Privatization at the Bottom of the Pyramid: Contracting Out Subsidized Food Distribution in Indonesian Villages[1]

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Abstract

Sophisticated contracts and auctions are used to outsource government service provision to private firms throughout the developed world. This paper asks whether the same concepts can work in developing country villages, with much lower levels of human capital and technical expertise. We analyze the effects of a large-scale, randomized field experiment across 572 Indonesian villages in which a procurement process was introduced that allowed private citizens to also bid for the right to implement a subsidized rice distribution program instead of the local government. On net, this led about 17 percent of villages to switch distributors. Applicants who proposed lower prices and applicants who had relevant experience as traders were more likely to be selected. Conversely, villages were more likely to retain the status quo distributor when the initial price charged was low and when baseline satisfaction levels were high. On net, the procurement treatment led to an 8 percent reduction in the markup paid by households. Households reported that the quality of the rice also improved. Mandating higher levels of competition further improved outcomes: introducing a minimum number of bids requirement in a random subset of villages with the procurement process increased the number of applicants who placed a bid and led to a larger reduction in markups than villages without this requirement. We also compare the outcomes in the villages with the bidding mechanism to a random subset of control villages in which local governments provided the same information about the program functioning that they also revealed during the procurement process. We find no effect on price or quality in these villages, implying that the observed effect from the bidding process came from competition and privatization, and not increased transparency. On net, the results suggest that privatization has the potential to improve performance, even at the village level.

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I. INTRODUCTION

A basic question in economics is whether the state should directly provide a public service or whether it should instead contract out the delivery to a private provider. In the seminal paper by Hart, Shleifer, and Vishny (1997, HSV henceforth), the trade-off between these two actions comes from the fact that governments, through procurement auctions and other means, can provide stronger incentives to private contractors than they can to their own employees. This can potentially lead private contractors to deliver better services, i.e. higher quality and/or at a lower price. However, it is possible that these stronger incentives will also push the contractors to lower quality below socially efficient levels in order to cut costs. For this reason, in certain settings where non-contractible quality dimensions may be important—prisons is the example that HSV offer—public provision may be preferable to contracting out services to private providers.

This theoretical argument over whether a service should be provided publically or contracted out presupposes that the contracting process can be done correctly. In practice, however, it may be plagued by many problems, ranging from incompetence in assessing the quality of bids to collusion among bidders to outright corruption or nepotism (see, for example, Tran, 2008; Bandeira, Pratt, and Villetti, 2009). Politicians who value being able to control the allocation of rents may also resist privatization if doing so makes it harder for them to capture the rents (Shelifer and Vishny, 1994). And, even when the politician is honest and the procurement process is run fairly, procurement auctions can end up with a type of winners-curse, where those who win systematically underestimate the cost of delivering a high quality product, leading to either low quality or costly ex-post renegotiation of contracts (e.g., Hong and Shum, 2002). Given the complexity of the issues involved, there is no guarantee that allowing privatization will work, especially if the decision-making body lacks any special expertise in designing and implementing these types of mechanisms.

In this paper, we use a randomized control trial across 572 villages in Indonesia to explore the efficacy of giving villages the option to privatize a publically provided service. The service under consideration was Raskin, Indonesia's largest targeted transfer program a subsidized rice transfer program

(annual budget of over US$1.5 billion) in which eligible households can receive a monthly allocation of subsidized rice. As is typical in most developing countries, even though this is a central government program, it is actually administered at the local level by village officials.[2] In the default arrangement, the village government appoints someone in the village government to be in charge of distribution, usually either the village head himself or someone he designates as village social welfare coordinator.

There are substantial reasons to believe that there is room for improvement in the program. For example, while the monthly co-pay for the rice should be Rp. 1600, the average buyer pays a mark-up of Rp 657 on top of this. Rice goes missing through the system and ineligible households also illegally purchase Raskin rice. Thus, eligible households only receive about one-third of their monthly allotment of rice. And, there are many complaints about the poor quality of the rice distributed and how the rice is distributed (e.g. location, timing, etc.). On the other hand, is it not clear if privatizing the delivery would improve outcomes. As in the HSV case, there are important non-contractible elements, such as the quality of the rice. And, on top of this, there are the challenges of procurement: there may be limited competition for the distribution job, citizens have limited experience in the intricacies of procurement, and the village leaders may have rents to protect and may try to sabotage the process – or even use it as an opportunity to extract even more rents.

To examine these questions, in 191 randomly selected villages out of the 572, the central government introduced an alternative procedure involving an auction for the right to be the Raskin distributor was adopted. The selection rule used in the auction was not imposed externally, but instead a small committee of appointed members examined the bids and chose the winner. The incumbent local government-appointed distributor was given the option to bid in the auction, along with private citizens. The village therefore retained the option of continuing with the status quo and not privatizing the service. In short, this created a process that would allow citizens to compete with the government leaders for the job, and for the village to privatize the service if it thought this was a good decision.

[2] This is often the case of many large programs. For example, India's guaranteed work program is centrally dictated, but locally run, as is China's urban Di Bao program (Gustafsson and Quheng 2011)), which is among the largest cash transfer programs in the world.

We also conducted two additional treatments to help us understand mechanisms through which the option for private provision may operate. First, in order to run the bidding meetings, one must explain the current process to citizens so that people could understand it and decide whether and how to bid. Thus, if there is an effect, it may be due to the bidding process, or it could simply be due to an increase in transparency. Thus, we also randomly assigned an additional 96 villages (out of the 572) to have the same meetings to describe the current processes, etc., but to not have the actual auction process. This information-only treatment serves in some sense as a placebo comparison group that to allows us to disentangle whether any observed effects are driven by allowing for private distributors to enter or simply arise from increased transparency.

Second, to differentiate between the extensive margin of introducing any private sector competition from the intensive margin of additional competition, in 96 randomly selected villages of the 191 villages assigned to the bidding process, we instituted a policy encouraging a minimum of 3 bids in order to explore the impact of increased competition in the bidding process.

Overall, we find that offering villages the opportunity to privatize led to substantial changes in the distribution process. Villages that were randomly chosen to have the bidding process have, on average, more than 2 bidders for the distribution job. In other words there is, at least on paper, real competition, which, prima facie, could affect the distribution outcomes, even though, as it turned out, the eventual winner in about half the villages is the incumbent. Indeed, we find that being assigned to have a bidding process leads to a reduction in prices paid by households, with the mark-up falling by 7.5 relative to the information only villages and 4.8 percent relative to pure control villages. Given that we find no effect of the information-only treatment on the mark-up that households pay, this suggests that this observed effect was not simply driven by just the additional information provided at the meetings. We find no declines on other dimensions: the quantity of rice received did not change, nor did the quality of the distribution process (e.g. quality of the rice, time to pick up rice) decline. In fact, households actually report a higher quality of rice in the bidding group. Thus, the distributors were not cutting quality to compensate for the price changes. This is not just a reduction in rents – as HSV predict, there does appear to be an efficiency

gain, as the distributors present at endline in the bidding villages report much lower transportation costs (by about a third) than those in the information-only treatment.

The improvements that we observe are consistent with villages making what appear to be broadly sensible choices among bids, rather than being driven by capture or plagued by technical incompetence. Comparing all winning and losing bids, the winners placed bids that had a 17 percent lower mark-up than the losers. Winning bids were also more likely to promise to bring the rice closer to households than losing bids. While winners were more likely to require that households pay upfront for the rice (which could be a negative for households), the committee may have also chosen winners who realistically understood that they needed upfront funds to be able to procure the rice from the central government; moreover, the winners were more likely to offer credit in their bids to offset the fact that they were more likely to require an upfront payment.

Villages also appear to select individuals who have relevant skills and characteristics as distributors. Specifically, being a trader is advantageous, with villages willing to pay about Rp 250/kg (30 percent of the control mean) in terms of higher prices for a trader. Having access to transportation that could be used in the Raskin distribution is similarly valued in the village decision rule, worth about Rp 140/kg. Interestingly, while winners discussed their "good reputation" as a top reason why they were chosen, we do not observe any differences between winners and losers on an experimental measure of dishonesty that we had tasked them with. There is no evidence of elite capture on average: once one conditions on bid characteristics and relevant professional experience, there is no incumbency advantage, nor are village officials more likely to win.

Taking these results together, it appears that the decision rule focuses primarily on price – 82 percent of villages with multiple bidders chose the low price bid – but deviates from it in directions that predicts higher credibility as a supplier (being a trader, owning transportation). While we do not of course know whether decision-makers are making the optimal choice, it is reassuring that there is no prima facie proof of obvious errors and that the decisions they make look broadly sensible if one is trying to maximize some combination of low price and high reliability.

This evidence suggests that villages were able to handle the auction stage of the process. But, what happened after the auction? As discussed above, one can imagine several possible issues. First, it might be the case that when there is a strong incentive to bid low to win the contract – as there appears to have been in this case – there is a risk of ex post default (or renegotiation based on threat of default), which ends up making the outcome worse. To examine this, we look in our endline survey to see what fraction of winning bidders are no longer distributing at endline – and if so, what happens. We do observe 38 cases (out of 185) where the winner says that he or she is no longer distributing at the endline, but only in about third of those villages the stated reason for that is the inability to handle the job rather than being prevented from doing so by other government actors. If we examine the gap between the endline price as reported by households (which is after the default would have taken place) and the baseline price, we find that this gap is if anything lower in default villages (i.e the gap is more favorable in the "default" villages). In other words, to the extent there are defaults, there is no prima facie evidence that the default generated worse outcomes than might have occurred otherwise.

The fact that winners were prevented from delivering in some of the villages flags a second, perhaps larger, concern about existing village elites blocking privatization to protect their rents (e.g. Acemoglu and Robinson AER 2000). The incumbent supplier is often the village head or someone closely related to him. The village head therefore has some reason to put road-blocks in the privatization process. In fact, 165 of the 192 villages did carry out the bidding process and obtain at least 1 bidder, and in almost half of villages the winner was someone who was not the incumbent. However, noted above, not all those winners ended up distributing, in part perhaps because they were blocked by the village authorities from doing so.

One would imagine that the village head will have a stronger incentive to find ways to defend his incumbency when he obtaining substantial rents from the current process. Although we cannot necessarily measure those rents directly, one indicator that there may be substantial rents in the system is a high markup for Raskin at markup, especially conditional on geographic characteristics that may influence actual distribution costs. A village head trying to protect his rents could do this by blocking the bidding

process from occurring, preventing anyone from bidding, by putting pressure on the committee to not pick the lowest priced supplier, or by simply not allowing the new winner to do his job. However, we see no evidence that villages were less likely to pick a new supplier in areas where there appear to have been more rents. In fact, we find that new distributors were more likely to win when the baseline price reported by households was high and when baseline satisfaction with the program was low, even conditional on other characteristics. Similarly, in our endline, the old distributor (regardless of whether they legitimately won the auction or not) is still most likely to remain the distributor in areas where the baseline price was low, rather than where the baseline price was high. While this does not rule out the possibility that the incumbents are pushing back and preventing the bidding process from having as large an effect as it could have had, it suggests that the capture of the bidding process by the village leaders is, at worst, relatively limited.

Increased competition on the intensive margin further improves outcomes. In villages that were randomly assigned to the "minimum number of bids" treatment, more people bid (2.74 bidders) than when there was no requirement (2.14). We find the largest decreases in price-markups in the villages that had the minimum number of bids compared to auction areas that did not (with no other observable declines in quality). Interestingly, the bids themselves were note lower with more competition; rather, villages appear to have followed a slightly different decision making process, using the extra degrees of freedom generated by having more competition to choose bidders with relevant experience, who may have been more able to deliver on what they promised. The results suggest that additional increases in competition further improve outcomes, rather than worsen them as could occur in a common values winners curse situation.

In short, we find that giving villages the option to contract out delivery of government services by increasing competition within the system, and that even local governments in a country like Indonesia—where education levels among village decision-makers are relatively low and reported corruption is relatively high—can benefit from using a bidding mechanism to choose its distributor rather than automatically relying on the village government to do the job. However, it is important to note that while

we observe a decline in the price mark-up, the gains were relatively modest, and there was no increase in the total quantity of rice distributed – which may be the largest source of leakage in our context. Simple changes to the process such as requiring a minimum bid to further increased competitive pressures led to higher gains, suggesting that it is important in future research to further think about (and test) what types of policy choices in how one structures the government's engagement with the private sector are needed to better align the incentives of service providers with citizens.

The remainder of the paper proceeds as follows. Section II describes the setting, experimental design, and data. Section III presents the overall results on the impact of the bidding process, compared to the information-only placebo group and the pure control group. Section IV explores three key channels: how villages decided among multiple bids they received; whether the process was able to overcome existing vested interests protecting their rents; and whether and how additional increases in competition further improved outcomes. Section V concludes.

II. Setting, Experimental Design and Data

A. Setting

We examine Indonesia's subsidized rice program, known as "Raskin" (Rice for the Poor). First introduced in 1998, the program entitles 15.5 million low-income households to purchase 15 kg of rice per month at a co-pay price of Rp. 1,600 per kg (US$0.15), or about one-fifth of the prevailing market price. This intended subsidy is substantial, equaling about 4 percent of beneficiary households' monthly consumption. It is Indonesia's largest permanent, targeted social assistance program, with an annual budget of over 1.5 billion USD intended to distribute 3.41 million tons of subsidized rice each year (Indonesian Financial Note and Revised Budget, 2012).

Although it is a national program, much of the day-to-day logistics are handled at the local level. The central governmental logistics agency procures the rice and delivers it to its warehouses across the country, typically located in district capitals. The village government is responsible for picking up the

village's allotment of rice --on average, 5550 kg of rice each month -- from the a central distribution point (either the warehouse itself or a central point located in the subdistrict capital), located, on average, about 7 kilometers from the village. While there is some heterogeneity in how the program is run, in most cases, the village head appoints someone in the village government to be in charge of picking up the rice and the distribution, usually either the village head himself or someone he designates as village social welfare coordinator.

While picking up the rice at the warehouse, the local leader has to remit the co-payment for the rice to the central government. Once they transport the rice back to the village, there is substantial heterogeneity in where they distribute it—at the village office, at the homes of hamlet or neighborhood heads within the village, or even transporting it directly to beneficiaries' houses. Village governments are not only responsible for the time and effort required to distribute the rice, but they also assume the transportation costs, which in control areas cost an average of Rp. 244,161 (US$21) each month.[3]

In practice, Raskin faces a number of challenges. Villagers complain that quality of rice is often very low. Rice may go missing at all stages in the distribution chain—from the central government to the sub-district distribution point to within hamlets (Olken, 2006; World Bank, 2012). Moreover, when the rice does make it to the village, it may be given to ineligible households rather than the eligible ones. On top of this, the village governments often charge households a much higher co-pay price to receive the rice than the central government intends. As shown in Appendix Table 1, in the baseline data, buyers paid an average mark-up of about Rs 660 per kg (about a 40 percent mark-up on the official co-pay price).[4]

It is important to note that these facts do not necessary imply malfeasance: the village governments may be diverting rice to deserving, but ineligible households, or they may charge a higher co-pay for legitimate reasons, for example, to cover the transportation costs of distributing the rice.

[3] There is, however, regional heterogeneity in these costs. In some areas, sub-district or district governments help subsidize these transport costs; in other areas, the sub-district may also deliver the rice directly to the village.
[4] As Appendix Figure 1 shows, there is considerable heterogeneity in mark-up, with relatively few households buying at the official rate.

However, the distributors in our control group report transport costs that only account for about 12.4 percent of the difference in price. Thus, it is likely that much of the higher price and missing rice is lost through corruption.

B. Sample

This project was carried out in 6 districts in Indonesia (2 each in the provinces of Lampung, South Sumatra, and Central Java). 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). To further ensure heterogeneity across institutions, we ensured that the sample consisted of about 40 percent urban and 60 percent rural villages. 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.[5]

C. Experimental Design

Stratifying by geographic location and the previous experiments, we randomly assigned the 572 villages to one of three treatment assignments—pure control, bidding, and information-only—as follows:

Pure Control: We randomly assigned 285 villages to the control group (see Appendix Table 2). These villages reflect the status quo distribution process detailed above, where the village leadership primarily assumes leadership for local pick-up and distribution.

[5] Due to a constrained timeline for providing feedback into policy, we conducted the experiment in an area where we had previously conducted an experiment on an unrelated cash transfer program that is run by a different government ministry than the Raskin program (see Alatas et al. (2013a) and Alatas et al. (2013b)). We also conducted a separate Raskin experiment on transparency (see Banerjee, et al 2014). As we discuss below, we stratified the treatment assignments in this project by the previous experiments in order to ensure balance across the previous interventions.

Bidding: We randomly assigned 191 villages to a process where private individuals could bid for the right to become the official Raskin distributor, i.e. to purchase the rice at the sub-district, transport it to the village, and sell the Raskin rice within the village.

The bidding process proceeded as follows: a local facilitator would arrive at the village, accompanied by an official letter from the central government, to explain to the village leader that the village had been selected to have a procurement process for Raskin distribution. The village leader would then be asked to organize a meeting in which the current distributor would describe the current distribution process and then the procurement process would be announced. At this meeting, citizens were told that anyone who wanted to—from both within and outside the village—could bid for the right to distribute Raskin by submitting a bidding form within in 10 days. The bidding form was a standard form that was provided to the local village government, which included, but was not limited to, the price that the prospective bidding would charge citizens, the process (e.g. where the rice would be distributed in the village, whether the households would have to pay upfront for the rice, etc), and the bidder qualifications (e.g. access to credit, owning a truck, etc). Note that the central government was insistent that households should receive their full allotment as they allocated regardless of who distributed the rice and so the quantity of rice that the potential distributor would allow households to buy was not included on the forms. Bidders did not necessarily know the number of other bidders at the time they submitted bids, and bids remained sealed until the bidding meeting.

Individuals were told that the winner would have the right to distribute Raskin for 6 months and that there would be another meeting at that time to determine whether the village wanted to continue with the winning bidder, revert to previous status quo distributor, or set up a new bidding process. In addition, a small committee was formed during this organizational meeting to oversee the bidding process and monitor its outcomes.[6]

[6] The committee was supposed to be drawn from an existing village government oversight committee (the LPM). Often, the LPM was defunct, so the committee was put together at the organizational meeting by the local leaders. To avoid conflicts of interest, members of this committee were not currently involved in Raskin distribution.

Note three important details about the process. First, in addition to trying to spread information about the bidding process via word of mouth from the village attendees, informational posters were posted in key locations within the village and within the sub-district capital in order to advertise both inside and outside the village. Second, the current distributor—who was generally a village leader or another member of the local government—was also allowed to bid if they wanted to, and in fact, the current distributor bid in 66 percent of the cases where there was at least one bid.

Third, we randomized whether there was a minimum number of bids that was needed for the procurement process to commence.[7] Specifically, in 96 randomly selected villages, we required a minimum of 3 bids, while there was no requirement in the remaining villages (Appendix Table 2). If three bids were not submitted by the deadline, the deadline was extended by 10 days. If after the extension, there were still not enough bids, the process continued with the realized number of bidders. If more than five bids were submitted (which only happened in 7 villages), the committee chose only the best five to be presented at the meeting so that there would be sufficient time to present and discuss all of the bids.

After the window to submit bids, but looking at the bids, the committee developed a set of criteria by which to select the winner. The committee was given some suggestions, including: proposed Raskin retail prices, distribution methods, pick-up locations for households, household payment methods, distributors' assets and capital ownership, projected costs of distribution, bidders' experience level, and bidders' overall character. However, the criteria were left open so that committee could set their own priorities for what constituted a good proposal. At this point, the committee also had the option to reject proposals that were not considered serious (11.8 percent of bids were rejected at this stage).

Next, each bidder presented his proposal to the committee at a meeting. Although the facilitator was present during the meeting to take notes, their participation was minimal and a committee

[7] We also randomized two other aspects of the committee formation and function. First, we randomized whether we required that a third of the committee needed to be female. Second, we randomized whether the facilitators suggested that the committee hold a follow-up meeting within three months to discuss the state of the distribution process. However, no follow-up or monitoring was done by the facilitators to ensure that the committee followed through with this meeting. Appendix Tables 3 and 4, respectively, provide results examining these changes.

representative led the meeting. During each presentation, the key information from the proposal was written on a large notepad that everyone could see to facilitate discussion and questions on the proposals. Bidders were allowed to improve upon their bids during the meeting in response to questions or in response to other bids.

After the presentations, the committee privately scored each proposal according to their criteria and summed the proposal's scores to determine the winner. Note that each bid was scored 1-10 on each criterion, so that committees de facto had substantial leeway in how they assessed various bids. The committees always had an odd number of members (3 or 5) to ensure no ties. They also had the option of rejecting all of the bids, and reverting to the status quo, if they deemed that none were of high enough quality. At the end of the meeting, the village head issued a letter establishing the winner as the official distributor for the next six months; this letter was also provided to relevant sub-district and district officials so that the winner could pay for and pick up the Raskin rice at the warehouse.

The facilitators returned to the village about six month later. The current distributor made a presentation about the Raskin distribution process as it operated at that time and the committee discussed their views on the process. They also decided whether or not to extend the new winner (if there was one), choose a new distributor, or revert back to the old process.

Information-Only: In addition to allowing for competition over who runs the Raskin program locally, the bidding process also naturally provides greater transparency on the current distribution process. Specifically, one must provide information about the distribution process, so that potential bidders can decide if they want to bid and, if so, provide realistic bids. Therefore, if one observes an effect of the bidding process, it could be due to competition, or it can simply be due to this increase in information. To control for the information effects of the bidding treatment, in addition we also randomly selected 96 villages for an information-only treatment, where a community facilitator coordinated with the village leader to set up the organizational meeting where the current distributor described the current process. At that meeting, a committee was also organized to discuss and monitor the process, following the same

procedures as in the bidding treatment. Again, a follow-up information meeting was also carried out at the end of 6 months to again provide information on the distribution process (i.e. at the same time as the re-evaluation meeting of the bidding treatment). This treatment was, therefore, identical to the bidding treatment in terms of providing information to citizens and organizing a committee who could potentially monitor the process, but did not include the privatization and bidding process.[8] We therefore use this treatment as a comparison group for the bidding treatment to isolate the pure effect of the bidding process above the increased transparency it provides.[9]

D. Randomization Design, Timing, and Data

Appendix Table 2 shows the number of villages randomly assigned to each treatment. We stratified by 6 geographic strata (districts) and the previous experimental treatments.

The timeline was as follows (see Appendix Figure 2): in April-July 2013, after the baseline survey was completed for the entire sub-district, both treatments were conducted. During the following six months, facilitators maintained a call center to address any on-the-ground issues, though only 17 calls were received in total. In January and February 2014, after the endline survey was completed in that sub-district, the facilitators returned to villages six months after the initial treatment was conducted to hold follow-up meetings.

[8] As in the bidding process, we also randomly allocated half of the villages in this treatment to have a third of the committee be female, and for half to be encouraged to hold a follow-up meeting at three months on their own (without any facilitators, etc) to discuss the state of the distribution. Appendix Tables 4 and 5 provide these results.

[9] A potential concern is that a bidding meeting might be more interesting, and hence draw more attention, than an information only meeting. Appendix Table 18 compares what happened at the information only and bidding meetings, and shows that they while the meetings were obviously not identical, they were broadly comparable in terms of intensity of activity, as measured by meeting length, number of people attending, and number of questions / comments. Specifically, information-only meetings were slightly shorter than bidding meetings (1.58 hours vs. 1.74 hours, so bidding meetings were 9.6 minutes longer on average), but had slightly more particpants (28.5 vs 21.7) and slightly more questions/comments (6.5 questions in information meetings vs. 4.3 in bidding meetings).

E. Data Collection

All surveys were conducted by SurveyMeter, an established independent survey organization. Two household surveys serve as our baseline, one conducted in October and November 2012 and one in April and May 2013. Each survey was conducted in a separate randomly selected hamlet within the village. In total, across both survey waves, we randomly sampled between 15 and 19 households per village, for a total of 10,277 households.[10] We then surveyed the households on their background and their experiences with Raskin. In addition, at the time of each baseline survey, we also interviewed the village head.

In December 2013 to January 2014, just before the six month follow-up meetings were held in the treatment villages, an endline survey took place in which we interviewed 6 randomly selected households from each of the two baseline surveys (12 households per village), for a total of 6,864 households. As in the baseline surveys, we also surveyed the village leader.

At the time of the endline, we also conducted a "distributor survey" in order to better understand who was selected by the bidding process. We interviewed all then-current Raskin distributors. In the bidding and information villages, we also interviewed the old distributor (if different than the currently active distributor), as well as the winner in the bidding villages (if different than the current, which could occur, for example, if the winner was denied permission to distribute or quit). In the bidding villages, we also randomly selected one losing candidate and interviewed him as well. In this survey, we gathered professional information on all candidates (e.g. tested their ability, asked about their management experience, etc.) and asked information about the distribution process if they were involved in it.

Finally, we have access to administrative data from the bidding forms filled out by prospective bidders and facilitators of the bidding process.

[10] We oversampled households on the list of households eligible for the Raskin program to ensure adequate representation of these types of households in the survey. There are more households in baseline than in endline as the baseline was used for other purposes (Banerjee et al 2014).

F. Experimental Validity

Appendix Table 6A provides a check on the randomization of villages to the control, bidding treatment and information treatment. We provide the difference, conditional on strata, between bidding and pure control (Column 5), information-only and pure control (Column 6), and bidding and information-only (Column 7). Of the 45 differences that we estimate between the groups, only 5 (11 percent) are significant at the 10 percent level, which is consistent with chance. The joint p-value across all 15 variables is 0.23, 0.50, and 0.20 in Columns 5-7, respectively. In Appendix Table 6B, we also conduct a randomization check on required minimum bids versus open bidding process. Again, the treatments appear balanced across the treatments with none of the individual differences statistically significant at the 10 percent level with a p-value for a joint significance test of 0.71.

G. Descriptive Statistics on the Bidding Process

We begin in Figure 1 by plotting the flow of the 191 villages through the bidding process to document how the bidding process was implemented. We also provide the average price markup of Raskin rice reported in the household surveys at both baseline and endline for the villages that completed each step in the process.

A few descriptive facts from this flowchart are worth noting. First, almost all – 185 out of 191 – of villages randomized to the bidding treatment villages conducted the procurement processes, though 20 of these villages received no bids and reverted back to the status quo. However, the procurement process did not necessarily result in a new distributor: of the 165 villages that received at least 1 bid, 86 of them (52 percent) selected the original distributor.

Second, the baseline markup seems to be an important predictor of how the bidding process proceeded. In particular, there appears to be more competition in places with higher markups: in places where there were no bidders, the baseline price markup averaged only Rp. 370; the baseline price markup is then monotonically increasing in the number of bidders all the way to 4 bidders, where it averaged Rp.

Rp 766. The winner is a new bidder in villages with baseline markup of Rp. 773, compared to a baseline markup of Rp. 638 in villages that selected the original distributor as the winner. On the other hand, there is some suggestive evidence that ex-post blocking behavior from village leaders occurs when there are more rents to protect: the 6 villages where the winner was blocked from distributing by the village or subdistrict had a baseline price almost double the average. The fact that the Raskin price at baseline predicts the number of bidders, rejecting the old bidder, and ex-post blocking by local elites suggests that the price may be a good proxy for the rents that were in the system. Though these are only descriptive statistics (in particular, they do not control for regional differences, other characteristics, etc), they are suggestive, and we explore these issues in more detail below.

Table 1 presents descriptive statistics on the bidding process. In Column (1), we present the overall mean, while in Columns (3) and (5), respectively, we present the means for villages randomly assigned either to the open process or to having a minimum number of bids for the process to occur. In Column (7), we present the p-value of the difference of means across the open and minimum bids.

Citizens did bid for the right to distribute Raskin (Panel A). On average, we observe 2.43 bids placed, with 2.16 passing the initial selection and thus to be presented at the meeting. However, it is also worth noting that the bidding meetings may have been dominated by the opinions of a few, particularly the elites (Panel B of Table 1).[11] On average, we observed about 22 individuals at the bidding meetings (the average population size in these villages of 1,299 households). Local leaders comprised a fair share of the participants, with about 9 official leaders attending, on average. About 8 of the individuals who attended the meetings claimed to be Raskin beneficiaries. The facilitators reported that relatively few people spoke at these meetings, with no discussion from the crowd in 9 percent of cases and with less than 10 percent of attendees talking at 43 percent of the meetings (Panel C). In only 3 percent of the meetings did the facilitator report that more than half of the crowd participated.

[11] In Appendix Figures 3, we present reasons reported by the winners and losers, respectively, on their outcomes. The three biggest reasons that winners attributed their success were their reputation, support from village leaders, and their level of commitment (Panel A). On the other hand, the top reasons for losses were high purchase price and lack of support from village leaders (Panel B). This is also suggestive that the process may have been influenced by the local officials, whom the process was designed to circumvent or place pressure upon to improve.

Importantly, requiring a minimum number of bids led to more legitimate bids considered at the meeting, but did not change the probability of selecting a new distributor (Panel A). There were 2.74 bids in villages randomized to the minimum bid treatment as opposed to 2.14 in the open treatment; this difference is statistically significant with a p-value of 0.01. One worry is that to fulfill the requirement, we would observe more bids that would not pass the initial screening process for quality. This was not the case: in the minimum bid villages, we still observe a statistically significant increase in bids that pass the screening process (2.44 relative to 1.88; p-value 0.01). There were more meetings with no discussion (15 percent versus 3 percent when it was left open), but this may have been due to the fact that there were more proposals to present at the meeting. On net, a new distributor won in 45 percent of minimum bid villages as opposed to 51 percent in the open villages; this difference is not significant at conventional levels (p-value of 0.49).

III. IMPACTS OF THE BIDDING PROCESS

A. Who is in charge of distribution?

We examine whether the Raskin distributor characteristics changed as a result of the bidding treatment using the endline data. Specifically, we estimate the following regression:

$$y_{is} = \alpha_s + \beta(BIDDING\ or\ INFO)_{is} + \gamma BIDDING_{is} + \epsilon_{is}$$

where i represents a village and s represents one of our geographic strata. We include an indicator variable for whether the village had either the bidding or information-only treatment ($(BIDDING\ or\ INFO)_{is}$) and an indicator variable for just the bidding treating ($BIDDING_{is}$). Thus, the coefficient γ captures how the bidding villages differ from those that received only the information-only (i.e. placebo) treatment, and is thus the key coefficient of interest for understanding the impact of the bidding treatment. We also report the p-value of the difference of the full bidding treatment against the pure control group in the row labeled "Bidding = Ctl" at the bottom of each table.

Table 2A and 2B presents the results. The dependent variable in each column is a different characteristic of the *current* distributor from the endline survey, which took place approximately six months after our intervention. This specification thus captures the net effect of the treatment in practice, taking into account the fact that the distributors may naturally change over time and that winning bidders may be blocked, resign, or be otherwise forced out. In fact, as shown in Figure 1, the new distributor never distributed Raskin in 22 cases (28 percent of the time the winner was a new distributor).

Six months after the implementation of the treatments, villages that were assigned to the bidding treatment were substantially more likely to have a new distributor relative to the other groups (Table 2A). Specifically, the distributor in the bidding villages was 17 percentage points—or 21 percent—less likely to have had Raskin responsibilities prior the intervention than the information-only group (Column1), and about 20 percentage points more likely relative to the pure controls.

The remaining columns explore the identity of who the distributor was. In the pure control group, almost about 85 percent of the distributors were a village official, hamlet official, or the spouse of one (Columns 2, 3, and 4). In the bidding group compared to the pure control group, village leaders were significantly less likely to be in charge (column 2), but their spouses/relatives and hamlet level-leaders were then more likely to be in charge (columns 3 and 4); thus, overall elite participation after the bidding process was not greatly different than in the pure control group. Interestingly, this same pattern was occurring in the information-only group as well, and while the effects are qualitatively bigger in the bidding group than the information-only group, the differences are not statistically significant. This suggests that some of the change in leadership may have been due to greater information.

The more noticeable change was that there was a large increase in the probability that the distributor was a trader by occupation in the bidding villages, relative to both the information-only and pure control group, suggesting that the distribution was more likely to be run by individuals that have skills relevant to distributing Raskin, even if they are “elites” (Column 5).

In Table 2B, we explore several characteristics of the individuals who are distributing Raskin. We find that the new distributors are more likely to have a personal savings account for business, which

suggests that they have some financial access necessary for handing the copayments involved in the process (Column 5). However, we find no difference in the propensity to own a truck or boat, no difference in score in a digit span test (e.g. ability), no difference in education level, and no difference in dice score points (e.g. a measure of dishonesty) for distributors in bidding and information villages (Columns 1-4).[12]

On net, the results suggest that while the bidding treatment changed the identify of those distributing Raskin, the results were largely about redistributing the role within the existing village government elite rather than moving it out from the elite altogether – but within the elite, moving the job and towards someone with relevant experience as a trader and capital that might be required to do the job effectively.

A. Impact on program outcomes

The next natural question is whether the bidding process led to a change in actual program functioning, as well as satisfaction with the process (Table 3). To examine this, we use the household survey level data. We estimate the same equation as in Table 2 using OLS, but since we use household level data, we cluster standard errors at the village level to take into account the fact that the randomization was conducted at the village level. We also control for the baseline value of the outcome variable in all regression except rice quality in Column (4), for which we do not have baseline data.[13]

Before we turn to the results, note two important aspects regarding the interpretation of the findings. First, we estimate the intent-to-treat effects, rather than the IV impact on those villages where there was a new winner. This is because the act of having to compete for the distribution rights may have changed the outcomes, even if the old distributor won the distribution (as is often the case). Second, as

[12] A modified version of the task in Fischbacher and Follmi-Heusi (2013), the task involves the survey respondent tossing a die 42 times, away from the prying eye of the surveyor, and recording the number on the face of the die on each roll. Participants would then receive Rp. 100 (USD 0.01) for each die point that they record. The task outcome predicts real-world corruption: Hanna and Wei (2013) conducted this task with government nurses in India and show that a high score (i.e. potentially lying about the points) is correlated with fraudulent absenteeism; in our sample, we observe distributors with a high score in the task indeed do charge a higher price (Appendix Table 7).

[13] Appendix Table 8 replicates Table 3 omitting the baseline controls. The results are qualitatively similar.

neither the bidding nor information treatment had an effect on the relative propensity to buy Raskin rice across eligible and ineligible households, nor on the relative total quantities that these households bought, we pool eligible and ineligible households. Thus, the regression results provide outcomes and perceptions of service quality for all citizens, regardless of eligibility status. In Appendix Table 9A and 9B, we disaggregate Table 3 by eligibility status and show that findings are qualitatively similar, regardless of who bought the Raskin rice (but greater precision in estimates for eligible households in terms of price changes); since the results are similar for both groups, we report result for all citizens together from here on.

As shown in Table 3, we find that the bidding treatment led to a reduction in the Raskin co-pay price, which as we discuss below was the key dimension that bidders competed on. We observe a Rp 49/kg reduction in price markup relative to the information-only treatment (statistically significant at the 5 percent level): this constitutes about an 8 percent reduction in the markup charged (Column 2).

One worry is that to compensate for the lower price, more rice would go missing. This may particularly be the case because as the central government had mandated that distributors were supposed to provide the correct quantity of rice—and provide it to eligible households—so this was not a category in the application form for the bid, and thus was not a criterion in which bidders were evaluated – even though correct distribution is an important issue in practice Put another way, since all distributors were in theory supposed to distribute all the rice, it wasn't possible for bidders to compete on this dimension. In any case, the overall quantity of rice bought did not change (Column 3).

Another worry, articulated by the HSV theory, is that distributor may shirk and reduce non-contractible dimensions of quality. In our case, a key dimension is quality of the rice. Distributors can increase the quality of by refusing to accept low quality deliveries from the government, or by stopping a practice of selling high quality rice on the market and substituting lower quality rice for Raskin. On the other hand, quality is non-contractible: measurement of quality is fairly subjective (i.e., does the rice smell bad?), and distributors can always blame quality problems on the government warehouse where they received the rice.

To examine what happens on this non-contractable dimension, we asked households to assess the quality of the rice they received (Column 4). We observe increases in satisfaction with the quality of rice – about 3.7 percent higher compared to information only, and about 4.9 percent higher compared to pure controls (p-value < 0.01).

Looking at other dimensions of the distribution process, such as physical distance to purchase point, time need to get to that point (which may differ from distance depending on road quality and other roadblocks), or whether the households paid for rice in advance (Columns 5-7), we do not find that these measures of quality worsened to compensate for the price change. If anything, households—particularly those in min bid villages—report that the time to travel to pick up the rice falls (Column 6). Finally, we examine changes in overall satisfaction with the Raskin process across the treatment groups (Column 8). Overall satisfaction with the program actually fell in the information treatment, with no additional difference for just the bidding process. This suggests that the increased transparency of how the process differs from the official process may have reduced overall program satisfaction, but against this baseline, there was no additional change in satisfaction associated with the bidding.

Overall, we observe a decrease in the price mark-up and an increase in quality, implying that allowing for competition through the bidding process, on average, improved the Raskin program.

B. Impact on Distribution Costs

As HSV theoretically point out, contracting out may have an efficiency gain if it leads to lower costs. To investigate this, at the endline, we interviewed the distributor (whoever that may have been) and asked about their costs of distributing Raskin.

Note two aspects of the cost measures. First, they are self-reported; given the informal nature of the economy, one cannot track these costs through credit card or bank transactions. Nevertheless, they may shed some light on how the distributers functioned, especially since distributors may have felt pressure to have their math add up so that the total costs they reported was equal to the total revenues they received. Second, the reported costs often *increase* in the information treatment relative to the pure

control: this may be because the information treatment forces distributors to have better compute their actual costs (again to make sure they add up), or because the greater scrutiny forces them to report their true costs. Given this, it is important to compare bidding and information to information only, rather than to pure control, to hold the effect of greater transparency constant.

Table 4 shows that, indeed, we observe a decrease in transportation costs in the bidding treatment overall, relative to just pure information (Column 1). This is consistent with the view suggested by HSV that contracting out government services can lead to efficiency improvements. We observe total costs falling (Column 4) by about 25 percent, and though this not statistically significant at conventional levels (p-value 0.133).

IV. CHANNELS

The results thus far suggest an overall improvement in the administration of the program with contracting out of service provision, though the results are quantitatively not enormous. The overall results, though, are the product of a number of forces that can potentially go in offsetting directions, and to the extent there are forces in conflicting directions this could potentially explain the modest overall magnitudes of the results. In this section, we break down the bidding process to analyze its components: how do villages select among the multiple bids they receive; do local elites block the process – either ex-ante or ex-post – in order to protect their existing rents; and does increases in competition on the intensive margin improve outcomes by encouraging bidders to bid more aggressively or does it lead to a winner's curse.

A. The Village Selection Decision

Selecting among bidders is complex. To the extent there are noncontractable dimensions of quality, those running procurement auctions may wish to use characteristics that predict quality, such as a bidder's reputation or other predictors of performance. One may also be concerned that bidders may attempt to renegotiate ex post if the winner cannot be forced to honor his original proposal; indeed, such

renegotiations may be optimal in order to share risk if there is some amount of information about the job that is only revealed ex-post. But if renegotiations are allowed, one may worry that a firm might adopt the strategy of bidding low to win the auction and then turn around and ask for better terms (or abandon the job) and that this may ultimately lead to higher costs for the government (see Chang, Salmon and Saral, 2013, for an excellent discussion of the theory and evidence on this issue; see also Decarolis 2014 for a real-world example of this phenomenon from Italy)[14] Others (see for example, Gil and Oudot (2009)) have recommended abandoning auctions altogether and negotiating with the bidders.[15] Given the complexity of the issues involved, there is no guarantee that the right mechanism will automatically be chosen, especially if the decision-making body has no special expertise in designing mechanisms (as will be the case in both the context we study and many local governments worldwide).

Although we have seen that, on net, the bidding process improved outcomes, it is instructive to examine what villages actually did with the bids they received – i.e. did they essentially choose based on price, or did they consider other factors as well that may predict performance?

We start by comparing winning and losing bids in Table 5, we present information on what the applicants proposed in bidding forms, by winning (Column 1) and losing (Column 3) bids. In Column 5, we present the p-value of the difference between winners and losers; note that this is clustered by village.

Winning bids look different than the losing ones on a number of different characteristics. On average, the winners propose a lower mark-up on the co-pay price (472 Rph/kg) than the losers (567 Rph/kg); this 17 percent difference in mark-up is statistically significant at the 1 percent level. The average winning bid proposed a mark-up that was about 28 percent lower than the baseline mark-up of 654 Rph/kg. These averages mask considerable heterogeneity. Appendix Figure 4 shows the winning

[14] In fact, to avoid this possibility the US Department of Defence allows unrealistic bids to be rejected before implementing the first price auction, while other government agencies in the United States and Europe use what is called an Average Bid Auction, in which the bidder who is the closest to the average of all the bids wins, though this can also facilitate collusion.

[15] However in a well-known theoretical paper, Bulow and Klemperer (1996) show that an auction with N+1 bidders always yield higher revenues than negotiations with N possible providers. Their result covers a wide class of auction mechanisms, but the possibility of ex post default by the winner of the auction is not considered.

price mark-up, by average baseline price-markup as reported by households. Most winning bids propose a price mark-ups that is below the baseline, with the largest differences in villages were the mark-ups were initially very high. However, in areas where the mark-up was initially very low, we also observe some winning bids proposing higher prices; in these cases, the winners were more likely to propose other amenities, such as delivering straight to the households.

The winners also generally promised to transport the rice closer to citizens, with more winners promising to bring the rice to the hamlet rather than households having to pick it up centrally within the village. On the other hand, the winners were more likely to expect households to pay for the rice prior to delivery (44 percent upfront as opposed to 39 percent during delivery) than the losers (36 percent as opposed to 47 percent), presumably for their own assurance that they would recover their costs.

Table 5 includes all bids, regardless of whether or not there was more than one bid. To more formally analyze how villages selected among the various bids that they receive, we restrict our sample to villages with multiple bids and estimate a conditional logit discrete choice model, with standard errors clustered by village, in Table 6. In column (1), we explore each bid characteristic one by one on the probability winning (i.e., each cell in column (1) reports the result from a separate regression). In column (2), we include all bid proposal characteristics jointly (i.e. all coefficients in column (2) come from the same regression), and in column (3), we also add individual characteristics of the bidder to the specification from column (2).

We observe some clear patterns on how villages chose among multiple bids in Table 6. For example, the proposed price is a significant predictor of winning, even conditional on other proposal characteristics (Column (2)) or individual characteristics (Column (3)), which further suggests that price enters strongly into the villagers' decision rule. In fact, in these villages with multiple bids, the lowest bidder wins 82 percent of the time. There is evidence of some incumbency advantage -- being the distributor at the time of bidding is also advantageous (Column (1)) -- but the effect becomes smaller in magnitude and insignificant when we control for proposal characteristics, suggesting that most of the

incumbency advantage comes from being able to propose a more attractive bid, rather than an incumbency advantage per se.

There is also evidence that, even conditional on price, villagers select distributors with skills that may make them more likely to be able to deliver effectively. Specifically, those bids that come from traders have an advantage, even conditional on other bid characteristics. Moreover, the committee appeared to choose winners who had access to transportation that was necessary for the distribution process, were more educated, and had a savings account that can be used for business (Column 1). Note, however, these are no longer significant at conventional levels once you control for other characteristics such as being a trader, in Column 3.

These effects are quantitatively large and suggests that villages are willing to pay substantially for distributors with these characteristics. Dividing the coefficient on 'being a trader' in column (3) by the coefficient 'price' yields a willingness to pay for a trader: Rp 247/kg, which is 30 percent of the control mean. Having access to transportation that could be used in the Raskin distribution is similarly valued in the village decision rule, worth about Rp 140/kg in additional markup. On net, the results suggest that village decision rules are not captured by incumbents; instead, they seem to largely select based on price, with some deviations that favor those with relevant experience or capital.

B. Eliminating or Protecting Rents?

The previous analysis showed that there was no incumbency advantage in how villages selected among bids. But that analysis is conditional on which bids were proposed – as well as on the biding process being held in the first place. And just because a winner was chosen doesn't mean that the winner was allowed to implement his bid, or that the bid was implemented as promised – as shown in Figure 1, implementation was sometimes blocked by local elites, perhaps as a way to protect their rents.

To examine these issues, we explore heterogeneity analysis in how the treatment worked as a function of the price of Raskin observed at baseline, as well as other metrics of Raskin program quality. Although the price of Raskin includes real transportation costs, it also is a likely proxy to some extent for

rents being obtained from the system.[16] And while one might expect substantial heterogeneity based on the baseline level of price, the sign is not completely obvious. On one hand, one might expect that when the baseline price is higher, there is more room for alternate bidders to undercut the incumbent, so the number of bids and the differential response to the treatment could be greater. On the other hand, one might be concern that elites would work harder in high rent areas to block any changes from the status quo (Acemoglu and Robinson, 2000). In addition to the baseline price markup, we also investigate the differential response based on baseline levels of the satisfaction with the program, the quantity of rice received, and several other characteristics of the Raskin process.

Table 7 begins by investigating blocking ex-ante – i.e. of the 191 villages randomized to bidding, where did the bids actually take place? We regress a dummy variable for a village either not holding the bidding meeting, or the bidding meeting being held but there being no bids. As in the previous table, each cell in column (1) shows results from a separate regression; columns (2) – (4) report the results from a single regression in each column. [17] The results show quite clearly that meetings are more likely to be held when the baseline price was higher. To give a sense of magnitudes, a one standard deviation increase in baseline markup (Rp. 427) would increase the odds a meeting was held by 0.30. This suggests that, overall, the phenomenon of meetings not happening or nobody bidding was largely because the existing process was going well, rather than it was going very poorly and was blocked by village elites.

The next set of results in Table 7 investigates the probability that the original distributor was chosen as the winner of the bidding process as a function . This is defined for all 191 villages randomized to bidding; we code the original distributor as winning if the bidding process was not held and the original distributor continued by default. The results suggest that the incumbent is less likely to chosen when prices were high at baseline, are more likely to be chosen when households ex-ante reported being satisfied with the Raskin program, and are more likely to be chosen when the rice is distributed close to

[16] Indeed, looking in our control villages, the Raskin price is correlated with villager's perception of how corrupt the village head and Raskin distributor are (see Appendix Table 14). Of course, this is not dispositive – it may be just that villagers infer there is corruption when they see a high price – but it is nonetheless suggestive.
[17] Appendix Table 13 replicates Table 7, but using OLS. The findings are, again, robust to OLS estimation.

recipients houses. Appendix Table 14 shows that these results are virtually unchanged when we control for objective characteristics that might predict how difficult or expensive it would be to deliver Raskin in the village, such as the number of hamlets in the village, log village population, and distance to the subdistrict (which is where the rice is often dropped off by the village government). These results suggest that on net, villages are more likely to remove the old distributor when the program was poorly functioning, either in the sense of high prices or low satisfaction – despite the potential ability of the incumbent to block the process.

The final set of results in Table 7 examines whether the original distributor is still distributing six months later. This variable combines the previous two effects (was bidding held, was someone other than the original distributor picked as distributor), with a third effect – namely that the original distributor could block the winner from taking effect or could cause them to lose at the re-evaluation meeting held after 6 months. In some sense, this captures the net effect of what actually happened at the village level. The results continue to suggest an effect for price -- the original distributor was able to maintain control of the process when the mark-up of the Raskin rice in the village was lower in the baseline (Column 5 and 8), suggesting again that the original distributor was only able to maintain control (regardless of whether they won or not) when they did not charge too high a price. This confirms the idea that the procurement process seems to reject those who were performing worse, even after taking into account the fact that they could block implementation ex-post.

In contrast to the price-markup, the total quantity that households were able to buy was not predictive of whether the old distributor won, even though reduced quantity to eligible households accounts for a large share of leakage.[18] This is suggestive of the fact that households view that the price is within the distributor's control, but that missing rice is not (or not as visible to households).

The results in Table 7 were just looking within bidding villages. Comparing bidding treatment areas to control areas reveals that the effects of the program are largely concentrated in those areas with

[18] Note that this is not due to households having low demand for Raskin rice. When informed of their eligibility rights and quantity entitlements, households buy much more Raskin rice (Banerjee, et al 2013).

high baseline prices – confirming the view that the program is most effective when it is eliminating large existing rents or other types of inefficiencies. To see this, Figure 2 plots non-parametric Fan (1992) regressions of several outcome variables against the baseline Raskin price markup. Since the information treatment was not substantially different from the pure control, and since non-parametric regressions are quite data-intensive compared to OLS estimates of mean effects, we focus on figures that show the estimated relationships between outcome and baseline price markup for both bidding villages and the combination of information pure control villages. Bootstrapped 95 percent confidence intervals are shown as dashed lines and we include the 45 degree line for ease of comparison.

Figure 2, Panel A shows the endline price plotted against the baseline price in bidding vs information/control villages. In areas where the markup was low to begin with—the bottom half of the distribution—we do not observe any treatment effect whatsoever. The reduction in price instead appears to be driven from large declines higher up the baseline price distribution (i.e. between about Rp. 1,000/kg and Rp 2,000 /kg). This is suggestive that the treatment was most effective in areas where problems were worse – despite the fact that vested interests may have had more rents to protect. In Panel B, we instead look at the quality of rice as reported by households; we again find that the bidding treatment seems to improve the program in areas with relatively high baseline prices, with no improvements in areas with low prices.

Finally, in Panel C, we examine the probability that the distributor at the end of the experiment is different from the distributor at baseline. Overall, the probability of replacing the distributor slopes up in baseline price, a sign that in general villages retain those distributors with lower prices. However, the bidding treatment seems to lead to a uniform increase in the probability of replacing the distributor – even in places with low markups where the treatment does not lead to a reduction in price.

Combined, these results suggest that while the bidding treatment has its main effects in places with high baseline place values, there is one potential cost of the bidding treatment: since the increase in the probability of changing distributors does not vary with the baseline price, but the program improvements are only in places with higher baseline prices, it suggests that the increased probability of

replacing the distributor in areas with low baseline prices is inefficient. That is, when the program is working reasonably well, the bidding process still induces villages to change distributors, even when this achieves no benefits.

C. How does increased competition matter?

A third mechanism we examine is the impact of increased competition on the bids of incumbents and the village decision rule. That is: when the process becomes more competitive, is there a winner's curse through which villages end up choosing firms that bid too low, and who then renege (as in Bulow and Klemperer 2002). Or, do villages adjust their bidding rules in other dimensions, potentially to avoid a winner's curse problem?

To examine these questions, we take advantage of the minimum number of bids subtreatment. Recall that in a randomly-selected half of bidding villages, the village was informed at the start of the process that the bidding deadline would be extended if fewer than 3 bidders bid at the end of the initial 10 day bidding period. As shown in Table 1, this generates randomly-induced variation in the number of bids – from a mean of 2.14 bids in bidding villages without this rule to 2.74 bids in villages with the minimum number of bids rule.

Table 10 examines how this extra induced competition changed bids, controlling for the baseline markup present in the village. We focus on the price dimension. Although the results are somewhat noisy, columns (1) and (2) examine how the incumbent's bid changed based on the minimum bid treatment; we find no evidence that the incumbent reduced their bid in response to the minimum number of bidders treatment, and in fact the point estimates are positive. Columns (3) and (4) show that the average bid also did not change. Even more surprisingly, columns (5) and (6) show that the minimum bid did not change.

Did increased competition help matters, or did it lead to a winner's curse? The results suggest that increased competition led to improved outcomes – largely because winners channeled cost reductions into lower prices for consumers rather than higher payments for members of the distribution team. Table 8 re-

examines the basic results in Table 3, separately for minimum number of bids and open bids treatments. The results suggest that, in fact, most all of the effect on price was driven by minimum number of treatment: the bidding treatment with a minimum of 3 bids led to a Rp. 74/kg reduction in markup (11 percent), compared to only Rp. 24/kg for the bidding treatment without minimum bids (p=0.06). Moreover, the increase in quality occurred equally in village with and without the minimum number of bids, so this reduction in price was not coming at the expense of quality.

Table 9 examines the impacts on bidder's cost structure based on the minimum number of bids treatment. Both bidding treatments seem to lead to a similar reduction in transportation costs (the p-value of comparing open versus minimum bid is 0.396). However, minimum bid treatment also led to a reduction in payments to others and total other costs, whereas the bidding treatment without it did not. All of this combined (Column 4), the net effect of bidding with minimum number of bidders was a substantial reduction in costs, whereas the net effect of bidding without a minimum number of bidders was no change (p-value of difference 0.01). One potential interpretation of these results is that either bidding treatment selected a more efficient supplier (i.e. one with lower actual transportation costs), but without the minimum number of bidders requirement, the winning bidder was able to offset this efficiency gain by not changing the price relative to the pure controls nearly as much, and instead captured these efficiency gains through the nebulous 'payments to others' category.

Combined these results present a bit of puzzle: how could outcomes improve even though, as shown in Table 10, bids did not perceptively change? Table 11 suggests that the difference comes from the village decision rule. Specifically, we re-estimate Table 6, interacting the various components that enter the village decision rule with the minimum bids treatment. In the minimum number of bids treatment, when citizens are given more choice over candidates, we find that citizens prefer the candidates who promise that they do not have to pay before receipt, those who live in their village, and those with trading experience and transportation access, but do not find an observable difference in choosing on price mark-up.

These results suggest an explanation for why outcomes might improve with more competition, even though the lowest price offered does not decline: the increase in competition seems to allow villages to exercise preferences over aspects of the bid other than pure price. It may be that with more choice, villagers are able to choose a candidate who is more reliable on other dimensions for the same promised price, and that these candidates actually deliver on what they promise, rather than ex-post reneging and channel profits to amorphous 'payments to others' (Table 4).

V. CONCLUSION

This paper examined the role in which contracting out of a public good can play in improving service delivery, in a low human capital environment. We found that local villages made what appear to be broadly sensible decisions among bidders, focusing primarily on reducing prices, but also giving weight to relevant distributor experience which may be correlated with quality, such as having previous experience as traders. Villages seems to use the opportunity to eliminate existing rents: they were more likely to change distributors when the baseline price charged was high – which we show was correlated with having a more dishonest distributor.

On net, we found that allowing villages the ability to outsource led to improvements in both price and quality. The improvements were concentrated in those villages where the price was highest at baseline. We show that the improvements were even greater when we experimentally encouraged more bidders in the process. The procurement process chose more led to efficiency improvements (measured by reduced transportation costs), and when we experimentally encouraged more competition, to lower profits being taken by distributors.

That said, it is important to note that the improvements we saw were largely on price and quality of rice. Procurement did not improve what is quantitatively a much more important source of losses in this context: rice that simply goes missing and is not distributed at all in the village. One reason for this may be that quantity is harder to observe; unlike prices or rice quality, which one can readily see from a random sample of the rice distribution, figuring out missing quantities and seeing whether distributors are distributing the entire allocation of rice requires careful counting and monitoring the entire process which

most villages do not do (as in Olken 2009). It is also the case that the bidding forms didn't discuss quantity: since everyone was supposed to distribute *all* the rice in any case, it would not be possible to have an explicit competition on this issue. This suggests that the results of contracting out may be better when all the relevant dimensions of leakage or poor performance can be explicitly documented and competed.

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Table 1: The Bidding Process (Conditional on Bidding Meeting Occuring)

	Overall		Open Bids		Min # Bids		P-Value
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Open Bids = Min # Bids
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Bids Submitted							
Number of Bids	2.43	1.66	2.14	1.68	2.74	1.59	0.01**
Number of Bids, after Initial Screening	2.16	1.50	1.88	1.47	2.44	1.48	0.01**
Old distributor Wins	0.52	0.50	0.49	0.50	0.55	0.50	0.49
Panel B: Meeting Attendance							
Attendees	21.69	9.13	21.09	10.31	22.31	7.75	0.36
Raskin Beneficiaries	8.28	8.32	8.78	8.95	7.77	7.63	0.41
Village or Hamlet Officials	9.42	5.83	8.80	5.95	10.06	5.66	0.14
Panel C: Meeting Participation							
No discussion at meeting	0.09	0.29	0.03	0.18	0.15	0.36	0.01***
<10% of people talk	0.43	0.50	0.46	0.50	0.41	0.49	0.51
10-50% of people talk	0.45	0.50	0.50	0.50	0.40	0.49	0.16
>50% of people talk	0.03	0.16	0.01	0.11	0.04	0.21	0.18

Notes: This table provides summary statistics on the number of bids submitted, as well as the attendance and participation during the bidding meeting. All data come from the forms that the facilitators used to document the bidding process. We first present the sample statistics for the 184 villages where a bidding meeting was held and then we disaggregate the data by whether the village was randomly assigned to the minimum bid requirement (91 villages) or it was left open (93 villages). *** p<0.01 ** p<0.05 * p<0.1.

Table 2A: Who Distributes Raskin Six Months After Intervention?

	In charge of any responsibilities before May 2013	Distributor/ spouse is/was village official	Distributor/ spouse is related to a village official	Distributor/ spouse is/was or is related to subvillage official	Is a trader	Lives in village
	(1)	(2)	(3)	(4)	(5)	(6)
Info or Bidding	-0.035	-0.059	0.043	0.038	0.008	-0.027
	(0.048)	(0.050)	(0.044)	(0.055)	(0.027)	(0.042)
Bidding	-0.165***	-0.076	0.046	0.043	0.071**	-0.019
	(0.051)	(0.053)	(0.047)	(0.058)	(0.028)	(0.045)
P-Value						
Bidding = Ctl	0.000	0.001	0.013	0.063	0.000	0.175
Control Mean	0.803	0.347	0.160	0.327	0.034	0.816

Note: In this table, we explore the characteristics of bidders across the experimental groups, six months after the intervention. We regress each characteristic on indicator variables for the bidding and information treatments and strata fixed effects. All regressions are estimated by OLS. Each column has 587 observations. *** p<0.01, ** p<0.05, * p<0.1.

Table 2B: Who Distributes Raskin Six Months After Intervention?

	Owns a truck and/or a boat	Avg Digit Span above median	Raw dice score above median	Years of education	Has personal savings account for business
	(1)	(2)	(3)	(4)	(5)
Info or Bidding	-0.039	-0.009	-0.021	0.233	0.036
	(0.026)	(0.059)	(0.059)	(0.325)	(0.055)
Bidding	0.022	0.002	0.023	-0.453	0.105*
	(0.028)	(0.062)	(0.062)	(0.345)	(0.058)
P-Value					
Bidding = Ctl	0.432	0.875	0.963	0.397	0.001
Control Mean	0.071	0.449	0.519	12.139	0.313

Note: In this table, we explore the characteristics of bidders across the experimental groups, six months after the intervention. We regress each characteristic on indicator variables for the bidding and information treatments and strata fixed effects. All regressions are estimated by OLS. Each column has 587 observations. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Raskin Distribution Process

	Bought Raskin	Price markup	Amount purchased	Satisfied with rice quality	Distance to purchase point (meters)	Time to purchase point (minutes)	HH paid for rice in advance	Satisfied with Raskin program
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Info or Bidding	-0.009	18.770	0.151	0.006	-1.080	0.441	0.013	-0.020*
	(0.02)	(24.07)	(0.23)	(0.01)	(14.37)	(0.27)	(0.02)	(0.01)
Bidding	0.021	-49.023**	-0.002	0.019*	7.754	-0.501*	-0.009	0.006
	(0.03)	(24.91)	(0.24)	(0.01)	(15.15)	(0.28)	(0.03)	(0.01)
P-Value								
Bidding = Ctl	0.55	0.09	0.39	0.00	0.55	0.75	0.82	0.06
Number of observations	6,860	5,886	6,858	6,533	6,194	6,247	6,394	6,782
Control Mean	0.76	652.39	5.76	0.51	190.96	5.94	0.43	0.59

Note: This table explores the effect of the treatments on the actual program functioning. All data come from the household endline survey that we conducted about six months after the intervention. We regress each outcome on indicator variables for the bidding and information treatments, the baseline value of the outcome, and strata fixed effects. All regressions are estimated by OLS and standard errors are clustered by the village. In Column 4, we do not control for baseline quality because we do not have this variable in the baseline survey. Columns 4 and 8 are categorical variables with 4 options on a scale of 0-1. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Endline Costs to Current Distributor

	Total transportation costs (Rp) (1)	Compensation to others (Rp) (2)	Total other costs (Rp) (3)	Total costs of Distribution (Rp) (4)
Info or Bidding	88,038*	121,875	40,716**	318,287
	(52,052)	(174,163)	(18,745)	(211,403)
Bidding	-101,616*	-94,256	-30,531	-317,960
	(54,924)	(179,950)	(19,678)	(219,985)
P-Value				
Bidding = Ctl	0.695	0.836	0.445	0.998
N	574	574	574	574
Control Mean	244,161	961,974	84,166	1,315,030

Note: This table explores the effect of the treatments on the program costs. All data come from the endline distributor survey that we conducted about six months after the intervention. We regress each outcome on indicator variables for the bidding and information treatments and strata fixed effects. All regressions are estimated by OLS with robust standard errors. If we have data for at least one cost variable, we replace missings with zeros for other cost categories. "Total costs of distribution" is the sum of Columns (1)- (3). The top one percentile of values for each cost are dropped. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Comparison of Winning and Losing Bids in Bidding Treatment Villages

	Winners		Losers		P-Value
	Mean	Std. Dev.	Mean	Std. Dev.	Losers = Winners
	(1)	(2)	(3)	(4)	(5)
Price markup (Rp/kg) promised by bidder	471.62	270.20	566.53	295.90	0.00***
Pay Before Receipt	0.44	0.50	0.36	0.48	0.04**
Pay During Receipt	0.39	0.49	0.47	0.50	0.03**
Pay After Receipt	0.19	0.40	0.20	0.40	0.74
Raskin is distributed at village level	0.27	0.44	0.32	0.47	0.15
Raskin is distributed at subvillage level	0.76	0.43	0.68	0.47	0.01**
Raskin is distributed at household level	0.03	0.17	0.04	0.20	0.37
Bidder offers credit	0.17	0.38	0.15	0.36	0.43

Note: This table reports on various dimensions of bidder's proposals, by winning and losing bids (403 bids total). All data come from the application forms. Column 5 provides the p-value of the difference in mean between losers and winners, clustered by village. *** p<0.01 ** p<0.05 * p<0.1

Table 6: Who was Selected in the Bidding Villages?

	1-by-1	Joint (Form)	Joint (All)
	(1)	(2)	(3)
Panel A: Proposal Characteristics			
Price markup (Rp/kg) promised by bidder	-0.010***	-0.009***	-0.007***
	(0.003)	(0.003)	(0.003)
Pay Before Receipt	-0.606	-0.614	-2.473
	(0.531)	(1.170)	(4.086)
Bidder offers credit	0.731	1.148	1.021
	(0.599)	(0.743)	(0.816)
Bidder lives in distribution village	-1.207*	-1.029	-0.792
	(0.643)	(1.054)	(1.179)
Bidder is Raskin distributor at time of bidding	0.531*	0.297	0.344
	(0.285)	(0.346)	(0.362)
Bidder is a trader	1.120**	2.303***	1.734***
	(0.527)	(0.714)	(0.576)
Bidder has means of transportation supportive of Raskin distribution	1.387***	1.295***	0.983
	(0.422)	(0.494)	(0.622)
Panel B: Individual Characteristics			
Distibutor/spouse is related to a village official	0.223		0.035
	(0.317)		(0.358)
Distibutor/spouse is/was village official	0.479		0.115
	(0.454)		(0.702)
Raw dice score above median	0.197		0.105
	(0.279)		(0.332)
Years of education of Distributor	0.093**		0.008
	(0.043)		(0.055)
Distributor has personal savings account that be used for business	0.849**		0.611
	(0.356)		(0.446)
Joint P-Value		0.000	0.010

Note: In this table, we explore the characteristics that are associated with winning, in villages where there were multiple bids. For the sample of bids, we regress a dummy variable for whether the applicant was won on proposal and applicant characteristics, as well as village fixed effects. We estimate all coefficients using a conditional logit and cluster the standard errors by village. In Column (1), we estimate the effect of each characteristic individually. In Column (2), we estimate the joint effect of all proposal characteristics, while we additionally control for all individual characteristics in Column (3). Proposal characteristics come from the application forms, while individual characteristics come from the distributor survey that we conducted. *** p<0.01, ** p<0.05, * p<0.1

Table 7: When Did Original Distributor Win and Continue Distributing?

	Where Was No Bidding Meeting Held, or Meeting Had No Bids?				Where Did Original Distributor Win?				Where is Original Distributor Still Distributing?			
	1-by-1	Joint (Household Only)	Joint (Form Only)	Joint (All)	1-by-1	Joint (Household Only)	Joint (Form Only)	Joint (All)	1-by-1	Joint (Household Only)	Joint (Form Only)	Joint (All)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Reported by Households in Baseline												
Avg Price Markup (Rp/kg)	-0.003***	-0.003**		-0.004***	-0.002***	-0.001**		-0.002**	-0.001**	-0.001		-0.001
	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)		(0.00)
HH Bought Raskin in Last 2 Months	0.369	0.517		0.358	1.467**	1.034		1.120	0.217	0.551		0.669
	(0.89)	(1.24)		(1.30)	(0.60)	(0.73)		(0.73)	(0.59)	(0.69)		(0.70)
Avg Amount of Raskin Purchased (kg)	0.024	-0.043		-0.041	0.062	0.002		-0.003	-0.030	-0.030		-0.033
	(0.07)	(0.09)		(0.10)	(0.07)	(0.06)		(0.06)	(0.05)	(0.06)		(0.06)
Avg Satisfaction with Program Quality	3.887*	1.408		0.599	4.519***	3.910**		4.162**	-0.010	-1.254		-1.342
(0-1 scale)	(2.24)	(2.99)		(2.98)	(1.68)	(1.83)		(1.87)	(1.55)	(1.58)		(1.63)
Avg Distance to Purchase Point (meters)	0.001	-0.001		-0.001	-0.001	-0.002*		-0.002*	0.000	0.000		0.000
	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)		(0.00)
HH purchased Raskin in advance	1.470***	1.103**		1.160**	1.067***	0.453		0.474	0.435	0.346		0.346
	(0.49)	(0.53)		(0.55)	(0.36)	(0.42)		(0.42)	(0.35)	(0.40)		(0.41)
Panel B: From Facilitation Forms												
Old Distributor Provides Credit if	-0.688		-0.613	0.125	-0.368		-0.433	0.058	-0.291		-0.374	-0.214
Recipient Cannot Afford Raskin	(0.65)		(0.68)	(0.74)	(0.37)		(0.38)	(0.42)	(0.37)		(0.38)	(0.41)
Costs of Rental Vehicle and/or Fuel to	-0.006		-0.006	-0.012	-0.000		0.001	0.005	0.002		0.003	0.005
Old Distributor	(0.01)		(0.01)	(0.01)	(0.00)		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)
Non-Transportation Costs to Old	-0.000		0.000	0.003	-0.001		-0.001	0.000	-0.001		-0.001	-0.000
Distributor	(0.00)		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)
Joint P-Value		0.00	0.60	0.01		0.00	0.54	0.01		0.44	0.53	0.64
Mean		0.13	0.14	0.14		0.59	0.59	0.60		0.57	0.58	0.57

Note: In this table, we explore what characteristics predict that the village with actually have bidders present at meeting, what characteristics predict that existing distributor will win (or that the committee will immediately throw out all the bids and return to the existing process), and what characteristics predict the continuation of the existing distributor's distribution. We regress a dummy for the existing distributor as the outcome of the bidding process on baseline characteristics from the household survey (Panel A) and from the baseline information forms on process (Panel B). All regression are estimated as a logit. The top 1% of transportation and other costs are dropped; costs are reported in Rp 10,000. *** p<0.01 ** p<0.05 * p<0.1

Table 8: Raskin Distribution Process, by Min Bids

	Bought Raskin	Price markup	Amount purchased	Satisfied with rice quality	Distance to purchase point (meters)	Time to purchase point (minutes)	HH paid for rice in advance	Satisfied with Raskin program
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Info or Bidding	-0.009	18.842	0.151	0.006	-0.999	0.441	0.013	-0.020*
	(0.02)	(24.06)	(0.23)	(0.01)	(14.38)	(0.27)	(0.02)	(0.01)
Open Bids	0.014	-23.645	-0.016	0.016	25.315	-0.430	0.011	0.001
	(0.03)	(29.94)	(0.28)	(0.01)	(18.11)	(0.33)	(0.03)	(0.01)
Min # Bids	0.027	-73.551***	0.012	0.022	-9.368	-0.569*	-0.028	0.011
	(0.03)	(26.51)	(0.27)	(0.01)	(16.62)	(0.31)	(0.03)	(0.01)
P-Value								
Open = Min	0.66	0.06	0.92	0.66	0.04	0.63	0.17	0.38
Open = Ctl	0.84	0.84	0.55	0.05	0.10	0.96	0.29	0.06
Min = Ctl	0.46	0.01	0.44	0.01	0.44	0.57	0.52	0.29
Number of observations	6,860	5,886	6,858	6,533	6,194	6,247	6,394	6,782
Control Mean	0.76	652.39	5.76	0.51	190.96	5.94	0.43	0.59

Note: This table explores the effect of the treatments on the actual program functioning. All data come from the household endline survey that we conducted about six months after the intervention. We regress each outcome on indicator variables for the bidding and information treatments, the baseline value of the outcome, and strata fixed effects, disaggregating the bidding effect by whether the village was randomized into the minimum number of bids requirement or whether it was left open. All regressions are estimated by OLS and standard errors are clustered by the village. In Column 4, we do not control for baseline quality because we do not have this variable in the baseline survey. Columns 4 and 8 are categorical variables with 4 options on a scale of 0-1. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Endline Costs to Current Distributor, By Min Bids

	Total transportation costs (Rp)	Compensation to others (Rp)	Total other costs (Rp)	Total costs of Distribution (Rp)
	(1)	(2)	(3)	(4)
Info or Bidding	87,943*	123,544	40,726**	320,069
	(52,102)	(174,286)	(18,740)	(211,657)
Open Bids	-124,126**	154,902	-13,578	-51,869
	(59,089)	(222,663)	(22,454)	(263,152)
Min # Bids	-79,174	-349,228*	-47,098**	-590,262***
	(62,837)	(180,668)	(21,563)	(223,414)
P-Value				
Open = Min	0.396	0.009	0.093	0.013
Open = Ctl	0.373	0.137	0.113	0.189
Min = Ctl	0.850	0.095	0.693	0.084
N	574	574	574	574
Control Mean	244,161	961,974	84,166	1,315,030

Note: This table explores the effect of the treatments on the program costs. All data come from the endline distributor survey that we conducted about six months after the intervention. In Panel A, we regress each outcome on indicator variables for the bidding and information treatments and strata fixed effects, disaggregating the bidding effect by whether the village was randomized into the minimum number of bids requirement or whether it was left open. All regressions are estimated by OLS with robust standard errors. If we have data for at least one cost variable, we replace missings with zeros for other cost categories. "Total costs of distribution" is the sum of Columns (1)- (3). The top one percentile of values for each cost are dropped. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Bid Markups

	Incumbent		Average		Minimum	
	All Villages	>1 Bidder	All Villages	>1 Bidder	All Villages	>1 Bidder
	(1)	(2)	(3)	(4)	(5)	(6)
Min Bids Treatment	10.435	3.360	-4.842	-13.886	-1.788	-0.996
	(46.351)	(58.060)	(37.216)	(41.760)	(32.012)	(38.558)
Baseline markup	0.563***	0.556***	0.563***	0.556***	0.566***	0.560***
	(0.074)	(0.088)	(0.069)	(0.076)	(0.053)	(0.060)
Constant	88.649*	108.682	129.450**	148.947**	57.352	60.637
	(53.424)	(68.749)	(51.027)	(58.434)	(39.338)	(47.031)
Observations	146	110	412	362	165	115
R-squared	0.436	0.423	0.405	0.397	0.487	0.489

Note: This table compares bid markups between minimum bids and open bids treatment villages. Columns (1)-(2) report results on bids from distribution incumbents (if they exist), columns (3)-(4) report average bids, and columns (5)-(6) report minimum bid in a village. Columns (1), (3), and (4) include all villages with at least one bid, while columns (2), (4), and (6) restrict to villages with multiple bids. Results from OLS with a dummy for missing baseline markup. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 11: Who was Selected in Bidding Villages, by the Minimum Number of Bids?

	1-by-1	Joint (Form)	Joint (All)
	(1)	(2)	(3)
Panel A: Proposal Characteristics			
Min # Bids * Promised Price Markup	0.002	0.005	-0.007
	(0.006)	(0.008)	(0.013)
Min # Bids * Pay Before Receipt	-1.188	-4.470*	-62.377***
	(1.338)	(2.619)	(5.065)
Min # Bids *Bidder Offers Credit	-2.016	-1.141	-33.758***
	(1.281)	(1.830)	(2.989)
Min # Bids * Bidder lives in village	12.896***	16.921***	8.911***
	(1.215)	(2.801)	(3.370)
Min # Bids * Bidder is distributor at time of bidding	0.830	1.006	1.191
	(0.577)	(1.023)	(1.236)
Min # Bids * Bidder is trader	1.031	2.630*	20.489***
	(1.054)	(1.539)	(2.061)
Min # Bids * Bidder has transportation	2.882**	2.936**	31.886***
	(1.193)	(1.434)	(1.767)
Panel B: Individual Characteristics			
Min # Bids * Distributor/spouse is related to a village official	0.723		1.661
	(0.654)		(1.109)
Min # Bids * Distributor/spouse is/was village official	1.503		6.112**
	(0.984)		(2.513)
Min # Bids * Raw dice score above median	-0.255		-0.611
	(0.568)		(0.838)
Min # Bids * Years of education of Distributor	0.059		0.110
	(0.088)		(0.186)
Min # Bids * Distributor has personal savings account	0.309		1.806
	(0.721)		(1.485)
Joint P-Value		0.000	0.000

Note: In this table, we explore the characteristics that are associated with winning in villages where we imposed the minimum number of bids requirement as compared to those that were left often, in villages where there were multiple bids. For the sample of bids, we regress a dummy variable for whether the applicant was won on proposal and applicant characteristics, an indicator for the minimum number of bids requirement, the interactions of the characteristics with the dummy variable for the minimum number of bids requirement and village fixed effects. We estimate all coefficients using a conditional logit and cluster the standard errors by village. In Column (1), we estimate the effect of each characteristic individually. In Column (2), we estimate the joint effect of all proposal characteristics, while we additionally control for all individual characteristics in Column (3). Proposal characteristics come from the application forms, while individual characteristics come from the distributor survey that we conducted. *** p<0.01, ** p<0.05, * p<0.1

Figure 1: Flow of Villages through Bidding Process

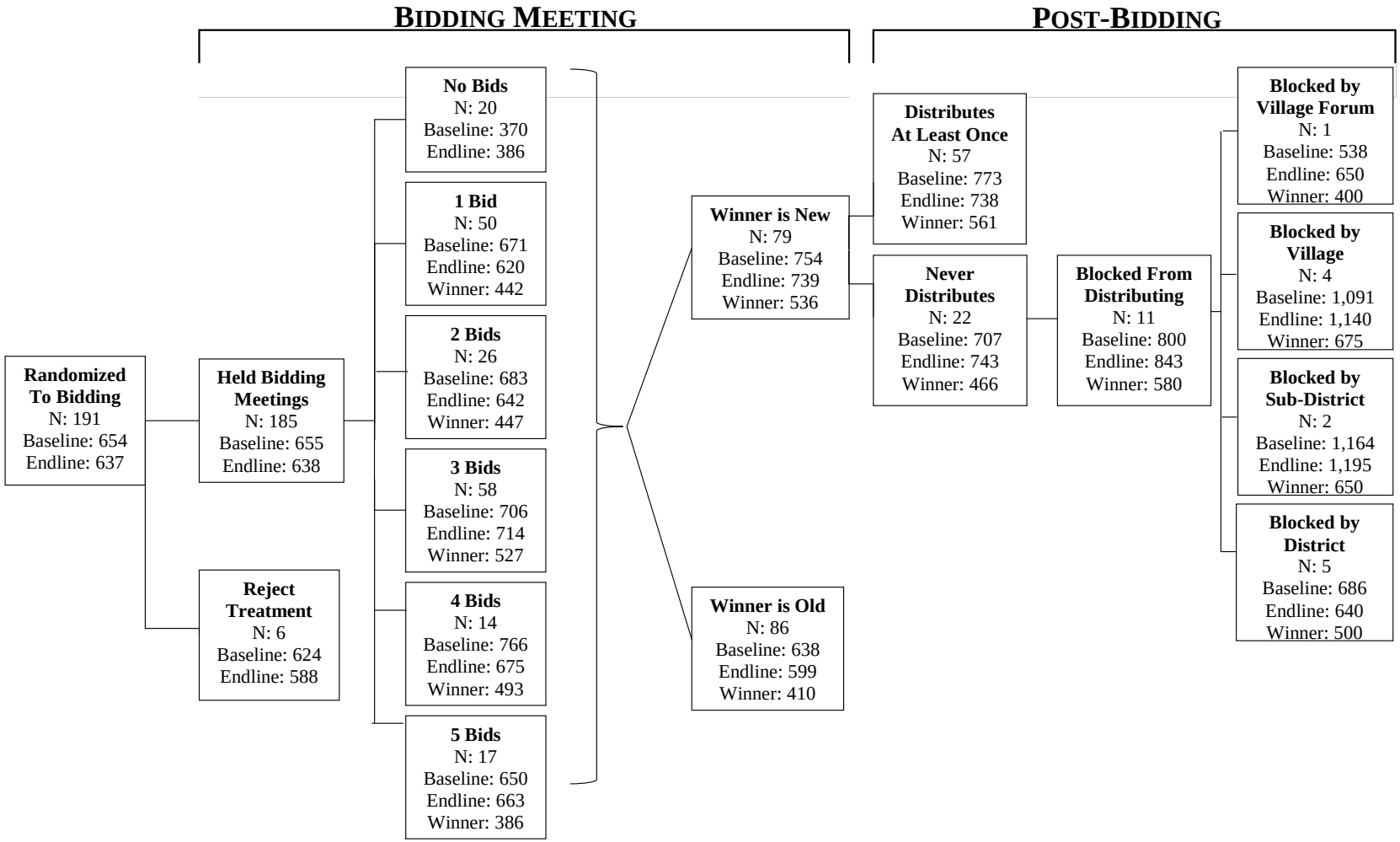


Figure 2: Non-parametric Estimations

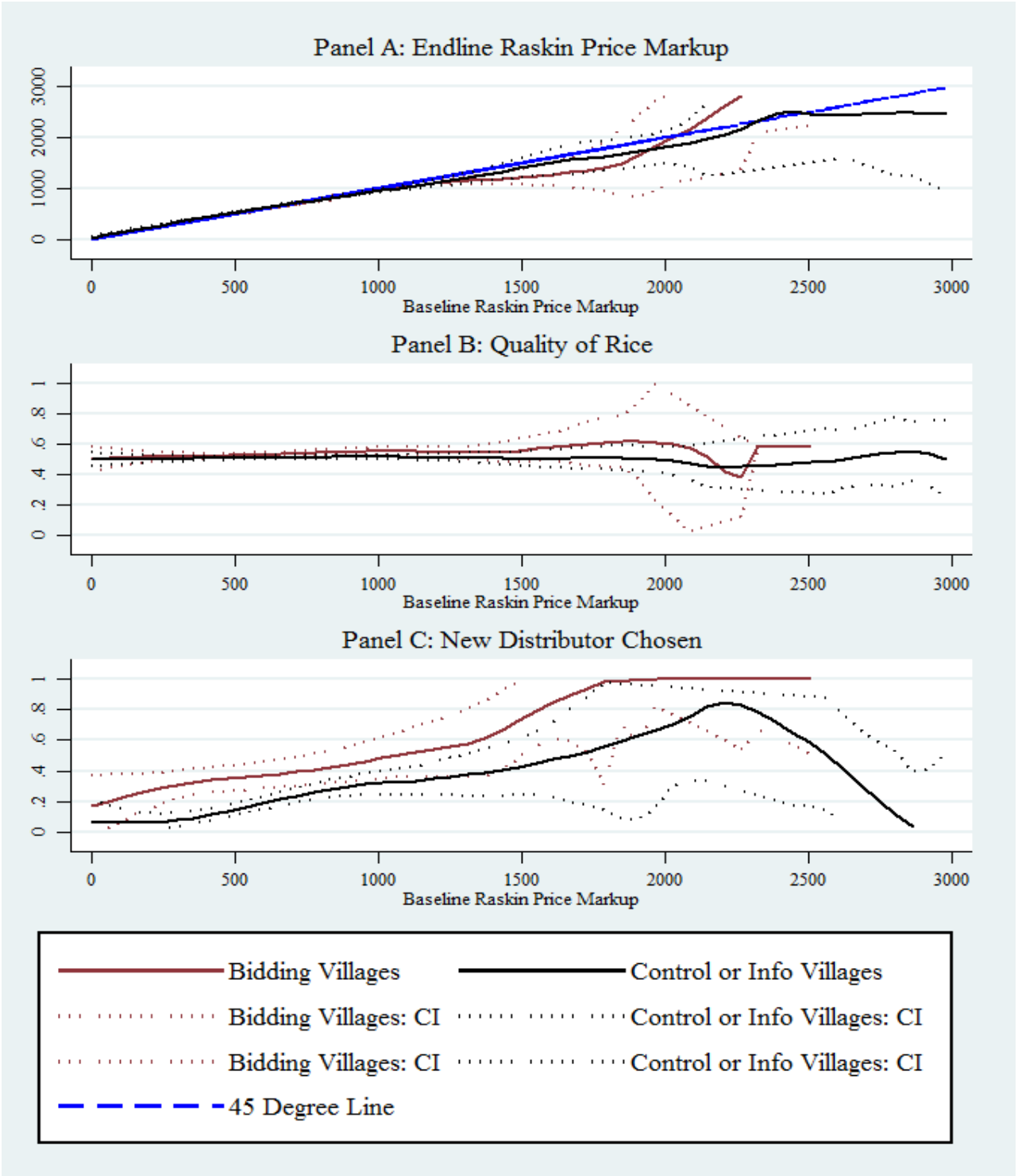
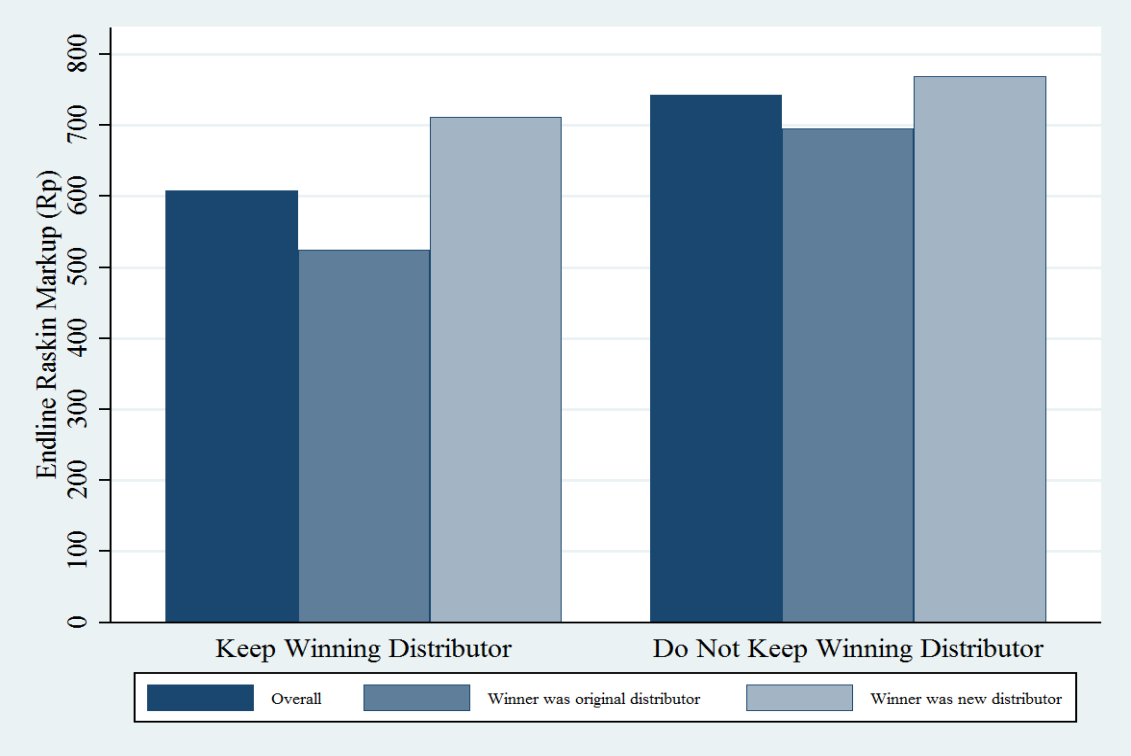


Figure 3: Endline Price Markup in Bidding Villages, By Results of Six-Month Re-Evaluation



villages that held bidding meetings with at least one bidder. This is divided by (1) the results of the six-month re-evaluation and (2) whether the winning distributor was the same person as the original distributor (pre-bidding).