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The challenges of universal health insurance in developing countries: Evidence from a large-scale randomised experiment in Indonesia*

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Abstract

To assess ways to achieve widespread, financially sustainable health insurance coverage in developing countries, we designed a randomised experiment involving almost 6,000 households in Indonesia who are subject to a nationally mandated government health insurance program (*Jaminan Kesehatan Nasional*: JKN). We assessed several interventions that simple theory and prior evidence suggest could increase coverage and reduce adverse selection: (i) substantial temporary price subsidies (which had to be activated within a limited time window and lasted for only a year); (ii) assisted registration; and (iii) information. Both temporary subsidies and assisted registration increased initial enrolment. Temporary subsidies attracted lower-cost enrolees, in part by eliminating the practice observed in the no-subsidy group of strategically timing coverage for a few months during health emergencies. As a result, while subsidies were in effect, they increased coverage more than eightfold at no higher unit cost. Even after the subsidies ended, coverage remained twice as high–again at no higher unit cost. However, the most intensive (and effective) intervention, however–assisted registration and a full one-year subsidy–resulted in only a 30 percent initial enrolment rate, underscoring the challenges to achieving widespread coverage.

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**TNP2K: *Tim Nasional Percepatan Penanggulangan Kemiskinan*: National Team for the Acceleration of Poverty Reduction.

Section One:

Introduction

As developing countries emerge from extreme poverty and enter middle-income status, many aim to expand their government-run social safety net systems (Chetty and Looney 2006). An important part of this process is the creation of universal health insurance policies 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 health insurance system funded entirely out of tax revenues and supplemented by small copayments at the time of service (Gruber, Hendren, and Townsend 2014) which has been shown to improve health but faces substantial funding challenges. Many other countries, such as Ghana, Kenya, the Philippines and Vietnam and 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 subsidised by tax revenues but everyone else is required to pay a premium that is collected through a payroll tax for formal sector workers and directly from individuals for everyone else.

The challenge with contributory systems, however, is that enforcing the insurance mandate for those who must pay premiums directly is difficult. While the political and administrative challenges of enforcing mandates are not unique to developing countries–for example, the Patient Protection and Affordable Care Act 2010 (commonly known as "Obamacare") legislation did not achieve universal coverage in the United States (Berchick 2018)–they are particularly difficult for them, again because the majority of their citizens are outside the tax net. This means that the types of penalties for noncompliance used initially in the United States under Obamacare–fines collected through the personal income tax system–are not an option.

Since developing countries have shown little appetite for enforcing the few possible remaining sanctions on the noncompliant population (for example, by denying delinquent households the ability to enrol their children in school), perhaps rightly, what they are left with is a toothless mandate. In theory, a toothless mandate can create two related challenges for governments that are trying to achieve universal or near-universal coverage: (i) low program enrolment; and (ii) adverse selection, where the least healthy are more likely to enrol, thereby raising program costs above the population average (Akerlof 1970; Einav and Finkelstein 2011). In practice, like other nations that have experimented with these policies, Indonesia has experienced both: despite the fact that mandatory, universal health insurance was launched in 2014, the contributory portion of the program, known as *JKN Mandiri*, had enroled only 20 percent of the

targeted population a year after its introduction, and its 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 enrolment, while also reining in the financial costs to the tax-funded government budget—in the context of mandatory, but weakly enforced, contributory health insurance programs. The aim is not necessarily for the government to break even—it is clear that some subsidies may be needed 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, in cooperation with the Indonesian Government, we designed a large-scale, multiarm experiment—involving almost 6,000 households—to assess three interventions that simple economic theory suggested could increase enrolment and reduce adverse selection in JKN. First, we examined the role of large, temporary subsidies: we randomised households to receive subsidies of either 50 percent ("half subsidy") or 100 percent ("full subsidy") for the first year of enrolment. To be eligible for the subsidy, households had to enrol 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, rather than traveling to a far-off insurance office to enrol. Third, we tested for information constraints by randomly advertising three different types of basic insurance information: (i) the financial costs of a health episode and how they relate to insurance prices; (ii) the two-week waiting period from enrolment to coverage (so that one could not wait to get sick to sign up); and (iii) the fact that insurance coverage is legally mandatory.

To assess the impacts of these interventions, we utilise a number of new data sources to examine the impact on enrolment and coverage. These data include the government's administrative insurance data on registration, premiums paid, and all claims made by program enrolees for up to 32 months after the intervention. We first use these data to examine the impact of the interventions on *enrolment* which we define as completing the initial registration process. As the decision to stay enroled is a dynamic one in which households need to pay a monthly premium, we also examine the impact of the interventions on *insurance coverage* which we define as having paid the premium for a given month to ensure insurance coverage for that month.

Given the extensive and detailed administrative data on claims, we then examine questions relating both to adverse selection and to the ultimate government costs per household insured under the various policy treatments. Finally, we supplement these administrative data with a short baseline assessment survey in which we collected data on demographics and self-reported health status prior to the intervention. Among other things, this baseline survey allows us to measure preintervention "health status" for all study participants, regardless of whether they subsequently enroled in the insurance program.

In the context of a toothless mandate, our findings reveal both opportunities and challenges for increasing coverage in contributory health insurance programs in developing countries. On the one hand, we find that temporary subsidies and assisted registration can both increase

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

enrolment. Moreover, temporary subsidies attract a much lower-cost population, enabling substantial increases in coverage at no higher cost per covered unit. This increased coverage persists (albeit at a lower rate) after the subsidies end. On the other hand, even our most intensive (and effective) intervention–assisted registration and a full one-year subsidy–resulted in only a 30 percent initial enrolment rate. This was a substantial increase on the status quo enrolment rate of 8 percent but still a far cry from universal coverage. Our analysis reveals specific obstacles to achieving widespread coverage stemming from limited state capacity to facilitate enrolment and to prevent strategic short-term coverage.

Our study explicitly builds on the literature on participation in public health insurance systems and in social protection programs more broadly. We not only test the impact of these individual policy tools on enrolment but also test the relative magnitudes of relieving different participation constraints against one another in a common real-world context.[2]

Theory suggests that the three constraints that we examine could each increase enrolment in various types of public programs including health insurance. In fact, the empirical evidence is consistent with this theory: the findings (Thornton et al. 2010; Asuming 2013; Fischer et al. 2018; Finkelstein, Hendren and Shepard 2019) from both developed and developing settings indicate that subsidies, reductions in transaction costs (Alatas et al. 2016; Bettinger et al. 2012; Dupas et al 2016), and information (Gupta 2017; Bhargava and Manoli 2015) all have the potential to increase participation in a variety of social insurance programs, motivating our experimental design. Importantly, our extensive high-frequency administrative data allow us to build upon this literature because we can precisely study whether these different types of interventions have persistent results over time as individuals make dynamic, and possibly strategic, decisions over insurance coverage each month.[3] This is particularly important for the temporary subsidies if "experience" with the health care system leads households to increase their perceived value of insurance and stay covered after the subsidies expire.[4]

We then go further to examine not only the impacts on overall enrolment but also whether these interventions affect the *type* of individual who enrols–as well as remains enroled–and thus whether it is possible to increase enrolment of low-utilisation individuals enough to reduce the per-participant cost of insurance. In the standard textbook models in which individuals differ only in their risk type, the interventions that we test could all potentially mitigate adverse selection since the marginal enrolees will be lower-cost than the average enrolees (Akerlof 1970). In the presence of multiple dimensions of heterogeneity, however, the impact of these

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[2] This study, in particular, is related to Thornton et al. (2010) which examined the impact of whether informal workers, recruited through a health insurance registration booth in the market, are randomised to receive a subsidy for contributory insurance through Nicaragua's social security system offices or through a microfinance organisation which could potentially have been more convenient for informal workers. Their study finds impacts of subsidies on enrolment and, therefore, on utilisation 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, there is little known about the longer-run impacts of improving health insurance take-up and selection through interventions. One notable exception is Asuming et al. (2018) which uses survey data to assess the impact of one-time subsidies on enrolment and subsequent health behaviours in Ghana, three years post-intervention. Our high-frequency, administrative data allow us to further 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 does so in a developed country setting, studying California's Affordable Care Act (Diamond et al. 2018).

[4] Delavallade (2017) provides evidence that a related "experience" effect could be important by showing that randomly providing households with a free preventive health visit increased their hypothetical willingness to pay for insurance in a subsequent survey.

interventions on adverse selection is theoretically ambiguous (Einav and Finkelstein 2011) and, indeed, existing evidence from health insurance studies indicates that while such interventions can ameliorate adverse selection (Fischer et al. 2018; Finkelstein, Hendren, and Shepard 2019), they can also, in different contexts, exacerbate it (Asuming et al. 2018; Handel 2013). It is, therefore, an empirical question whether varying the different insurance constraints, holding constant the setting, leads to a different type of enrolment, and in particular one that makes a meaningful difference in terms of participant costs.

More specifically, our three interventions produce three distinct sets of findings.

First, we find that the one-year full subsidies significantly boosted enrolment and improved selection leading to more people insured at the same cost to the government, even after the subsidies expired. Those offered the full subsidy were 20.9 percentage points (almost seven times) more likely to enrol than were those in the no-subsidy group during the active subsidy period. This increase was not driven simply by households who would have purchased insurance anyway ("harvesting") but rather represents a real net increase in enrolment. While some of these households did not elect to pay premiums at the end of the one-year subsidy, many did. As a result, in the year after the subsidy ended, insurance coverage in the full-subsidy group remained over twice as high as coverage in the no-subsidy group–consistent with the idea of health insurance as an experience good.

Despite the fact that more households enroled under the full-subsidy treatment, the net cost to the government per covered person–that is, the difference between revenues from premiums and payments to providers and, therefore, the amount that would need to be covered from the general government budget–was similar with and without the full subsidy. Remarkably, this was true *even* in the first year, when the subsidy was active and hence when the full-subsidy group brought in essentially no revenue. This is because the subsidies brought in substantially lower-cost enrolees. Relative to enrolees in the no-subsidy group, those receiving the full subsidy reported better health at baseline and had fewer claims (and, notably, fewer claims for chronic conditions) during their first year of enrolment. This cost difference may also, in part, reflect strategic timing decisions by the no-subsidy group, rather than fixed health differences alone. In fact, the no-subsidy enrolees submitted more claims than did full-subsidy enrolees in the first three months after enrolment, after which the difference between the two groups attenuates. Many enrolees in the no-subsidy group subsequently ceased paying premiums and dropped coverage after a few months. Such strategic enrolment timing was less of an option for full-subsidy enrolees because the subsidy offer was time-limited and, once enroled, they stayed covered for the full first year. When the full-subsidy group had to begin paying premiums in the second year, they brought in slightly more revenue to the government since more people were enroled (due to the experience effect highlighted above), but the value of their claims appears similar to the value of those in the control group.

In contrast, the half-subsidy offer was less effective than the full subsidy, enroled fewer people than the full subsidy–the treatment effect was about one-half that of the full subsidy–and did not appear large enough to generate an experience effect in the second year. Nor do we observe a large selection effect on claims. Taken together, in the first year, despite bringing in more revenue than the full subsidy, the half-subsidy treatment led to fewer households covered than the full subsidy at a higher per enrolee cost.

Second, while the subsidy treatments highlight that the financial cost of insurance is a barrier to enrolment, we find that hassle costs also appear to be a real barrier to participation, and one that we were not able to fully solve. Reducing hassles by assisting with Internet-based registration increased enrolment by 3.5 percentage points (41 percent). Importantly, however, many more people attempted to enrol than were actually able to do so: in fact, nearly as many people attempted to enrol in the assisted Internet-based registration as in the full-subsidy group. When offered *both* a full subsidy and assisted Internet registration, nearly 60 percent of households tried to enrol, but only about one-half were successfully able to do so.

Households' enrolment efforts were substantially muted by technical and administrative challenges with the government's online enrolment system. While also 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. Registry data on who is in each family is often inaccurate (Sumner and Kusumaningrum 2014) and, since whole families must be enroled 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. 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.

We also find that those who enroled in the assisted-Internet registration group stopped paying premiums at a faster rate than those who enroled under the status quo registration. This is possibly because those who selected in under this treatment might also be those who are easily discouraged by the hassle costs involved in making payments each month. Not surprisingly, given their high dropout rate, we do not observe any differences in the claims of the assisted-registration group as compared to the status quo registration group.

Third, none of the information treatments affected enrolment into the system. The fact that our various information treatments had no impact suggests that lack of information may not be a key barrier, although we cannot rule this out definitively. It does suggest, however, that while information and 'nudge' campaigns are often an attractive policy option given their low cost (Thaler and Sunstein 2009), this does not seem to be the primary constraint in this context.

Taken together, the most important takeaway from our results is that large, temporary subsidies can work. 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 term. 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 reducing adverse selection. This may be because many households in developing countries lack experience with insurance (Aacharya et al. 2012), suggesting an important role for registration drives featuring temporary subsidy periods to give people experience with insurance as part of campaigns to increase enrolment.

Despite the fact that we find that these large temporary subsidies can substantially boost enrolment, particularly among lower-cost enrolees, we did not find an immediate and effective solution that would lead to universal (or even close to universal) enrolment. Even the most intensive intervention–assisted registration plus free insurance

for one year–only resulted in a 30 percent initial enrolment rate. While this is substantially higher than the status quo initial enrolment rate of 8 percent, it is still a long way from universal enrolment; moreover, many newly enroled households dropped coverage over time.

Nevertheless, our findings offer important insights into how to further improve these types of programs on the margin:

- **First, a trial period of free insurance had significant positive effects–increasing enrolment rates while substantially mitigating adverse selection–at no additional cost to the government.**
- **Second, our results suggest that the dynamics of coverage decisions can exacerbate adverse selection.** A key administrative challenge, therefore, lies not just in enforcing the enrolment mandate–which was the premise for our interventions–but also in designing insurance regulations to prevent the strategic timing of gaining and dropping coverage.
- **Finally, as the assisted Internet registration treatment demonstrated, without substantial long-term investments in overall administration and infrastructure (for example, improved identification systems and better Internet connections), there will continue to be substantial hassles that prevent universal insurance coverage.**

The remainder of the paper is organised in four sections. Section Two presents the setting, the experimental design, and the data used in the analysis. Section Three presents the enrolment effects of the intervention as well as its impacts on coverage over time. Section Four presents the selection effects and discusses their implications for government costs, while Section Five provides the conclusions.

Section Two:

Setting, Experimental Design and Data

2.1 Setting: The JKN Mandiri Program

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 subprograms based on income and employment status. Non-poor informal workers who represent 30 percent of the country are covered through a subprogram called *JKN Mandiri*. Under *JKN Mandiri*, households must complete an initial registration process and then pay monthly premiums.[5] While insurance enrolment is legally mandatory, the mandate is hard to enforce in practice and there are currently no penalties imposed on households that do not enrol.

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 (for example, father, mother, and children) listed on their official Family Card (*Karta Keluarga*) which is maintained in the civil registry by another ministry (Department of Home Affairs).

The monthly premium per person for basic coverage (known as Class III) is IDR 25,500 (US$2.00) which corresponds to 3.5 percent of average monthly total expenditures for eligible households.[6] The premium that a household pays to have JKN coverage for a year is lower than the reported yearly out-of-pocket (OOP) health expenditures for 12 percent of all non-poor informal households without health insurance. This percentage reaches 66 percent, however, for households that had an inpatient episode in the last year, in which case the median "savings" from having health insurance are large (IDR 231,341 per month).[7]

The premium can be paid at any BPJS 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

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5 Those below the poverty line (about the bottom 40 percent) receive fully subsidised insurance. Formal workers are covered jointly by employers and the employee's own contributions that are withheld by the tax system.

6 There are three different classes that cover the same medical procedures but offer different types of accommodation should an inpatient procedure be required. The monthly premium per person during the period of the study was IDR 42,500 (~US$3.00) for class II (3-5 beds per room) and IDR 59,500 (~US$4.50) 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 enroling in Class III insurance.

7 For each household, we compute what would have been the yearly JKN premium based on household size and compare this with the yearly OOP expenditures reported in the survey using Susenas 2015 data (BPS 2015).

date, the household must pay arrears which are capped at a maximum of six months.[8] After the program's introduction, the government became concerned that individuals might only enrol in JKN when they had a health emergency. To limit this, in September 2015, the government introduced a two-week waiting period after enrolment, only after which households could submit an insurance claim.

An active membership provides coverage for health care costs incurred at public or affiliated clinics and hospitals with no copayments, although specific procedures (for example, cosmetic surgery, infertility treatments, and orthodontics) are excluded. Primary care clinics are reimbursed under a capitation system 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 Based Groups) in which amounts are determined jointly by primary diagnosis and severity of the condition.

2.2 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 proportion of their population was uninsured.[9] 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 subdistricts in Medan in February 2015 and in eight subdistricts in Bandung in November and December 2015. The subdistricts were selected from among those with the highest concentration of non-poor informal workers; within those subdistricts we randomly selected neighbourhoods for the study.[10] 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, 14.5 percent (7,629) 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 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 enroled, resulting in a sample of 5,996 households.

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[8] 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.

[9] Other large cities, such as DKI 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.

[10] Using the 2010 census, we chose subdistricts with a high fraction of non-poor informal workers. We excluded subdistricts with universities, large factories, or malls to avoid areas with a high concentration of temporary residents. We then randomly selected 12 kelurahan (urban municipal units) in the two subdistricts in Medan (out of 16 possible *kelurahan*) and four kelurahan in each subdistrict in Bandung (out of 41 possible *kelurahan*). Within each kelurahan, we randomly selected the neighbourhoods (rukun warga, also known as RW) to enumerate.

2.3 Experimental design

Upon identifying an eligible household, we administered a short baseline survey (see below for details). At the end of this survey the household was randomly assigned to three fully cross-treatment arms affecting the insurance price, the hassle cost of registration, and the information available (Figure 1).

2.3.1 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 covering one-half of a family's premiums for one year.[11] After the offer, the subsidy was valid for up to two weeks in Bandung and two weeks in Medan. To be conservative and ensure we captured all households that enroled during the subsidy period and to account for data lags, our definition of households enroled during the subsidy period includes all households that enroled within eight weeks of the offer date.

For logistical reasons, we could not pay one-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 one-half of 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 subsidised. In theory, the government regulated that all immediate household members be registered, so subsidising one-half of the household members was roughly equivalent to providing a 50 percent discount. The subsidy received for the subsidised members was conditional on payment for the non-subsidised members for the first month but unconditional thereafter in practice. Households in the full-subsidy period were not required to make any payments during the subsidy period.

2.3.2 Assisted Internet registration treatment

Registering for *JKN Mandiri* usually requires traveling to the BPJS office in the district capital so, to reduce the hassle costs of registration, we offered one-half of the study households the opportunity to complete 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 enrol but needed time to assemble the documentation, or had technical registration problems, the enumerators returned within a few days to continue the enrolment process.

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[11] In Medan, households with a positive subsidy offer were randomised to receive a one-week deadline, a two-week deadline, or the ability to choose either a one- or two-week deadline to enrol using the subsidy. In Bandung, we additionally offered a fourth subsidy subtreatment in which households that enroled 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 subtreatments 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.

[12] If a family had an odd number of members, we randomly assigned the household to receive a subsidy for or members with equal probability. If there was only one member, the member received a full subsidy.

2.3.3 Information treatments

All study households received basic information about the insurance service coverage, the premiums, and the procedure for registration. For randomly selected households in each city, we provided additional types of information to test whether various forms of knowledge constrained enrolment.

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 OOP 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 and a discussion of the OOP expenditures associated with accessing care. Based on discussions with the government, however, we then randomly assigned households to the following two treatments: (i) a "waiting period" treatment in which we informed households about the new two-week waiting period between enrolment and the start of coverage; and (ii) a "mandate penalties" treatment in which we reminded households that enrolment 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 and driver's license.

2.4 Randomisation design and timing

The study occurred in February 2015 in Medan and in November and December 2015 in Bandung. Subsidies were administered for 12 months after the offer for those who enroled within two weeks of the offer. Figure 1 shows the experimental design for Medan and Bandung separately,[13] while Figure 2 provides the experimental timeline.

2.5 Data and variable definitions

We compiled two new data sets 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 utilisation, and existing knowledge of the program.[14] Self-reported health was measured on a four-point scale from 1 (unhealthy) to 4 (very healthy); we analyse 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.

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[13] The number of households differs in each treatment for two reasons. First, while in Medan we maximised power to detect differences in enrolment, in Bandung we maximised power to detect differences in claims conditional on take-up. Since we expected greater take-up with a larger subsidy, we randomised 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 randomisation was used and reweight observations to obtain the intended cross-randomisation weights so that each main treatment group has the same mix of each crossed additional treatment.

[14] To minimise 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 BPJS or JKN.

Second, we use uniquely detailed government administrative data from February 2015 to August 2018 to measure enrolment outcomes, coverage, and health care utilisation.[15] We track all participants for 32 months after the baseline survey. We matched the study participants to the administrative data using individuals' unique national identification number (*Nomor Induk Kependudukan* or NIK).[16]

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

To measure health care utilisation, we analyse 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.[17] 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, since claims data are often noisy, we also examine the number of days until the first claim was submitted. Second, we use the diagnoses to code whether the claim was for a chronic condition.[18]

2.6 Balance

Appendix Table 1 provides a check on the randomisation by regressing various household characteristics measured in the baseline survey on treatment dummies. Only six out of the 54 coefficients are significantly different from zero at the 10 percent level, in line with what we would expect by chance.

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[15] 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 than 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 enrolment date or membership type changes in subsequent extracts, we retain the information as reported in the first extract in which the individual appears.

[16] 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 enrolment, those without a NIK are likely to not be enroled in JKN.

[17] 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 gets reimbursed under the same claim.

[18] We build our chronic 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.

Section Three:

Impacts on Enrolment and Subsequent Coverage

3.1 Enrolment

Table 1 and Table 2 examine the impacts of the various treatments on enrolment–that is, successfully completing the registration process. We measure enrolment over the first year after the intervention date–that is, the date the baseline survey occurred. We estimate the following regression:

$y_i=\beta_0+\beta_1$ *HALF SUBSIDY*$_i+\beta_2$ *FULL SUBSIDY*$_i+\beta_3$*INTERNET*$_i+$*INFO*$^{i\prime}\beta_4+X_i\delta+\varepsilon_i$ (1) where *HALF SUBSIDY*$_i$, *FULL SUBSIDY*$_i$ *and INTERNET*$_i$, are dummy variables equal to *1* if household 1 was randomly assigned to the respective treatment, and *INFO* 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 11), a dummy for the randomisation procedure (see footnote 13) and a dummy variable for city of residence. Regressions are weighted to reflect the desired cross-treatment randomisation design (see footnote 13). Given the household-level randomisation, we report robust standard errors.[19]

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 shows the half and full subsidy have the same treatment effect (that is, $\beta_1=\beta_2$) and from a test that the full subsidy and the assisted Internet registration have the same effect (that is, $\beta_1=\beta_3$). Column 1 examines whether the household was enroled within the 12 months that the subsidies were active. In Column 2, we examine whether households initiated the enrolment process, regardless of whether they successfully enroled.[20] In Column 3, we examine enrolment within eight weeks of offer date (that is, when the subsidy offer was valid plus some margin for error).[21] In Column 4, we consider enrolment after the subsidy offer expired but throughout the subsidy period (up to one year from the offer date).

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[19] Note that to facilitate comparisons, we separate out interventions reported in tables. Nevertheless, the full set of indicator variables is always included.

[20] For households assigned to the assisted Internet registration treatment, we set attempted enrolment equal to 1 if they stated that they wanted to enrol 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 enroling. 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 enrol if they were not ultimate ly successful in enroling. For these households, attempted enrolment is set equal to actual enrolment, 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 house holds who have a signup date in the administrative data within eight weeks from the offer date as having enroled using the subsidy to allow for potential delays in the data.

Subsidies substantially increased the probability of enrolment during the 12 months after the offer date, while assisted registration had a positive but smaller impact (Panel A, Column 1). Only about 9 percent of the no-subsidy group enroled within the 12-month period. Relative to this, offering the full subsidy increased enrolment by 18.6 percentage points (216 percent), while offering the half subsidy increased enrolment by 10 percentage points (116 percent). In contrast, the assisted Internet registration treatment only increased enrolment by 3.5 percentage points (40 percent).[22]

The enrolment measure by itself masks the fact that many more households–particularly those in the assisted Internet treatment–attempted to enrol than were successful. Assisted Internet registration led to a 23.8 percentage point increase in attempted enrolment during the first eight weeks (Column 2), but only a 4.3 percentage point increase in successful enrolment during that period (Column 3). This indicates that less than one-fifth of the households induced by the registration assistance to attempt enrolment were actually successful in doing so. The most common reason for unsuccessful enrolment was an inaccurate Family Card, the official identification document (see Appendix Table 3). To combat adverse selection, the government required that households enrol all nuclear family members as listed in this document which was automatically sourced from the digital records held by the Ministry of Home Affairs. This was problematic if the family composition had changed but the document had not been updated. In practice, updating the card is challenging–it cannot be updated online and requires at least one trip to a Home Affairs-linked administrative office, and can often incur delays and other additional costs. During in-person enrolment, social security administration officials use discretion to overrule the system for cause (for example, if households had documentation that the Home Affairs record was inaccurate) but the lack of flexibility in the online system made web enrolment nearly impossible for many.

The evidence in Column 3 of impacts on enrolment within the first eight weeks also raises the question of whether the interventions merely shifted forward in time an enrolment decision that would have occurred anyway (so-called "harvesting"). This seems particularly plausible given that both the offer of registration assistance and the subsidy offers were time-limited. Column 4, therefore, shows the probability of enroling after the subsidy offer expired–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 enroling in this period but the decline is significantly smaller than the increase due to the subsidies in the initial period (shown in Column 3). Harvesting is, therefore, relatively small–accounting for no more than about 10 percent of the total additional enrolment we observed in the first eight weeks.

3.2 Barriers to universal enrolment

The results in Table 1, Panel A, show enrolment impacts from the intervention but also indicate that even with a full subsidy for one year, most people do not enrol. One explanation is that the hassle costs of enrolment 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

[22] Appendix Tables 2a and 2b replicate Table 1 but disaggregate the data by city. Overall, subsidies had similar effects on actual enrolment in the two cities.

the (cross-randomised) subsidy treatments and the assisted intervention treatment. Column 1 shows that, even with a full subsidy and assisted Internet registration, enrolment only reached 30 percent. Column 2 shows that less than 60 percent of households even *tried* to enrol 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 enrol.

We, therefore, explored other potential barriers such as information barriers (Table 2). 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 (that is, that enrolment is mandatory and that households must enrol 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 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).[23]

3.3 Coverage dynamics

Insurance coverage is not a one-time decision–after the initial decision to enrol, households must decide whether to continue to pay their monthly premiums to remain covered at any given point in time. We now turn to the administrative data on premium payments to examine these monthly payment decisions. Figure 3 plots coverage by month and subsidy group since the offer date. 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. All households in the full subsidy group who successfully enrol are, therefore, covered for 12 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. Many enrolees quickly dropped coverage, however, as one-quarter of enroled control group households had stopped paying their premiums three months after enrolment, and nearly one-half of the enrolees in the no-subsidy group had stopped paying their premiums a year post-enrolment (Appendix Figure 1). The steady increase in coverage for the no-subsidy group in Figure 3 implies that the rate of new enrolments was large enough so that net coverage rates continued to increase despite the dropout effect.[24]

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 enroled in the first two months after the offer and their coverage remained constant during the first year when the subsidies were active.[25]

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

[24] The steady increase in enrolment of the no-subsidy group throughout the study period is in line with the number of enrolees going from ap proximately 10 million in January 2015 to more than 15 million in January 2016.

[25] 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 enroled after the subsidy period was over.

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 after the offer date.[26] The fact that those brought in with the temporary full subsidy stayed enroled in the second year suggests a strong "experience effect," that is, that these individuals may not have understood the benefits of insurance until they experienced it. This implies that temporary subsidies can help boost enrolment past their expiration date and may be an important tool in boosting insurance coverage in low-enrolment 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 summarises the coverage patterns in Figure 3.[27] In Column 1, we report the percentage of households that enroled 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 enroled within one 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 three 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 (β_1=0 and β_2=0), as well as whether the assisted-registration coverage rates differ from the status quo registration (β_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 withdrawn. 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. 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.

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[26] Appendix Figure 1 shows the coverage rate for the sample of those who enroled in the first year, by month of enrolment. There is 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.

[27] 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 13, so that each treatment group shown has the same (weighted) combination of subtreatments; that is, half subsidy has the same weighted mix of status quo vs. assisted Internet registration as full subsidy, and so on.

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 subsidised 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.

Section Four:

Selection Impacts and Their Implications for Government Costs

4.1 Impacts on selection

Subsidies are a textbook response to concerns about adverse selection since in standard models they will induce lower-cost individuals to enrol. To examine the types of people who enrol under different intervention arms, we draw on two sources of data: (i) self-reported health from the baseline survey; and (ii) administrative claims data among those who enroled. While these two measures capture different objects–namely, health and health care usage–perhaps, not surprisingly, enrolees with better self-reported health indeed tend to have fewer claims (see Appendix Table 6).

Table 4 shows various measures of health and health care use for those who enroled and had coverage for at least one month during the first year (that is, as measured in Column 1 of Table 3).[28] Column 1 indicates that the marginal household that received coverage in response to the subsidies had a higher level of self-reported health at baseline than enrolees in the no-subsidy group. Those enroling with the subsidies had an average self-reported health score that is about 4.5 percent higher than that of no-subsidy enrolees, 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.[29]

The remaining columns of Table 4 examine health care usage of households that enroled and had coverage for at least one month during the first year. We examine all claims for the 12 months after the enrolment date. By examining a fixed number of months since enrolment date regardless of when households enroled, we can abstract from the feature that temporary subsidies may drive households to enrol earlier in a calendar year, thereby mechanically affecting length of insurance coverage. We focus on three main indicators: (i) whether the household had any claim (Column 2); (ii) the total number of visits made (Column 6); and (iii) 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 which can provide greater precision than the value of claims which tend to have a large right tail (Aron-Dine et al. 2015).

[28] The regressions that calculate these p-values are provided in Appendix Table 7.

[29] Appendix Table 8 shows that the results also hold if self-reported health is measured as the self-reported health of the least healthy family member. In addition, households that enroled under the full-subsidy treatment were also less likely to have a family member over 60 years of age.

Consistent with the results on self-reported health in Column 1, the claims analysis in the remaining columns also indicates that those who enroled under the full subsidy were healthier and lower-cost. Households in the full-subsidy group were also 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, the full-subsidy group were less likely to lodge "large claims" that suggest a substantial health emergency. This is shown in Figure 4 that reports the probability distribution function of the value of inpatient claims submitted within 12 months since enrolment by treatment status for those who enroled within one 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 subsidised 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, waited 30 percent longer before submitting their first claim (Column 11; *p*-value 0.006).

4.2 Dynamics and selection

An important question is whether the fact that households can time enrolment and dropout decisions exacerbates adverse selection. We investigate both: (i) whether households in the no-subsidy group who do not face a time-limited enrolment period are more likely to time enrolment to when they are likely to have a claim; and (ii) how those who choose to retain coverage differ from those who drop. Figure 5 begins by plotting the number of claims by month since enrolment, separately by subsidy treatment groups among households who enroled within one year since offer and had coverage for at least one month over that period, along with 95 percent confidence intervals.

Those who enroled without the subsidy appear to have submitted more claims in the first few months upon enrolment than did the households in the full-subsidy group. Over time this difference became less stark, however, 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.[30] Combined with the payments findings in the previous section (that is, Figure 3), this suggests that no-subsidy households may have had large claims once they enroled but then stopped paying premiums (that is, 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 and several results are worth highlighting. For each treatment, we divide those who enroled 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.

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.[31] 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.

4.3 Implications for government costs

The selection patterns indicate that the subsidies brought in healthier enrolees, while the coverage dynamics indicate that not only were no-subsidy enrolees sicker and higher-cost but that they strategically timed enrolment to coincide with major health expenditures and were quicker to drop coverage (that is, stopped paying premiums) after a few months. In Table 6, we examine the implications of these results for the net costs to the government. The results indicate that the subsidies covered more households at similar cost per covered household.[32]

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[30] Appendix Table 9 formally confirms this result. In the first three months that the households were enroled in insurance (Panel A), full-subsidy households were less likely to submit inpatient or outpatient claims, had fewer 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 12 after enrolment (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.

[31] 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.

[32] 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.

Table 6 shows net revenues with and without accounting for capitation payments by month, which can be decomposed into revenues[33] from premiums and government expenditures as a result of claims.[34] 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. As an alternative way of presenting the same results, Columns 6 through 9 report the results for all households in the sample regardless of whether they enroled, 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.

While the subsidy was active, on net the government lost around IDR 125,000 (~US$9.00) per household-month covered in the no-subsidy group (Column 5, Panel A). By comparison, over this same period, on net the government lost only about IDR 50,000 (~US$3.50) per household-month covered in the full-subsidy group. In other words, the net cost to the government per covered household-month in the subsidy group was no higher than in the no-subsidy group (*p*-value = 0.19), *even taking into account that the government received essentially no revenue from the subsidy group*. This is because the decline in average claims 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).[35] 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 so this policy does entail an increase in the total amount spent by the government. Looking over the entire sample (that is, not conditioning on enrolment), Column 9 indicates that in the no-subsidy group the government cost was IDR 3,000 (~US$0.20) per eligible household per month, while with the full-subsidy the government cost was IDR 6,000 (~US$0.40) per eligible household per month.

Panel B explores what happened in the year *after* the subsidies were withdrawn–as shown in Table 3, there was a persistent increase in coverage in the full-subsidy group. Table 5 shows, however, that despite being healthier initially, households in the full-subsidy group that retained coverage (that is, 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. As a result, those who retained coverage had similar average claims after the subsidy ended to those in the no-subsidy group.

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[33] Revenues are defined as premiums paid by enrolees. 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 enroled after the time period the subsidy offer was in effect and, therefore, had to pay premiums.

[34] Capitation payments depend on the number of enrolees who declare the facility as their primary provider, the total number of practitioners, the ratio of practitioners to beneficiaries, and operating hours. These range between IDR 3,000-6,000 per enrolee for puskesmas and IDR 8,000-10,000 for clinics. Given that approximately 80 percent of JKN Mandiri enrolees 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 enrolee per month. Capitation payments are only paid to health care facilities in months in which the household paid the premium.

[35] 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.

On net, 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 (Table 6, Panel B, Column 5). Nevertheless, the point estimates suggest that the government lost about half as much per covered household-month in the full-subsidy group compared to the no-subsidy group (IDR 47,000 in net government costs per covered household-month in the full-subsidy group; IDR 102,000 in net government costs per covered household-month in the no-subsidy group). In the year after the temporary full subsidy ended, we, therefore, 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). Looking over the entire sample (that is, not conditioning on enrolment), Column 9 indicates virtually identical government expenditures (about IDR 5,000 per person) 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 by offering a temporary full subsidy to a given population. In the year the subsidy was in effect, the government doubled its net budgetary contribution for this population (from IDR 3,000 to IDR 6,000 per person 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 person in the population–regardless of treatment. But the full-subsidy group had, on net, 58 percent more people 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.

Section Five:

Conclusions

As incomes have risen in emerging economies, there has been a growing move to increase coverage of social insurance programs, however, insurance mandates can be difficult to enforce. We examine the impact of temporary insurance subsidies–which must be taken up within one month of offer and only last one year–reduced hassle costs, and information provision on insurance coverage in a mandated insurance setting.

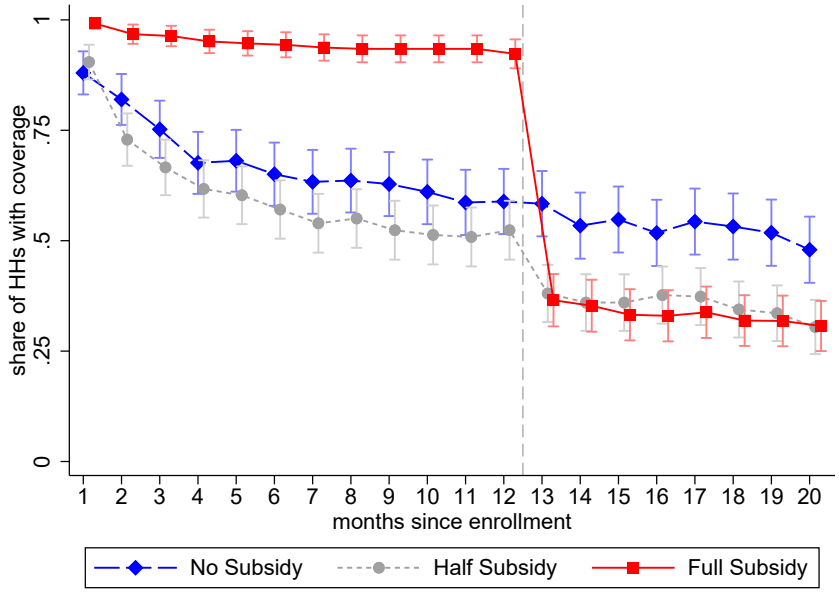
We find that offering a full, but temporary, subsidy was effective at increasing enrolment and helped to attract healthier enrolees. Because of the healthier selection and also the strategic dynamic adjustment of coverage and claims in the no-subsidy group–the no-subsidy group timed its enrolment to 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, subsidies induced higher enrolment even after they 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.

At the same time, however, our findings also highlight challenges that governments face when aiming to achieve universal health coverage through a contributory system.

While both subsidies and assisted enrolment increased enrolment rates, even the most aggressive interventions–a full subsidy for a year and Internet-assisted enrolment–only led to 30 percent enrolment. This is a substantial increase from the 8 percent enrolment in the status quo group but is far short of universal enrolment. Some of this reflects administrative challenges: almost 60 percent of households in the full subsidy, Internet-assisted registration treatment tried to enrol–double the numberr who actually did so. This underscores how weak social insurance infrastructure (in this case, the underlying social registry) can create obstacles to universal enrolment and suggests that long-term solutions to universal coverage are only feasible through strengthening overall administrative structures.

Appendix

Appendix Figure 1: Insurance Coverage, by Month since Enrollment and Subsidy Treatment



Note: This figure shows mean insurance coverage by month since enrollment for households who enrolled under different subsidy treatments, 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 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.

Appendix Table 1: Randomization Balance

	Has NIK	Self-reported health	Outpatient	Inpatient	Any chronic	Family member 60+	HH finished highschool	HH employed	HH size
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Full subsidy	-0.001	0.028	0.009	0.010	0.021	-0.029	-0.008	-0.001	0.155**
	(0.017)	(0.024)	(0.022)	(0.012)	(0.022)	(0.019)	(0.023)	(0.013)	(0.067)
Half subsidy	0.021	0.015	-0.022	0.008	0.016	0.006	0.031	-0.014	0.002
	(0.014)	(0.020)	(0.019)	(0.010)	(0.019)	(0.016)	(0.019)	(0.012)	(0.055)
Assisted internet registration	0.001	-0.011	0.007	0.011	0.002	-0.003	-0.004	0.000	0.002
	(0.011)	(0.015)	(0.014)	(0.008)	(0.014)	(0.012)	(0.014)	(0.009)	(0.042)
Information on cost of	-0.001	-0.030	-0.002	0.005	0.021	0.034	-0.035	0.040*	0.006
treatment for heart attack	(0.026)	(0.037)	(0.034)	(0.020)	(0.033)	(0.031)	(0.035)	(0.021)	(0.111)
Information on possible	-0.003	-0.015	-0.021	-0.002	-0.009	0.004	0.040***	-0.012	0.014
mandate penalties	(0.011)	(0.016)	(0.015)	(0.008)	(0.015)	(0.012)	(0.015)	(0.009)	(0.042)
Information on two weeks	0.004	0.039**	-0.004	-0.008	-0.026*	0.004	-0.014	0.009	0.085**
waiting period	(0.011)	(0.016)	(0.015)	(0.008)	(0.015)	(0.012)	(0.015)	(0.009)	(0.042)
Observations	5996	5964	5964	5964	5964	5996	5964	5996	5964

Note: This table shows covariate balance across subsidies, registration and information treatment assignment. We regress each of the outcomes on indicator variables for treatment assignment, an indicator variable for the randomization procedure used and an indicator variable for the study location (equation (1)). All regressions are estimated by OLS and weighted to reflect the intended randomization. Robust standard errors are reported in parentheses. All data is from the baseline survey. The smaller sample size for some outcomes is explained by households participating in the listing and treatment assignment, but refusing to complete the baseline survey. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 2a: Effect of Temporary Subsidies and Assisted Internet Registration on Year 1 Enrollment, Medan

		Decomposition		
	Enrolled within 1 year	Attempted to enroll within 8 weeks of offer date	Enrolled within 8 weeks of offer date	Enrolled after 8 weeks, but within 1 year of offer date
	(1)	(2)	(3)	(4)
Panel A: Main effects				
Full subsidy	0.200***	0.319***	0.228***	-0.027
	(0.040)	(0.040)	(0.036)	(0.022)
Half subsidy	0.131***	0.199***	0.130***	0.002
	(0.033)	(0.040)	(0.028)	(0.020)
Assisted internet registration	0.019	0.371***	0.024	-0.005
	(0.028)	(0.029)	(0.025)	(0.016)
No subsidy mean	0.075	0.140	0.017	0.058
P-value of test of hypothesis				
Half subsidy = full subsidy	0.085	0.004	0.008	0.169
Assisted internet registration = full subsidy	0.001	0.328	0.000	0.451
Panel B: Interacted specification				
Full subsidy and assisted internet registration	0.195***	0.649***	0.222***	-0.027
	(0.049)	(0.044)	(0.042)	(0.029)
Full subsidy and status quo registration	0.228***	0.247***	0.240***	-0.013
	(0.055)	(0.049)	(0.047)	(0.033)
Half subsidy and assisted internet registration	0.176***	0.555***	0.175***	0.002
	(0.057)	(0.063)	(0.048)	(0.036)
Half subsidy and status quo registration	0.106**	0.100***	0.089***	0.017
	(0.042)	(0.034)	(0.032)	(0.030)
No subsidy and assisted internet registration	0.022	0.258***	0.007	0.015
	(0.030)	(0.028)	(0.015)	(0.027)
No subsidy, status quo registration mean	0.064	0.013	0.013	0.051

Note: This table shows the effect of subsidies and assisted internet registration on enrollment in year 1 in Medan. The sample size is 1446 households. In Panel A, we regress each outcome on indicator variables for treatment assignment, 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 and status quo registration for the assisted internet registration treatment. The p-values reported are from a test of the difference between the half subsidy and full subsidy treatments ($\beta_1 = \beta_2$) and assisted internet registration and full subsidy treatments ($\beta_1 = \beta_3$). Panel B shows the effect of the interacted treatments on enrollment in year 1. 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.

Appendix Table 2b: Effect of Temporary Subsidies and Assisted Internet Registration on Year 1 Enrollment, Bandung

		Decomposition		
	Enrolled within 1 year	Attempted to enroll within 8 weeks of offer date	Enrolled within 8 weeks of offer date	Enrolled after 8 weeks, but within 1 year of offer date
	(1)	(2)	(3)	(4)
Panel A: Main effects				
Full subsidy	0.188***	0.263***	0.202***	-0.014
	(0.022)	(0.023)	(0.021)	(0.011)
Half subsidy	0.091***	0.153***	0.112***	-0.021***
	(0.016)	(0.017)	(0.014)	(0.008)
Assisted internet registration	0.040***	0.194***	0.049***	-0.010
	(0.011)	(0.011)	(0.009)	(0.007)
No subsidy mean	0.088	0.090	0.033	0.055
P-value of test of hypothesis				
Half subsidy = full subsidy	0.000	0.000	0.000	0.560
Assisted internet registration = full subsidy	0.000	0.006	0.000	0.739
Panel B: Interacted specification				
Full subsidy and assisted internet registration	0.241***	0.494***	0.265***	-0.024
	(0.033)	(0.034)	(0.031)	(0.015)
Full subsidy and status quo registration	0.145***	0.163***	0.163***	-0.018
	(0.030)	(0.026)	(0.026)	(0.018)
Half subsidy and assisted internet registration	0.121***	0.345***	0.160***	-0.039***
	(0.026)	(0.029)	(0.024)	(0.010)
Half subsidy and status quo registration	0.075***	0.101***	0.091***	-0.016
	(0.018)	(0.015)	(0.015)	(0.012)
No subsidy and assisted internet registration	0.014	0.140***	0.027***	-0.013
	(0.012)	(0.012)	(0.008)	(0.010)
No subsidy, status quo registration mean	0.081	0.019	0.019	0.062

Note: This table shows the effect of subsidies and assisted internet registration on enrollment in year 1 in Bandung. The sample size is 4550 households. In Panel A, we regress each outcome on indicator variables for treatment assignment, 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 and status quo registration for the assisted internet registration treatment. The p-values reported are from a test of the difference between the half subsidy and full subsidy treatments ($\beta_1 = \beta_2$) and assisted internet registration and full subsidy treatments ($\beta_1 = \beta_3$). Panel B shows the effect of the interacted treatments on enrollment in year 1. 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.

Appendix Table 3: Reasons for Failing to Enroll

	N	%
	(1)	(2)
No reason reported	6	1.060
Technical reasons (internet, website)	21	3.710
Family card issues	468	82.686
Family card not registered in the online system	11	2.350
Family already has insurance according to the online system	80	17.094
Family card does not match the family members listed in the online system	91	19.444
Other family card issues	286	61.111
Other issues	71	12.544

Note: The sample includes households assigned to assisted internet registration treatment that attempted to enroll within six weeks from offer date but failed to complete the registration process. Data is from the enumerator forms that capture the enrollment process.

Appendix Table 4: Effect of Additional Treatments on Year 1 Enrollment, by City

		Decomposition		
	Enrolled within 1 year	Attempted to enroll within 8 weeks of offer date	Enrolled within 8 weeks of offer date	Enrolled after 8 weeks, but within 1 year of offer date
	(1)	(2)	(3)	(4)
	Panel A: Medan			
Two week deadline	0.048	0.012	0.047	0.001
	(0.045)	(0.047)	(0.044)	(0.020)
Choice between one or two week deadline	0.031	0.023	0.001	0.030
	(0.048)	(0.051)	(0.043)	(0.028)
No subsidy mean	0.075	0.140	0.017	0.058
	Panel B: Bandung			
Bonus subsidy	0.037***	0.061***	0.040***	-0.003
	(0.013)	(0.013)	(0.010)	(0.009)
No subsidy mean	0.088	0.090	0.033	0.055

Note: This table shows the effect of the deadline and the bonus subsidy treatment on enrollment in year 1, by city. The sample size is 1446 households in Medan and 4550 households in Bandung. We regress each of the enrollment measures on indicator variables for treatment assignment and an indicator variable for the randomization procedure used (equation (1)). The omitted category is one week deadline for the deadline treatment and no subsidy for the bonus subsidy 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.

Appendix Table 5: Insurance Coverage, by Temporary Subsidies and Assisted Internet Registration

	Enrolled within 1 year of offer date							
		Dropouts	Stayers					
	Had coverage for at least 1 month	Did not have coverage in month 15	Had coverage in month 15	P-Value (2) vs (3)	Had coverage in month 15	P-Value (1) vs (5)	Had coverage in month 20	P-Value (1) vs (7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full subsidy	0.200***	0.142***	0.058***	0.153***	0.048***	0.208***	0.045***	0.210***
	(0.019)	(0.017)	(0.012)	(0.015)	(0.013)	(0.018)	(0.013)	(0.018)
Full subsidy interaction				-0.105***		-0.168***		-0.174***
				(0.020)		(0.016)		(0.016)
Half subsidy	0.100***	0.073***	0.027***	0.078***	0.022**	0.104***	0.010	0.106***
	(0.014)	(0.011)	(0.009)	(0.011)	(0.010)	(0.014)	(0.010)	(0.014)
Half subsidy interaction				-0.057***		-0.087***		-0.101***
				(0.016)		(0.014)		(0.015)
Assisted internet registration	0.022**	0.022***	-0.000	0.022***	0.001	0.022**	-0.008	0.022**
	(0.010)	(0.008)	(0.007)	(0.008)	(0.007)	(0.010)	(0.008)	(0.010)
Assisted internet registration interaction				-0.023**		-0.021**		-0.030***
				(0.011)		(0.009)		(0.010)
Observations	5996	5996	5996	11992	5996	11992	5996	11992
No subsidy mean	0.063	0.024	0.038	0.031	0.053	0.058	0.067	0.065

Note: This table shows insurance coverage by temporary subsidies and assisted internet registration. A household is considered as having insurance coverage if the premium was paid for all its members. In Columns (1), (2), (3), (5), and (7) we regress each outcome on indicator variables for treatment assignment, an indicator variable for the randomization procedure used and an indicator variable for study location (equation (1)). Column (4), Column (6), and (8) report coefficients from regressions in which we stack the outcomes being compared and regress them on indicator variables for treatment assignment, the interaction of these indicators with an indicator for the outcome the observation refers to, an indicator variable for the randomization procedure used and an indicator variable for study location. All regressions are estimated by OLS and weighted to reflect the intended randomization. Robust standard errors are reported in parentheses in Columns (1)-(3), (5), and (7) and standard errors clustered at the household level are reported in parentheses in Columns (4), (6), and (8). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 6: Relationship between Self-Reported Health and Year 1 Health-Seeking Behavior

	Had a claim				Total # of visits				Claims	
	Of any type	Outpatient	Inpatient	Chronic	Of any type	Outpatient	Inpatient	Chronic	Value of claims	Days to first claim
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Self-reported health	-0.091**	-0.096**	-0.054*	-0.090**	-1.040	-0.884	-0.156	-0.093*	-0.885*	22.701*
	(0.040)	(0.040)	(0.033)	(0.037)	(0.696)	(0.645)	(0.097)	(0.054)	(0.503)	(12.561)
R2	0.035	0.040	0.027	0.027	0.028	0.027	0.032	0.025	0.035	0.044

Note: This table shows the coefficients from a regression of claims in months 1 to 12 since enrollment date on self-reported health. For self-reported health, higher values correspond to better self-reported health. The sample is restricted to households who enrolled within a year from offer date and had coverage for at least one month over the same time period. The sample size is 749 households. The value of claims in Column (8) is winsorized at the 99% level and only refers to hospital claims. Each regression additionally controls for indicator variables for treatment assignment, an indicator variable for the randomization procedure used and an indicator variable for the study location (equation (1)). 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.

Appendix Table 7: Self-Reported Health and Claims in 12 Months since Enrollment, by Temporary Subsidies and Assisted Internet Registration

	Self-reported health	Had a claim				Total # of visits				Claims	
		Of any type	Outpatient	Inpatient	Chronic	Of any type	Outpatient	Inpatient	Chronic	Value of claims	Days to first claim
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Full subsidy	0.138**	-0.118**	-0.119**	-0.051	-0.089*	-2.115**	-2.022**	-0.092	-0.146**	-0.706*	47.159***
	(0.057)	(0.058)	(0.058)	(0.047)	(0.051)	(0.940)	(0.898)	(0.083)	(0.069)	(0.423)	(17.185)
Half subsidy	0.146**	-0.084	-0.080	0.028	-0.024	-0.986	-1.043	0.057	-0.030	0.237	28.996
	(0.060)	(0.059)	(0.059)	(0.046)	(0.051)	(1.082)	(1.044)	(0.081)	(0.071)	(0.484)	(17.963)
Assisted internet registration	0.076*	-0.033	-0.036	-0.026	0.009	-1.000	-0.984	-0.016	-0.010	-0.173	14.433
	(0.044)	(0.044)	(0.044)	(0.033)	(0.033)	(0.636)	(0.610)	(0.059)	(0.044)	(0.309)	(13.018)
No subsidy mean	3.099	0.622	0.612	0.181	0.272	6.167	5.906	0.262	0.339	1.637	176.259

Note: This table shows self-reported health and claims submitted in months 1 to 12 since one's enrollment date by temporary subsidies and assisted internet registration. The sample is restricted to households who enrolled within a year from offer date and had coverage for at least one month over the same time period. The sample size is 749 households. In Column (1), higher values of the outcome correspond to better self-reported health. The value of claims in Column (10) is winsorized at the 99% level and only refers to hospital claims. Each regression additionally controls for indicator variables for treatment assignment, an indicator variable for the randomization procedure used and an indicator variable for the study location (equation (1)). 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.

Appendix Table 8: Self-Reported Health and Family Composition of Enrolled Households, by Subsidy and Assisted Internet Registration Treatments

	Self-reported health, min	Family member over 60
	(1)	(2)
Full subsidy	2.895	0.182
	[0.651]	[0.386]
Half subsidy	2.956	0.281
	[0.697]	[0.451]
No subsidy	2.801	0.259
	[0.750]	[0.440]
Assisted internet registration	2.927	0.231
	[0.686]	[0.422]
Status quo registration	2.821	0.244
	[0.714]	[0.430]
P-value of test of hypothesis		
Full subsidy = no subsidy	0.060	0.073
Half subsidy = no subsidy	0.019	0.770
Assisted internet registration = status quo	0.051	0.709

Note: This table shows the effect of subsidies and assisted internet registration on the minimum self-reported health across household members and family composition. 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), self-reported health is defined as the minimum self-reported health of all family members and higher values of the outcome correspond to better self-reported health. We regress each outcome on indicator variables for treatment assignment, 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 ($\beta_3 = 0$) and between the status quo and assisted internet registration treatments ($\beta_4 = 0$). All regressions are estimated by OLS and weighted to reflect the intended randomization.

Appendix Table 9: Effect of Temporary Subsidies and Assisted Internet Registration on Claims, Disaggregated by First Quarter versus Rest of the Year

	Had a claim				Total # of visits				Claims	
	Of any type	Outpatient	Inpatient	Chronic	Of any type	Outpatient	Inpatient	Chronic	Value of claims	Days to first claim
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Months 1 to 3 since enrollment date										
Full subsidy	-0.166***	-0.169***	-0.035	-0.047	-0.965***	-0.896***	-0.069	-0.047	-0.380	13.883***
	(0.055)	(0.054)	(0.028)	(0.035)	(0.357)	(0.334)	(0.053)	(0.041)	(0.233)	(3.759)
Half subsidy	-0.132**	-0.127**	0.001	0.010	-0.229	-0.255	0.026	0.034	0.050	9.373**
	(0.055)	(0.055)	(0.029)	(0.035)	(0.375)	(0.354)	(0.047)	(0.042)	(0.241)	(3.903)
Assisted internet registration	-0.044	-0.043	-0.018	-0.001	-0.318	-0.327*	0.009	0.003	0.066	3.459
	(0.040)	(0.039)	(0.020)	(0.022)	(0.216)	(0.197)	(0.040)	(0.028)	(0.166)	(2.797)
No subsidy mean	0.452	0.448	0.080	0.108	1.782	1.687	0.095	0.113	0.597	59.021
Panel B: Months 4 to 12 since enrollment date										
Full subsidy	-0.072	-0.051	-0.021	-0.078	-1.150	-1.126	-0.023	-0.110**	-0.237	
	(0.058)	(0.058)	(0.042)	(0.048)	(0.752)	(0.726)	(0.060)	(0.054)	(0.284)	
Half subsidy	-0.063	-0.052	0.027	-0.037	-0.757	-0.788	0.031	-0.047	0.120	
	(0.058)	(0.058)	(0.041)	(0.049)	(0.863)	(0.838)	(0.063)	(0.058)	(0.345)	
Assisted internet registration	-0.061	-0.061	-0.021	-0.003	-0.682	-0.657	-0.025	-0.016	-0.278	
	(0.042)	(0.041)	(0.029)	(0.031)	(0.518)	(0.501)	(0.041)	(0.035)	(0.203)	
No subsidy mean	0.527	0.498	0.127	0.236	4.385	4.219	0.166	0.274	1.012	

Note: This table shows the effect of subsidies and assisted internet registration on claims submitted for months 1 to 3 since enrollment date (Panel A) and from month 4 to 12 since enrollment date (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. The value of claims in Column (9) is winsorized at the 99% level and only refers to hospital claims. We regress each outcome on indicator variables for treatment assignment, 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. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 10: Year 1 Claims by Retention in Year 2, by Assisted Internet Registration Treatment

	Self-reported health	Had a claim				Total # of visits				Claims	
		Of any type	Outpatient	Inpatient	Chronic	Of any type	Outpatient	Inpatient	Chronic	Value of claims	Days to first claim
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Assisted internet registration											
Dropouts	3.233	0.405	0.376	0.185	0.148	2.639	2.369	0.270	0.169	1.419	241.731
	[0.504]	[0.492]	[0.485]	[0.390]	[0.356]	[5.101]	[4.768]	[0.686]	[0.427]	[3.847]	[145.876]
Stayers	3.193	0.697	0.685	0.146	0.338	6.556	6.317	0.239	0.388	1.289	176.465
	[0.551]	[0.461]	[0.466]	[0.354]	[0.474]	[9.366]	[9.153]	[0.711]	[0.590]	[3.426]	[141.792]
P-value of test of hypothesis											
Dropouts = stayers	0.491	0.000	0.000	0.359	0.000	0.000	0.000	0.716	0.000	0.769	0.000
Panel B: Status quo registration											
Dropouts	3.173	0.485	0.461	0.167	0.184	3.069	2.867	0.201	0.221	0.932	227.274
	[0.467]	[0.501]	[0.500]	[0.374]	[0.388]	[5.456]	[5.277]	[0.499]	[0.498]	[2.637]	[144.664]
Stayers	3.125	0.627	0.614	0.230	0.245	7.330	6.983	0.347	0.313	2.118	173.817
	[0.542]	[0.485]	[0.488]	[0.422]	[0.432]	[12.985]	[12.620]	[0.800]	[0.657]	[4.719]	[152.519]
P-value of test of hypothesis											
Dropouts = stayers	0.424	0.016	0.008	0.173	0.187	0.000	0.000	0.062	0.156	0.005	0.002

Note: This table shows mean self-reported health and claims in the first year since enrollment, separately by registration treatment 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), higher values of the outcome correspond to better self-reported health. The value of claims in Column (10) is winsorized at the 99% level and only refers to hospital claims. The p-values are from an interacted specification where the outcome is regressed on indicator variables for treatment assignment interacted with an indicator variable for whether the household has coverage in month 15, an indicator variable for the randomization procedure used and an indicator variable for study location. All regressions are estimated by OLS and weighted to reflect the intended randomization. Appendix Table 11 provides the regression estimates behind the numbers reported in this table.

Appendix Table 11: Year 1 Claims by Retention in Year 2, by Temporary Subsidy and Assisted Internet Registration Treatment

	Self-reported health	Had a claim				Total # of visits				Claims	
		Of any type	Outpatient	Inpatient	Chronic	Of any type	Outpatient	Inpatient	Chronic	Value of claims	Days to first claim
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Full subsidy	0.028	-0.093	-0.099	-0.143**	-0.069	-0.711	-0.544	-0.167*	-0.165	-1.188**	45.628*
	(0.079)	(0.079)	(0.079)	(0.071)	(0.074)	(0.964)	(0.903)	(0.097)	(0.110)	(0.594)	(23.850)
Full subsidy interaction	0.239**	0.038	0.050	0.182**	-0.013	-1.370	-1.544	0.173	0.066	0.989	-17.912
	(0.107)	(0.108)	(0.108)	(0.089)	(0.095)	(1.702)	(1.625)	(0.141)	(0.132)	(0.777)	(32.717)
Half subsidy	0.075	-0.080	-0.077	-0.044	-0.056	-0.357	-0.368	0.012	-0.110	-0.282	33.343
	(0.088)	(0.084)	(0.084)	(0.075)	(0.074)	(1.009)	(0.938)	(0.113)	(0.111)	(0.699)	(26.055)
Half subsidy interaction	0.122	0.098	0.101	0.118	0.135	0.638	0.566	0.073	0.237	0.960	-32.530
	(0.122)	(0.121)	(0.121)	(0.096)	(0.108)	(2.030)	(1.942)	(0.176)	(0.154)	(1.014)	(36.749)
Assisted internet registration	0.070	-0.087	-0.092	0.018	-0.031	-0.528	-0.592	0.064	-0.048	0.416	17.612
	(0.056)	(0.060)	(0.060)	(0.044)	(0.042)	(0.599)	(0.562)	(0.076)	(0.055)	(0.385)	(17.767)
Assisted internet registration interaction	0.017	0.170*	0.178**	-0.089	0.126*	-0.089	0.070	-0.158	0.133	-1.139*	-20.585
	(0.087)	(0.088)	(0.088)	(0.062)	(0.071)	(1.325)	(1.273)	(0.115)	(0.089)	(0.590)	(26.106)

Note: This table shows the difference in self-reported health and claims by treatment and by whether households kept or dropped coverage at month 15 since offer date. 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), higher values of the outcome correspond to better self-reported health. The value of claims in Column (10) is winsorized at the 99% level and only refers to hospital claims. We regress each outcome on indicator variables for treatment assignment, the interaction of these indicators with an indicator whether the household retained coverage at month 15, an indicator variable for the randomization procedure used and an indicator variable for study location. 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.

Appendix Table 12: Expenditures and Revenues, by Assisted Internet Registration

		Per covered household-month				Per household-month			
	Coverage	Revenues	Claims expenditures	Net revenues	Net revenues including capitation	Revenues	Claims expenditures	Net revenues	Net revenues including capitation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Panel A: Months 1 to 12 since offer date					
Assisted internet registration	1.065	0.032	0.083	-0.051	-0.072	0.003	0.007	-0.004	-0.006
	[3.094]	[0.042]	[0.723]	[0.723]	[0.723]	[0.016]	[0.217]	[0.216]	[0.216]
Status quo registration	0.862	0.033	0.126	-0.093	-0.113	0.003	0.009	-0.006	-0.008
	[2.759]	[0.046]	[1.248]	[1.243]	[1.243]	[0.016]	[0.336]	[0.334]	[0.334]
Observations	5996	5558	5558	5558	5558	71952	71952	71952	71952
				P-value of test of hypothesis					
Status quo = assisted	0.032	0.230	0.357	0.394	0.407	0.128	0.616	0.504	0.586
				Panel B: Months 13 to 20 since offer date					
Assisted internet registration	0.586	0.070	0.161	-0.091	-0.110	0.006	0.012	-0.006	-0.007
	[1.901]	[0.047]	[1.517]	[1.516]	[1.516]	[0.023]	[0.413]	[0.411]	[0.411]
Status quo registration	0.606	0.067	0.110	-0.043	-0.061	0.005	0.008	-0.003	-0.004
	[1.926]	[0.055]	[0.904]	[0.903]	[0.904]	[0.024]	[0.251]	[0.249]	[0.249]
Observations	5996	3565	3565	3565	3565	47968	47968	47968	47968
				P-value of test of hypothesis					
Status quo = assisted	0.669	0.397	0.198	0.222	0.210	0.505	0.371	0.418	0.411

Note: This table shows mean revenues and expenditures by assisted internet registration, for months 1 to 12 from offer (Panel A) and for months 13 to 20 from offer (Panel B). Column (1) reports mean number of months with insurance coverage. Means are weighted to reflect the intended randomization. Standard deviations are in brackets. Observations are at the household level. Columns (2) to (5) and (6) to (9) show mean net revenues, net revenues including capitation payments, revenues (premiums paid by enrollees) and expenditures (total value of claims) in millions IDR for household-months in which households had coverage and for all household-months. Observations are at the household-month level. The value of claims in Columns (5) and (9) 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 treatment assignment, 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 (9) standard errors are clustered at the household level. The p-values reported are from a test of the difference between the status quo and assisted internet registration treatments ($\beta_3 = 0$). All regressions are estimated by OLS and weighted to reflect the intended randomization. Appendix Table 13 provides the regression estimates behind the numbers reported in Appendix Table 12. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 13: Expenditures and Revenues, by Temporary Subsidies and Assisted Internet Registration

		Per covered household-month				Per household-month			
	Coverage	Net revenues	Net revenues including capitation	Revenues	Claims expenditures	Net revenues	Net revenues including capitation	Revenues	Claims expenditures
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Panel A: Months 1 to 12 since offer date					
Full subsidy	0.200***	0.111	0.105	-0.071***	-0.182**	-0.001	-0.006	-0.001***	-0.001
	(0.019)	(0.081)	(0.081)	(0.004)	(0.081)	(0.005)	(0.005)	(0.000)	(0.005)
Half subsidy	0.100***	-0.026	-0.028	-0.032***	-0.006	-0.009*	-0.010**	0.002***	0.011**
	(0.014)	(0.090)	(0.090)	(0.005)	(0.091)	(0.005)	(0.005)	(0.000)	(0.005)
Assisted internet registration	0.022**	0.039	0.037	-0.003	-0.042	0.002	0.002	0.001	-0.002
	(0.010)	(0.045)	(0.045)	(0.003)	(0.045)	(0.003)	(0.003)	(0.000)	(0.003)
Observations	5996	5558	5558	5558	5558	71952	71952	71952	71952
No subsidy mean	0.063	-0.108	-0.125	0.075	0.183	-0.003	-0.003	0.002	0.005
				Panel B: Months 13 to 20 since offer date					
Full subsidy	0.045***	0.019	0.014	0.003	-0.016	-0.002	-0.004	0.005***	0.007
	(0.013)	(0.056)	(0.056)	(0.006)	(0.055)	(0.004)	(0.004)	(0.001)	(0.005)
Half subsidy	0.010	-0.068	-0.071	0.005	0.073	-0.006	-0.006	0.002***	0.008
	(0.010)	(0.083)	(0.083)	(0.005)	(0.083)	(0.006)	(0.006)	(0.001)	(0.006)
Assisted internet registration	-0.008	-0.063	-0.064	0.003	0.066	-0.003	-0.003	0.000	0.003
	(0.008)	(0.051)	(0.051)	(0.004)	(0.051)	(0.004)	(0.004)	(0.001)	(0.004)
Observations	5996	3565	3565	3565	3565	47968	47968	47968	47968
No subsidy mean	0.067	-0.085	-0.102	0.068	0.153	-0.005	-0.006	0.004	0.009

Note: This table shows the difference in revenues, coverage, and expenditures by subsidies and assisted internet registration. Column (1) reports mean number of months with insurance coverage. Observations are at the household level. Columns (2) to (5) and (6) to (9) show mean net revenues, net revenues including capitation payments, revenues (premiums paid by enrollees) and expenditures (total value of claims) in millions IDR for household-months in which households had coverage and for all household-months. Observations are at the household-month level. The value of claims in Columns (5) and (9) is winsorized at the 99% level and only refers to hospital claims. We regress each outcome on indicator variables for treatment assignment, 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. In Column (1) standard errors are robust, while in Columns (2) to (9) standard errors are clustered at the household level. *** p<0.01, ** p<0.05, * p<0.1.

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