# Information is Power:
## *Identification Cards and Food Subsidy Programs in Indonesia*

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Abstract

This paper studies how governments can improve aid programs by providing information to beneficiaries. In our model, programs are administered by local officials, who bargain with recipients over how much the official will keep. We test how information affects the distribution of aid empirically through a large-scale, randomized field experiment in over 550 Indonesian villages in which identification cards with information about beneficiaries' rights were mailed out by the central government to beneficiaries of a subsidized rice aid program. To tease out mechanisms through which the cards matter, we randomly varied the number of cards distributed, the precise information written on the cards, and whether information about the program was publicly posted. We find that overall, eligible households received about 25 percent more of the subsidy in identification card villages. Ineligible households did not get less, so this represents a reduction in leakage. Adding a single line to the cards showing the official copay price led to a 12 percent increase in subsidy received, compared to cards without the copay price information. We show that providing public information increased higher-order beliefs about program eligibility, and also led to a 15 percent increase in subsidy received compared to the cards with only private information. On net, the results show that increased transparency about program rules and eligibility can substantially reduce leakage and improve aid programs.

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## I. Introduction

Throughout the developing world, central governments face the problem of ensuring that rules and laws are implemented as intended. These rules need to be implemented by someone—such as a local politician or a local bureaucrat—who has his own interests, from preventing conflict in his jurisdiction to promoting his career all the way to lining his own pockets. To the extent that the implementing official's interests differ from the central government's intentions, the rules and policies that citizens experience might differ considerably from those on the books.

Consider, for example, a local official in charge of implementing a transfer program, e.g. a subsidized food program or a work-fare scheme. There are a myriad of rules: who is eligible, what benefits should they receive, what they need to do to receive them, etc. In practice, the local official may have substantial leeway in their implementation. Citizens can challenge him, perhaps by appealing to an outside authority, like the central government, if they believe that they have been cheated. However, it is hard to effectively do so if citizens do not fully understand what they are entitled to under the rules. The fact that it is costly to complain—with no guarantee of redress—may further exacerbate the problem.

This sets up a simple bargaining game between the local official and the program beneficiary. The beneficiary's beliefs play a key role: those who believe that they are entitled to more would be more inclined to challenge the implementing official, conditional on receiving the same offer. We show that increased transparency of the program rules leads beneficiaries to receive a greater share of their entitlement through raising mean beliefs, or simply by reducing variance of the said beliefs even if does not change the mean.

Empirically testing this model is challenging. In addition to providing information to citizens that they can then use in negotiation with the local officials, greater transparency may also provide information to the local officials as to what rules the central government cares about.[1] Thus, the impact of transparency could simply stem from the local official updating his beliefs as to the focus and level of monitoring by the central government. If this is the case, there may be more direct, cost effective mechanisms to impose this greater central government accountability on local officials than taking the time and expense to inform the populace.

We experimentally test these ideas within the context of a subsidized rice program in Indonesia. The program, known as "Raskin" (Rice for the Poor), is designed—in theory—to provide 15 kg of subsidized rice per month to eligible households, who represent about 27 percent of the population. With an annual budget of US$1.5 billion, and a targeted population of 17.5 million households, the program is Indonesia's largest, targeted transfer program. In practice, the program is not implemented as intended: our survey reveals that while 83 percent of eligible households bought the subsidized Raskin rice, they did not receive their full entitlement. Some rice was diverted to others, with nearly 70 percent of ineligible households also purchasing Raskin rice in the same period; other rice was siphoned off along the way (Olken, 2006; World Bank, 2012). Beneficiaries pay an inflated price, nearly 40 percent higher than the official copay amount. On net, eligible households received only about a third of the intended subsidy.[2]

[1] In fact, conceptually, this is the case in many government programs. For example, consider an experiment that provided new textbooks to teachers: did the new textbooks improve learning or is it the signal from the central government that textbook should be used? Use of the textbooks could increase in both cases, either from teachers and students believing that the new textbooks are better or from simply believing that the government thinks that they should use textbooks more.

[2]Leakages and mis-targeting are common to many aid programs, both in government-run programs and those that are supported by foreign aid. For example, Niehaus, Atanassova, Bertrand, and Mullainathan (2013) describe how many ineligible households buy subsidized products through India's public distribution system and the price charged is, on average, higher than the stated price. Nunn and Qian (forthcoming) describe how much of food aid supplied by foreign aid goes missing; for example, the UN World Food Program has released reports 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).

Working with the Government of Indonesia, we designed an experiment 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 provide clarity on their eligibility status and the quantity of rice that they were entitled to. As we discuss in greater detail below, the government also experimentally varied the way the card program was implemented in different villages, allowing us to provide insights into whether the program changed the bargaining power of citizens through providing information about program rules or simply increased local officials' perception of central government accountability.

We then surveyed both eligible and ineligible households in villages where cards were mailed out and in the control villages, both two months and eight months after the cards were mailed. Since the cards could affect the amount of rice, as well as the price paid, 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.

We find that the beneficiary card distribution strikingly increased the subsidy amount received by eligible households. It is worth noting that this occurred despite the fact that the card distribution itself was not fully implemented as the central government intended: eligible households in treatment villages were only 28 percentage points more likely to have received a card relative to those in the control villages. Yet, despite this, eligible, treatment households received a 25 percent increase in subsidy relative to equivalent households in control villages, stemming from both an increase in quantity purchased and a decrease in the copay price.[3] On net,

[3] Note that this is the reduced form effect for all eligible households (regardless of card receipt), so the implied treatment-on-treated effect would be three times as large, assuming no spillovers to those who did not receive a card.

there was no reduction in total subsidy received by ineligible households, implying that the cards reduced leakage of rice, increasing the total amount of rice distributed in the villages by 8 percent, rather than just transferring rice from ineligible to eligible households.

Note that the increase in subsidy received by beneficiaries was similar at both the two month and eight month follow-up. If this impact was due simply to the local official believing that the central government was imposing greater accountability on them for these particular rules that the cards made salient, it seems plausible that the effect would dissipate by the eight-month mark—once the local official learned that the central government was not doing anything extra for violations of this particular program's rules (violations continue, though at a reduced rate, so they should be able to observe any difference). The sustained effect is more consistent with the story that citizens and local officials have negotiated a new equilibrium subsidy based on the citizen's updated information.

Explicitly providing even *more* information to citizens directly reinforced the effects we observed in the basic intervention. In a randomly chosen group of villages, in addition to mailing out the cards, the beneficiary list was publicly posted throughout the village and information about the cards was played over a village loudspeaker system. This could change knowledge in two ways: First, beneficiaries and non-beneficiaries alike (including the village elites) had a chance to learn about the program rules, the list and the cards. Secondly, it promoted second order knowledge—villagers were made more conscious of the fact that other villagers knew about these things, potentially making it easier for villagers who were being denied their rights to collaborate with other villagers in trying to get redress.

We find that this additional public information does have these second-order knowledge effects: households were not only more likely to have seen the beneficiary list, they were also more likely to believe that others had seen it as well. In turn, the effects of the cards are substantially higher in this treatment: On net, eligible households received twice as much additional subsidy as they do under cards with the standard information treatment. We show that the additional impacts of public information, however, is only experienced by those assigned to receive the cards, suggesting that public information facilitates coordination among those who have cards rather than by changing the village government's approach more generally.

Three additional pieces of evidence strongly suggest that the impacts were due to citizen use of the information, rather than due to changes in the official's belief about central government monitoring. First, in half of the villages chosen at random, the cards were printed with information about the official copay price (Rp. 1,600 per kg) in addition to the quantity of rice eligible households were to receive (15 kg per month); in the rest, the cards only contained the information about quantity. This treatment should not have additional accountability effect, since what was done from the government side was exactly the same in both cases (e.g. the official program documentation and frequently-asked questions sheet sent to village heads included the copay price information in all cases) and therefore this can be seen as close as possible to a pure information intervention. We find that the total subsidy received by eligible households in villages where price was printed on the cards was about double that that in villages where cards were printed without price information, confirming that there is a strong pure transparency effect. Interestingly, the impact of printing the copay price on the cards largely occurred through an increased quantity of rice received by eligible households, not a lower price, suggesting that it affected bargaining rather than attention to the price *per se*, which further suggests a bargaining story rather than an accountability story.

Yet another variant of the program helps us argue, albeit somewhat more tentatively, that in fact the entire effect of the program came from transparency: in a random set of villages, cards were only mailed to the bottom decile of households, as opposed to mailing them to all beneficiaries. The full list of beneficiaries given to the village head was identical in both treatments, so the leader's information about who is eligible was the same. If the fact that the cards had been sent had had any influence on the leader's beliefs about the likelihood of a government audit, we would expect the eligible households not in the bottom decile to benefit even when they themselves did not receive a card. We see no evidence of this. Eligible households only experienced an improvement in receipt of rice in villages where they were randomized to receive the cards.[4]

Finally, our collection of interventions included one that was purely focused on accountability. In half the villages, the cards had clip-off coupons to be collected by the village leader from those whom he gave the rice and remitted to the central government. This could make it costlier for the village leader to give the rice to the wrong person, since there would be no coupon to show. At the same time it could increase the bargaining power of eligible villagers vis-à-vis the village head because they have the option of refusing to give him the coupon if he does not give them what they want.

We find support for the first effect but not the second: the ineligible villagers receive less in the coupon villages, but the eligible do not receive more. One reason could be that the villagers do not understand the systems of governance well enough to understand the role played by the coupons and how they could exploit it.

[4] Someone who is keen on the accountability view could argue that the leader believed that the central government only cares about those who were sent the cards, but given that the leaders received a list from the government that listed all eligible villagers at the same time, this seems implausible.

The fact that the non-eligible villagers were hurt by the last intervention flags a general concern about governance interventions, which has to do with multitasking. Transparency could encourage officials to reorient their focus to only those aspects of the program that become visible, at the expense of other dimensions that remain hard to observe. For example, in our context, the broad goal of the program was to distribute rice to the poor. Improving compliance with the government-issued beneficiary listing is only a means to get there. Given that the listing is often inaccurate (see, e.g., Alatas et al 2012), one could imagine a scenario where a benevolent local official would deviate from the list to ensure that poorer (excluded) households receive access to the program; distributing cards in this context may simply force the bureaucrat to comply with the listing and thus reduce access for the ineligible poor. We find, however, no evidence that the poor, ineligible households were hurt by the cards, suggesting this is not a major concern in this context.

We also see no evidence of spillovers within the eligible group. The total subsidy paid out by the leader is much higher in the case where the entire eligible group got cards than where they were only mailed to the bottom ten percent, but the subsidy per person in the bottom percent remains the same. Finally we see do some evidence of a positive spillover from the eligible population to the ineligible, but only on prices: In the case where the price was printed on the card, the actual price paid goes down even for those who were not eligible, perhaps because the information gets around (Olken 2009). This suggests that it is easier for village officials to discriminate based on quantities than copay prices. It also may explain why, in response to the information where price was printed on the cards, village officials responded by allocating larger quantities to those who received the cards rather than lowering the price: they could increase quantities just for those who had the additional information, but would have had to cut prices for all.

While transparency is almost universally perceived as vital in reducing the rampant corruption observed in many developing countries–so much so, that the leading international anti-corruption organization is called "Transparency International"—there is surprisingly little rigorous evidence that transparency actually directly (as against through its effects on electoral outcomes) reduces leakages and improves government performance.[5] One reason why transparency may be ineffective is that citizens lack the power to use the information, but another might be that the information is not provided in a form that is easy to process. One striking fact that emerges from our study is the close tie between the card itself and the outcome. The bottom ten percent treatment had no effect on eligible households outside the bottom ten, despite the fact that all eligible households were entitled to the same amount of rice and that amount was printed on the cards that went to the bottom ten and that information could have easily spread to the rest. Likewise the enhanced socialization seems to have had no effect on the knowledge of those eligible households who did not have a card (i.e. in villages where only the bottom ten got cards) though it had big effects on those who had the cards. In other words the original affirmation of the information through the possession of the cards was key for the villagers to take advantage of the information provided through the enhanced socialization. In this sense the cards may have

[5] Thus far, the literature is relatively small, often with contrasting results: for example, using data from Uganda, Reinikka and Svensson and Reinikka (2004, 2005) found that when the government implemented a national campaign to advertise the grant, schools closer to a newspaper outlet were more likely to receive a larger share of the entitlement. On the other hand, Banerjee, Banerji, Duflo, Glennerster and Khemani (2010) find no effects of providing information about the (poor) performance of schools to parents on any form of collective action vis-à-vis the schools and the educational system, and the only effects on tests scores came from private initiatives to teach the children outside school. Ravallion, van de Walle, Dutta, and Murgai (2013) report the results of an intervention that provided 40 villages in India (randomly selected out of 150) with a 25 minute video that provided information on India's work for cash program; while the program increased knowledge of their entitlements, they find no impact on actual program outcomes, thereby arguing that supply-side changes must also be made.

been an especially effective way to convey the relevant information (who were eligible, for how much, at what price).

The remainder of the paper proceeds as follows. Section II describes the setting, experimental design and data. Section III shows a simple model to outline how the cards could change bargaining and the amount of subsidy households receive. We discuss the overall effects of the card treatment in Section IV, and we explore different potential mechanisms in Section V. Section VI concludes.

## II. Setting, Experimental Design and Data

### *A. Setting*

This project explores a set of different mechanisms for increasing citizens' information about Indonesia's subsidized rice program, known as "Raskin" (Rice for the Poor). The program was first implemented in 1998; in 2012, the program targeted 17.5 million low-income households, allowing them to purchase 15 kg of rice at a copay price of Rp. 1,600 per kg (US$0.15), which is about one-fifth of the prevailing market price. On net, the value of the intended subsidy is substantial, equaling about 4 percent of the beneficiary households' monthly consumption. It is the largest permanent, targeted social assistance program in Indonesia: in 2012, the government allocated a budget of Rp. 15.7 trillion—approximately $1.5 billion, or 53 percent of all targeted social assistance expenditures by the central government—to distribute 3.41 million tons of subsidized rice (Indonesian Financial Note and Revised Budget, 2012).

Beneficiaries, however, do not necessarily receive these benefits. Leakages are abundant, with a substantial amount of rice never reaching citizens (Olken, 2006; World Bank, 2012). A second problem relates to targeting: local government officials administer the distribution, and thus have a high degree of *de facto* discretion over who can access the program.[6] Even when they distributed the rice, for a variety of reasons (such as political pressure, views of fairness, to keep social accord, and so forth), local officials distribute Raskin more widely than intended by the central government when it designed the program: 68 percent of officially ineligible households in our control group had purchased Raskin rice at least once during the last two months. Since these ineligible households are generally richer than the eligible households, diverting the rice to these households reduces the redistributive success of the program. Third, local leaders often charge a higher copay price for the rice than the government intends: in the control group, eligible households believed the copay was 25 percent higher than the official copay, and in some cases households were charged as much as 40 percent more than the official Raskin copay price.[7] The higher price may also dissuade households from purchasing their allocation, particularly if they are credit constrained. On net, the combination of these problems – leakage of rice from the program, diversion of rice from eligible to ineligible households, and inflated copay prices – add up eligible households receiving only a third of their intended subsidy.[8]

### *B. Sample*

This project was carried out in 6 districts in Indonesia (2 each in the provinces of Lampung, South Sumatra, and Central Java). Due to the constrained timeframe for providing feedback into the national policy, we chose to conduct the experiment in villages where we had previously

[6] Alatas et al (2013a) show that the manipulation of the beneficiary lists by local leaders likely happens during the distribution of the rice, rather than through the determination of the official eligibility lists.

[7] Some of this stems from the fact that local leaders bear real transport costs in collecting and distributing the rice (e.g. trucks rentals, storage space), but qualitative research suggests that higher price often exceeds the real distribution costs (Smeru 2008).

[8] Authors' calculation from control group of sample.

worked and thus had household level data that could serve as a baseline for the new experiment.[9] As we discuss below, we stratified the treatment assignments in this project by the previous experiment to ensure balance across the previous interventions. 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).

Within these districts, we had originally randomly sampled 600 villages. Prior to conducting the randomization, we dropped 28 villages that were deemed too unsafe to send survey teams. Thus, the final sample comprised 572 villages. Note, again, to capture important heterogeneity across institutions, we ensured that the sample was consisted of about 40 percent urban and 60 percent rural villages.

### *C. Experimental Design*

Within the sample of 572 villages, we first randomly assigned the villages to one of two primary treatment assignments: those in which eligible households received identification cards and those in which they did not. The treatment status was stratified on geographic strata (i.e. subdistrict) and the previous experiment conducted in this region. Details of the treatment assignments are as follows:

<u>Status Quo</u>: We randomly assigned 194 villages to the current status quo for Indonesia (see Figure 1). In these villages, the government mailed a soft-copy list of the beneficiaries to districts with instructions to send one hard copy listing to the village government. The government also mailed an informational packet on program rules directly to village governments, including

[9] 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).

instructions to post the beneficiary list in a public place and to distribute rice only to households on the list. In these villages, households did not receive the Raskin identification cards or any other form of information from the central government about their eligibility status.

Identification Cards: As shown in Figure 1, the remaining 378 villages were randomly assigned to receive the identification cards, as well as instructions on how to use them, from the national government. In addition, the government still informed the district and village officials of the beneficiary lists as in the status quo and also requested that leaders post the list, as in the status quo.

An example of an identification card is shown in Figure 2. As is evident from the Figure, the identification cards contain the name of the household head, the address, a unique Raskin identification number, and instructions that the beneficiaries are entitled to receive 15 kilograms of subsidized rice per month. The government in Jakarta mailed the cards via the postal service directly to beneficiary households. Postmen delivered the cards directly to households when possible; however, as is the case in most developing countries, the postal service has a limited ability to reach individual households, particularly in rural areas. As such only 15 percent of the households that received a card report that they received it directly from a postal worker; the remaining households received it from local officials.

Within the identification card treatment group, we further varied the treatments along a number of dimensions to further explore how different types of transparency mechanisms function. First, we randomly varied who received the cards: in 188 randomly chosen villages, cards were only mailed to those in the lowest decile of per capita household consumption (or 32 percent of eligible households). The other eligible households still were on the lists and posters that were provided to the local officials and they were still eligible to receive Raskin rice despite not having a card. This allowed us to test if actually receiving a card *per se* would affect the distribution of rice. Second, we varied whether the cards had attached coupons that were supposed to be clipped at each Raskin distribution. In 189 randomly chosen card villages, households additionally received tear-away coupons for each month that the card was valid (September 2012-December 2013), which were supposed to be remitted to the central government. The coupons could increase the bargaining power of eligible, since they could simply withhold their coupon if they disagreed with the amount of Raskin they were being provided. Similarly, they could increase the bargaining power of village heads vis-à-vis ineligibles demanding rice, since ineligibles did not have the coupons.

Third, in 187 randomly chosen card villages, the government printed the price on the card (see Figure 2). In the remaining villages, the price was not printed. This was done to understand if holding constant the card receipt, whether simply increasing the information that was printed on the card would increase the subsidy received.

Finally, we experimentally varied the degree to which there was public information provided about the cards. We randomly assigned 186 villages to receive the standard information campaign discussed above, wherein the central government provides information to the district government who, in turn, sends the information down to more local government officials who are expected to advertise to the communities. In the remaining 192 villages that received cards, additional public information was provided regarding both the presence of the cards and who should receive them. The goal of this sub-treatment was to not only increase knowledge of one's own eligibility status, but to also increase common knowledge about beneficiary status and cards within the village. Thus, in these villages, a community facilitator went to each village and hung up additional posters—announcing the presence of the cards and publicizing the beneficiary lists—within different neighborhoods of the village and played a pre-recorded announcement about the cards in the local language over the loudspeaker of the village mosque (a common advertising technique in Indonesia).[10] A facilitator spent about 2 days in each village, and so the relative cost of this additional public information was only about US$1.40 per beneficiary household.[11]

*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 and the previous experimental treatments. For all of the other experimental variations (price, public information, and coupon), we stratified on the six districts, previous experimental treatments, and whether all beneficiaries received cards or only the bottom 10th decile did so.

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

[10] Appendix Figure 1 contains an example of the posters used to socialize the presence of cards in the village. There were eight variants of the poster to reflect the various 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.

[11] The facilitators had a coordination meeting with the village leaders to gain permission to hang up the posters. The meetings were attended by relatively few households (an average of 20 people out of 1380 households in a village) and they were short; the facilitators were instructed to stay on script and not provide information about the Raskin program. So, it is highly unlikely that information was widely spread directly as a result of the coordination meeting.

*E. Data Collection*

We conducted two follow-up surveys: one in October to November 2012, at least two months after the receipt of the identification cards, and a second in March to April 2013, to allow about eight Raskin distributions to occur since the card distribution. Both surveys were conducted by SurveyMeter, an independent survey organization. In both surveys, we independently visited randomly selected households and asked about their experience with Raskin, as well as a host of other characteristics. We oversampled households on the list of eligible households to ensure adequate representation of these types of households in the survey. In the second survey, a portion of respondents consisted of those who had been previously surveyed in our previous experiment (Alatas et al 2013b), to take advantage of pre-treatment information on those households. Additional details about the sampling can be found in the Appendix.

*F. Summary Statistics and Experimental Validity*

Table 1 provides sample statistics for both eligible and ineligible households from the control villages to provide a description of the Raskin program in the absence of the cards. On average, 83 percent of households that were eligible to buy Raskin rice in the last two months did so. However, 68 percent of ineligible households bought Raskin 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. 2,263, which is considerably higher than the official cop-pay price of Rp. 1,600. Combined, this implies that eligible households received an average subsidy of Rp. 28,781, or 32 percent of the total subsidy (Rp. 88,680) that they are entitled to receive.[12] Note that 6 percent of eligible and 5 percent of ineligible households report having an identification card for rice

[12] 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.

purchases in the control group, which may stem from the fact that some local governments had previously issued identification cards for Raskin.

Appendix Tables 1 and 2 provide a check on the randomization for the main card treatment and subtreatments, respectively. The ten variables chosen for the randomization check were specified prior to the randomization. Column 5 of Appendix Table 1 shows the difference between the card treatment and the control, controlling for strata effects. Only one out of 10 differences is significant at the 10 percent level or more, which is consistent with what one would expect by chance. A joint test of the treatment status across the 10 variables leads to a p-value of 0.60.

For each of the set of subtreatments within the card treatment, we compute the baseline difference in the subtreatments, conditional on the stratification variables. Out of the 40 differences, only 4 are significant at the 10 percent level or more, which is what one would expect by chance. We compute the joint test across variables for each subtreatment, and find that out of the four sets of subtreatments, only one is significant (coupons versus no coupons with a p-value of 0.05).

## III. Model

### *A. Setup*

We propose a simple model of bargaining that helps us understand the effects of our interventions on the negotiation between the village leader and a potential Raskin beneficiary over the division of the benefits from the program. A key result is that additional information about program rules can increase the amount received by Raskin beneficiaries even if it merely reduces the variance of their beliefs, without changing mean beliefs.

The total value of the Raskin benefits is denoted by $V$. The leader has to decide on $X$, which is how much of these benefits he wants to offer to the villager, and he retains the remainder, $V - X$. The bargaining process is simple: the leader makes a take or leave it offer to the villager. If the villager accepts, he gets $X$ and the leader gets $V - X$. If the villager does not accept, he has the option of complaining to an outside authority. Complaining costs $C$ but has the potential to yield higher benefits for the villager; whenever there is a complaint the leader gets zero.

Villagers do not know exactly how much they will get by complaining. However, they have a distribution for how much they will obtain, $Y$, given by the function $G(Y)$. The leader knows $G(Y)$ but not the $Y$ of the particular villager he is dealing with.

*B. Analysis of Model*

Given these assumptions, the villager will complain as long as $Y - C \geq X$. Therefore the leader maximizes

$$G(X + C)[V - X]$$

by his choice of $X$. This yields the first order condition

$$= V - X.$$

To see what this means, assume that $G(X + C)$ is uniform on $[B - D, B + D]$, with $B < V$. Then

$$G(X + C) = \frac{X + C - B + D}{2D}$$

and

$$g(X + C) = \frac{1}{2D}.$$

Assuming that an interior solution exists, the leader will offer the villager:

$$X = \frac{V + B - C - D}{2}.$$

The probability that the villager will accept the offer is

$$G(X + C) = \frac{V - B + C}{4D} + \frac{1}{4}.$$

An interior solution will exist when $G(X + C) = \frac{V - B + C}{4D} + \frac{1}{4} < 1$ or $V - B + C < 3D$. Under this assumption we have the following simple results:

<u>Result 1</u>: An increase in the mean perceived outside option ($B$ goes up*)* will increase the amount the leader offers the villager and reduce the probability that the villager accepts.

<u>Result 2</u>: A reduction in the cost of complaining ($C$ goes down*)* will increase the amount the leader offers the villager and reduce the probability that the villager accepts.

<u>Result 3</u>: A reduction in the dispersion of views about perceived outside option ($D$ goes down*)* will increase the amount the leader offers the villager and the probability that the villager accepts.

Each of these results should make intuitive sense. The reason an increase in $B$ increases disagreements in equilibrium is because the village head is maximizing the product of the probability of agreements and the amount conditional on agreement; when bargaining power of the village increases, the village head adjusts on both margins. A reduction in $C$ should be like an increase in $B$*,* because both make complaining more attractive—we should therefore see more "complaints" and less agreement; expecting that, the leader should offer a bit more to the villager to try to buy his compliance. The reason why a reduction in $D$ is different is because it reduces asymmetry of information between the leader and the villager and therefore allows the leader to tailor the offer to what the villager would accept. This leads to both an improvement in the offer and a reduction in disagreements.

### *C. Interpretation of Results*

The model makes clear that there are several ways one can interpret the identification cards, and each of them lead to an increase in the share of benefits received by the villagers, either because it persuaded villagers that the government was actively supporting their rights and therefore shifted the distribution of their perceived outside options to the right ($B$ went up) or because it made everyone more aware of what the program rules are—which we interpret as convergence in the perceived outside options ($D$ goes down).

In addition, the act of making the list of beneficiaries public is likely to have reduced the cost of complaining. It is well-known that unelected elites play an important mediating role in Indonesian villages—the list being public makes it easier for the villagers to go to these other leaders with evidence that the elected leader, who is normally the only one with information about the list, was violating what was promised to them. According to Result 2, this should lead to better offers.

We also noted that these alternative interpretations have somewhat different implications for the likelihood of complaints on the equilibrium path. Unfortunately, the data we have does not permit us to reliably test the predictions about complaints. While we do have a measure of protests and this measure actually does increase significantly in response to the card treatment (results available from the authors), we cannot distinguish in our data between protests by those who are eligible and protests by those who are not. If our model were exactly right, there would of course be no protests by the ineligible; but it seems plausible that when the rules change and this restricts the ability of the ineligible to benefit from the program, there will be a round of protests in the treatment villages trying to get the rules changed again. We are unable to rule out the possibility that this is what is going on instead of on-equilibrium path protests by the eligible of the sort predicted by the model. Therefore we will confine ourselves to reporting data about the economic outcomes (i.e. the total amount of the intended subsidy actually received by eligible households).

In taking the model to the data on economic outcomes, it is worth noting that we observe the amount of Raskin that the villager obtains. This is not necessarily the leader's initial offer; instead, it is possible that this is a combination of the leader's original offer if accepted, and the outside option if the villager rejects that offer and complains. In the model, our measure of the villager's benefits would then correspond to the expected gross payoff to the villager

$$P = G(X + C)X + [1 - G(X + C)]\, B^*,$$

where $B^*$ is the actual outside option. Note that $B^*$ could be the mean of the distribution of perceived outside options $B$, but as we already discussed, perceptions could also be systematically wrong.

Simple differentiation tells us that

$$= G(X + C)\frac{\partial X}{\partial I} + (X - B^*)\frac{\partial G(X + C)}{\partial I}$$

where $I = B, C, D$. From this and the above results, it is evident that $\frac{\partial P}{\partial B}$ and $\frac{\partial P}{\partial C}$ will both be positive as long as $B^* - X$ is not too negative. In particular, in the case where by complaining the villager obtains the true amount he is entitled to, so that $B^* = V < X$, then both $\frac{\partial P}{\partial B}$ and $\frac{\partial P}{\partial C}$ will be positive. If however people overestimate their outside options, they might end up with less as a result of the campaign. On the other hand, $\frac{\partial P}{\partial D}$ is guaranteed to be positive even when $B^* - X$ is negative—if complaining actually hurts, then reducing asymmetry of information is good for the villager. Finally in the special case where the perceived outside option is exactly right on average ($B = B^*$) one can show that show that both reducing $C$ and reducing $D$ always increases $P$.

## IV. The impact of identification cards

### *A. Do people get the cards, and do they learn from them?*

We first begin by understanding whether households in the card treatment villages received the cards and whether or not they were more likely to know their beneficiary status. Table 2 provides these results. Unless otherwise noted, we estimate the following regression:

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

where $k$ represents a geographic stratum, $s$ represents a type of household sampled, $t$ represents a survey round, $v$ represents a village, and $i$ represents an individual household respondent. In general, we pool the results of both follow-up surveys in the analysis and include survey sample dummies interacted with the survey round dummy, as well as stratum fixed effects (geographic controls and experimental treatments in the previous study).[13] 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 (i.e., those households eligible for Raskin according to the official central government list), while in columns 4-6 the

[13] Appendix Table 3 provides the results disaggregated by survey round. We do not observe significant differences between the two follow-up surveys. Appendix Table 4 replicates the results with varying levels of controls; the results are near identical across the different specifications. Appendix Table 5 estimates the treatment effect on and off Java. We find the eligible households in Java were more likely to receive the card than eligible households off Java. However, even off Java, where we expect weaker institutions, there is a strong and positive effect on card receipt.

sample is ineligible households (randomly selected households whom we could not locate in the official central government Raskin list).[14]

Eligible households were 28 percentage points more likely to receive the cards than those in the control villages (Column 1). There are a variety of reasons that households may not receive cards: cards getting lost in the mail system, addresses difficult to assess, village leaders blocking cards to ether particular households or the entire village, etc. Nonetheless, it is a statistically significant and meaningful increase in the number of cards that were distributed. In comparison, ineligible households in the treatment group were only 2 percentage points more likely to receive cards (Column 4). Ineligible households may receive cards for a variety of reasons—corruption, reallocations at the village levels of slots from poor to rich, imperfect matching of survey data to government rolls, and so forth—but the overall level is dramatically lower than those who were eligible.

In villages where the cards were mailed out, we observe an increase in card use. Eligible households were 14 percentage points more likely to use a card to purchase Raskin rice; note, however, that even if one did not use the card, the act of *getting* a card may still be important. Qualitatively, some households that we interviewed explained that they were told to keep the card with their important documents, and thus did not necessarily use it for the distribution.

From the perspective of the theory, an important question is whether the card treatment increased people's beliefs about what they were entitled to (i.e. increased V in the model). To

[14] Note that for a randomly selected fraction of card villages, the cards were only mailed out to households in the bottom 10 decile. Thus, for these villages, only households that were eligible to receive a card are included in the eligible sample; those who are eligible for the Raskin program, but not for the identification cards, are dropped from from the main analysis (we explore outcomes for these households in Tables 5A and 5B). We then reweight the villages so that all three sets of villages (control, full card, and bottom 10$^{th}$ decile) so that their total weights in the regressions are consistent with the fraction of eligible households for the Raskin program in each of the 58 geographic strata. Note also that it is possible that a small number of ineligibles whom we could not match to the official Raskin list may have in fact been on the list.

test this, we ask households in the survey whether they believe they are entitled to receive the rice. Eligible households were 9 percentage points, or 30 percent over the control mean, more likely to correctly know their eligibility status (Column 3). Similarly, ineligibles were 4 percentage points, or 11 percent over the control mean, more likely to know they were ineligible in villages where cards were distributed (Column 6). This suggests that the cards increased information, and in particular, increased eligible households' beliefs about what they were entitled to.

*B.* *Impacts on subsidy received*

Table 3 explores the impacts of the cards on how much subsidy households actually received. Specifically we investigate whether one bought Raskin rice in the last two months, the amount purchased, the copay price paid, and the overall subsidy received. The sample structure and regressions are the same as in Table 2.[15] Note that the quantity and subsidy variables are coded as zero if no purchase was made, and thus capture both the intensive and extensive program effects. Price, however, is conditional on purchase, since it is not observed for households that do not purchase the rice.

The results in Table 3 show that the card treatment substantially improves the amount of transfer received by eligible households. Eligible households were no more likely to buy Raskin rice in the two months prior to the survey (Column 1). But, we observe large changes in both the quantity purchased and price paid: eligible households in card villages bought 1.20 kilograms more rice than the control (Column 2) and paid a copay price of Rp. 60 less than the control villages (Column 3). This translates to a Rp. 7,136—or about a 25 percent—increase in the

[15] Appendix Table 6 replicates Table 3, but with varying sets of controls; the results are near identical across the specifications. Appendix Table 7 provides the results for villages that are on and off Java; the gain in subsidy attributable to the card is larger in Java, but not significantly so.

subsidy received (Column 4). Appendix Table 8 provides the results separately by survey round, for comparable samples; the subsidy gain by eligible households is the same in both follow-up surveys (after about two monthly distributions and eight monthly distributions, respectively), which suggests that the effect of the cards is not falling over time. Only the effect on the copay price changes significantly over time: copay price decreases by even more over time.

This increase for poor households appears to be indicative of a reduction in leakages, rather than just a transfer of rice between ineligible and eligible households. Ineligible households were 7 percentage points less likely to purchase Raskin in the last two months (Column 5). However, we do not observe a significant difference in the total amount of rice purchased by ineligible households (Column 6); this is because the quantity conditional on purchase also rose for ineligible households that continued to buy after the treatment (Appendix Table 9). Thus, on net, there was no change in subsidy received by the ineligible households (Column 8).

Since the cards increased the quantity received by eligible, but did not decrease the quantity received by ineligibles, this implies that on net, the cards resulted in a substantial reduction in the amount of rice that went missing from the village. 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 8 percent. This corresponds to an approximately 25 percent reduction in “lost” rice.

### *C. Multitasking*

One concern with transparency programs is that by focusing on particular aspects of the program that can be made transparent, officials may respond to those aspects of the program that are transparent and neglect other aspects of the government’s objective function that are not focused on. In the case we study here, the objective of the program is to distribute rice to the poor. The government's official eligibility list are an imperfect measure of poverty – since they are based on assets, which are a noisy predictor of consumption levels, the eligibility list predicts poverty, but not perfectly. Part of the redistribution from eligible to ineligibles could be to correct these types of errors in the government list and ensure that the poor, ineligible households receive transfers.

To examine this, in Table 4, we test whether the card treatment shifted resources away from poor households, as measured by their baseline per capita consumption (i.e. measured before the Raskin experiment began). To estimate this, we interact the treatment with baseline log per capita, consumption, i.e. we estimate:

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

Where $LOGCONSUMPTION_i$ is the household's per-capita consumption, measured at baseline. We estimate this regression separately for eligible and ineligible households.

The first 4 columns of Table 4 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 significant at conventional levels (Column 4). Similarly, the remaining columns show no evidence that poorer, ineligible households are losing the subsidy as a result of the cards (Column 8). This is related to the point discussed earlier that the cards did not, in general, reduce the amount of subsidy received by the non-poor.

## V. MECHANISMS

Providing information through the identification cards potentially affects the bargaining between households and village officials in multiple ways. The cards can make a difference only to those who receive them, or could provide general improvements for everyone in the village. The information printed on the cards itself can matter by informing people about program rules, and the way the cards are publicized can affect both individual and common knowledge about program rules and eligibility. As discussed above, how the cards impacted rice receipts along these three dimensions also helps distinguish between the information story and the accountability story. In this section we explore each of these issues in turn.

### *A. Does receiving a card matter?*

A first question is whether receiving a physical card *per se* matters for the amount of rice a household receives. Receiving a card could matter in many ways: by providing information to those who receive it, or as a proof of eligibility that could be shown to a village head or other official to protest maltreatment. We explore whether actually being one of the households who receives a card *per se* (as opposed to being in a village where cards were distributed) matters in two ways: by varying who received the cards, and by adding coupons to the cards.

First, in half of the villages, we experimentally varied whether cards were mailed out to everyone or just to the very poor. Specifically, in half (randomly-selected) of the card villages, the government mailed out cards to all eligible households; in the remaining half, cards were only provided to households in the bottom decile (about 32 percent of eligible households). In all villages, the government mailed the complete list of eligible households to the village leaders with instructions that all eligible households should be allowed to purchase their full Raskin allotment. Thus, we can see whether the improvements in the program were general, or whether there were differential impacts based on receiving a card. If, in the language of the model, the card increased $Y$ or decreased $C$, then if village heads know this they could tailor offers specifically to those who received the cards; if they do not know this, one might expect the impacts to be broader.

To examine this, 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 when cards are mailed only to the bottom 10). We regress each outcome on indicator variables for "cards to the bottom decile" and "cards to all," and thus the coefficients reflect differences from villages where no cards were distributed. For ease of comparison, we also provide the difference in outcomes for the "cards to the bottom decile" and the "cards to all" villages, as well as the standard error of these differences. Table 5A and Table 5B provide these findings for each of the three categories of households. Note that all regressions include the variables we stratified on, are estimated by OLS, and are clustered at the village level.[16]

Providing cards to just the bottom decile did not change the allocation to these households relative to villages in which all households received cards. Households in the bottom decile were just as likely to receive cards and use cards across both types of villages (Columns 1 and 2 of Table 5A). They were also equally likely to know their beneficiary status. 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 5B). However, the outcomes for the other eligible households greatly differed based on whether or not they resided in "cards

[16] Appendix Table 10 and 11 provide the results also conditional on the other sub-treatments. The difference in outcomes between the bottom 10 and cards to all is similar regardless of whether one controls for the other sub-treatments.

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" villages were just as likely to receive a card as those in the bottom decile (Columns 1 vs 4 of Table 5A) and received an increase in subsidy that was just as large (Columns 2 vs 8 of Table 5B). Other eligible households that resided in villages where only the bottom decile received a card, by comparison, did not experience any gains in quantity purchase, price or subsidy (Column 6-8 of Table 5B). Thus, taking these results together, these results imply that an increased focus by the government on the bottom decile did not spur officials to relatively improve the program for these households, but rather the act of receiving a card—regardless of program focus—mattered. This suggests that the results are driven by bilateral bargaining between villagers and officials rather than by an overall change in program implementation. Only those who received the cards had additional information, and received better outcomes; eligible households who did not receive the cards had no change in their knowledge, and no change in their outcomes.

Second, we also varied whether the cards included a tear-off coupon that was supposed to be remitted to the government. In the language of the model, including the coupons should be a reduction in $C$, the cost of disagreement. Without coupons, an eligible villager would actively need to seek out a government official to complain or protest; with the coupons, all a villager needs to do is to withhold his or her coupon, and the government would find out automatically since it would reduce the number of coupons that it received.

To investigate this, in half the villages, the central government also mailed a set of coupons along with the cards that local officials were supposed to collect at the time of purchase and submit to the central government. In practice, the government did not do anything based on coupons it collected, consistent with the reality of many top-down monitoring programs in developing countries, but villagers did not know this *ex ante*.

Tables 6A and 6B explore the effect of the coupons.[17] Eligible households were just as likely to receive and use the cards across the two types of villages (Columns 1 and 2 of Table 6A), but were 6 percentage points more likely to submit a coupon at the time of purchase in coupon villages (Column 3 of Table 6A). This implies that the coupons were collected in roughly half of the cases that the card was used.

Interestingly, although the point estimates are positive, the coupons did not have a statistically significant or quantitatively large impact on the subsidy that was received by eligible households (Column 4 of Table 6B). Instead, they reduced the probability of purchase and amount subsidy received by ineligible households (Columns 5 and 8 of Table 6B). It appears that an (unintended) consequence of the coupons may have been to increase $C$ for ineligible households: if the complaints system switches to coupons, then ineligible households don't have them, and therefore are not able to complain. The coupons thus seem to have strengthened the hand of the village heads vis-à-vis the ineligibles, even though it did not substantially improve the situation for eligible households.

*B. Isolating the role of information*

To isolate the role of the information contained on the card (as opposed to having a card), we randomly varied what was printed on the front of the card. In all villages where cards were distributed, the cards contained information on the quantity of rice households were eligible to receive, the fact that rice was supposed to be delivered each month, the fact that the copayment was supposed to be in cash, the fact that the card had to be carefully saved, and the fact that the

[17]Appendix Table 12 and 13 present these results also conditional on the other sub-treatments. The results are near identical to Table 6A and 6B.

recipients were required to show the card to receive the rice. In half of card villages (randomly selected), the cards included all of that information, plus an additional line stating the copayment price (Rp. 1,600 per kg). In both cases, the official program rules that were distributed to village leaders contained the official copay price of Rp. 1,600 per kg, so this is purely an intervention affecting the information received by villagers. By comparing the villages that received the cards with the price information to the villages that received the cards without the price information, we can precisely identify the role of additional information *per se*, controlling for the physical aspects of receiving the cards.

The results are provided in Table 7A and Table 7B. Again, all regressions are estimated by OLS, include controls for the variables we stratified on, and are clustered by village.[18] The probability of receiving the cards is the same in both types of villages, but eligible households are 5 percentage points more likely to use the cards to purchase Raskin rice in villages where the price is printed on the card (Table 7A); this represents a 63 percent increase in card use relative to households that received a card without the printed price.

As shown in Table 7B, eligible households in villages where the copay was printed received more than double the subsidy than in villages where it was not printed. Interestingly, the difference is primarily through the quantity purchased margin, rather than the price margin.[19] Specifically, eligible households receive Rp. 3,583 per month more in subsidy in villages where

[18] Appendix Table 14 and 15 provide the results from 7A and 7B, additionally conditional on the sub-treatments. Again, we observe no difference treatment effects if we include these additional controls. We also tested the effect of the cards in the standard information versus public information treatments, since the public information may had an effect on people's perception of price (Appendix Table 16). We find that the effect of printing the price on cards is similar in both the standard and public information treatments.

[19] One potential reason for the quantity increase is that households think the price is lower, so this represents 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 the maximum they could. Moreover, the quantity effects are sufficiently large that demand for rice would need to be very elastic to explain these effects, which seems unlikely for an important staple.

price was printed on cards compared to eligible households in villages that received cards without price information; of this Rp. 3,585 increase in subsidy, about 95% of the change was due to increase in quantity received (which increased from 0.49 without price printed to 1.12 kg with price printed) while only about 5% of the change was due to a reduction in the copay price (which fell by Rp. 34 without price printed and Rp. 55 with price printed).[20]

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 in the form of lower copay prices or higher quantities. Information about copay price should increase the subsidy received, but whether it comes through the price or quantity margin is arbitrary and depends on which approach is more cost-effective for village heads. In fact, the difference in the amount of subsidy received—Rp. 3,583 per household—is almost exactly equal to the difference in value from taking existing quantities of rice and pricing them at the correct copay price instead of the marked up price (i.e. the 5.3 kg received in controls multiplied by the difference between the actual Rp. 2,263 copay price and the correct Rp. 1,600 copay price is Rp. 3,513). One potential reason that increasing quantities may be more cost-effective for village heads is that it allows them to discriminate between eligible households and ineligible households; the evidence suggests that price reductions may have spilled over to ineligible households since there may be pressure to charge a uniform price (Appendix Table 17). The fact that it affects the quantity dimension, rather than the price dimension, is consistent with the bargaining story rather than an accountability story: if one was concerned that by printing the price the government was signaling a higher degree of auditing over the price, one

[20] Note that since price is only available conditional on buying Raskin, which may change in response to the treatment, we also report regressions on the minimum and maximum price reported by any of our respondents in the village. Appendix Table 17 suggests that, relative to pure controls, the cards with printed price reduce the maximum printed price in the village by about Rp. 90, or about 9 percent of the control group levels of price markups above the official Rp. 1,600 copay price.

would expect strong results of the price treatment only on prices. By contrast, in a bargaining model, the villager cares only about the total subsidy received, regardless of whether it comes in the form of higher quantity or lower price.

On net, the results in this section suggest two important conclusions: giving information *per se* (holding the physical aspects of the cards constant) substantially increases the amount received by eligible households, and the fact that it affects the quantity dimension, not the price dimension, suggests that it is through a bargaining channel rather than through village officials becoming more worried about compliance with official prices.

### *C. Public Information*

Challenges to authority may feature strategic complementarities: a village head may be able to retaliate against a lone individual who challenges his authority, but it is much harder to retaliate if many people challenge him. A villager deciding whether to challenge a village head may therefore be more likely to do so if he can coordinate with other villagers. But doing this requires knowing not just knowledge about what you are entitled to, but also being confident that everyone else knows more or less what they are entitled to as well (Chwe 1991, Olken 2007).

To investigate these issues, we varied whether the degree to which information was about the cards and beneficiaries was provided publicly in a way that could generate common knowledge. Specifically, in half of the card villages (randomly-selected), the government conducted their "standard" program information treatment: village leaders received the list of the beneficiaries and were requested to post a copy of this in a visible place in the village. In addition, they were told to generally to advertise the list and arrival of cards. In the remaining villages ("public information"), a community facilitator visited and dropped off a number of additional copies of the poster and the list, and coordinated with the village leaders to ensure that the poster and listing were hung up in each hamlet in the village; the facilitators also played a pre-recorded message about the cards on the mosque radio.

In Table 8, we test whether receiving the cards under the different forms of information both improved access to information, and changed people's belief's about other's access to information (i.e. higher order beliefs).[21] To test access to information, in Panel A, for each of the four key demographic groups we survey (eligible, non-eligible, village officials, and informal leaders), we regress a dummy variable that indicates whether the respondent has seen the list of beneficiaries on dummies variables for the standard information and enhanced public information. To test higher order beliefs, i.e. beliefs about whether *others* have seen the information, in Panel B, for everyone we surveyed, we ask how likely it is that members of each of the four demographic groups have observed the list, where 0 corresponds to "have not seen the list" and 3 corresponds to "most have seen the list." All regressions include the stratification variables, are estimated by OLS, and are clustered at the village level. For both panels, the answer "do not know" is coded as zero; Appendix Table 18 shows that the findings are the same even if do not know is coded as missing.

The public information treatment significantly increased the probability of seeing the list (Panel A). In the non-card areas, the fraction of households who saw the full beneficiary list was low (7 percent of beneficiaries and 6 percent of non-beneficiaries). Even among those in formal leadership positions within the village (e.g. village head, village secretary, hamlet heads), only about a third had reported that they had seen the full list, while only about 13 percent of informal

[21] 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 X knows), whereas common knowledge encompasses all higher-order beliefs (i.e. do you believe that X knows that you know, and so on), but is the highest-order belief we were practically able to elicit from Indonesian villagers. 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.

community leaders had seen it. The “standard” government publicity did not significantly increase the percentage of individuals who report having seen the list across any of the demographic categories. In contrast, the public information treatment greatly increased everyone’s access. The number of beneficiaries who had seen the list nearly trebled relative to no cards (from 7 percent to 21 percent in Column 1) and was 11 percentage points higher than in the standard publicity approach. The percentage of non-beneficiaries who had had seen the list was 10 percentage points higher in the public information treatment relative to the standard approach (Column 2), and the percentage of village leaders was 17 percentage points higher (Column 3).

In fact, not only were individuals more likely to see the list themselves, they were more likely to believe that *others* had seen the list (Panel B). In the card villages with the standard information, households were more likely to report that beneficiaries had seen the list relative to areas with no cards (Column 1), but they were no more likely to report that the non-beneficiaries or leaders (Column 2-4) had seen it. In contrast, in the public information villages, households were more likely to report than everyone had seen the list both relative to the control of no cards and to the standard card villages.

We also examine actual knowledge by asking households whether they themselves were entitled to the program, and by asking the whether a given set of other households were officially entitled to the program. Interestingly, the public information resulted in an increase in knowledge of their one’s own beneficiary status, but not that of others (Table 9). With no cards, 30 percent of beneficiaries can correctly identify their status; beneficiaries in villages with cards and standard information are 5 percentage points more likely to correctly identify their status relative to no cards (Column 1 of Panel A). However, with the public information, they are 7 percentage points more likely to do so relative to the standard information—this is a 40 percent increase in knowledge relative to no cards and about a 20 percent increase relative to cards with standard information. With just the cards, non-beneficiaries were no more likely to know their correct status than under no cards, but they were 7 percentage points (or 20 percent) more likely to know their status under public information (Column 2). However, as shown in Panel B, on average, individuals were equally likely to correctly identify anyone else's status under both standard or enhanced. One might have suspected that this would vary by whether the person is a beneficiary (under the assumption that beneficiaries have more to gain from monitoring and therefore may invest more in figuring out who else is on the list), but as Appendix Table 19 shows, there is no observed difference between beneficiaries and non-beneficiaries.

To sum up then, under the public information treatment, individuals were more likely to have seen the eligibility list and to be informed of their own beneficiary status. While they were no more likely to be able to identify the beneficiary status of others, they did believe that others in the village were much more likely to have seen the eligibility list and thus posses this knowledge. The natural question that follows is how this affected the Raskin subsidy: these results are presented in Tables 10A and 10B.[22]

Eligible households were both more likely to receive their card and use it under public information, but we find no difference in either of these variables for ineligible households (Table 10A). The magnitude of these differences for the eligible households is large: they were 24 percent more likely to have received a card and 45 percent more likely to use it than under the standard socialization. The fact that they were more likely to receive the card with the public information treatment suggests that one potential reason for the low overall card distribution rate was because the village heads blocked the distribution of cards; since the post-office doesn't

[22] We also report the results of Table 10A and 10B when we additionally control for the other sub-treatments in Appendix Tables 20 and 21; the results are near identical. In Appendix Tables 22 and 23, we report the results, by survey round, for comparable samples. The results are similar across the first and second follow-up surveys.

know addresses in many rural areas, they cannot deliver cards without the assistance of local officials, and the results suggest that when there was inadequate public information about the cards they may have used this power to prevent the distribution of the cards.

Table 10B shows that the public information translated into large gains in the subsidy received by eligible households. In fact, the subsidy level received under enhanced was nearly double than that received under standard (Column 4), with this difference being driven by both an increase in the quantity bought (Column 2) and a decrease in the price (Column 3). Again, we are not observing a difference in the quantity purchased by ineligible households, which implies that the gain is less about program resources being diverted from ineligible to eligible, but rather a decrease in the theft of rice before it reaches citizens.

Given these findings, a natural question that arises is whether the public information treatment worked by simply increasing the number of cards that made their way to beneficiaries or had broader effects beyond the receipt of 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 enhanced in Table 11; we also provide the p-value of the difference in estimates.[23] If the additional knowledge in enhanced reduced leakages simply through increased card receipt, we would expect that the treatment effect of receiving a card would be the similar in both sets of villages. However, the effect is much larger in enhanced: the IV estimate of receiving the card on the subsidy is 31,052 in enhanced, while it is 17,310 in standard. We can reject equality of these estimates with a p-value of 0.06. This implies that the public information had effects on the subsidy that households received that were independent of just an increase in the probability of card receipt. The fact that providing public

[23] The corresponding first stage and reduced form regressions are presented in Appendix Table 24.

information to villagers substantially improves the impact of the cards above and beyond the impact on delivering cards again suggests that it is citizens' information, not village officials' fear of a crackdown from above, that is driving the results.

## V. Conclusions

Despite widely-held beliefs in the importance of transparency for improving governance, there has been surprisingly little rigorous evidence on the role that additional information plays in improving the delivery of services to citizens. In this paper, we tested the role of information by providing identification cards to eligible beneficiaries of a subsidized food program in Indonesia, and by varying the content of information printed on the cards, to whom they were distributed, and whether there was public information about the presence of the cards and the beneficiary list in addition to the private information people obtained by receiving the cards. We show that the cards matter: on average, eligible beneficiaries in villages randomly chosen to receive the cards received about 25 percent more subsidy than eligible beneficiaries in villages 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 almost doubled 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 also further increased the effectiveness of the cards, again suggesting an important role for information.

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 find out what the rules are, particularly for general rules like how many kilos you are entitled to and at what price you should pay for it.

Given that, as we show, providing this information has significant material benefits, the next question is why. There are a number of possible answers: perhaps people simply don't 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 change all the time, 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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## Appendix: Sampling

In the first follow up survey, we identified the set of hamlets in each village that had at least one targeted beneficiary and had not been randomly selected to be surveyed in the previous experiment; we then randomly selected one hamlet from this set. Within each hamlet, we selected 8 households to be surveyed for a total of 4,571 households. Of these 8 households, we oversampled beneficiaries as they were the focus of the study, and thus we randomly selected 5 households from the beneficiary listing and 3 from a hamlet census.[24] The survey contained detailed questions on card receipt and use, rice purchases through the Raskin program, knowledge of both one's own and other households' eligibility status, and program satisfaction. Additionally, we interviewed the village head in order to assess his views on the program functioning.

For the second follow-up survey (March and April 2013), we returned to the hamlet that we had surveyed in the previous experiment, so that we could survey a fraction of the households for whom we had baseline data.[25] We surveyed 10 to 11 households per village, for a total of 5,706 households using a similar questionnaire to the first follow-up. We oversampled the beneficiaries (between 6 to 7 per village) and then sampled the remaining households from the random sample of households we surveyed in the previous experiment.[26] Again, we surveyed the village head.

[24] The 5 randomly chosen beneficiaries were stratified by whether they were classified as very poor (the bottom 10th decile) or other eligible households. A fraction of those who were randomly chosen from the census were eligible and thus are classified as such in the analysis.

[25] There is substantial heterogeneity across hamlets within a village, and thus the strategy of sampling in different hamlets allowed us to better capture this variation. Moreover, since the households surveyed in the second follow-up were in a different hamlet than those in the first, we are less concerned that any "monitoring effects" that could arise as a result of the first survey on the card use would influence how the card program functioned in the areas surveyed for the second follow-up (Zwane, Alix, et al. 2011).

[26] For the beneficiary sample, we sampled all eligible households that we had previously surveyed and then supplemented this with a random sample of eligible households from the government listing. In a few cases, we did not have enough ineligible households to choose from in our previous survey; in these cases, we randomly selected additional households from the hamlet census.

Table 1: Summary Statistics for the Control Group

| | Eligible Households | | | Ineligible Households | | |
|---|---|---|---|---|---|---|
| | Observations | Mean | Std. Dev | Observations | Mean | Std. Dev |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | *Panel A: Card Receipt and Use* | | | | | |
| Received Card | 2,275 | 0.06 | 0.25 | 1,207 | 0.05 | 0.21 |
| Used Card | 2,275 | 0.06 | 0.24 | 1,207 | 0.05 | 0.23 |
| Knows Own Status | 2,275 | 0.30 | 0.46 | 1,207 | 0.35 | 0.48 |
| | *Panel B: Rice Purchases and Price* | | | | | |
| Bought in the Last 2 Months | 2,275 | 0.83 | 0.37 | 1,207 | 0.68 | 0.47 |
| Amount Purchased (Kg) | 2,274 | 5.30 | 4.32 | 1,207 | 3.42 | 3.67 |
| Price (Rp.) | 1,923 | 2,263 | 453 | 813 | 2,272 | 464 |
| Subsidy (Rp.) | 2,274 | 28,781 | 23,939 | 1,207 | 18,428 | 19,987 |

Note: This table provides summary statistics for key outcome variables for the control group, by official eligibility status. Data are pooled from the first and second follow-up survey.

Table 2: Reduced Form Effect of Card Treatment on Card Receipt and Use

| | Eligible Households | | | Ineligible Households | | |
|---|---|---|---|---|---|---|
| | Received Card | Used Card | Knows Own Status | Received Card | Used Card | Knows Own Status |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Card Treatment | 0.28*** | 0.14*** | 0.09*** | 0.02** | 0.03** | 0.04* |
| | (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 | 0.06 | 0.06 | 0.30 | 0.05 | 0.05 | 0.35 |

Note: This table provides the reduced form effect of belonging to the card treatment group on card receipt, use, and knowledge by eligibility status. 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 reweight the treatment groups by sub-district so that the ratio of all three income groups is the same. Each column in this table comes from a separate OLS regression of respective outcome on the treatment, sub-district fixed effects, survey sample dummies and dummy variables for the previous experimental design. Standard errors are clustered by village. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: Reduced Form Effect of Card Treatment on Rice Purchases and Price

| | 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 | 0.01 | 1.20*** | -60*** | 7,136*** | -0.07*** | 0.07 | -37 | 555 |
| | (0.01) | (0.24) | (18) | (1,373) | (0.02) | (0.19) | (23) | (1,044) |
| Observations | 5,693 | 5,690 | 4,880 | 5,690 | 3,619 | 3,619 | 2,283 | 3,619 |
| Control Group Mean | 0.83 | 5.30 | 2,263 | 28,781 | 0.68 | 3.42 | 2,272 | 18,428 |

Note: This table provides the reduced form effect of belonging to the card treatment group on rice purchases by eligibility status. 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 reweight 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. 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. Each column in this table comes from a separate OLS regression of respective outcome on the treatment, sub-district fixed effects, survey sample dummies and dummy variables for the previous experimental design. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Reduced Form Effect of Card Treatment on Rice Purchases and Price, 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 | 0.01 | 1.20*** | -60*** | 7,136*** | -0.07*** | 0.07 | -37 | 555 |
| | (0.01) | (0.24) | (18) | (1,373) | (0.02) | (0.19) | (23) | (1,044) |
| Log Consumption | -0.00 | 0.11 | -11 | 574 | -0.14*** | -0.66*** | -19 | -3,496*** |
| | (0.02) | (0.21) | (19) | (1,115) | (0.02) | (0.11) | (20) | (597) |
| Treatment x Log Consumption | -0.00 | -0.23 | 22 | -1,446 | 0.05** | 0.06 | 35 | 288 |
| | (0.02) | (0.30) | (24) | (1,591) | (0.02) | (0.14) | (25) | (740) |
| Observations | 1,266 | 1,266 | 1,148 | 1,266 | 1,925 | 1,925 | 1,235 | 1,925 |
| Control Group Mean | 0.87 | 5.05 | 2309 | 26417 | 0.67 | 2.99 | 2304 | 15674 |

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. 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 reweight 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. 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. Each column comes from a separate OLS regression and includes sub-district fixed effects, survey sample dummies and dummy variables for the previous experimental design. Standard errors are clustered by village.

*** p<0.01, ** p<0.05, * p<0.1

Table 5A: Effect of Distributing Cards Only to the Bottom 10 Percent on Card Receipt and Use

| | Bottom 10 Households | | | Other Eligible Households | | | Ineligible Households | | |
|---|---|---|---|---|---|---|---|---|---|
| | Received Card | Used Card | Knows Own Status | Received Card | Used Card | Knows Own Status | Received Card | Used Card | Knows Own Status |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Card to Bottom 10 | 0.24*** | 0.08*** | 0.04 | 0.03 | 0.01 | 0.00 | 0.02 | 0.02 | -0.00 |
| | (0.03) | (0.03) | (0.03) | (0.02) | (0.02) | (0.03) | (0.02) | (0.02) | (0.03) |
| Cards to All | 0.25*** | 0.12*** | 0.05* | 0.27*** | 0.14*** | 0.09*** | 0.04** | 0.05** | 0.01 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.02) | (0.02) | (0.03) |
| *Difference:* Bottom 10 – All | -0.01 | -0.04 | -0.01 | -0.24*** | -0.13*** | -0.08*** | -0.02 | -0.03 | -0.01 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.02) | (0.02) | (0.02) |
| Observations | 3,683 | 3,683 | 3,683 | 2,968 | 2,968 | 2,966 | 3,619 | 3,619 | 3,619 |
| Control Group Mean | 0.07 | 0.07 | 0.32 | 0.06 | 0.06 | 0.28 | 0.05 | 0.05 | 0.35 |

Note: This table provides the reduced form effect of belonging to the bottom ten and all cards treatment groups on card receipt and use, by eligibility status, as compared to the control group. Data are pooled from the first and second follow-up survey. Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, sub-district fixed effects, survey sample dummies, dummy variables for the previous experimental design, and a dummy for whether the village was also in the public information treatment. We also provide the difference in the two card treatments. Standard errors are clustered by village.*** p<0.01, ** p<0.05, * p<0.1

Table 5B: Effect of Distributing Cards Only to the Bottom 10 Percent on Rice Purchases and Price

| | 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 | 0.77$^{**}$ | -45$^{*}$ | 4,662$^{**}$ | 0.01 | 0.26 | -2 | 1,624 | -0.03 | 0.11 | -23 | 691 |
| | (0.02) | (0.34) | (23) | (1,911) | (0.02) | (0.32) | (30) | (1,783) | (0.03) | (0.24) | (26) | (1,338) |
| Cards to All | 0.00 | 0.71$^{*}$ | -50$^{*}$ | 4,484$^{**}$ | -0.01 | 0.79$^{**}$ | -43 | 4,779$^{**}$ | -0.07$^{**}$ | 0.09 | -26 | 690 |
| | (0.02) | (0.40) | (26) | (2,238) | (0.02) | (0.33) | (29) | (1,869) | (0.03) | (0.26) | (30) | (1,409) |
| *Difference:* Bottom 10 – All | 0.03 | 0.06 | 5 | 178 | 0.02 | -0.53 | 41 | -3155$^{*}$ | 0.04 | 0.02 | 3 | 1 |
| | (0.02) | (0.37) | (22) | (2091) | (0.02) | (0.32) | (28) | (1833) | (0.03) | (0.23) | (25) | (1257) |
| Observations | 3,683 | 3,682 | 3,188 | 3,682 | 2,968 | 2,966 | 2,507 | 2,966 | 3,619 | 3,619 | 2,283 | 3,619 |
| Control Group Mean | 0.84 | 5.43 | 2,271 | 29,457 | 0.82 | 5.15 | 2,252 | 27,941 | 0.68 | 3.42 | 2,272 | 18,428 |

Note: This table provides the reduced form effect of belonging to the bottom ten and all cards treatment groups rice purchases, by eligibility status, as compared to the control group. 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. 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. Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, sub-district fixed effects, survey sample dummies, dummy variables for the previous experimental design, and a dummy for whether the village was also in the public information treatment. We also provide the difference in the two card treatments. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 6A: Effect of Distributing Cards with Coupons on Card Receipt and Use

| | Eligible Households | | | Ineligible Households | | |
|---|---|---|---|---|---|---|
| | Received Card | Used Card | Used Coupon | Received Card | Used Card | Used Coupon |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Cards with Coupons | 0.24*** | 0.11*** | 0.06*** | 0.03** | 0.03 | 0.01 |
| | (0.03) | (0.03) | (0.01) | (0.02) | (0.02) | (0.01) |
| Cards without Coupons | 0.25*** | 0.11*** | -0.01 | 0.03* | 0.04** | -0.00 |
| | (0.03) | (0.03) | (0.01) | (0.02) | (0.02) | (0.01) |
| *Difference:* | | | | | | |
| Coupons – No Coupons | -0.01 | 0.00 | 0.06*** | 0.00 | -0.01 | 0.01* |
| | (0.03) | (0.03) | (0.02) | (0.02) | (0.02) | (0.01) |
| Observations | 5,693 | 5,693 | 5,693 | 3,619 | 3,619 | 3,619 |
| Control Group Mean | 0.06 | 0.06 | 0.01 | 0.05 | 0.05 | 0.01 |

Note: This table provides the reduced form effect of belonging to the Coupons and No Coupons treatment groups on card receipt and use, by eligibility status, as compared to the control group. 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 reweight the treatment groups by sub-district so that the ratio of all three income groups is the same. Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, sub-district fixed effects, survey sample dummies, dummy variables for the previous experimental design, and a dummy for whether the village was also in the public information treatment. We also provide the difference in the two card treatments. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 6B: Effect of Distributing Cards with Coupons on Rice Purchases and Price

| | 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 Coupons | 0.03 | 0.91*** | -30 | 5,515*** | -0.08*** | -0.19 | -2 | -961 |
| | (0.02) | (0.34) | (23) | (1,943) | (0.03) | (0.23) | (28) | (1,261) |
| Cards without Coupons | 0.00 | 0.70** | -62** | 4,220** | -0.02 | 0.40 | -46 | 2,439* |
| | (0.02) | (0.35) | (27) | (1,964) | (0.03) | (0.27) | (29) | (1,480) |
| *Difference:* | | | | | | | | |
| Coupons – No Coupons | 0.02 | 0.21 | 31 | 1,294 | -0.06** | -0.59** | 44* | -3,400*** |
| | (0.02) | (0.33) | (25) | (1,879) | (0.03) | (0.23) | (26) | (1,263) |
| Observations | 5,693 | 5,690 | 4,880 | 5,690 | 3,619 | 3,619 | 2,283 | 3,619 |
| Control Group Mean | 0.83 | 5.30 | 2,263 | 28,781 | 0.68 | 3.42 | 2,272 | 18,428 |

Note: This table provides the reduced form effect of belonging to the Coupon and No Coupon treatment groups on rice purchases by eligibility status. 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 reweight 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. 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. Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, sub-district fixed effects, survey sample dummies, dummy variables for the previous experimental design, and a dummy for whether the village was also in the public information treatment. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 7A: Effect of Printing Price on Cards on Card Receipt and Use

| | Eligible Households | | Ineligible Households | |
|---|---|---|---|---|
| | Received Card | Used Card | Received Card | Used Card |
| | (1) | (2) | (3) | (4) |
| Cards with Printed Price | 0.25*** | 0.13*** | 0.04** | 0.06*** |
| | (0.03) | (0.03) | (0.02) | (0.02) |
| Cards without Price | 0.25*** | 0.08*** | 0.03** | 0.02 |
| | (0.03) | (0.03) | (0.02) | (0.02) |
| *Difference:* | | | | |
| Price - No Price | -0.00 | 0.05* | 0.01 | 0.03* |
| | (0.03) | (0.03) | (0.02) | (0.02) |
| Observations | 5,688 | 5,688 | 3,615 | 3,615 |
| Control Group Mean | 0.06 | 0.06 | 0.05 | 0.05 |

Note: This table provides the reduced form effect of belonging to the price and no price treatment groups on card receipt and use, by eligibility status, as compared to the control group. 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 reweight the treatment groups by sub-district so that the ratio of all three income groups is the same. Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, sub-district fixed effects, survey sample dummies, dummy variables for the previous experimental design, and a dummy for whether the village was also in the public information treatment. Standard errors are clustered by village. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7B: Effect of Printing Price on Cards on Rice Purchases and Price

| | 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 | 0.01 | 1.12*** | -55** | 6,659*** | -0.06** | 0.10 | -36 | 819 |
| | (0.02) | (0.36) | (25) | (2,061) | (0.03) | (0.26) | (29) | (1,411) |
| Cards without Price | 0.02 | 0.49 | -34 | 3,076* | -0.05 | 0.08 | -6 | 462 |
| | (0.02) | (0.32) | (24) | (1,808) | (0.03) | (0.25) | (27) | (1,349) |
| *Difference:* | | | | | | | | |
| Price - No Price | -0.01 | 0.63* | -21 | 3,583* | -0.01 | 0.02 | -29 | 357 |
| | (0.02) | (0.36) | (25) | (2,051) | (0.03) | (0.24) | (25) | (1,277) |
| Observations | 5,688 | 5,685 | 4,876 | 5,685 | 3,615 | 3,615 | 2,281 | 3,615 |
| Control Group Mean | 0.83 | 5.30 | 2,263 | 28,781 | 0.68 | 3.42 | 2,272 | 18,428 |

Note: This table provides the reduced form effect of belonging to the price and no price treatment groups on rice purchases by eligibility status. 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 reweight 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. 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. Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, sub-district fixed effects, survey sample dummies, dummy variables for the previous experimental design, and a dummy for whether the village was also in the public information treatment. We also provide the difference in the two card treatments. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Effect of Public Information on Seeing the Eligibility List

| | Eligible (1) | Ineligible (2) | Village officials (3) | Informal Leaders (4) |
|---|---|---|---|---|
| *Panel A: Respondent has seen the list* | | | | |
| Public Information | 0.14*** | 0.10*** | 0.20*** | 0.13** |
| | (0.02) | (0.02) | (0.06) | (0.05) |
| Standard Information | 0.02 | 0.01 | 0.03 | 0.02 |
| | (0.01) | (0.01) | (0.06) | (0.05) |
| *Difference:* Public - Standard | 0.11*** | 0.10*** | 0.17*** | 0.12** |
| | (0.02) | (0.02) | (0.06) | (0.05) |
| Observations | 5,685 | 3,619 | 497 | 385 |
| Control Group Mean | 0.07 | 0.06 | 0.33 | 0.13 |
| *Panel B: Respondent believes that stated category of individuals has seen the list* | | | | |
| Public Information | 0.35*** | 0.26*** | 0.24*** | 0.24*** |
| | (0.04) | (0.03) | (0.05) | (0.05) |
| Standard Information | 0.07* | 0.01 | 0.03 | 0.06 |
| | (0.04) | (0.03) | (0.05) | (0.04) |
| *Difference:* Public - Standard | 0.28*** | 0.25*** | 0.22*** | 0.18*** |
| | (0.05) | (0.04) | (0.06) | (0.05) |
| Observations | 9,304 | 9,304 | 9,304 | 9,304 |
| Control Group Mean | 0.31 | 0.15 | 1.03 | 0.47 |

Note: This table provides the reduced form effect of the public information treatments on seeing the eligibility list. 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). Panel B includes all survey respondents. The outcome is whether the respondent believes that individuals of the stated category have seen the list; the variable is scaled between 0 and 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. Data are pooled from the first and second follow-up survey. Each regression is estimated by OLS and includes sub-district fixed effects, survey sample dummies and dummy variables for the previous experimental design. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Effect of Public Information on Beneficiary Status Knowledge

| | Eligible (1) | Ineligible (2) | Village officials (3) | Informal Leaders (4) |
|---|---|---|---|---|
| *Panel A: Respondent correctly identifies own status* | | | | |
| Public Information | 0.12*** | 0.07*** | 0.25*** | -0.00 |
| | (0.02) | (0.02) | (0.06) | (0.07) |
| Standard Information | 0.05** | 0.01 | 0.14** | 0.04 |
| | (0.02) | (0.02) | (0.06) | (0.08) |
| *Difference:* | | | | |
| Public - Standard | 0.07*** | 0.07*** | 0.12* | -0.04 |
| | (0.03) | (0.03) | (0.06) | (0.07) |
| Observations | 5,683 | 3,619 | 497 | 385 |
| Control Group Mean | 0.30 | 0.35 | 0.42 | 0.50 |
| *Panel B: Respondent correctly identifies status of other households* | | | | |
| 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 | 0.02 |
| | (0.01) | (0.01) | (0.04) | (0.04) |
| *Difference:* | | | | |
| Public - Standard | -0.01 | -0.02 | -0.02 | -0.04 |
| | (0.01) | (0.02) | (0.04) | (0.04) |
| Observations | 64,540 | 34,755 | 4,162 | 4,215 |
| Control Group Mean | 0.66 | 0.32 | 0.59 | 0.64 |

Note: This table provides the reduced form effect of the public information treatments on correctly identifying the beneficiary status. In Panel A, 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. 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. Each regression is estimated by OLS and includes sub-district fixed effects, survey sample dummies and dummy variables for the previous experimental design. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 10A: Effect of Public Information 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*** | 0.16*** | 0.02 | 0.03 |
| | (0.02) | (0.02) | (0.01) | (0.02) |
| Standard Information | 0.25*** | 0.11*** | 0.03** | 0.04** |
| | (0.03) | (0.02) | (0.01) | (0.02) |
| *Difference:* Public - Standard | 0.06* | 0.05* | -0.01 | -0.01 |
| | (0.03) | (0.03) | (0.02) | (0.02) |
| Observations | 5,685 | 5,685 | 3,619 | 3,619 |
| Control Group Mean | 0.06 | 0.06 | 0.05 | 0.05 |

Note: This table provides the reduced form effect of public information treatment groups on card receipt and use, by eligibility status, as compared to the control group. 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 reweight the treatment groups by sub-district so that the ratio of all three income groups is the same. Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, sub-district fixed effects, survey sample dummies and dummy variables for the previous experimental design. We also provide the difference in the two card treatments. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Table 10B: Effect of Public Information on Rice Purchases and Price

| | 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 | 1.57*** | -79*** | 9,272*** | -0.09*** | 0.09 | -50* | 678 |
| | (0.02) | (0.30) | (21) | (1,693) | (0.03) | (0.23) | (27) | (1,258) |
| Standard Information | 0.01 | 0.80*** | -42* | 4,833*** | -0.06** | 0.07 | -26 | 524 |
| | (0.02) | (0.30) | (22) | (1,692) | (0.03) | (0.22) | (25) | (1,218) |
| *Difference:* | | | | | | | | |
| Public - Standard | -0.00 | 0.78** | -37* | 4,439** | -0.03 | 0.03 | -24 | 153 |
| | (0.02) | (0.36) | (22) | (2,021) | (0.03) | (0.25) | (25) | (1,336) |
| Observations | 5,685 | 5,682 | 4,872 | 5,682 | 3,619 | 3,619 | 2,283 | 3,619 |
| Control Group Mean | 0.83 | 5.30 | 2,263 | 28,781 | 0.68 | 3.42 | 2,272 | 18,428 |

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. Eligible households that did not receive a card under the bottom ten treatment are dropped from the sample and we reweight 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. 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. Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, sub-district fixed effects, survey sample dummies and dummy variables for the previous experimental design. We also provide the difference in the two card treatments. Standard errors are clustered by village.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 11: Does Public Information Affect Subsidy Only Through Card Receipt? Implied Instrumental Variables Estimation

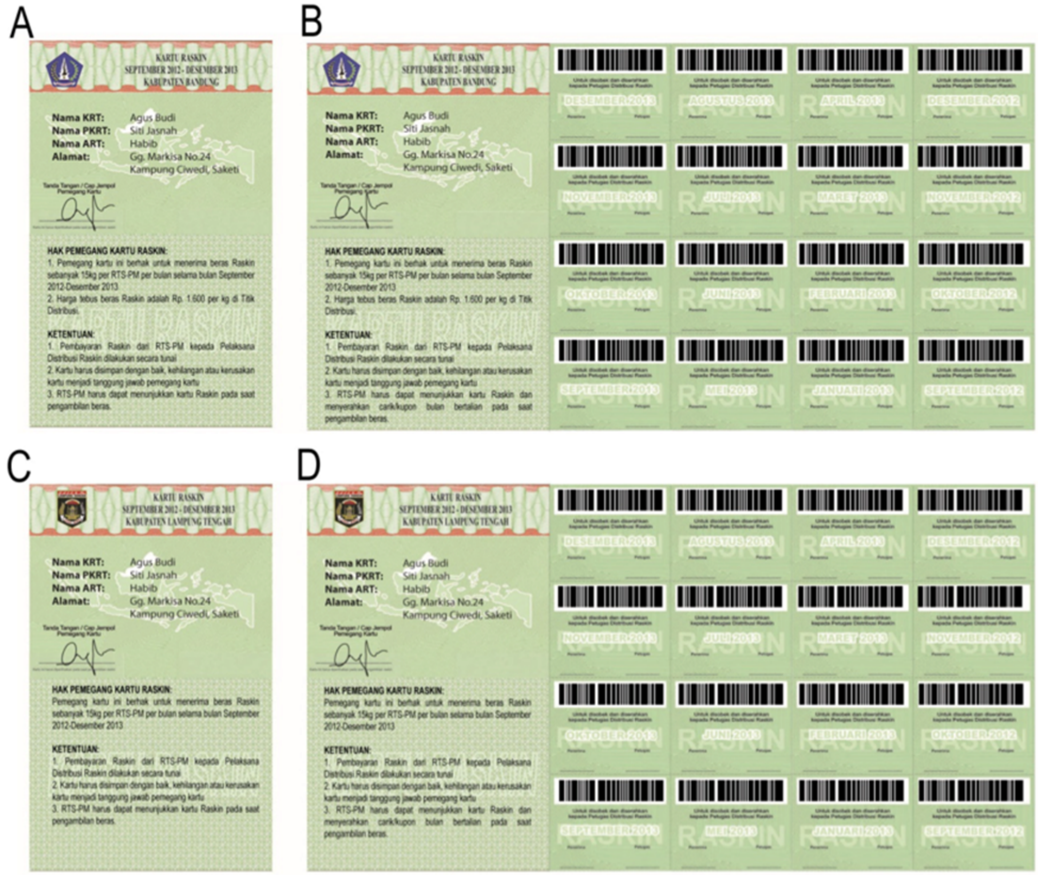
| | Public Information | Standard Information |
|---|---|---|
| | (1) | (2) |
| Received Card | 31,052*** | 17,310*** |
| | (5,344) | (6,482) |
| | | |
| Observations | 3,999 | 3,957 |
| Control Group Mean | 28,781 | 28,781 |
| P-value (1)-(2) | | 0.06 |

Note: This table provides the instrumental variables estimation of the effect of receiving the card on the subsidy. The two instruments used are the enhanced public information treatment (Column 1) and the standard information treatment (Column 2). Eligible households that did not receive a card under the bottom ten treatment are dropped from the sample and we reweight the treatment groups by sub-district so that the ratio of all three income groups is the same. For each household, the subsidy is an average over the past four months; the current month is dropped if the interview occurred before the 25th day of the month. The subsidy is set equal to zero if the household does not purchase any Raskin rice. Each column in this table comes from a separate IV regression of the subsidy on the endogenous variable received card, with instruments given by the column header, and other regressors: the sub-district fixed effects, survey sample dummies and dummy variables for the previous experimental design. The last row contains the p-value of the difference between the two implied coefficients for the received card variable from columns (1) and (2). Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

Figure 1: Experimental Design

| | Total | Card Sub-treatments: Socialization Level: Standard | Enhanced | Distributed to: All | Bottom 10 | Printed Price: Yes | No | Includes Coupons: Yes | No |
|---|---|---|---|---|---|---|---|---|---|
| Identification Cards | 378 | 186 | 192 | 190 | 188 | 187 | 191 | 189 | 189 |
| No Identification Cards | 194 | | | | | | | | |
| Total Villages | 572 | | | | | | | | |

Figure 2: Raskin Identification Cards




Note: Figure 2A shows Raskin identification cards with printed price and no coupons. Figure 2B shows Raskin identification cards with printed price and coupons. Figure 2C and 2D show Raskin identification cards without printed price and without and with coupons respectively.

Figure 3: Project Timeline