Tangible Information and Citizen Empowerment

Identification Cards and Food Subsidy Programs in Indonesia[°]

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Abstract

Redistribution programs in developing countries often "leak" because local officials do not implement programs as the central government intends. We study one approach to reducing leakage. In an experiment in over 550 villages, we test whether mailing cards with program information to targeted beneficiaries increases the subsidy they receive from a subsidized rice program. On net, beneficiaries received 26 percent more subsidy in card villages. Ineligible households received no less, so this represents substantially lower leakage.

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I. Introduction

Throughout the developing world, governments face the problem of ensuring that their rules and laws are implemented as conceived by local officials who exercise significant discretion and whose interests may differ from those of the central government and/or from those of the local community.

Transfer programs, for example, typically have rules about eligibility, benefit amounts, application procedures, and the like, but in practice a local official will often have substantial leeway in interpreting these rules. Citizens may not know enough about program rules to effectively advocate for their rights under the program.[1] As such, many experts advocate providing greater information to citizens about their rights under different policies and programs in order to improve service delivery (World Bank 2004).

However, it is not clear that providing citizens more information will actually help: citizens may not be able use the information to demand more of their entitlement, local leaders may not care about citizen demand or complaints, or citizens may already have sufficient information to begin with.[2] Whether information empowers citizens is therefore an empirical question, but of the 16 experimental and quasi-experimental studies on transparency and accountability reviewed by Kosack and Fung (2014), only a few study the effects of providing just information.[3]

We experimentally test the effect of providing information to citizens in the context of Indonesia's "Raskin" program ("Rice for the Poor"). Raskin is designed – in theory – to

[1] As a result, leakages are common, both in government-run programs and those that are supported by foreign aid. For example, Niehaus, Atanassova, Bertrand, and Mullainathan (2013) show high leakage rates in India's public distribution system. Nunn and Qian (forthcoming) describe how much of the foreign-supplied food aid goes missing; for example, the UN World Food Program has reported that as much as half of their food aid sent to Somalia (about $485 million in 2009) went missing (New York Times, 3/9/10).

[2] In addition, providing more information even has the possibility of making things worse because reducing the possibility of future illicit rents may motivate a local official to steal more today. Niehaus and Sukhtankar (2013) describe these "golden goose" effects in the context of changes in citizen benefits in India's workfare program, NREGA.

[3] For example, Bjorkman and Svensson (2007) show large effects on health of a community-monitoring program that brought together community members and health care providers to discuss the health centers and create an action plan for change. Thus, it tests both information and coordination at the same time. Ravallion, van de Walle, Dutta, and Murgai (2013) find that a 25 minute video on NREGA that was shown in 40 villages in India (randomly chosen from 150) increased citizen knowledge, without impacting program outcomes. Other studies have tried to measure the effects using quasi-experimental designs. Reinikka and Svensson (2004, 2005) find that when the Ugandan government implemented a national advertising campaign, schools that were closer to a newspaper outlet received more of the advertised grant. Francken et al (2009) show an association between media access and leakages in public expenditures in Madagascar.

provide 15 kg of subsidized rice per month (about half of a household's rice consumption) to eligible households. With an annual budget of US$1.5 billion, and a targeted population of 17.5 million households, it is Indonesia's largest targeted transfer program. In practice, local officials often do not follow the national rules. In data we introduce below, we find that beneficiaries seldom receive their full entitlement and they pay 42 percent more than the official copay price—thus, on net, eligible households receive only about one-third of the intended subsidy.

Working with the Government of Indonesia, we designed a set of field experiments to provide information directly to eligible households. In 378 villages (randomly selected from among 572 villages spread over three provinces), the central government mailed "Raskin identification cards" to eligible households to inform them of their eligibility and the quantity of rice that they were entitled to. The government also experimentally varied how the card program was run along three key dimensions— whether an additional rule (the copay price) was also listed on the card, whether information about the beneficiaries was also made public, and whether cards were sent to all eligible households or only to a subset.

We surveyed both eligible and ineligible households in all villages, two months, eight months, and eighteen months after the cards were mailed. Since the cards could affect both the amount of rice received and the price, we focus on understanding the impacts on the total subsidy received, defined as the quantity of rice purchased multiplied by the difference between the market price of rice and the copay that the household paid.[4] We also measure individual beliefs about the program, as well as the protests and complaints to local leaders, to understand whether citizens gained and used the information.

On net, across all of the variations of the program, we find that the cards led to a large increase in subsidy received by eligible households. Eligible households in treatment villages received a 26 percent (std. error: 5 percent) increase in subsidy, stemming from both an increase in quantity and a decrease in the copay price.[5] Not only did eligible

[4]Welfare analysis generally focuses on prices rather than quantities because of the envelope-theorem idea that small changes in quantities do not matter. However, in the case, this logic is not appropriate: the price is about one-fifth of the market price and households have excess demand for rice at this price, implying that changes in both prices and quantities have first-order effects on welfare.

[5] This is the reduced form effect for eligible households (regardless of card receipt). The implied treatment-on-treated effect would be thrice as large, assuming no spillovers to those who did not receive a card.

households receive more rice, but ineligible ones in total received no less, implying that the cards reduced leakage by 1 kg (std. error: 0.46) to 1.6 kg (std. error: 0.55) per eligible household, which represents a 33 (std. error: 15 percent) to 58 percent (std. error: 27 percent) reduction in leakage. This occurred despite imperfect implementation: eligible households in treatment villages were only 30 percentage points more likely to have received a card relative to the control.

Importantly, the information listed on the card mattered: Printing the copay price nearly doubled the additional subsidy eligible households received relative to the effect of providing a card without the copay information.

If the intervention puts too much pressure on the local leaders to reduce leakage and satisfy all eligible households, they might be unable or unwilling to implement the program at all.[6] The central government therefore also implemented an alternative intervention where the cards were only mailed to the bottom decile of households, rather than the bottom 30 percent who are typically eligible, thereby offering the leader more "flexibility" in his or her decisions.[7] This treatment arm was no more effective than full distribution of the cards: Households who received cards experienced the same increase in subsidy regardless of whether everyone else received them. Eligible households that were assigned not to receive a card – and ineligible households – saw no change in subsidy relative to in the control areas.

Another reason why information could be counterproductive is that deviations from program rules may have been for purely altruistic reasons. The government's list of eligible households is known to be imperfect and socially-minded village leaders may deliberately deviate from it in order to include the poor excluded households (Alatas et al. 2012). In this case, if information compels the leaders to comply with the government's list, welfare may actually fall. In practice, this was not the case: poor, ineligible households were no

[6] For example, in the past, protests about errors in the targeting list led some village leaders to resign rather than defend the beneficiary lists to their constituents: over 2,000 village officials refused to participate in a new government transfer program for this reason (See, for example, "BLT Bisa Munculkan Konflik Baru" (BLT may create new conflicts), Kompas, May 17, 2008; "Kepala Desa Trauma BLT" (A village head's trauma with BLT), Kompas, May 24, 2008; "Ribuan Perangkat Desa Tolak Salurkan BLT" (Thousands of village officials refuse to distribute BLT), Kompas, May 22, 2008; and "DPRD Indramayu Tolak BLT" (District parliament of Indramayu refuses BLT), Kompas, May 24, 2008.).

[7] The full beneficiary list that was given to the village head was identical in both treatments. Therefore, the leader's information about who is eligible was the same and only the citizens' information was varied.

less likely to receive the rice as a result of the cards.

The experiment also allows us to test the effect of public relative to private information about benefit entitlement. In half of the card villages (randomly selected), the beneficiary list was posted all over the villages and information about the cards was played on the village mosque loudspeaker ("public information"), in addition to mailing out the cards ("standard information"). Eligible households in the public information villages received twice as much additional subsidy as they did under the cards treatment with the standard information only. Part of this effect may have been driven by the fact that households were more likely to receive their cards, but we show that even conditional upon receipt, cards had a much larger effect in public information villages relative to those villages that received standard information. While public information increased everyone's knowledge about their own eligibility status, this treatment appears to have also promoted second order knowledge; for example, it also made citizens of all types more conscious of the fact that others knew about the official eligibility list. These results suggest that public information could have made it easier for villagers who were being denied their rights to coordinate with others in trying to get redress.

There are several possible mechanisms that could explain these results. Is it that information empowered citizens to better negotiate with local leaders for their fair share? Or does the information reduce the social stigma of transfer programs by legitimizing the entitlement and therefore encourage citizens to become more aggressive about claiming their rights? Is the information just a signal that local governments will be monitoring local leaders more? While it is challenging to identify a single mechanism that drives the result – and, indeed, the results could be driven by a combination of these mechanisms – we show that the data is consistent with the explanation of citizens having better negotiating power.

Several pieces of evidence support the idea that bargaining is at least one important channel through which cards are improving outcomes. For example, we show, in the context of a simple bargaining model, that providing information can affect how citizens engage with local leaders; indeed, empirically we find an increase in protests in cards villages. Moreover, we find that impact of printing the copay price on the cards was driven by an increase in the *quantity* of rice that eligible households received, rather than a reduction in the price paid. This is consistent with a bargaining model, where officials and

villagers care only about the total subsidy that the villagers would receive. The price information appears to have changed the subsidy along the more cost-effective margin for the local leaders, changing the quantity just for eligible households, for example, rather than changing the price for all households. This is harder to reconcile with other stories, such as a perceived increase in monitoring by the central government, because that should have increased compliance with the law (i.e. charging the right price).

In short, these findings strongly argue for the view that information about citizens' rights is very scarce, at least in poor populations, in developing countries, even when the rules have existed for a long time. Thus, providing information can be a powerful tool to improve service delivery. Just providing information directly to beneficiary households had a large effect on their ability to receive their entitlements, and it does so in a cost effective manner: the cards yield an increase in subsidy received by households greater than 7 times their cost, even under the assumption that effects last only eighteen months.

The remainder of the paper proceeds as follows. Section II describes the setting, experimental design and data. Section III provides the main findings on the effects of the card program. Section IV tests the idea of whether "too much" information may potentially backfire, while Section V explores the effect of additionally providing public information. Section VI discusses potential mechanisms that may drive the results. Section VII concludes.

II. Setting, Experimental Design and Data

A. Setting

This project explores the impact of providing information to citizens within Indonesia's subsidized rice program, known as "Raskin" (Rice for the Poor). Introduced in 1998, by 2012, the program targeted 17.5 million low-income households (the poorest 30 percent) based on a proxy-means test that is updated every three years. Targeted households are allowed to purchase 15 kg of rice–about half of a typical household's monthly rice consumption–at a copay price of Rp. 1,600 per kg (US$0.15), about one-fifth of the market price. The intended subsidy value—about 4 percent of beneficiary households' monthly consumption—is substantial. It is Indonesia's largest permanent, targeted social assistance program: in 2012, the budget for Raskin was over US$1.5 billion, and it distributed 3.41 million tons of subsidized rice (Indonesian Budget 2012).

The Raskin program is implemented at the local level by local officials appointed by the head of the local government. Indonesian villages (known as *desa*), and their urban equivalents (known as *kelurahan*), can have one of two systems of government. In *desa* governments, which tend to be in rural areas, the head of the government, known as the *kepala desa* (literally: village head), is elected, usually for 5 year terms. *Kepala Desa*, during the period of our study, were largely compensated in the form of usufruct rights over village land reserved for this purpose (known in Java, for example, as *tanah bengkok)*. These elections are quite competitive.[8] In *kelurahan* governments, which tend to be in more urban areas (and are required for all districts that are formally recognized cities, known as *kotamadya*), the head of government is the *lurah*, who is a civil servant appointed by the directly-elected district head, and who receives a civil service salary. We hereafter refer to both *kepala desa* and *lurah* as "village heads" for simplicity. Our sample consists of approximately 70% *desa* and 30% *kelurahan*, roughly mirroring the rural/urban split of our sample.

Typically, the village head appoints one villager to be sub-head for people's welfare (*Kepala Urusan Kesejahteraan Rakyat,* or *Kaur Kesra*).[9] This individual is in charge of picking up the rice once a month from the central distribution point (either in the nearby subdistrict or in the district capital), collecting copays from households, setting up a location where households can receive the rice (either in the village office or in each neighborhood), and remitting copays to the central government. There is little central government oversight, so local officials have substantial *de facto* control over implementation of the program at the local level. Our sample villages have an average of 336 eligible households, which means that the distribution team is typically responsible for distributing about 5 tons of rice per month.

Beneficiaries, however, do not necessarily receive all of the intended benefits. Leakages are abundant – a substantial amount of rice disappears (Olken 2006; World Bank

[8] In a previous survey conducted in 2009 in Indonesia by Olken, Onishi, and Wong (2014), we found that incumbent village heads chose to run for re-election only 40 percent of the time. Conditional on running for re-election, incumbents won only 59 percent of the time. Given this, being re-elected as village head is far from a sure thing.

[9] Exactly which local officials are in charge of Raskin distribution within the village varies by village. In 93% of villages in the sample, a combination of the village head, other village officers (i.e. village secretary), and neighborhood heads are in charge of Raskin distribution.

2012). Targeting is also a problem: the local officials who administer the distribution have a high degree of *de facto* discretion over who can access it. Local officials distribute Raskin more widely than the central government intends: 63 percent of the officially ineligible households in our control group had purchased Raskin rice at least once in the last two months.[10] This means that eligible households cannot purchase their full entitlement: 83 percent of eligible households in our control group reported that they wanted to buy more Raskin rice during the last distribution. Of these, 84 percent say that local Raskin officials prevented them from doing so.[11] Third, the local leaders often inflate the copay, with eligible households paying 42 percent above the official price. While this may reflect the fact that local leaders bear real transport costs for the distribution (e.g. truck rentals, storage space) that are not covered by the central government, qualitative research (Smeru 2008) and our own estimates (reported in Banerjee et al. 2015) suggest that this higher price often exceeds these costs. Putting this together, eligible households receive only a third of the intended subsidy.

Existing research suggests that, while Raskin is a highly salient and well-known program, intended beneficiaries have little information on program rules and beneficiary status (Smeru 2008; World Bank 2012). This means they may not realize that they are receiving a low share of their intended subsidy. In our sample, only 30 percent of the beneficiaries know that they are on the official eligibility list, and the average eligible household believes that the official copay price is 13 percent higher than the true price.

On the one hand, it is surprising that citizen information is so low for such an established program. However, government efforts to publicize the program (what is called "socialization" in the Indonesian context) have focused on the local officials that implement the program rather than on the citizens (Smeru 2008; World Bank 2012). Moreover, the fact that there are legitimate reasons for deviations from program rules muddies the waters. For example, the fact that distribution costs are not covered by the central government provides an excuse to raise prices beyond the copay amount. However,

[10] There is a long history of local deviations from official eligibility lists in Raskin and its predecessor programs (Olken et al. 2001; Olken 2006). Alatas et al. (2013) show that these changes to beneficiary lists by local leaders likely happens during the distribution of the rice, rather than through the determination of the official eligibility lists.

[11] By contrast, only 19 percent of households in control villages in our sample report that they could not buy more Raskin rice because they were credit-constrained at the time of distribution.

villagers may not know how large an increase can be justified by this argument, allowing officials to pad the amount. Similarly, the fact that some poor households are indeed excluded from official eligibility lists due to the inevitable errors in the implementation of proxy-means tests (Alatas et al. 2012) means that there is a legitimate (and legally allowed) reason to take some rice from the eligible and give it to the ineligible. Once that occurs, and eligible households are not getting the full 15 kg allotment they are supposed to, it requires careful checking to make sure that all the rice is redistributed properly and that none leaks out. Given that these deviations include both legitimate and illegitimate deviations from program rules, it is important to check not only whether the interventions increase compliance with program rules, but whether they do so by reducing leakage or at the expense of legitimate deviations (i.e. helping the poor). We explore these issues in the empirical work below.

B. Sample

This project was carried out in six districts (two each in the provinces of Lampung, South Sumatra, and Central Java). Importantly, the districts are spread out across Indonesia—specifically, on and off Java–in order to capture important heterogeneity in culture and institutions (Dearden and Ravallion 1988). Due to the constrained timeframe for providing feedback into national policy, we chose to conduct the experiment in villages where we had previously worked and thus had household-level data that could serve as a baseline survey.[12] Thus, we stratified the treatment assignments in this project based on status in the previous experiment to ensure balance.

Within these districts, we had originally randomly sampled 600 villages. We dropped 28 unsafe villages prior to conducting the randomization, for a final sample of 572 villages (40 percent urban and 60 percent rural villages).

C. Experimental Design

As shown in Figure 1, out of the 572 villages in the sample, we chose 378 to receive the Raskin cards. In the 194 remaining control villages, the government continued to run the

[12] The previous experiment was on an unrelated conditional Cash Transfer Program, known as PKH, targeted at the very poorest population and administered through a different ministry and funds distribution program (see Alatas et al. 2016 for a description of the previous experiment).

program under the status quo. The government mailed a soft-copy beneficiary list to districts with instructions to send one hard copy to the village government. The government also mailed an informational packet on program rules directly to village governments, including instructions to publically post the beneficiary list and to distribute rice only to those on the list. In these villages, households did not receive Raskin identification cards or any other form of information from the central government.

In the 378 card villages, the central government did everything they did in the control villages, but also mailed out "Raskin cards," along with instructions on how to use them, to beneficiary households via the postal service. Figure 2 shows an example of a card, which contains the household's identifying information plus instructions that they are entitled to receive 15 kg of subsidized rice per month. Postmen delivered the cards directly to households when possible; however, as in most developing countries, the postal service has a limited ability to do so, particularly in rural areas. As such, only 15 percent of the households that received a card reported receiving it directly from a postal worker; the rest received it from local officials.

We explore three variants of the cards' treatment.[13] First, in 187 randomly chosen card villages, the government printed the copay price on the card (see Figure 2). In the remaining villages, it was not printed. The quantity of rice households were entitled to (15 kg) was printed in both cases.

Second, in half the card villages (randomly selected), all eligible households (on average, 30 percent of the village for our sample) received cards. In the remaining card villages, cards were only mailed to those in the lowest decile of predicted per capita household consumption (32 percent of eligible households, or, on average, 14 percent of the whole village for our sample). The other eligible households were still on the lists and posters provided to the local officials and they were still eligible to receive Raskin despite

[13] The government also administered a fourth intervention where the government mailed coupons to beneficiaries, along with the cards. Local officials were supposed to collect the coupon each month when a beneficiary bought Raskin rice, and send the coupons to the central government, which was supposed to check them. The intervention was designed to test whether additional "monitoring" by the central government resulted in less leakage. However, in fact, the central government did not actually tabulate the coupons they received, or follow up based on the coupons or lack thereof. We show in the Appendix Tables 1A and 1B that in fact, the coupons increased the bargaining power of the official relative to ineligible households, as officials were able to deny ineligible households access to the program, but did not increase access to eligible ones.

not having a card.

Finally, we experimentally varied the degree to which information was public. In 192 villages (randomly chosen) that received cards, additional public information, beyond the status quo information, was provided regarding both the presence of the cards and eligibility. The goal was to not only increase knowledge of one's own eligibility status, but to also increase common knowledge within the village. To this end, a community facilitator hung up additional posters – announcing the cards and publicizing the beneficiary lists – within different neighborhoods of the "public" villages. They also played a pre-recorded announcement about the cards in the local language over the village mosque loudspeaker (a common advertising technique in Indonesia).[14] The facilitator spent about two days in each village, and the marginal cost of this additional information was only about US$1.40 per beneficiary household.[15]

D. Randomization Design, Timing, and Data

Figure 1 shows the number of villages randomly assigned to each treatment. For the assignments of control, card, and card only to the bottom decile, we stratified by 58 geographic strata (sub-districts) interacted with the previous experimental treatments. For the price and public information sub-treatments, we stratified by district, previous experimental treatments, and cards.

Figure 3 shows the timeline of the experiment. In July 2012, the central government mailed the program guidelines and the new list of eligible households to local governments. In August, the government mailed the cards to eligible households in card treatment villages. In September and October, the additional public information treatment was conducted in the villages that were randomly assigned to receive it.

E. Data Collection

We conducted two primary follow-up surveys: one in October to November 2012, at least two months after cards were mailed, and a second in March to April 2013, about eight

[14] Appendix Figure 1 shows an example of the posters used to announce the cards. There were eight variants of the poster to reflect the combinations of the sub-treatments: with and without price, with and without coupons, and distributed to all eligible households or only to the bottom decile.

[15] The facilitators had a coordination meeting with the village leaders to gain permission to hang up the posters. The meetings were attended by few households (an average of 20 out of 1,380 households in a village) and they were short; the facilitators were instructed to stay on script and not provide program information. So, it is highly unlikely that information was widely spread directly as a result of the meeting.

months afterwards. In both surveys, SurveyMeter, an independent survey organization, visited randomly selected households and asked them about their experience with Raskin, as well as other characteristics. We oversampled eligible households to ensure sufficient power for this group. In the second survey, we also sampled some respondents who had been surveyed in our previous experiment (Alatas et al. forthcoming), to take advantage of pre-treatment information. Additional sampling details can be found in Appendix 1.

We also conducted a third follow-up survey in December 2013 to January 2014, 18 months after the intervention, to be used as the endline survey for another experiment that we conducted after this one (see Banerjee et al. 2015). In July 2013, prior to the 18-month survey but after our second (8 month) survey, the government distributed new cards nationwide (i.e. in both the control and treatment areas) for all social protection programs. While the new social protection cards were officially to be used for all programs, including Raskin, the publicity surrounding the social protection cards was heavily focused on a new temporary cash transfer program that was rolled out concurrently and there was comparatively little information about the Raskin program.[16] Thus, we report the results of this endline separately to shed light on longer term effects of the original Raskin card, with the caveat that these 18 month results may have been affected by these other interventions.

F. Summary Statistics and Experimental Validity

Appendix Table 2 provides sample statistics from the control villages to provide a description of Raskin in the absence of the intervention. On average, 79 percent of eligible households bought Raskin in the last two months; however, 63 percent of the ineligible households did so as well. Eligible households typically bought only a third of their official allotment (5.3 kg out of 15 kg) at an average price of Rp. 2,276, over 40 percent higher than the official copay price of Rp. 1,600. Combined, this implies that the eligible households received an average subsidy of Rp. 28,605, or 32 percent of their entitlement

[16] This final endline reveals that 91 percent of eligible households in treatment areas and 93 percent in control areas received a Social Protection Card mailed out in July 2013. However, while 99 percent of card recipients report that the Social Protection Card was used for the cash transfer program, just 1 percent report it was used for Raskin. These percentages are similar in treatment and control group.

(Rp. 88,680).[17] Seven percent of eligible and 5 percent of ineligible households report having a card for Raskin in the control group, which may be because a few local governments had previously issued cards.

Appendix Table 3 provides the randomization check for the main card treatment, and Appendix Table 4 provides the check for card variants. The variables were specified prior to the randomization. Only 2 out of 20 differences in Appendix Table 3 and only two out of 30 differences shown in Appendix Table 4 are significant at the 10 percent level, consistent with chance, suggesting the experimental groups are balanced.

III. Overall Impact of Information

A. Did Households Receive the Cards?

We begin by examining whether households in the card treatment villages received the cards, and whether this intervention translated to increased knowledge of eligibility status. Table 1 provides the results. Unless otherwise noted, we estimate:

$$y_{kvist} = \alpha_k + \alpha_{st} + \beta TREAT_v + \epsilon_{kvist}$$

where k represents a stratum, s represents a type of household sampled, t represents a survey round, v represents a village, and i represents a household. Since the results are similar across survey rounds, we pool them for most of the analysis, but we also provide the disaggregated analysis below. We include sample dummies interacted with the survey round dummy, as well as stratum fixed effects.[18] Each column comes from a separate OLS regression of the respective outcome on the treatment, with standard errors clustered by village; we provide p-values from randomization inference in brackets.[19] In Columns 1-3, the sample is eligible households (those who were on the official central government list), while in columns 4-6 the sample is ineligible households (randomly selected households

[17] The total subsidy is the difference between the prevailing local market price for rice of similar quality and the copay price multiplied by the quantity purchased.

[18] Appendix Table 5 replicates the specifications in Table 1, with varying levels of controls; the results are near identical with either no or additional controls. Appendix Table 6 shows that the sample weights do not drive the results. Appendix Table 7 shows that the eligible households in Java were more likely to receive the card than those off Java. However, even off Java, where we expect weaker institutions, there is a strong and positive effect on card receipt for eligible households (Column 1).

[19] To construct randomization-inference p-values, we re-ran our original randomization code 1,000 times with different seed values to construct alternative pseudo-randomizations that completely reflect stratification and other elements of our randomization design. We use these pseudo-randomizations to construct randomization-inference p-values.

who were not on that list).[20] The last row provides randomization-inference p-values comparing the coefficients of eligible and ineligible households for each respective variable. Note that the $TREAT_v$ variable is defined based on the randomization results, irrespective of whether cards were actually distributed in the village or not, so all regressions that we report in the paper estimate intent-to-treat effects.

Eligible households in the treatment group were 30 percentage points more likely to receive the cards than those in the control villages (Column 1 in Table 1). Households may not receive cards if they get lost in the mail system or if addresses are difficult to access. Moreover, village leaders have the power to block the distribution of cards because, in most rural areas, the post office does not know households' addresses, and instead relies on local leaders to help postmen identify who lives where. Anecdotally, in a number of cases, when the facilitators arrived at the village for the public information treatment, they found that the cards were still in a drawer in the village head's office, undistributed, suggesting that indeed village heads may have been blocking their distribution. This blocking was likely happening in practice, as we observe that households were more likely to receive cards in areas that were seen as "lower corruption" areas at baseline (Appendix Table 8).

In comparison to the eligible households, ineligible households in the treatment group were only 3 percentage points more likely to receive cards (Column 4); the difference between eligible and ineligible households is significant at the 1 percent level. Ineligible households may receive cards for a variety of reasons—corruption, reallocations at the village level of slots from poor to rich, imperfect matching of the survey data to government rolls, and so forth – but the overall level is dramatically lower than those who were eligible.

All cards included instructions that the card was to be presented when Raskin was purchased. In villages where the cards were mailed out, card use increased: eligible households were 15 percentage points more likely to use a card to purchase Raskin rice. Note that even if one did not use it, the act of *getting* a card may still be important. In fact,

[20] As already noted, for some randomly selected card villages, the cards were mailed only to households in the bottom decile. For these villages, only households that were mailed a card are included in the eligible sample; those who are eligible for the Raskin program, but who were not mailed a card, are dropped from the main analysis (we explore their outcomes in below). We reweight the regressions so that, on average, the weighted fraction of households from the two types of eligible households (bottom decile and other eligible) are identical in treatment and control areas in each of the 58 geographic strata.

in qualitative interviews some households explained that they were told to simply store the card with their important documents rather than use it.

We then ask whether the card treatment increased people's beliefs about their eligibility.[21] Eligible households were 9 percentage points, or 30 percent (std. error: 7 percent) compared to the control mean, more likely to correctly know their eligibility status in the treatment group than the control (Column 3). Similarly, the ineligible were 5 percentage points, or 14 percent (std. error: 6 percent) compared to the control mean, more likely to know their status in the treatment villages (Column 6).[22] This suggests that the cards increased information, and in particular, increased eligible households' beliefs about what they were entitled to.

B. Impacts of Card on Distribution Outcomes

Table 2 explores the impact of the cards on the purchase of Raskin rice in the two months prior to the survey, quantity, price paid, and the overall subsidy received. The sample structure and regression specifications are the same as in Table 1.[23] The quantity and subsidy variables are coded as zero if no purchase was made and thus capture both the intensive and extensive treatment effects. Price, however, is conditional on purchase, since it is unobserved for households that do not purchase rice.

The card treatment substantially increases the subsidy received by eligible households. While they were no more likely to buy Raskin in the last two months (Column 1 in Table 2), we observe large changes in both quantity and price: eligible households in card villages bought 1.25 kg more rice and paid a copay price of Rp. 57 less than control villages (Columns 2-3). This translates to a Rp. 7,455—or about a 26 percent—increase in subsidy received (Column 4).

These findings are not likely to be driven by reporting or recall error. For example, one might be concerned it is hard to distinguish a 1.2 kg difference in rice – although this

[21] The mean for this variable is low for both eligible and ineligibles; this is because many households of both types answer "don't know," which we code as not knowing their status.

[22] All of the increase in ineligibles' knowledge comes from public information villages, with no change in ineligibles' information in standard information villages (Table 7).

[23] Appendix Table 9 and 10, respectively, show that the results are near identical regardless of adding or removing controls and in dropping the sample weights. Note that eligible household received larger subsidy in Java than off-Java (Appendix Table 11) and in "low-corruption" areas at baseline (Appendix Table 12), consistent with treatment households in these types of areas being more likely to receive cards.

difference is proportionally quite large – and therefore the fact that households say that they purchase more rice in treatment villages is based on a misperception. This would be true, for example, if leaders responded to the cards by telling everyone that rice sacks contained 6.5 kg of rice, while still giving them only 5.3 kg. To check this, we separately tested whether households could accurately assess the quantity of rice, and found that households were easily able to detect an extra 1 kg of rice in a sack.[24] We also find qualitatively similar treatment effects if we constrain the sample to areas where village officials weigh the Raskin rice in front of you, where villagers directly observe the quantity they are receiving rather than having to guess its weight (see Appendix Table 15).

Alternatively, while one may be concerned that the results are due to recall bias, we find that if we restrict the answers to distributions in the last month, where recall is most likely less of an issue, we find qualitatively very similar results (Appendix Table 14). Moreover, recall bias could not fully explain the fact that the variations of the card treatment that we discuss below (i.e. cards with and without price printed on them) had differential effects since everyone received a card in that case. Finally, let us add that qualitatively, we have observed that Raskin distributions are extremely salient to households. Rice in general plays a central role in Indonesian communities, and Raskin is the main government rice program. In our experience, households have no problem remembering precisely when the last Raskin distribution took place, how much rice they received, or the price they paid.

Ineligibles in the card villages were 6 percentage points less likely to purchase Raskin in the last two months than those in control villages (Column 5). However, there is no treatment effect on the total amount purchased by ineligibles (Column 6), since the card treatment increased the quantity conditional on purchase for the ineligibles (Appendix Table 16). Thus, on net, there was no effect on the subsidy received by ineligible

[24] Specifically, we asked 18 eligible households in two different sample villages to guess the weights of 4 packets of rice (in random order) that weighed 4, 6, 7, and 8 kg. Respondents assessed packet weight with remarkable accuracy, guessing an average of 3.9, 5.5, 7.9, and 8.7 kg respectively. Most importantly, respondents consistently assessed the relative packet weights accurately. In a regression, where each respondent represents 4 observations (for each packet guess) and the standard errors are clustered by respondent, dummies for actual packet weight are highly significant (p-value=0.000), as are the estimated differences in weights between packets of size 6 and 7 kg and between 6 and 8 kg (p-value=0.000), showing that eligible households can accurately assess differences of the size of the observed treatment effects. See Appendix Table 13.

households (Column 8). The standard errors are such that the upper bound 95 percent confidence interval allows us to rule out an effect greater than about 13 percent of the mean subsidy level for ineligibles. As shown in the last row of Table 2, we can rule out that the treatment effects of eligible and ineligible households are the same.

Since the cards increased the quantity received by eligible households, but did not decrease the quantity received by ineligibles, this implies that on net, the cards resulted in a substantial reduction in leakages. We estimate that the cards reduce leakage by 1 kg (std. error: 0.46) to 1.6 kg (std. error: 0.55) per eligible household, which represents a 33 (std. error: 15 percent) to 58 percent (std. error: 27 percent) reduction in "lost" rice.[25]

Finally, we estimate the treatment effect of cards by survey round, i.e. at the two, eight and eighteenth month mark of the program.[26] As shown in the control means in Table 3, despite fluctuations of the program functioning over time (e.g. in both quantity and price), the estimates suggest that the card impact is remarkably persistent. The difference in subsidy for the eligibles, while larger in the first period (7,470 in the first round as compared to 4,538 in the second), is not statistically different across the two survey rounds. Remarkably, the treatment effect on the subsidy remains positive, large in magnitude, and significant at the 1 percent level 18 months after the intervention.

C. Impact of an Additional Piece of Information

The cards contained both individual-specific components – it was pre-printed with the names of household members to officially document program eligibility – as well as general information (the quantity of rice that eligible households can purchase). To isolate the role of a single piece of general-purpose information, the government randomly varied

[25] We estimate the reduction in leakage using a "gap measurement" method, similar to Olken (2006). We use administrative data from the Government of Indonesia on the size of the Raskin quota for the village. Then, we use household data on Raskin rice purchases to estimate the total amount of Raskin rice that "arrived" in the village; to arrive at the village-level estimate, we weight eligible and ineligible households based on their proportion in the village population. We derive a range of estimates for amount of Raskin rice that is purchased by households because there is measurement error in the value of the total number of households in the village. We measure the village population using three sources: the first round community survey with the village head, the second round community survey with the village head, and PODES 2011, a census of all villages in Indonesia collected by the Government of Indonesia. We calculate leakage as the difference between the village's Raskin quota and the estimates of the total amount of Raskin purchased within the village. We present the upper and lower bound estimates for leakage throughout the paper.

[26] We sampled slightly different sets of households in each survey round. We restrict analysis to a comparable sample and weight respondents in the 2nd and 3rd rounds to match the proportions in the first.

whether the copay price (Rp. 1,600 per kg) was printed on the card across villages. In all villages, the official program rules distributed to village leaders contained the official copay, so this is purely an intervention affecting the information received by villagers.

The results, provided in Table 4, show that just a single additional piece of information on the cards had a substantial effect: eligible households in the villages where the official price was printed on the card received a much larger increase in subsidy than in villages where it was not.[27] The difference is primarily through quantity, rather than price. Specifically, eligible households receive Rp. 3,602 more subsidy per month with the printed price than without; of this Rp. 3,602 increase in subsidy, about 94 percent of the change was due to increase in quantity received (which increased by 0.62 kg compared to cards without price) while only about 5 percent of the change was due to a reduction in the copay price (which fell by Rp. 43 compared to cards without price).[28]

IV. Can Information Backfire?

In the previous section, we showed that information improved outcomes. However, a potential concern – both in theory and voiced in practice by the Indonesian government – is that that "too much information" could be counterproductive, for two distinct reasons.

One potential issue is that local leaders may deviate from the program rules for purely altruistic reasons. The program's primary objective was to distribute rice to the poor. However, the government's official eligibility list is based on assets, which are a good, but imperfect, measure of poverty. One could imagine a benevolent village head redistributing from eligible to ineligible households to correct errors and ensure that the poor, ineligible households are taken care of. The cards intervention could prevent him from making these types of desirable transfers.

In Table 5, we test whether the card treatment shifted resources away from poor households, as measured by their per capita consumption prior to the experiment. We

[27] In addition, Appendix Table 18 shows while printing the price did not affect receipt of cards, it did increase the probability cards were used. We also tested the effect of the cards in the standard information versus public information treatments, since the public information may have had an effect on people's perception of price (Appendix Table 19). We find that the effect of printing the price on cards is similar across both.

[28] Since price is only available conditional on buying Raskin, the sample of people reporting prices may change in response to the treatment. Thus, we also report regressions on the minimum and maximum price reported by any of our respondents in the village. Appendix Table 20 suggests that, relative to pure controls, the cards with printed price reduce the maximum printed price in the village by about Rp. 110, or about 12 percent of the control group levels of price markups above the official Rp. 1,600 copay price.

interact the treatment with baseline log per capita consumption ($LOGCONSUMPTION_i$) and estimate:

$$y_{kvist} = \alpha_k + \alpha_{st} + \beta TREAT_v + \omega LOGCONSUMPTION_i + \gamma TREAT_v \times LOGCONSUMPTION_i + \epsilon_{kvist}$$

The first 4 columns of Table 5 show that, for eligible households, we find no evidence that the gain in subsidy received is concentrated among the rich; if anything, the treatment effect is smaller for those with higher income, albeit not statistically significant (Column 4). Similarly, the remaining columns show no evidence that poorer ineligible households are hurt as a result of the cards.

A second reason why the government was concerned is that too much information may have perverse effects on the incentives of the leaders. This is because the information, by putting pressure on the local leaders to deliver more to the villagers, reduces the rents that the village head can hold on to and thereby makes him less interested in continuing to administer the program. As a result, providing information to fewer individuals may actually improve outcomes for *all* eligible households, since the leader now has the flexibility to satisfy a smaller group and protect more of his rents.[29]

To examine the tradeoff between providing information to all and providing information to some, the government experimentally varied whether cards were mailed out to all eligible households or just to those in the bottom decile (about 32 percent of eligible households). In these villages, as in all treatment villages, the government mailed the complete eligibility list to the local leaders (and not just the list of eligible people in the bottom decile) with instructions that all eligible households be allowed to purchase their Raskin allotment.

To examine the impacts, we split our sample of "eligible" households into two groups, those in the bottom decile (who receive cards in all card treatment villages) and other eligible households (who do not receive cards where cards are mailed only to the bottom decile, but receive cards when they are mailed to all eligible households). We regress each outcome on indicator variables for "cards to the bottom decile" and "cards to all," and thus the coefficients reflect differences from the "no card" villages. Table 6

[29] To see this theoretically, please see Appendix 4.

provides these findings for each of the three categories of households.[30]

This treatment did reduce pressure on the local leaders: overall protests were significantly lower in the villages where cards were only given to the bottom decile rather than when cards were given to all (Appendix Table 22). However, providing cards to just the bottom decile did not change the allocation to these households relative to villages in which all households received cards: there was no detectable difference in the propensity to buy Raskin rice, the amount purchased, the price or the subsidy for those in the bottom decile across the two types of villages (Columns 1-4 of Table 6).

However, the outcomes for the other eligible households greatly differed based on whether or not they resided in "cards to all" villages, despite the fact that they were on the beneficiary list in both types of villages. The other eligible households in the "cards to all" received an increase in subsidy that was just as large as that of bottom decile households under "cards to all" (Columns 4 vs 8 of Table 6). Other eligible households that resided in villages where only the bottom decile received a card, by comparison, did not experience any gains (Column 5-8 of Table 6).

In short, we find no evidence that the additional information "backfired," either by reducing the ability of local leaders to "fix" bad national rules, or by placing so much pressure upon them that they actually misbehaved more.

V. The Effects of Providing Public Information

The discussion thus far has focused on private information: providing a card to an eligible household informing the recipient of what he or she is entitled to. However, it is also possible that providing public information, both about the existence of the cards and the information they contain, may matter – either by making sure that the cards are actually distributed, or by creating common knowledge about the cards and hence more scope for collective action.

To explore these issues, in half of the card villages (randomly selected), the government conducted the "standard" card procedures: local leaders received the

[30] Appendix Table 21 shows the impact on card receipt, use, and knowledge. Card receipt and knowledge is identical for bottom decile households in both types of villages but only increases for other eligible in "cards to all" villages.

beneficiary list and were told to hang it in a visible place in the village. In the remaining ones ("public information"), a facilitator ensured that three copies of the poster announcing the cards and beneficiary list were hung in each hamlet in the village; they also played a pre-recorded message about the cards on the mosque loudspeaker.

Table 7 begins by examining the impact on whether households have seen the beneficiary list. In Panel A, for each of four key groups (eligible, non-eligible, village officials, and informal leaders), we regress a dummy variable that indicates whether the respondent reports having seen the beneficiary list on dummy variables for the cards with standard information and the cards with the public information campaign. The "standard" card treatment did not significantly increase reports of having seen the list across any of the groups. In contrast, the "public information" treatment greatly increased exposure to the list: the number of eligible households who had seen it nearly tripled relative to no cards (from 7 to 21 percent in Column 1) and was 12 percentage points higher than in the standard approach. Ineligibles were 10 percentage points more likely to see it in the public versus the standard approach (Column 2), and village leaders were 18 percentage points more likely (Column 3).[31]

The public information increased knowledge of one's own eligibility status (Table 7, Panel B). With no cards, 30 percent of eligible households can correctly identify their status; those in villages with just cards are 6 percentage points more likely to correctly identify their status relative to no cards (Column 1 of Panel B). With the additional public information, they are 6 percentage points more likely to do so relative to just the card alone – this is a 40 percent (std. error: 7 percent) increase in knowledge relative to no cards and about a 17 percent (std. error: 8 percent) increase relative to the standard card approach. With just the cards, ineligibles were no more likely to know their status than under no cards, but they were 8 percentage points (or 22 percent (std. error: 8 percent)) more likely to know it under public information (Column 2).

The second mechanism through which the public treatment could have had an effect was by changing people's beliefs about others' access to information. This may be important if challenges to authority feature strategic complementarities: a village head may

[31] We coded anyone who reported not knowing whether they had seen the list as not having seen it. In Appendix Table 23, we drop those who reported "do not know" and find near identical results.

be able to retaliate against a lone individual, but it may be harder to retaliate against a group. Thus, a citizen deciding whether to challenge a village head may be more likely to do so if he can coordinate with others. However, doing this requires not just knowledge about what you are entitled to, but also confidence that everyone else knows more or less what they are entitled to as well (Chwe 2001).

To test whether higher order beliefs changed, in Table 8, Panel A, we ask all survey respondents how likely members of each of the four groups have seen the list, where 0 corresponds to "have not seen the list" and 3 corresponds to "most have seen it." Individuals under public information were more likely to believe that *others* had seen the list, whereas individuals under standard information were no more likely to report that any type of individual had seen it.[32] However, despite the fact that more people have seen the list, with everyone believing that everyone has more information, respondents were no more likely to correctly identify other people's status in public information than under the control (Panel B of Table 8).[33]

Tables 9 and 10 examine the impact of the additional information on program outcomes. Eligible households were both more likely to receive their card and to use them under public information, with no change for ineligible households (Table 9). The magnitude of these differences for eligible households is large: they were 19 percent (std. error: 9 percent) more likely to have received a card and 50 percent (std. error: 19 percent) more likely to use it than under the standard information.

The public information nearly doubled the subsidy increase that eligible households received relative to the standard information card villages (Table 10). This difference was driven by both an increase in quantity (Column 2) and a decrease in price (Column 3). Again, there is no difference in quantity for ineligibles, which implies that the gain is less about program resources being diverted from ineligible to eligible, but rather due to a decrease in the theft of rice.

One question is whether the public information worked by simply increasing the

[32] If we disaggregate by respondent type (eligible or ineligible), we see that eligible respondents are more likely to believe that others have seen the list under standard information, as well (see Appendix Table 24). This is consistent with the notion that the standard information treatment primarily gave information to eligible households.

[33] As Appendix Table 25 shows, there are not systematic differences between eligible and ineligible households.

number of cards distributed, or if it had broader effects beyond the receipt of the cards. To try to distinguish between these two scenarios, we estimate the implied instrumental variables effect of receiving a card in the standard villages and compare this effect to that in the public (see Appendix Table 27).[34] If the effect of the public treatment was simply through increased card receipt, the IV effect should be similar across both sets of villages. However, this is not the case: the IV estimate of receiving the card on the subsidy is Rp. 31,160 in public, while it is Rp. 18,833 in the standard treatment (p-value of difference is 0.08).[35] This implies that the public information had impacts beyond just handing out more cards.

On net, these results suggest that public information, through its combined effect on increasing what people know about their own rights and on higher-order knowledge, may be an important component of empowerment.

VI. Mechanisms

The cards greatly increased the amount of subsidy that eligible households received under the Raskin program. Here we consider the possible economic mechanisms behind our findings. One possibility is that cards changed the nature of bargaining between villagers and local officials, so that villagers were able to successfully demand more from local officials. A second possibility is that village officials see the cards as a signal that the central government is monitoring them more, encouraging them to comply more with program rules. Yet a third possibility is that the cards – and particularly the public treatment – somehow reduce stigma associated with the program, leading people to demand more.

In the subsections that follow we flesh out each of these mechanisms and discuss how they fit with the evidence. We also consider a few other possibilities that we argue are not consistent with the facts. We conclude that bargaining likely plays an important role, though our evidence admits a role for other mechanisms as well.

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[34] The corresponding first stage and reduced form regressions are presented in Appendix Table 28.

[35] Technically, it is possible that these differences could just reflect a different local average treatment effects (LATEs) for the different subpopulations induced to receive the cards under public, but this seems extremely unlikely. To see this, note that if the households who received cards under Standard Information continued to experience a LATE of 18,833, the 6 percent additional households induced to receive cards under public would have to have a LATE of 82,314. While this is in theory possible, this would be very surprising change in LATEs.

A. Information Effects on Bargaining

One possible channel is that by providing eligible households with better information, it allowed them to better bargain with government officials for their entitlements. To determine whether the evidence is consistent with this channel, it is first worth considering a simple bargaining model to explore the possible impacts of information on the negotiation between the village leader and a Raskin beneficiary over the division of program benefits. We lay out the setup and main intuition here; full formal details and results can be found in Appendix 2.

Suppose there is a population of risk-neutral potential beneficiaries of mass 1 indexed by i, each entitled to a total value of benefits denoted by B. The local leader must decide how much of these benefits ($X_i \in [0, B]$) to offer to each potential beneficiary.

The leader makes a take it or leave it offer to each villager. If the villager accepts, he gets X_i and the leader keeps $B - X_i$. If the villager does not accept, he has the option of complaining to an outside authority at cost C. Each villager has a prior p_i on the likelihood that he is eligible and, if so, his expected benefits B_i, conditional on complaining. Both p_i and B_i vary by individual, but what is relevant is the distribution of the expected value $Y_i = p_i B_i$. The leader knows the distribution of beliefs, $G(Y)$, but not the Y_i of the particular villager i with whom he is interacting. For a village head, complaints have both a monetary cost and also reduce his future reelection probability.

We model providing Raskin cards as inducing a shift in beliefs, $G(Y)$. This could take several possible forms. For example, receiving Raskin cards could lead to a reduction in the variance of $G(Y)$ for those who receive cards, if people previously had diffuse, but correct-on-average, priors about program rules. Alternatively, it could lead to an increase in the mean of $G(Y)$, if for example government officials misled them about program rules (such as the true copay price). It is also possible for mean and variance to change simultaneously; for example, if some eligible households did not know they were eligible, informing all eligible households they were eligible would increase the mean and reduce the variance $G(Y)$.

The model suggests that the impact of increasing information, perhaps surprisingly, is ambiguous. In fact, we show that even the effect of an increase in the information

available to eligible households on those households can be negative. Consider, for example, an increase in mean beliefs, i.e. in the mean of $G(Y)$, for eligible households. For a given offer from the village head, there are now fewer eligible people accepting the offer, which reduces the cost of sweetening the offer to them slightly and pushes towards raising *X*. On the other hand, complaints increase, decreasing the likelihood the village head stays in office in the future, and effectively increasing his discount rate because he is less likely to be in office to obtain rents in the future. Making him less forward-looking leads the official to reduce X, which counteracts the previous effect; which of these effects dominates is theoretically ambiguous. One can similarly show that the effect of a decrease in the variance of $G(Y)$ has ambiguous effects on *X*.

An important feature of this model is that there are complaints along the equilibrium path. This is because the decision to complain is based on the villager's expectation of what he can get by complaining, and the village head does not observe each particular villager's actual expectation, only the population distribution of such expectations. While the village head does try to reduce the number of complaints, asymmetric information prevents him from doing so perfectly.

A related characteristic of the model is that complaints do not necessarily go up when households are worse off; this is because complaints arise from *a disconnect between households' beliefs and what the village head offers them*. This means that increasing the mean beliefs of eligible villagers can increase the offers that the village head makes to them and at the same time *increase* their complaints, since in general the offer the village head makes to the villagers will not increase enough to fully offset the increase in mean beliefs. The model also shows that there are potential spillover effects of informing eligible households on the outcomes of ineligible households operating through changes in the village head's reelection probabilities. For example, if protests by eligibles go up and therefore the village head is less likely to be reelected, he may become more ruthless in rejecting the claims of the ineligible villagers.

The key point from the model is that the impact of even a simple change to information is not, ex-ante, as obvious as one might expect. The model also suggests that it is useful to look at complaints as separate data-points that are indicative of receiving an offer that is poor relative to one's beliefs, which contains information distinct from just the

amount of rice one ends up receiving.

There are several pieces of evidence in line with the bargaining model. First, the model suggests that complaints and protests can change in response to information, even on the equilibrium path. To examine this, we use data we collected on whether there were citizen "protests" and whether there were any of four different types of "complaints": complaints from those who receive rice, complaints from those who did not, complaints about the beneficiary selection process, and complaints about the distribution process.[36]

Table 11 shows that the likelihood of complaints is altered by the cards treatment. Specifically, protests increase substantially in card villages (Column 1). Complaints by those who do not receive Raskin increase by 8 percentage points in treatment areas – about a 36 percent (std. error: 14 percent) rise over the control group mean –while complaints fall for those not receiving Raskin rice. The treatment spurs more complaints about the beneficiary listing, and fewer complaints about the distribution process.[37] This suggests that the bargaining relationship between citizens and local officials has changed.

A second point of evidence in favor of the bargaining story is the fact that printing the information about the copay price increased the *quantity* of rice eligible households received, not the price they paid. From the perspective of the bargaining theory outlined above, officials and villagers would care only about the total subsidy X that villagers receive (the product of the price discount and the quantity), not whether it comes from lower prices or higher quantities. Revealing information about the true copay price of Raskin (as discussed in Section III C) should increase the total subsidy, but the margin through which it does so is arbitrary and depends on which approach is more cost-effective for the local leaders. The fact that we find that quantities went up in response to printing

[36] Protests generally refer to simultaneous protests by multiple people, whereas complaints are individual. Complaints about the beneficiary selection process are comprised of the following specific types of complaints: "Process of data collection and selection for program beneficiaries was not transparent," "There was practice of corruption/collusion/nepotism in determining beneficiaries," "The allocation was not fair," "Aid was given to those who were not suitable to the program," "Household that used to be eligible for Raskin is no longer eligible," and "The latest Raskin Beneficiary list was not accurate"; complaints about the distribution process include: "The amount of aid received was not matched," "Raskin came late," "The fee was not matched with the regulation," "The new Raskin quota did not meet the desired amount," "Location of Raskin pick up point was not pleasant," and "Raskin quality was poor."

[37] Interestingly, the increase in complaints about the targeting and beneficiary list tend to occur right after the intervention, while the decrease in complaints about distribution occur after households have had time to update their beliefs on the distribution process (Appendix Table 17).

the copay price can be reconciled with a bargaining story if increasing quantities is more cost-effective for the leader than lowering prices. This may be the case if it is easier for the leaders to discriminate between eligibles and ineligibles on quantities because there may be more pressure for a uniform price than for equal quantities.

Qualitative evidence supports the idea that bargaining between village leaders and villagers may be important. For example, a village head we met described a process whereby each year, the village would hold a meeting in which he described how Raskin rice would be allocated to both eligible and ineligible households. During the meeting, he would seek consent of the village for the proposed distribution scheme.[38] At the meeting he held after the cards had been distributed, however, eligible households refused to consent: he said that they knew what they were entitled to and refused to budge, and the village head had to deliver the full 15kg to all eligible households from thereon in.

B. Effects on perceptions of central government monitoring

An alternative story is that the information campaign simply increases the village officials' beliefs about how important the central government believes following the rules are, and how much the central government would be monitoring them on the rules in the future. The fact that the price treatment results in increases in quantities provides evidence against the idea that the central mechanism is a perception of greater central government monitoring: if one thought that by printing the price the government was signaling a higher degree of auditing on price, one would expect effects only on price. Other evidence also suggests that the results are not driven by perceived higher central government monitoring. In particular, we see strong results even 18 months after the cards were implemented, despite the fact that there was in fact no change in central government monitoring. If monitoring was really the driving force, one might expect a faster decay of the effects of the cards once people realized that the monitoring was not happening.

C. Effects on Stigma

[38] This anecdote actually came from a different part of the country, in NTT province, after the national scale-up of cards.

An alternative explanation for our results is that there is a stigma for receiving social programs, and that the cards "normalized" the idea of receiving the program, thereby reducing the stigma involved. However, the evidence suggests that this is unlikely. In the control group 79 percent of the eligible households were already purchasing Raskin rice, just not receiving their full entitlement and/or were paying a higher price; had stigma been an issue, we would have seen many fewer households buying Raskin rice to begin with. Indeed, Table 2 shows that the treatment only affects the intensive margin of how much people buy, not the extensive margin of whether they buy. One would imagine that if reducing stigma was the main channel, one would find more results on the extensive margin.

In this context, most households appear to want to be seen as poor rather than rich: in the baseline survey, when we asked individuals to assess their poverty level on a ladder from 1 (poorest) to 6 (richest), 19% of households list themselves at 1, 50% list themselves as either 1 or 2, and about 87% list themselves at 3 or below. In fact, less than 3% of households list themselves at 5 or 6, the two richest categories. Thus, it is unlikely that stigma was a substantial problem in this context.

D. Other Explanations

We also consider several other possible explanations for the effects. One view is that villages may have been maximizing a different social welfare function from the central government, and therefore, deviated from the program rules to ensure that resources are allocated to others. In this view, the cards simply made villages more likely to comply with the central government rules, and less likely to maximize resources allocation as they choose. While this is possible, it is unlikely to be the main driver of our results: the cards did not greatly change who accessed the Raskin program, but did greatly reduce leakages (i.e. rice theft).

Another possible explanation is that the large increases in Raskin quantity we observe are actually just a normal demand response to the change in price induced by the cards (i.e. cards affected only the price of Raskin, and then households responded to the price decline by increasing quantity, as they would with any good). Again, this seems unlikely: the Raskin price (even with markups) is already so far below market price that most households would want to buy as much as they could, especially since it is possible

to resell rice to traders. Moreover, the quantity effects are sufficiently large that the demand for rice would need to be very elastic to explain these effects – we calculate that it would require a price elasticity of about 10 -- which seems very unlikely for an important staple.[39] It is also worth noting that the outsourcing intervention studied in Banerjee et al. (2015) in this context led to a reduction in the price of rice but no essentially increase in quantities, lending further evidence against the idea that the effects here are demand effects.

VI. Conclusion

Despite widely-held beliefs about the importance of transparency for improving governance, there has been surprisingly little rigorous evidence on its effects on service delivery. In this paper, we tested the role of information by providing identification cards to eligible beneficiaries of a subsidized food program in Indonesia. Importantly, we varied several aspects of the card program to test how providing different information amounts and content affected the ultimate outcomes.

The cards mattered: on average, beneficiaries in villages randomly chosen to receive the cards received about 26 percent (std. error: 5 percent) more subsidy than those in the control group. The evidence points to a mechanism through which information increased citizens' bargaining power vis-à-vis village officials. In particular, adding a single line to the cards with the copay price information printed on it dramatically increased the impact of the cards on the amount of subsidy received, but it did so primarily by increasing the quantity of rice received as opposed to lowering the copay price paid, suggesting that it improved recipients' ability to bargain with village heads rather than leading village heads to comply exactly with program rules. Moreover, publicly posting the information about the cards and the beneficiary list further increased the effectiveness of the cards.

The increase in subsidy to eligible households was achieved in a cost-effective manner. Overall, the estimated increase in subsidy received by households over the course of eighteen months was more than 7 times the cost of the intervention. In fact, the benefits of the cards exceed the costs within just two months. Increasing the costs by 30%, or even 100%, to account for the marginal cost of public funds (Ballard et al. 1985; Devarajan et

[39] For example, Case (1991) estimates an elasticity of demand for rice in Indonesia of 0.48.

al. 2002; Kleven and Kreiner 2006) does not change the conclusion that such an intervention is strongly welfare-improving. It is worth noting that this calculation implicitly values the social cost of "leaked" rice (i.e. rents captured by corrupt officials) at zero.

At some level, the idea that additional information can empower citizens to more effectively demand the fulfillment of their rights seems surprising for well-established and long-lived programs like Raskin. After all, shouldn't people already have the information? One might have thought that it should not be that hard to learn the rules, particularly general ones like how many kilograms of rice you are entitled to and at what price.

Given that providing this information has significant material benefits, the next question is why. There are a number of possible answers: perhaps people simply do not know that there are rules – they assume that it is all left to the discretion of the village leadership. Perhaps they know that there are rules, but they have the wrong version of the rules. Perhaps they know that there are rules but assume that the rules constantly change, which is certainly true of some government programs. Understanding the actual reasons behind the lack of information in the status quo is both interesting and important, and an area we hope to address in future research.

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Table 1: Effect of Card Treatment on Card Receipt and Use

	Eligible Households			Ineligible Households		
	Received Card (1)	Used Card (2)	Knows Own Status on Official List (3)	Received Card (4)	Used Card (5)	Knows Own Status on Official List (6)
Card Treatment	0.30***	0.15***	0.09***	0.03**	0.04***	0.05**
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
	[<.001]	[<.001]	[<.001]	[.031]	[.006]	[.017]
Observations	5,693	5,693	5,691	3,619	3,619	3,619
Control Group Mean	0.07	0.06	0.30	0.05	0.04	0.36
P-Value: Eligible vs Ineligible				<0.001	<0.001	0.135

Note: Each column in this table comes from a separate OLS regression of respective outcome on the treatment, strata fixed effects, and survey sample dummies. Data are pooled from the first and second follow-up surveys. Eligible households that did not receive a card under the bottom decile treatment are dropped from the sample and we re-weight the treatment group by sub-district so that the ratio of all three income groups is the same. Standard errors (in parentheses below coefficient) are clustered by village. Randomization inference p-values are in brackets below standard errors. Randomization inference p-values for testing the equality of the treatment effect on eligibles versus ineligibles are shown in the last row. Stars are based on standard (not randomization inference) p-values. *** p<0.01, ** p<0.05, * p<0.1

Table 2: Effect of Card Treatment on Subsidy

	Eligible Households				Ineligible Households			
	Bought in the Last 2 Months (1)	Amount Purchased (Kg) (2)	Price (Rp.) (3)	Subsidy (Rp.) (4)	Bought in the Last 2 Months (5)	Amount Purchased (Kg) (6)	Price (Rp.) (7)	Subsidy (Rp.) (8)
Card Treatment	0.02	1.25***	-57***	7,455***	-0.06***	0.07	-35	526
	(0.01)	(0.24)	(18)	(1,328)	(0.02)	(0.19)	(24)	(1,035)
	[.264]	[<.001]	[.005]	[<.001]	[.024]	[.758]	[.162]	[.669]
Observations	5,693	5,692	4,881	5,692	3,619	3,619	2,283	3,619
Control Group Mean	0.79	5.29	2,276	28,605	0.63	3.46	2,251	18,754
P-Value: Eligible vs Ineligible					0.003	<0.001	0.349	<0.001

Note: Each column in this table comes from a separate OLS regression of respective outcome on the treatment, strata fixed effects, and survey sample dummies. Data are pooled from the first and second follow-up survey. Eligible households that did not receive a card under the bottom decile treatment are dropped from the sample and we re-weight the treatment groups by sub-district so that the ratio of all three income groups is the same. For each household, the variables for amount purchased, price and subsidy are averages over the past four months; the current month is dropped if the interview occurred before the 25th day of the month (as the Raskin rice is distributed after that day). The amount and subsidy are set equal to zero if the household does not purchase any Raskin rice. The price is defined only among purchasing households and is dropped if below Rp. 500 or above Rp. 10,000. Standard errors (in parentheses below coefficient) are clustered by village. Randomization inference p-values are in brackets below standard errors. Randomization inference p-values for testing the equality of the treatment effect on eligibles versus ineligibles are shown in the last row. Stars are based on standard (not randomization inference) p-values. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Effect of Card Treatment on Subsidy, By Survey Round

	Eligible Households				Ineligible Households			
	Bought in the Last 2 Months	Amount Purchased (Kg)	Price (Rp.)	Subsidy (Rp.)	Bought in the Last 2 Months	Amount Purchased (Kg)	Price (Rp.)	Subsidy (Rp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Survey Round 1 (Approximately 2 months)								
Card Treatment	0.03	1.25***	-23	7,470***	-0.07*	-0.13	-16	-683
	(0.02)	(0.35)	(23.17)	(1,974.78)	(0.04)	(0.48)	(37.49)	(2,669.30)
	[.157]	[.001]	[.406]	[.001]	[.102]	[.8]	[.685]	[.813]
Observations	2,225	2,225	1,801	2,225	897	897	519	897
Control Group Mean	0.79	5.76	2,264.17	32,013.19	0.64	4.11	2,218.22	22,943.87
P-Value: Eligible vs Ineligible					0.03	0.009	0.905	0.006
Panel B: Survey Round 2 (Approximately 8 months)								
Card Treatment	-0.01	0.71***	-88***	4,538***	-0.09***	-0.08	-23	-385
	(0.02)	(0.27)	(26.38)	(1,503.03)	(0.03)	(0.17)	(33.75)	(917.57)
	[.723]	[.017]	[.002]	[.008]	[.005]	[.699]	[.538]	[.736]
Observations	1,778	1,778	1,576	1,778	1,756	1,756	1,115	1,756
Control Group Mean	0.80	4.98	2,299.13	26,197.73	0.62	2.92	2,294.63	15,338.40
P-Value: Eligible vs Ineligible					0.018	0.025	0.11	0.009
Panel C: Survey Round 3 (Approximately 18 months)								
Card Treatment	-0.01	0.74***	-45**	4,398***	-0.07**	-0.04	-20	-121
	(0.02)	(0.27)	(18.62)	(1,439.84)	(0.03)	(0.24)	(30.41)	(1,201.60)
	[.566]	[.015]	[.042]	[.007]	[.05]	[.873]	[.528]	[.916]
Observations	2,944	2,943	2,764	2,943	1,714	1,714	1,196	1,714
Control Group Mean	0.86	6.33	2,262.55	32,154.76	0.68	4.08	2,290.81	20,540.02
P-Value: Eligible vs Ineligible					0.087	0.03	0.37	0.018
P-Value of Difference 1 – 2	0.297	0.331	0.139	0.333	0.858	0.684	0.704	0.673
P-Value of Difference 1 – 3	0.045	0.130	0.891	0.087	0.973	0.796	0.973	0.799
P-Value of Difference 2 – 3	0.493	0.560	0.092	0.432	0.776	0.759	0.651	0.692
P-Value of Joint Equality Test	0.200	0.283	0.171	0.198	0.963	0.894	0.885	0.856

Note: Each column in each panel of this table comes from a separate OLS regression of respective outcome on the treatment, strata fixed effects, and survey sample dummies. We also provide the p-value of the difference between survey waves, based on randomization inference. Only households sampled using comparable sampling frames in each survey wave are included in each regression. Eligible households that did not receive a card under the bottom decile treatment are dropped from the sample and we re-weight the treatment groups by sub-district so that the ratio of all three income groups is the same. For each household, the variables for amount purchased, price and subsidy are averages over the past four months; the current month is dropped if the interview occurred before the 25th day of the month (as the Raskin rice is distributed after that day). The amount and subsidy are set equal to zero if the household does not purchase any Raskin rice. The price is defined only among purchasing households and is dropped if below Rp. 500 or above Rp. 10,000. Standard errors (in parentheses below coefficient) are clustered by village. Randomization inference p-values are in brackets below standard errors. Randomization inference p-values for testing the equality of the treatment effect on eligibles versus ineligibles are also shown (note that for Column 7, Panel A, this is calculated using district*previous treatment, rather than subdistrict*previous treatment, strata). Stars are based on standard (not randomization inference) p-values. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Effect on Subsidy of Printing Price on Cards

	Eligible Households				Ineligible Households			
	Bought in the Last 2 Months	Amount Purchased (Kg)	Price (Rp.)	Subsidy (Rp.)	Bought in the Last 2 Months	Amount Purchased (Kg)	Price (Rp.)	Subsidy (Rp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cards with Printed Price	0.03	1.17***	-49	6,802***	-0.03	0.12	-43	861
	(0.03)	(0.36)	(35)	(2,017)	(0.03)	(0.28)	(34)	(1,555)
	[.287]	[.002]	[.101]	[.002]	[.384]	[.673]	[.185]	[.565]
Cards without Price	0.02	0.55	-6	3,200*	-0.03	0.14	18	664
	(0.03)	(0.35)	(29)	(1,935)	(0.03)	(0.28)	(32)	(1,514)
	[.457]	[.136]	[.832]	[.125]	[.450]	[.598]	[.579]	[.652]
Difference:								
Price - No Price	0.01	0.62*	-43	3,602*	-0.00	-0.02	-62**	197
	(0.02)	(0.35)	(28)	(1,974)	(0.03)	(0.26)	(28)	(1,436)
	[.813]	[.088]	[.156]	[.074]	[.954]	[.910]	[.032]	[.879]
P-Value: Eligible vs Ineligible								
Cards with Printed Price					0.076	0.005	0.861	0.005
Cards without Price					0.164	0.243	0.416	0.201
Price - No Price					0.771	0.095	0.482	0.112
Observations	5,688	5,687	4,877	5,687	3,615	3,615	2,281	3,615
Control Group Mean	0.79	5.29	2,276	28,605	0.63	3.46	2,251	18,754

Note: Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, strata fixed effects, survey sample dummies, and a dummy for whether the village was also in the public information treatment. Data are pooled from the first and second follow-up survey. Eligible households that did not receive a card under the bottom decile treatment are dropped from the sample and we re-weight the treatment groups by sub-district so that the ratio of all three income groups is the same. For each household, the variables for amount purchased, price and subsidy are averages over the past four months; the current month is dropped if the interview occurred before the 25th day of the month (as the Raskin rice is distributed after that day). The amount and subsidy are set equal to zero if the household does not purchase any Raskin rice. The price is defined only among purchasing households and is dropped if below Rp. 500 or above Rp. 10,000. Standard errors (in parentheses below coefficient) are clustered by village. Randomization inference p-values are in brackets below standard errors. Randomization inference p-values for testing the equality of the treatment effect on eligibles versus ineligibles are also shown. Stars are based on standard (not randomization inference) p-values. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Effect of Card Treatment on Subsidy, by Household's Baseline Consumption

	Eligible Households				Ineligible Households			
	Bought in the Last 2 Months (1)	Amount Purchased (Kg) (2)	Price (Rp.) (3)	Subsidy (Rp.) (4)	Bought in the Last 2 Months (5)	Amount Purchased (Kg) (6)	Price (Rp.) (7)	Subsidy (Rp.) (8)
Card Treatment	-0.02	0.52*	-54*	3,175*	-0.09***	0.01	-42	205
	(0.02)	(0.30)	(28)	(1,622)	(0.03)	(0.17)	(37)	(909)
	[.601]	[.146]	[.104]	[.089]	[.001]	[.982]	[.252]	[.886]
Log Consumption	0.00	0.18	-18	950	-0.11***	-0.59***	-17	-3,107***
	(0.02)	(0.21)	(19)	(1,078)	(0.02)	(0.12)	(21)	(651)
	[.855]	[.353]	[.322]	[.355]	[.221]	[.559]	[.389]	[.567]
Treatment x	-0.02	-0.32	33	-1,938	0.02	-0.03	32	-176
Log Consumption	(0.02)	(0.29)	(24)	(1,573)	(0.02)	(0.15)	(27)	(798)
	[.479]	[.283]	[.219]	[.208]	[.463]	[.832]	[.278]	[.839]
P-Value: Eligible vs Ineligible								
Card Treatment					0.013	0.08	0.715	0.052
Log Consumption					0.016	0.019	0.993	0.012
Treatment x Log Consumption					0.174	0.284	0.986	0.216
Observations	1,266	1,266	1,148	1,266	1,925	1,925	1,235	1,925
Control Group Mean	0.82	5.09	2,313	26,653	0.62	2.99	2,305	15,663

Note: Each column comes from a separate OLS regression and includes strata fixed effects and survey sample. The sample is a group of households in the second follow-up for whom we have baseline consumption data. Eligible households that did not receive a card under the bottom decile treatment are dropped from the sample and we re-weight the treatment groups by sub-district so that the ratio of all three income groups is the same. For each household, the variables for amount purchased, price and subsidy are averages over the past four months; the current month is dropped if the interview occurred before the 25th day of the month (as the Raskin rice is distributed after that day). The amount and subsidy are set equal to zero if the household does not purchase any Raskin rice. The price is defined only among purchasing households and is dropped if below Rp. 500 or above Rp. 10,000. Standard errors (in parentheses below coefficient) are clustered by village. Randomization inference p-values are in brackets below standard errors. Randomization inference p-values for testing the equality of the treatment effect on eligibles versus ineligibles are also shown. Stars are based on standard (not randomization inference) p-values. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Effect of Distributing Cards Only to the Bottom Decile on Subsidy

	Bottom Decile Households				Other Eligible Households				Ineligible Households			
	Bought in the Last 2 Months	Amount Purchased (Kg)	Price (Rp.)	Subsidy (Rp.)	Bought in the Last 2 Months	Amount Purchased (Kg)	Price (Rp.)	Subsidy (Rp.)	Bought in the Last 2 Months	Amount Purchased (Kg)	Price (Rp.)	Subsidy (Rp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Cards to Bottom Decile	0.03	0.75**	-46**	4,536**	0.03	0.14	-10	1,049	-0.02	0.03	-15	231
	(0.02)	(0.34)	(23)	(1,907)	(0.02)	(0.34)	(30)	(1,923)	(0.03)	(0.25)	(28)	(1,374)
	[.187]	[.068]	[.116]	[.043]	[.340]	[.714]	[.775]	[.627]	[.587]	[.926]	[.655]	[.880]
Cards to All	0.01	0.75*	-44*	4,694**	-0.01	0.80**	-56*	4,997***	-0.06**	0.03	-7	248
	(0.02)	(0.39)	(25)	(2,208)	(0.02)	(0.34)	(30)	(1,931)	(0.03)	(0.27)	(31)	(1,482)
	[.625]	[.071]	[.134]	[.040]	[.756]	[.038]	[.100]	[.027]	[.079]	[.923]	[.847]	[.873]
Difference:												
Bottom Decile – All	0.02	0.00	-1	-158	0.03*	-0.67**	46*	-3,948**	0.04	0.00	-8	-17
	(0.02)	(0.35)	(22)	(1,979)	(0.02)	(0.31)	(26)	(1,765)	(0.03)	(0.22)	(25)	(1,219)
	[.358]	[.999]	[.946]	[.957]	[.145]	[.073]	[.126]	[.056]	[.209]	[.998]	[.774]	[.988]
P-Value: *vs Bottom Decile*												
Cards to Bottom Decile					0.811	0.055	0.146	0.049	0.109	0.085	0.328	0.065
Cards to All					0.464	0.870	0.655	0.877	0.051	0.114	0.296	0.083
Difference					0.565	0.022	0.031	0.019	0.564	0.999	0.846	0.949
P-Value: vs Other Eligible												
Cards to Bottom Decile									0.205	0.778	0.865	0.698
Cards to All									0.156	0.045	0.140	0.030
Difference									0.895	0.088	0.053	0.071
Observations	3,683	3,683	3,189	3,683	2,968	2,967	2,507	2,967	3,619	3,619	2,283	3,619
Control Group Mean	0.80	5.37	2,280	29,015	0.78	5.09	2,263	27,566	0.63	3.45	2,251	18,692

Note: Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, strata fixed effects, survey sample dummies, and a dummy for whether the village was also in the public information treatment. For each household, the variables for amount purchased, price and subsidy are averages over the past four months; the current month is dropped if the interview occurred before the 25th day of the month (as the Raskin rice is distributed after that day). The amount and subsidy are set equal to zero if the household does not purchase any Raskin rice. The price is defined only among purchasing households and is dropped if below Rp. 500 or above Rp. 10,000. Data are pooled from the first and second follow-up survey. Standard errors (in parentheses below coefficient) are clustered by village. Randomization inference p-values are in brackets below standard errors. Randomization inference p-values for testing the equality of treatment effect on eligibles versus ineligibles are also shown. Stars are based on standard (not randomization inference) p-values. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Effect of Public Information Treatment on Seeing the Eligibility List

	Eligible (1)	Ineligible (2)	Village officials (3)	Informal Leaders (4)
Panel A: Respondent of type […] has seen the list				
Public Information	0.14***	0.10***	0.24***	0.12***
	(0.02)	(0.02)	(0.06)	(0.05)
	[<.001]	[<.001]	[<.001]	[.012]
Standard Information	0.02*	0.01	0.05	-0.01
	(0.01)	(0.01)	(0.05)	(0.04)
	[.147]	[.723]	[.362]	[.853]
Difference:				
Public - Standard	0.12***	0.10***	0.18***	0.13***
	(0.02)	(0.02)	(0.06)	(0.05)
	[<.001]	[<.001]	[.001]	[.010]
Observations	5,685	3,619	496	385
Control Group Mean	0.07	0.06	0.36	0.12
Panel B: Respondent of type […] correctly knows whether respondent is on list or not				
Public Information	0.12***	0.08***	0.25***	0.00
	(0.02)	(0.03)	(0.05)	(0.07)
	[<.001]	[.001]	[<.001]	[.977]
Standard Information	0.06***	0.01	0.14***	-0.02
	(0.02)	(0.03)	(0.05)	(0.07)
	[.014]	[.618]	[.009]	[.812]
Difference:				
Public - Standard	0.06**	0.07***	0.11**	0.02
	(0.03)	(0.03)	(0.05)	(0.07)
	[.030]	[.009]	[.036]	[.778]
Observations	5,683	3,619	496	385
Control Group Mean	0.30	0.36	0.44	0.48

Note: Each regression is estimated by OLS and includes strata fixed effects and survey sample dummies. In Panel A, the sample is the stated category in the column and the outcome is a dummy indicating whether the individual has seen the eligibility list. "Do not know" answers are coded as zero (not seen). In Panel B, the sample is restricted to the stated category in the column header. The outcome is whether the respondent household correctly identifies its own status. "Do not know" answers are coded as zero. Data are pooled from the first and second follow-up survey. Standard errors (in parentheses below coefficient) are clustered by village. Randomization inference p-values are in brackets below standard errors. Stars are based on standard (not randomization inference) p-values. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Testing for Changes in High Order Beliefs

	Eligible	Ineligible	Village officials	Informal Leaders
	(1)	(2)	(3)	(4)
Panel A: Respondent believes that the […] category of individuals has seen the list				
Public Information	0.36***	0.27***	0.24***	0.24***
	(0.05)	(0.04)	(0.06)	(0.05)
	[<.001]	[<.001]	[<.001]	[<.001]
Standard Information	0.08**	0.02	0.04	0.05
	(0.04)	(0.02)	(0.05)	(0.04)
	[.096]	[.631]	[.454]	[.259]
Difference:				
Public - Standard	0.28***	0.25***	0.20***	0.19***
	(0.05)	(0.04)	(0.06)	(0.05)
	[<.001]	[<.001]	[<.001]	[.001]
Observations	9,304	9,304	9,304	9,304
Control Group Mean	0.31	0.15	1.04	0.47
Panel B: Respondent correctly identifies status of other households of […] type				
Public Information	-0.01	0.01	-0.00	-0.03
	(0.01)	(0.01)	(0.03)	(0.04)
	[.621]	[.38]	[.958]	[.347]
Standard Information	-0.00	0.03**	0.03	0.00
	(0.01)	(0.01)	(0.04)	(0.04)
	[.842]	[.045]	[.438]	[.937]
Difference:				
Public - Standard	-0.00	-0.02	-0.03	-0.04
	(0.01)	(0.02)	(0.04)	(0.04)
	[.774]	[.228]	[.403]	[.431]
Observations	64,540	34,757	4,155	4,215
Control Group Mean	0.66	0.32	0.60	0.63

Note: Each column in this table comes from a separate OLS regression of respective outcome on the public information treatments, strata fixed effects, survey sample dummies. Panel A includes all survey respondents. The outcome varies from 0 to 3, where 0 corresponds to "have not seen the list" and 3 corresponds to "most have seen the list"; "Do not know" answers are coded as zero. In Panel B, the respondents include all individuals (regardless of income group). The outcome is whether the individual correctly identifies other households in their village within each of the categories listed in the columns. "Do not know" answers are coded as zero. Data are pooled from the first and second follow-up survey. Standard errors (in parentheses below coefficient) are clustered by village. Randomization inference p-values are in brackets below standard errors. Stars are based on standard (not randomization inference) p-values. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Effect of Public Information Treatment on Card Receipt and Use

	Eligible Households		Ineligible Households	
	Received Card	Used Card	Received Card	Used Card
	(1)	(2)	(3)	(4)
Public Information	0.31***	0.18***	0.03*	0.04***
	(0.03)	(0.03)	(0.01)	(0.02)
	[<.001]	[<.001]	[.054]	[.013]
Standard Information	0.25***	0.10***	0.03**	0.04**
	(0.03)	(0.02)	(0.01)	(0.02)
	[<.001]	[<.001]	[.038]	[.015]
Difference:				
Public - Standard	0.06*	0.08***	-0.00	0.00
	(0.03)	(0.03)	(0.02)	(0.02)
	[.072]	[.007]	[.944]	[.925]
P-Value: Eligible vs Ineligible				
Public Information			<0.001	<0.001
Standard Information			<0.001	0.001
Public - Standard			0.062	0.002
Observations	5,685	5,685	3,619	3,619
Control Group Mean	0.07	0.06	0.05	0.04

Note: Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, strata fixed effects, and survey sample dummies, from the first and second follow-up survey. Eligible households randomized under the bottom decile treatment not to receive cards are dropped from the sample and we re-weight the treatment groups by sub-district so that the ratio of all three income groups is the same. Standard errors (in parentheses below coefficient) are clustered by village. Randomization inference p-values are in brackets below standard errors. Randomization inference p-values for testing the equality of the treatment effect on eligibles versus ineligibles are also shown. Stars are based on standard (not randomization inference) p-values. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Effect of Public Information Treatment on Subsidy

	Eligible Households				Ineligible Households			
	Bought in the Last 2 Months (1)	Amount Purchased (Kg) (2)	Price (Rp.) (3)	Subsidy (Rp.) (4)	Bought in the Last 2 Months (5)	Amount Purchased (Kg) (6)	Price (Rp.) (7)	Subsidy (Rp.) (8)
Public Information	0.01	1.64***	-81***	9,666***	-0.08***	0.12	-46	764
	(0.02)	(0.30)	(26)	(1,703)	(0.03)	(0.24)	(30)	(1,293)
	[.618]	[<.001]	[.001]	[<.001]	[.003]	[.611]	[.113]	[.539]
Standard Information	0.02	0.83***	-24	4,839***	-0.03	0.10	-19	623
	(0.02)	(0.31)	(29)	(1,764)	(0.03)	(0.25)	(30)	(1,347)
	[.349]	[.012]	[.360]	[.010]	[.377]	[.695]	[.511]	[.641]
Difference:								
Public - Standard	-0.01	0.81**	-58**	4,827**	-0.06*	0.02	-27	140
	(0.02)	(0.36)	(28)	(2,031)	(0.03)	(0.26)	(30)	(1,419)
	[.717]	[.040]	[.034]	[.032]	[.068]	[.931]	[.370]	[.917]
P-Value: Eligible vs Ineligible								
Public Information					0.001	<0.001	0.166	<0.001
Standard Information					0.081	0.018	0.863	0.017
Public - Standard					0.132	0.048	0.289	0.036
Observations	5,685	5,684	4,873	5,684	3,619	3,619	2,283	3,619
Control Group Mean	0.79	5.29	2,276	28,605	0.63	3.46	2,251	18,754

Note: Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, strata fixed effects, and survey sample dummies. We also provide the difference in the two card treatments. Eligible households that did not receive a card under the bottom decile treatment are dropped from the sample and we re-weight the treatment groups by sub-district so that the ratio of all three income groups is the same. For each household, the variables for amount purchased, price and subsidy are averages over the past four months; the current month is dropped if the interview occurred before the 25th day of the month (as the Raskin rice is distributed after that day). The amount and subsidy are set equal to zero if the household does not purchase any Raskin rice. The price is defined only among purchasing households and is dropped if below Rp. 500 or above Rp. 10,000. Data are pooled from the first and second follow-up survey. Standard errors (in parentheses below coefficient) are clustered by village. Randomization inference p-values are in brackets below standard errors. Randomization inference p-values for testing the equality of the treatment effect on eligibles versus ineligibles are also shown. Stars are based on standard (not randomization inference) p-values. *** p<0.01, ** p<0.05, * p<0.1

Table 11: Effect of Card Treatment on Protests and Complaints

	Indicator for whether village leaders reports any…				
	"Protests"	"Complaints" by those who receive rice	"Complaints" by those who do not receive rice	"Complaints" about list of beneficiaries	"Complaints" about distribution process
	(1)	(2)	(3)	(4)	(5)
Card Treatment	0.07***	-0.09***	0.08***	0.08***	-0.06**
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
	[.003]	[.002]	[.006]	[.007]	[.052]
Observations	1,143	1,144	1,144	1,144	1,144
Control Group Mean	0.11	0.43	0.22	0.18	0.41

Note: Each column in this table comes from a separate OLS regression of respective outcome on the treatment, strata fixed effects, and survey wave indicator. Data are pooled from village leader module of the first and second follow-up surveys. Standard errors (in parentheses below coefficient) are clustered by village. Randomization inference p-values are in brackets below standard errors. Stars are based on standard (not randomization inference) p-values. *** p<0.01, ** p<0.05, * p<0.1

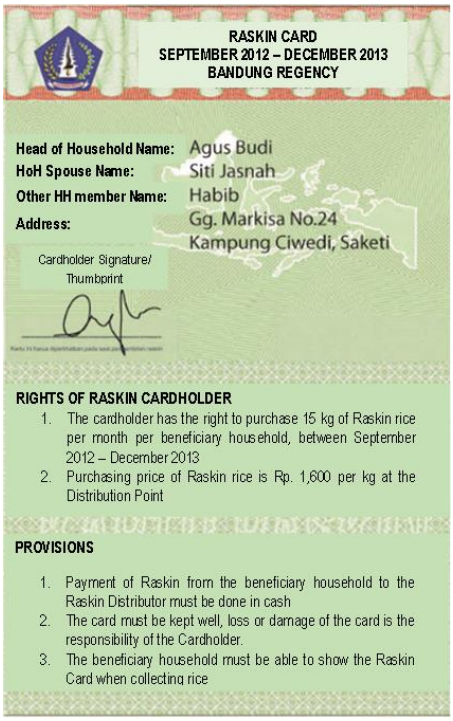
Figure 1: Experimental Design

		Cards Subtreatments					
		Information Type		Printed Price		Coupons	
	Total	Standard	Public	Yes	No	Yes	No
No Cards	194						
Cards to All	190	94	96	95	95	95	95
Cards to Bottom Decile	188	92	96	92	96	94	94
Total Villages	572	186	192	187	191	189	189

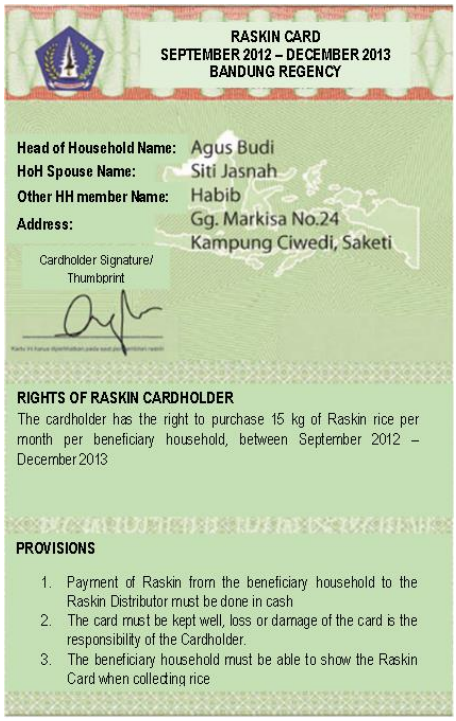
Note: This table lists the total number of villages randomly assigned to each of the treatments.

Figure 2: Raskin Cards with and without price

A: With Price



B: Without Price



Note: Figure 2A shows English translations of example Raskin cards with the printed price; Figure 2B shows the Raskin cards without the price printed. Original versions in Indonesian are available in the Appendix as Appendix Figure 3.

Figure 3: Project Timeline