 15 (column 5), and 3.9 percentage points (58 percent; *p*-value = 0.001) more likely than the no-subsidy group to have coverage at month 20 (column 7). This again suggests that health insurance is an experience good – those who were covered for free for a limited time were much more likely to pay for coverage afterwards than those who were never offered free insurance.

Second, despite the experience effect, we still document statistically significant declines in coverage in the subsidy treatments. As shown in Figure 3, we observe significantly higher coverage rates for both the full-subsidy and half-subsidy group in the first year, when the subsidy was still active (Column 1). By 20 months, after all subsidies had expired, coverage had fallen substantially and the coverage rates at 20 months were no longer statistically distinguishable between the half-subsidy and no-subsidy group. Even for the full-subsidy group, where we document the persistence of coverage above, comparing Columns 1 and 7 shows that about 61 percent of those who ever had coverage in the first year had dropped coverage by month 20 (10.6 percent in month 20 covered compared to 27.7 percent covered at some point in the first year; *p*-value < 0.001). These results suggest that while temporary subsidies can lead to substantial increases in coverage even after the subsidies are over, only about 40 percent of those subsidized continue to retain coverage.

Finally, it is important to note that while the assisted-registration group saw a slight increase in coverage initially (Column 1), their coverage rate quickly converged to that of the control group. This suggests that some of the households brought into the insurance system by reducing hassles may have been particularly sensitive to the hassles of paying each month, leading to the increased dropout rate. One possible reason is that while the assisted internet registration made registration easier, it did not resolve the hassles of paying one's premium, which still needed to be done at an office, ATM, or convenience store.

IV. Selection Impacts and its Implications on Government Costs

Subsidies are a textbook response to concerns about adverse selection, since in standard models they will induce lower-cost individuals to enroll (e.g., Akerlof 1970). However, in the presence of multiple dimensions of heterogeneity, the impact of subsidies – as well as the other interventions that we study – on adverse selection is theoretically ambiguous (Einav and Finkelstein 2011); and indeed, existing evidence from health insurance studies indicates that while such interventions in some contexts ameliorate adverse selection (e.g., Fischer et al. 2018; Finkelstein, Hendren, and Shepard 2019), they can also, in different contexts, exacerbate it (Asuming et al. 2018; Handel 2013). The impact of our interventions on selection – as well as on government costs – is therefore an empirical question.

A. Impacts on Selection

To examine the types of people who enroll under different intervention arms, we draw on two sources of data: self-reported health from the baseline survey, and administrative claims data among those who enrolled. 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).

Table 4 shows the results.[27] It shows various measures of health and healthcare use for those who enrolled and had coverage for at least one month during the first year (i.e., as measured in column 1 of Table 3). Column 1 indicates 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. Those enrolling with the subsidies had an average self-reported health score that is about 4.5 percent higher than that of no-subsidy enrollees, with both subsidy treatment effects significant at the 5 percent level. The effects of assisted internet registration were smaller, but in the same direction, and statistically significant at the 10 percent level.[28]

The remaining columns of Table 4 examine healthcare usage of households who enrolled and had coverage for at least one month during the first year. We examine all claims for the 12

[27] The regressions that calculate these *p*-values are provided in Appendix Table 7.
[28] Appendix Table 8 shows that the results holds also if self-reported health is measured in as the self-reported health of the least healthy family member. In addition, households who enrolled under the full subsidy treatment were also less likely to have a family member above 60.

months after the enrollment date; by examining a fixed number of months since enrollment date regardless of when households enrolled, we can abstract from the feature that temporary subsidies may drive households to enroll earlier in a calendar year thus mechanically affecting length of insurance coverage. We focus on three main indicators: whether the household had any claim (column 2), the total number of visits made (column 6), and the total value of claims paid (column 10). We then subdivide claims into outpatient, inpatient, and chronic. We also examine the number of days to first claim, to try to increase precision (Aron-Dine et al. 2015).

Consistent with the results on self-reported health in column 1, the claims analysis in the remaining columns also indicates that those who enrolled under the full subsidy were healthier and lower-cost. Households in the full-subsidy group were less likely to submit claims. 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). Those in the full-subsidy group were also less likely to have had a claim for a chronic, ongoing condition: 27 percentage points for the no-subsidy group compared to 17 percentage points for the full-subsidy group (column 5; p-value = 0.082). Results for the half-subsidy group are mostly qualitatively similar to the full-subsidy group, but smaller in magnitude and never statistically significantly different from the no-subsidy group. The same is true of the results for the assisted internet registration group.

In addition to having fewer overall claims, claims in the full-subsidy group were less likely to be "large claims" that suggest a substantial health emergency. This is shown in Figure 4, which reports the probability distribution function of the value of inpatient claims submitted within twelve months since enrollment, by treatment status, for those who enrolled within a year since offer date and paid for at least one month. The distribution of values of claims for the full-subsidy group is markedly left-shifted 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.

On net, the fact that the full-subsidy group had fewer claims, and that these claims were small, results in substantial reductions in claims expenditures from the insurer. In particular, the full-subsidy group had average claims that were 40 percent lower in value than those in the no-subsidy group (column 10 of Table 4; *p*-value = 0.095) and on average waits 30 percent longer before submitting their first claim (column 11; *p*-value = 0.006).

B. Dynamics and Selection

An important question is whether the fact that households can time enrollment and dropout decisions exacerbates adverse selection. We investigate both a) whether households in the no-subsidy group, who do not face a time limited enrollment period, are more likely to time enrollment to when they are likely to have a claim, and b) how those who choose to retain coverage differ from those who drop.

Figure 5 begins by plotting the number of claims by month since enrollment, separately by subsidy treatment groups among households who enrolled within one year since offer and had coverage for at least one month over that period, along with 95 percent confidence intervals. Those who enrolled without the subsidy appear to have submitted more claims in the first few months upon enrollment than did the households in the full-subsidy group, but over time this difference became less stark and, by the end of the period, they displayed similar patterns in number of claims. Households in the half-subsidy group also 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.

We present similar analysis in regression form in Appendix Table 9. In the first 3 months that the households were enrolled in insurance (Panel A), full-subsidy households were less likely to submit inpatient or outpatient claims, had lower overall claims than the control group, and their inpatient claims were on average for lower values. The coefficient on the half-subsidy group is generally negative, but the difference is not always statistically significant. Months four through twelve after enrollment (Panel B) show that, over time, the difference disappears: all of the treatment groups display a very similar pattern of claims, although the coefficient for the full-subsidy group is overall still negative.

Combined with the payments findings in the previous section (i.e., Figure 3), 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).

Table 5 investigates differential selection in terms of who retained coverage. 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 coverage rates of these two groups are shown in Table 3. Several results are worth highlighting. In the full-subsidy group, those who retained coverage had *higher* baseline self-reported health than those who did not (column 1; *p*-value = 0.068). On the other hand, the stayers were also more likely to have had claims (column 2; *p*-value = 0.005) and to have had more visits (column 6; *p*-value = 0.002). These were particularly likely to be outpatient claims/visits and those for chronic conditions, rather than inpatient claims. The half-subsidy group showed a similar pattern of claims.[29] The pattern for the no-subsidy group is more ambiguous, with the dropouts more likely to have had an inpatient claim, but having had fewer overall visits.

The results from the subsidy treatments continue to suggest an experience effect: those who stayed were those who made use of the system, even for smaller outpatient or chronic conditions. They also raise the possibility that allowing relatively small payments from a plan (as opposed to a high-deductible plan that only covers catastrophic expenses) may be important for continuing to entice healthy people to remain covered.

C. Implications for Government Costs

The selection patterns indicate that not only were the no-subsidy enrollees sicker and higher-cost than those who enrolled with full subsidies, but that the no-subsidy enrollees strategically timed enrollment to coincide with major health expenditures, and were quicker to drop coverage (i.e., stop paying premiums) after a few months. In Table 6, we examine the implications of these results

[29] Appendix Table 10 presents the equivalent results broken down by the assisted internet registration treatment, and finds a similar pattern: stayers were more likely to have had claims, particularly inpatient and outpatient claims. Appendix Table 11 presents the regressions from which we calculate the *p*-value of the difference in means reported in Table 5 and in Appendix Table 10.

for the net costs to the government, i.e., the difference between premiums received (net of subsidies) and claim expenditures. The results indicate that offering full subsidies covers more households at similar cost per covered household.[30]

Panel A shows the results during the time period the subsidy was in effect. Column 1 shows the total number of covered household-months in each treatment group. Since households in the full-subsidy group are more likely retain coverage than those in the no-subsidy group, the differences in initial enrollment shown above translate into even larger differences in the total number of covered household-months: the total number of covered household-months in the full-subsidy group is *eight times* that of the no-subsidy group; coverage for the half-subsidy group is about 3 times that of the no-subsidy group.

The remaining columns show net revenues with and without accounting for capitation payments by month, which can be decomposed into revenues from premiums and government expenditures as a result of claims.[31] Revenues are defined as premiums paid by enrollees.[32] Claims expenditures are defined as the value of claims paid. In columns 2 to 5, we focus on revenues and expenditures per household-month covered; this provides us with estimates of the additional revenue and expenditure for each additional household covered in a given month. In, columns 6 through 9, we then report the results for all households in the sample regardless of whether they enrolled in the insurance, thus providing us the total revenues and costs of offering the policy; these estimates reflect the corresponding cells in columns 2 through 5, scaled by the number of covered households in that group.

Panel A, column 5 shows that, while the subsidy was active, on net the government lost around IDR 125,000 (~$9.25) per household-month covered in the no-subsidy group compared to

[30] Appendix Table 12 presents equivalent results split by assisted internet registration vs. status quo registration, and finds no substantial differences in net costs to the government. Appendix Table 13 reports the regressions that correspond to the *p*-values reported in Table 6 and Appendix Table 12.
[31] 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 IDR 3,000-6,000 per enrollee for *puskesmas* and IDR 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 IDR 6,800 per enrollee per month. Capitation payments are only paid to healthcare facilities in months in which the household paid the premium.
[32] They should therefore 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.

losing only about IDR 50,000 (~$3.75) per household-month covered in the full-subsidy group. In other words, the net cost to the government per covered household-month in the full-subsidy group was no higher than in the no-subsidy group (p-value = 0.198), *even taking into account that the government received essentially no revenue from the subsidy group*. This is because the decline in average claims expenditures between the full-subsidy and no-subsidy groups (Column 3: decline of IDR 152,000 per covered household-month; p-value = 0.026) was even larger than the forgone revenue from not collecting premiums (Column 2: decline of IDR 71,000 per covered household-month; p-value < 0.001).[33] As a result, the full subsidy resulted in over eight times more covered household-months (Column 1), at no higher cost to the government per household-month covered (Column 5).

Of course, there are more people covered, and on net the policy does entail an increase in the total amount spent by the government in total during the period the subsidies were in effect. Looking over the entire sample (i.e., not conditioning on enrollment), Column 9 indicates that in the no-subsidy group the government cost was IDR 3,000 (~$0.22) per eligible household per month, while with the full-subsidy the government cost was IDR 12,000 (~$0.88) per eligible household per month, though these differences are not statistically different.

Panel B explores what happened in the year *after* the subsidies were withdrawn. As shown in Table 3, there was some persistent increase in coverage in the full-subsidy group. Table 5 shows that despite being healthier initially, households in the full-subsidy group that retained coverage (i.e., paid premiums) after the subsidy ended were more likely to have had a claim during the first year than those who dropped coverage after the subsidy ended. They are, however, still healthier in terms of baseline health status than stayers from the no-subsidy group; similarly, the point estimates in Table 6, column 3, suggest that the claims from this group were somewhat lower than from the no subsidy group, though the differences are not statistically significant. On net, Table 6, Panel B (column 5) indicates that government costs were not statistically different per covered household-month for the no-subsidy group and the full-subsidy group in the period after the subsidy ended; in fact, the point estimates suggest that that net costs to the government per covered

[33] For household-months covered in the half-subsidy group, the net losses are similar to those in the no-subsidy group (about IDR 160,000 per covered household-month); again, the fact that net revenue losses are only slightly larger for the half-subsidy group than for the no-subsidy group – despite mechanically lower revenues – reflects the healthier composition of the half-subsidy pool.

household-month in the full-subsidy group were about half those in the no-subsidy group (IDR 47,000 in net government costs per covered household-month in the full-subsidy group; IDR 101,000 in net government costs per covered household-month in the no-subsidy group). Thus, in the year after the temporary full subsidy ended, we estimate that twice as many household-months were covered (Panel B, column 1), at no higher cost per covered household-month (Panel B, column 5). In fact, looking over the entire sample (i.e., not conditioning on enrollment), column 9 indicates virtually identical government expenditures (about IDR 5,000 per household) in the year after the subsidies end for the full-subsidy group compared to the no-subsidy group.

Putting this all together, one can calculate the bottom-line implications for the government for offering a temporary full subsidy to a given population. In the year the subsidy was in effect, the government quadrupled its net budgetary contribution for this population (from IDR 3,000 to IDR 12,000 per household offered). During that year, coverage expanded dramatically – from 6.3 percent of the population to 27.7 percent of the population. In the subsequent year, the bottom line for the government was the same – about IDR 5,000 per household in the population – regardless of treatment. But the full-subsidy group had, on net, 58 percent more households covered, with the same *total* government expenditure. This is very far from universal coverage – the full-subsidy group had 10.6 percent covered at 20 months after the project started, compared to 6.7 percent in the no-subsidy group – but it represents meaningfully more people covered with no additional ongoing 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, insurance mandates can be very difficult to enforce, particularly given the institutions present in most developing countries. Countries are therefore turning to complementary policy tools to boost coverage.

In this paper, we examine the impact of temporary insurance subsidies – which must be taken up within a month of offer and only last a year – reduced hassle costs associated with enrollment, and information provision on insurance coverage in a mandated insurance setting. We

find that offering a full, but temporary, subsidy lead to an eight-fold increase in the total number of household-months covered by insurance during the first year, and helped attract much healthier enrollees. Because of the healthier selection and also the strategic dynamic adjustment of coverage and claims in the no-subsidy group – the no-subsidy group timed its enrollment to coincide with high expenditures and quickly dropped coverage a few months later – the net cost to the government per covered household-month of the full subsidy is no higher, *even despite the cost of the subsidies*. Importantly, the full monetary subsidy induced higher enrollment even after it expired, in line with health insurance being an experience good. As a result, after the subsidy period was over, the government was able to cover substantially more people at a roughly similar net cost per household covered – and possibly even at no higher total cost to the government.

At the same time, our findings also further highlight the challenges that governments face when aiming to achieving universal health coverage through a contributory system. While both monetary subsidies and assisted enrollment increased enrollment rates, even the most aggressive interventions – a full subsidy for a year and internet-assisted enrollment, only led to 30 percent enrollment – a substantial increase from the 8 percent enrollment in the status quo group, but a far cry from universal enrollment. Some of this reflects administrative challenges: almost 60 percent of households in the full subsidy, internet assisted registration treatment tried to enroll – double the amount who actually did so. This underscores how weak social insurance infrastructure (in this case, the underlying social registry) can create obstacles to universal enrollment and suggests that long-run solutions to universal coverage are only feasible through strengthening overall administrative structures.