Tangible Information and Citizen Empowerment

Identification Cards and Food Subsidy Programs in Indonesia

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Abstract

Local officials in developing countries often exercise considerable discretion in implementing the central government's policies, resulting in changes in program functioning and, in some cases, outright theft. While providing information about their rights could empower eligible beneficiaries to demand more, we show in a simple dynamic bargaining model that it could also backfire if it makes local officials less forward-looking because there are fewer rents to extract in the future. We investigate these issues by conducting an experiment in over 550 Indonesian villages in which the government mailed personalized cards confirming eligibility status and providing program information to beneficiaries of a subsidized rice program. Beneficiaries received 26 percent more subsidy in card villages. Ineligible households received no less, so this represents lower leakage. Consistent with the model, the evidence suggests that this effect is driven by improved citizen bargaining with local officials. Information about uniform program rules matters as well: experimentally adding the official price to the cards in some villages increased the subsidy by 11 percentage points compared to cards without price information. Additional public information increased higher-order knowledge about eligibility and led to a 14 percentage point increase in subsidy compared to just distributing cards. In short, providing information empowered citizens to reduce leakages and improve program functioning.

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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. These rules typically need to be administered by local politicians or bureaucrats who have their own interests, ranging from preventing conflict in their jurisdiction to promoting their career to lining their own pockets. To the extent that the implementing officials' interests differ from the government's intentions, the policies and programs that citizens experience might differ considerably from those on the books.

Consider, for example, a local official who administers a government transfer program, such as a subsidized food program or a work-fare scheme. There are many rules: who is eligible, what benefits they should receive, what they need to do to obtain the benefits, etc. In practice, however, the local official may exercise substantial leeway in how he implements the program. Citizens can challenge him if they think that they have been cheated, but it is hard to effectively do so without a full understanding of the official rules. The fact that it is costly to complain—with no guarantee of redress—may further exacerbate this problem. This sets up a simple bargaining game between the official and the program beneficiaries, where providing tangible information to the beneficiaries confirming their eligibility and explaining their rights under the program may change their beliefs about what they are entitled to, their bargaining with village officials, and ultimately what they receive.

This idea that an informed citizenry is essential to improving service delivery is widely accepted—for example, it was one of the main messages of the World Development Report (WDR) on "Making Services Work for Poor People" (World Bank 2004). It turns out, however, that this claim is less obvious than it may seem. Indeed, we show that in even a relatively simple dynamic bargaining model, more information does not always help, because reducing the possibility of future illicit rents may motivate the local official to steal more today.[1] Moreover, despite its importance, there has been relatively little empirical work on this question: of the 16 experimental and quasi-experimental studies on transparency and accountability reviewed by Kosack and Fung

[1] Niehaus and Sukhtankar (2013) describe these "golden goose" effects in the context of changes in citizen benefits in India's workfare program, NREGA.

(2014), only a few study the effects of providing just information, particularly for services where the government is the monopoly provider.[2]

We experimentally test these ideas within Indonesia's "Raskin" program ("Rice for the Poor"). Raskin is designed—in theory—to provide 15 kg of subsidized rice per month 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. Despite the fact that we observe citizen complaints about the implementation process, beneficiaries seldom receive their full entitlement and they pay 40 percent more than the official copay price—thus, on net, eligible households receive only about one-third of the intended subsidy.[3]

Working with the Government of Indonesia, we designed a set of field experiments to provide information 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. To unbundle the mechanisms through which different forms of information may affect program outcomes, 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 very public, and whether cards were sent to all eligible households or only to a subset.

We then surveyed both eligible and ineligible households in all villages, two months, eight months, and eighteen months after the cards were mailed.[4] Since the cards could affect both the amount of rice received and the price, we focus on understanding

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[2] Using a quasi-experimental design, 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 a larger share of the advertised grant. 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, but did not impact program outcomes.

[3] 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).

[4] The primary paper results are from the two and eight month follow-up surveys. After that, the government implemented other policies that may have also affected the control group. Nevertheless, we conducted another follow-up survey at 18 months and still find strikingly persistent treatment effects.

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. 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 and to shed light on the mechanisms that we outline in our model.

Eligible households received a large increase in subsidy due to the cards. Eligible households in treatment villages received a 26 percent increase in subsidy, stemming from both an increase in quantity and a decrease in the copay price.[5] Not only did eligible households receive more rice, but ineligible ones in total received no less, implying that the cards increased the total amount of distributed rice by 17 percent. This occurred despite an imperfect implementation: eligible households in treatment villages were only 30 percentage points more likely to have received a card relative to the control.

Two pieces of evidence suggest that these results are driven by a bargaining process along the lines we lay out in our model, and not simply that the officials thought that the cards implied greater central government "monitoring" of the program. First, we observe that protests and complaints to the local officials react directly to the card treatment, suggesting that more information drives changes in the negotiating process between officials and citizens as in our model. Second, while all of the cards provide a set of important information (e.g. eligibility status, entitled quantity, etc.), we randomly varied whether the co-pay price was listed on the card or not. This allowed us to trace the effect of a single, additional piece of information, conditional on card receipt. Printing the price nearly doubled the additional subsidy eligible households received relative to the effect from just providing cards. Interestingly, while all households (both eligible and ineligible) experienced small price declines in response to the price treatment, the effect of printing the co-pay price on the subsidy was driven by an increase in the quantity of rice that eligible households received. This is consistent with our 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, e.g. changing the quantity for just eligible

[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 rather than changing price for all.

One concern highlighted by our model -- and a genuine and oft-articulated worry for the Indonesian government in designing the interventions – is that that "too much information" could be counterproductive. 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. 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% who are typically eligible, thereby offering the leader more "flexibility" in his decisions.[6] In our model, under some assumptions, this has the potential to be better for the entire group of eligible households than sending cards to all because the leader now only has to satisfy a smaller group.

The empirical results, however, suggest otherwise: providing information to fewer households did not improve the overall functioning of the program. 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—looked no different than those in the control areas.

Another reason why information may be counterproductive is that deviations from program rules may have been 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 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 less likely to receive the rice as a result of the cards.

Another possibility, starting from our base intervention, is to provide public information in addition to private information. A distinctive feature of transfer programs is that the benefit in question is a private good and a citizen who is provided with private information can, in principle, use this information to more effectively claim his entitlement. However, it is also possible that providing more public information can generate collective action, where citizens can support one another in staking their

[6] 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.

individual claims. We therefore also explore the effect of providing public information to all citizens rather than just private information to beneficiaries. Specifically, 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 as we show 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, i.e. it also made citizens of all types more conscious of the fact that others knew about the official eligibility list. This higher-order knowledge could have potentially made it easier for villagers who were being denied their rights to coordinate with others in trying to get redress, and indeed, we find more organized protests in these villages.

Taken together, these findings strongly argue for the view that information about their rights is very scarce at least in poor populations in developing countries and even when the rules have existed for a long time. And, that providing citizens with clear information about their rights can enable them to better negotiate with officials to receive their entitlements.[7] Note that our findings on "bottom up" monitoring by a more informed citizenry – which persist even 18 months after the information intervention – stand in stark contrast to recent evidence from "top-down" monitoring interventions by central governments of local officials, which have often proved more difficult to sustain over time (Banerjee, Glennerster and Duflo, 2010; Dhaliwal and Hanna, 2014).

The remainder of the paper proceeds as follows. Section II describes the setting, experimental design and data. Section III outlines a simple dynamic bargaining model to underscore how information could change relative bargaining power and then provides

[7] And, do so in cost effective manner: the cards yield subsidy returns greater than 6 times their cost, even assuming the effect lasts only one year.

the overall program effects. Section IV explores whether providing information can backfire, while Section V explores the effect of additionally providing public information. Section VI concludes.

II. Setting, Experimental Design and Data

A. Setting

This project explores the impact of proving 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, allowing them to purchase 15 kg of rice – about half 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.[8] The intended subsidy value—about 4 percent of the beneficiary households' monthly consumption—is substantial. It is Indonesia's largest permanent targeted social assistance program: in 2012, the budget 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 village level by local officials appointed by the village head. 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 from the central distribution point (either in the nearby subdistrict or in the district capital), collecting co-pays 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. They may hire local laborers or rent trucks to assist in this process. 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, 2012). Targeting is also a problem: the local officials who administer the

[8] Officially, Raskin targets about the poorest 30% of the Indonesian population, based on a proxy-means test. Official eligibility status is updated every 3 years based on a national poverty census.

[9] There is a long history of local deviations from official eligibility lists in Raskin and its predecessor programs (Olken et al 2001; Olken 2005). Alatas et al (2013a) 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.

distribution have a high degree of *de facto* discretion over who can access it. For many reasons (such as political pressures, views of fairness, maintenance of social accord, and so forth), local officials distribute Raskin more widely than the central government intended: 63 percent of the officially ineligible households in our control group had purchased Raskin rice at least once in the last two months.[10] Since these ineligible households are generally richer than eligible ones, diverting rice to them reduces the program's redistributive goals. This also means that eligible households could not purchase as much as they want: 82 percent of eligible, control-group households reported that they wanted to buy more Raskin rice during the last distribution. Of these, 73 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) suggests 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% of the beneficiaries know that they are on the official eligibility list and they believe that the official copay price is 12% higher than the true price.

On the one hand, it is surprising that citizen information is so low on such an established program. However, government socialization efforts have focused on the local officials that implement the program rather than on 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

[10] 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 (e.g. village secretary), and neighborhood heads are in charge of Raskin distribution.

[11] By contrast, only 16 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.

amount; once that occurs, villagers may not know how large an increase is reasonable, allowing officials to pad the amount. Similarly, the fact that some poor households are indeed excluded from official eligibility lists due to natural errors in 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, so eligible are not getting the full 15kg allotment they are supposed to, it requires careful checking to make sure that all rice is redistributed properly and none leaks. 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 6 districts (2 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 the 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 by 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 program under the status quo. The government mailed a soft-copy beneficiary list to

[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, 2013a, 2013b for a description of the previous experiment).

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" and 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. This was done to understand if holding constant the card receipt, adding on an additional piece of information about a general program rule would increase the subsidy received.

Second, in half the card villages (randomly selected), all eligible households (30 percent of the village) 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 10 percent of the whole village). 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 not having a card. This allows us to shed light on

[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.

what happened when fewer people in the village are informed, theoretically reducing the pressure on local leaders to satisfy everyone.

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 2 days in each village, and so the relative 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 10th 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. 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

[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 10 percent.

[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.

two months after cards were mailed, and a second in March to April 2013, about eight 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-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 for all programs, 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, but caveat that these 18 month results may be affected by these other activities.

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 kilograms out of 15) 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 (Rp. 88,680).[17] Seven percent of eligible and 5 percent of ineligible

[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.

[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.

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. Model

Before beginning the empirical analysis, it is 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. This is important to formally analyze: the prevailing belief is that greater transparency will always improve citizen outcomes, but as we show, the impact may be more nuanced once we take into account the village official's incentives and how information changes the distribution of citizens' beliefs. We lay out the setup and main intuition here; full formal details and results can be found in Appendix 2.

In our framework, we suppose there is a population of potential beneficiaries of mass 1 indexed by i, who are 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, i.

The bargaining process is simple: 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. Complaining can yield higher benefits, but the (risk-neutral) villagers do not exactly know by how much. However, each villager has a prior p_i on the likelihood that he is eligible and, if so, conditional on complaining, he expects to receive B_i. 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 as well as potentially affecting 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 is surprisingly ambiguous. In fact, we show that if even one increases information available to eligible households, the effects even for those households are ambiguous. 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. On the other hand, complaints would increase, decreasing the likelihood the village head stays in office in the future, and effectively increasing his discount rate. This tends to lead the official to reduce X, and 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.

In the model, complaints do not necessarily arise when households are worse off; they arise from a disconnect between households' beliefs and what the village head offers them. This means that increasing the mean beliefs for eligibles can increase the offers village heads make to them *and* increase their complaints, since in general village heads offers will not increase enough to fully offset the increase in mean beliefs. The model also shows that there are potentially spillover effects of informing eligible households on the outcomes of ineligible households that operate through the village head's re-election probabilities.

The key point from the model is that the impact of even a simple change to information are not, ex-ante, as obvious as one might expect. The model also suggests looking at complaints as separate datapoints 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. We examine these issues in the empirical work below.

B. 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 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. 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 who were not on that list).[19] 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 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, addresses are difficult to access,

[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 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] 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.

village leaders block delivery to particular households or to the entire village, etc.[20] Nonetheless, it is a statistically significant and economically meaningful increase in the number of cards. By comparison, ineligible households in the treatment group were only 3 percentage points more likely to receive cards (Column 4). 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. Qualitatively, some households we interviewed 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 over 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 over 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.

C. 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

[20] Village leaders have the power to block distribution of cards because, in most rural areas, the post office does not know households' addresses, and instead rely on local leaders to help postmen identify who lives where. Anecdotally, in a number of cases, when our 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.

[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.

[23] Appendix Table 7 shows that the results are near identical regardless of adding or removing controls. Appendix Table 8 shows a larger gain in subsidy for eligible household in Java than off-Java, consistent

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 the 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).[24]

Ineligibles were 6 percentage points less likely to purchase Raskin in the last two months (Column 5). However, there is no significant difference in the total amount purchased by ineligibles (Column 6), since the quantity conditional on purchase also rose for the ineligibles that were able to buy after the treatment (Appendix Table 10). Thus, on net, there was no change in subsidy received by ineligible households (Column 8).

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. Weighting the eligible and ineligibles by their respective shares in the village, we estimate that the cards increase the total amount of rice distributed by 17 percent—thus, there was a 36 percent reduction in "lost" rice.

Finally, we estimate the treatment effect of cards by survey round, i.e. at the two, eight and eighteenth month mark of the program.[25] As shown in the control means in Table 3, despite fluctuations of the program functioning over time (e.g. in both quantity

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with treatment households in Java being more likely to receive cards.

[24] One might be concerned it is hard to distinguish a 1.2kg difference in rice—although this 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 6.5kg of rice, while still giving them only 5.3kg. Thus, we tested whether households could accurately assess the quantity of rice (Appendix Table 9). 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 standard errors 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 7kg and between 6 and 8kg (p-value=0.000), showing that eligible households can accurately assess differences of the size of the observed treatment effects.

[25] 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.

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.

D. *Impact of Cards on Protests and Complaints*

The bargaining model we lay out suggests that an important mechanism through which transparency could matter is through complaints or the threat thereof. Thus, we next explore whether the information from the cards changed the experience of the village head in his interactions with citizens. In Table 4, we investigate 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 beneficiary selection process, and complaints about the distribution process.[26]

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—about a 36 percent rise over the control group mean—in the treatment areas, while those who purchase Raskin rice complain less. The model in Appendix 2 gives an example of how the change in beliefs induced by the cards could produce exactly this pattern of changes in benefits and complaints. The treatment spurs more complaints about the beneficiary listing, and fewer complaints about the distribution process.[27]

In short, the fact that protests and complaints respond to the cards treatment in theoretically sensible ways suggests that citizen negotiation is a mechanism through which the cards ultimately improved the subsidy.

[26] 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."

[27] 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 11).

E. 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, we randomly varied 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 are provided in Table 5.[28] 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. The difference is primarily through quantity, rather than price.[29] 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).[30]

From the perspective of bargaining theory, 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. Price information 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. Increasing quantities may be more cost effective if it allows leaders to better discriminate between

[28] Appendix Table 12 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 13). We find that the effect of printing the price on cards is similar across both.

[29] One potential reason for the quantity increase is if households thought the price is lower, thus representing a demand effect. This seems very unlikely, however, since 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. Moreover, the quantity effects are sufficiently large that the demand for rice would need to be very elastic to explain these effects, which seems unlikely for an important staple.

[30] 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 14 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.

eligibles and ineligibles, i.e. there may be more pressure for a uniform price than for equal quantities. Importantly, though, the fact that it affects the quantity dimension is consistent with the bargaining story rather than one of perceived greater central government accountability: 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.

IV. Can Information Backfire?

In the previous section, we showed that information improved outcomes and that the likely mechanism was through citizens better being able to bargain with local leaders. However, a potential concern – both from the 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. In this setting, the program objective is 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 6, we test whether the card treatment shifted resources away from poor households, as measured by their per capita consumption measured prior to the experiment. We 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 6 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.

The second reason for the government's concerns – highlighted by the theory – is that too much information may put pressure on local leaders to reduce leakage and satisfy all eligible households if it changes the effective discount rate of the village head. In fact, not only is the overall effect of information ambiguous in the model, but we show that there can be cases where providing information to fewer individuals may actually improve outcomes for *all* eligible households as compared with providing information to everyone, since the leader now has the flexibility to satisfy a smaller group. (See Appendix 4).

To examine the tradeoff between providing information to all and providing information to some, we 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 all villages, the government mailed the complete eligibility list to the local leaders with instructions that all eligible households were allowed to purchase their Raskin allotment.

To examine the impacts, we split our sample of "eligible" households into two groups, those in the bottom 10 percent (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 10, 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 7 provides these findings for each of the three categories of households.[31]

This treatment did reduce pressure on the local leaders: overall protests were significantly lower in the villages where only cards were given to the bottom 10 rather than when cards were given to all (Appendix Table 16). 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 difference in propensity to buy, amount purchased, price or subsidy for those in the bottom percentile across the two types of villages (Columns 1-4 of Table 7).

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

[31] Appendix Table 15 shows the impact on card receipt, use, and knowledge. Card receipt and knowledge is identical for bottom 10 households in both types of villages but only increases for other eligible in "cards to all" villages.

all" received an increase in subsidy that was just as large (Columns 4 vs 8 of Table 7). Other eligible households that resided in villages where only the bottom decile received a card, by comparison, did not experience any gains (Column 6-8 of Table 7). This suggests that providing cards to the other eligible households directly increased their information, compared to when cards were provided only to bottom 10 households.

In short, we find no evidence that the additional information "backfired," either by reducing the ability of local leaders to "fix" bad national rules, nor by placing so much pressure upon them that the program stalled entirely.

V. The Effects of Providing Public Information

Finally, we explore the effect of providing public information to all citizens, regardless of eligibility status. In half of the card villages (randomly selected), the government conducted the "standard" card procedures: local leaders received the 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. This public information campaign may have had two types of effects: it could have increased households' information and it could have increased households' higher-order beliefs about what other households knew, potentially reducing their cost of complaining by facilitating coordination.

Table 8 begins by examining the impact on whether households had seen the beneficiary list. In Panel A, for each of four key demographic 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 dummies 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 demographic categories. In contrast, the "public information" treatment greatly increased access: 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).[32]

The public information increased knowledge of one's own eligibility status (Table 8, 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 increase in knowledge relative to no cards and about a 17 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) more likely to know it under public information (Column 2).

The second way the public treatment could operate was by changing people's beliefs about others' access to information (i.e. higher order beliefs).[33] This may be important if challenges to authority feature strategic complementarities: a village head may be able to retaliate against a lone individual, but it may be harder to retaliate against a group. In the language of the model, the per-person cost C may be decreasing in the number of people who complain. A villager deciding whether to challenge a village head may therefore 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 9, Panel A, we ask all survey respondents how likely members of each of the four demographic 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

[32] We coded anyone who reported not knowing whether they had seen the list as not having seen it. In Appendix Table 17, we drop those who reported "do not know" and find near identical results.

[33] Specifically, we test for whether respondents of type X believe that respondents of type Y have seen the list of beneficiaries, for all X and Y of eligible households, ineligible households, formal village leaders, and informal leaders. This is technically a second-order belief (i.e. do you believe that Y knows), whereas full common knowledge encompasses all higher-order beliefs (i.e. do you believe that Y knows that you know, and so on), but is the highest-order belief that we were practically able to elicit during a survey. Given that the treatment involved posting the list publicly, and we see results on second-order beliefs, it is likely that we moved towards full common knowledge as well.

likely to report that any type of individual had seen it. 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 9).[34]

With respect to the model, one can interpret the public information treatment as potentially affecting three things. First, the information set of eligible households improved. Second, the information set of ineligible households improved. Third, if common knowledge reduced the cost of complaining (e.g. because it is easier to coordinate), then C went down. We show in Appendix 2 (Result 4) that under plausible assumptions, improving everyone's information and reducing C should lead to an increase in the amount received by the eligible and a reduction in their complaints. The impact on the ineligible is theoretically ambiguous. In Appendix 3, we provide a numerical example where everyone's information improves, C goes down, and as a consequence, eligibles receive more, complain less, and the opposite happens for the ineligible.

Tables 10 and 11 examine the impact of the additional information on program outcomes. Eligible households were both more likely to receive their card and use them under public information, with no change for ineligible households (Table 10). The magnitude of these differences for the eligible is large: they were 19 percent more likely to have received a card and 50 percent more likely to use it than under the standard socialization. Addresses in rural areas are difficult to find, and so the post-office relies on local leaders for help in locating households; the fact that beneficiaries were more likely to receive the card in the public information treatment suggests that without public knowledge, village leaders were able to block cards to maintain their rents, but were less able to do so once information about the cards was publically provided.

The public information nearly doubled the subsidy that eligible households received relative to the standard information card villages (Table 11). 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

[34] As Appendix Table 18 shows, there is no difference between eligible and ineligible households.

decrease in the theft of rice. Although as discussed above, the impact on complaints and protests is theoretically ambiguous, we also observe more protests and complaints about the beneficiary list from those who do not receive the rice (Appendix Table 19).

One question is whether the public information worked by simply increasing the 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 20).[35] 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). 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. 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 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

[35] The corresponding first stage and reduced form regressions are presented in Appendix Table 21.

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 also further increased the effectiveness of the cards, again suggesting an important role for increasing information more broadly.

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 kilos 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 (which then raises the question, why does political competition not fix that?). Perhaps they know that there are rules, but assume that the rules constantly change, which is certainly true of some government programs. If so, this introduces a potential cost of trying to reform 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	Used Card	Knows Own Status on Official List	Received Card	Used Card	Knows Own Status on Official List
	(1)	(2)	(3)	(4)	(5)	(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)
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

Note: This table provides the reduced form effect of belonging to the card treatment group on card outcomes and knowledge by eligibility status. 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 ten 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 are clustered by village. *** 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	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)
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)
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

Note: This table provides the reduced form effect of belonging to the card treatment group on rice purchases by eligibility status. 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 10 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, whereas the price is calculated among purchasing households. Standard errors are clustered by village. *** 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 (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)
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)
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
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)
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
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)
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 of Difference 1 – 2	0.14	0.15	0.03	0.16	0.65	0.93	0.87	0.91
P-Value of Difference 1 – 3	0.06	0.13	0.36	0.09	0.95	0.85	0.91	0.81
P-Value of Difference 2 – 3	0.92	0.89	0.10	0.92	0.45	0.86	0.94	0.83
P-Value of Joint Equality Test	0.16	0.28	0.10	0.23	0.75	0.97	0.99	0.95

Note: This table provides the reduced form effect of belonging to the card treatment group on rice purchases and use by eligibility status, separately for each of the survey's three rounds. 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. 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 ten 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, whereas the price is calculated among purchasing households. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Effect of Card Treatment on Protests and Complaints

	Indicator for whether village leaders reports any…				
	"Protests" (1)	"Complaints" by those who receive rice (2)	"Complaints" by those who do not receive rice (3)	"Complaints" about list of beneficiaries (4)	"Complaints" about distribution process (5)
Card Treatment	0.07***	-0.09***	0.08***	0.08***	-0.06**
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
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: This table provides the reduced form effect of belonging to the card treatment group on village leaders' reports of protests or complaints related to the Raskin program in the 12 months preceding the survey. 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 are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Effect of Printing Price on Cards on Subsidy

	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)
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)
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)
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: This table provides the reduced form effect of belonging to the Price and No Price treatment groups on rice purchases by eligibility status. 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. We also provide the difference in the two card treatments. Data are pooled from the first and second follow-up survey. Eligible households that did not receive a card under the bottom ten 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, whereas the price is calculated among purchasing households. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Effect of Card Treatment on Subsidy, by Consumption

	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)
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)
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)
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)
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: This table provides the reduced form effect of belonging to the card treatment group on rice purchases by eligibility status, interacted with the z-score of pre-treatment log consumption. 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 ten 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, whereas the price is calculated among purchasing households. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Effect of Distributing Cards Only to the Bottom 10 Percent on Subsidy

	Bottom 10 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 10	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)
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)
Difference:												
Bottom 10 – 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)
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: This table provides the reduced form effect of belonging to the Bottom Ten and All Cards treatment groups on rice purchases, by eligibility status, as compared to the control group. 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. We also provide the difference in the two card treatments. 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, whereas the price is calculated among purchasing households. Data are pooled from the first and second follow-up survey. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 8: 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)
Standard Information	0.02*	0.01	0.05	-0.01
	(0.01)	(0.01)	(0.05)	(0.04)
Difference:				
Public - Standard	0.12***	0.10***	0.18***	0.13***
	(0.02)	(0.02)	(0.06)	(0.05)
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 it is on list or not				
Public Information	0.12***	0.08***	0.25***	0.00
	(0.02)	(0.03)	(0.05)	(0.07)
Standard Information	0.06***	0.01	0.14***	-0.02
	(0.02)	(0.03)	(0.05)	(0.07)
Difference:				
Public - Standard	0.06**	0.07***	0.11**	0.02
	(0.03)	(0.03)	(0.05)	(0.07)
Observations	5,683	3,619	496	385
Control Group Mean	0.30	0.36	0.44	0.48

Note: This table provides the reduced form effect of the public information treatments on seeing the beneficiary list and correctly identifying own beneficiary status. 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 each 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 are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 9: 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)
Standard Information	0.08**	0.02	0.04	0.05
	(0.04)	(0.02)	(0.05)	(0.04)
Difference:				
Public - Standard	0.28***	0.25***	0.20***	0.19***
	(0.05)	(0.04)	(0.06)	(0.05)
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)
Standard Information	-0.00	0.03**	0.03	0.00
	(0.01)	(0.01)	(0.04)	(0.04)
Difference:				
Public - Standard	-0.00	-0.02	-0.03	-0.04
	(0.01)	(0.02)	(0.04)	(0.04)
Observations	64,540	34,757	4,155	4,215
Control Group Mean	0.66	0.32	0.60	0.63

Note: This table provides the reduced form effect of the public information treatments on beliefs about others seeing the eligibility list and ability to identify others' beneficiary status. 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 are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 10: 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)
Standard Information	0.25***	0.10***	0.03**	0.04**
	(0.03)	(0.02)	(0.01)	(0.02)
Difference:				
Public - Standard	0.06*	0.08***	-0.00	0.00
	(0.03)	(0.03)	(0.02)	(0.02)
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 ten 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 are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 11: Effect of Public Information Treatment on Subsidy

	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)
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)
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)
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)
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: This table provides the reduced form effect of public information treatment groups on rice purchases, by eligibility status, as compared to the control group. 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 ten 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, whereas the price is calculated among purchasing households. Data are pooled from the first and second follow-up survey. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

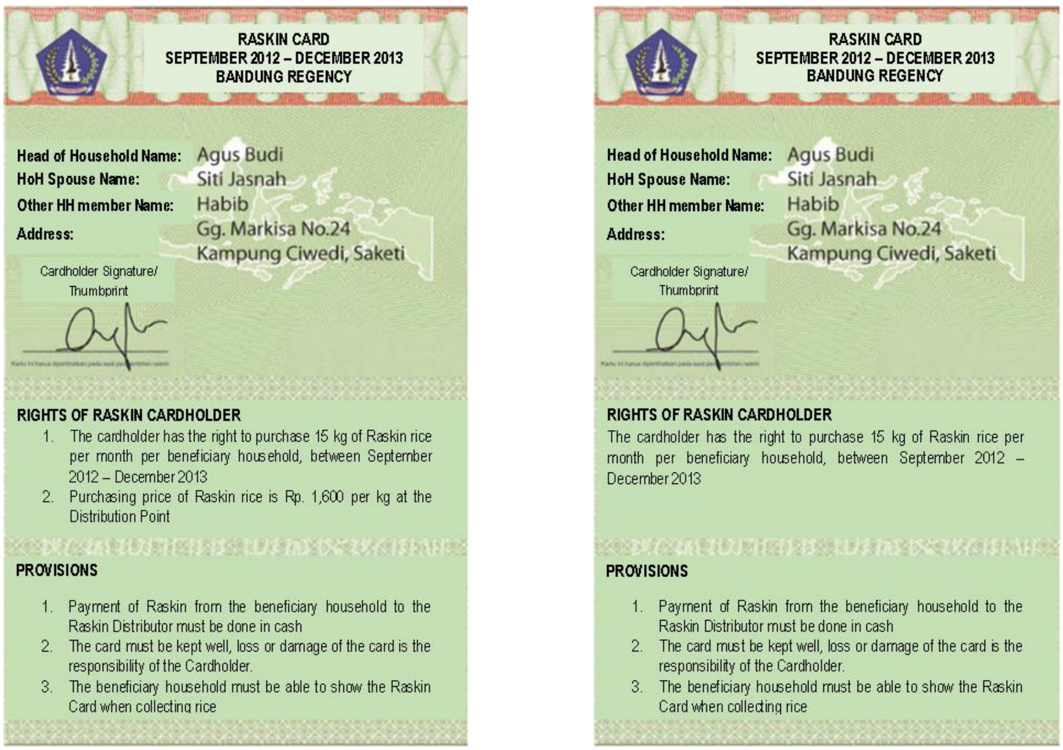
Figure 1: Experimental Design

		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 10	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