The Role of Competition in Effective Outsourcing: Subsidized Food Distribution in Indonesia [1]

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Abstract

Should government service delivery be outsourced to the private sector? If so, how? We conduct the first randomized field experiment on these issues, spread across 572 Indonesian localities. We show that allowing for outsourcing the last mile of a subsidized food delivery program reduced operating costs without sacrificing quality. However, prices paid by citizens were lower only where we exogenously increased competition in the bidding process. Corrupt elites attempted to block reform, but high rents in these areas also increased entry, offsetting this effect. The results suggest that sufficient competition is needed to ensure citizens share the gains from outsourcing.

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I. INTRODUCTION

Should the state directly provide public services, or should it contract out service delivery to a private provider? In the seminal paper by Hart, Shleifer, and Vishny (1997, HSV henceforth), private contractors may potentially deliver better services—i.e. higher quality and/or a lower price—because governments can provide stronger incentives to private contractors than to their own employees. These stronger incentives may potentially lead to efficiency improvements, but come with a risk that a contractor may lower quality below socially efficient levels to cut their costs. Therefore, in settings where non-contractible quality dimensions may be important—prisons are HSV's example—public provision may be preferred.

Even if contracting out can potentially improve efficiency, there is no guarantee that the public will benefit for two distinct reasons. First, if there is limited competition for the contract, the gains may accrue entirely to the contractor rather than the public. While enacting regulations to ensure sufficient competition is the standard remedy to this problem, it is possible that competitive bidding processes can also exacerbate the quality problems discussed above or lead to undercutting to win contracts.[2] Indeed, Bajari, McMillan, and Tadelis (2009) argue that if the contracts are sufficiently complex, competitive bidding may perform worse than simply negotiating with a single firm. Moreover, while the standard view is that more bona fide bidders improves outcomes,[3] the regulatory imposition of a requirement that there be a minimum number of bidders may bring in non-serious bidders and muddy the choice process.

The second reason may be even more serious: All of the arguments so far about whether to outsource, and if so how, presuppose that the decisions are made by agents acting in the public interest. However, powerful vested interests may seek to undermine an outsourcing process. After all, beyond

[2] Spulber (1990) suggests that the penalties on private parties who renege on contract terms may be constrained by bankruptcy protection, leading to adverse selection problems. Procurement agencies can partially counteract this by focusing on firm reputations rather than simply awarding contracts to the lowest bidder, but this then introduces substantial subjectivity in the process that increases the risk of capture. Bajari and Tadelis (2001) emphasize the challenge of ensuring that the contract covers all possible contingencies ex-ante, resulting in a need for ex-post renegotiation, which in turn is anticipated by the contractors, creating moral hazard. Bajari, Houghton and Tadelis (2014) show with US data that such ex-post renegotiation is quantitatively important and anticipated by contractors.

[3] Bulow and Klemperer (1996) formalize the value of additional competition by showing that an auction with N+1 bidders always yield higher revenues in expectation than any possible set of negotiations with N providers.

efficiency gains, part of the point of outsourcing is to transfer rents from the government insiders to lay consumers, who are typically less powerful. As a result, there is a very legitimate concern that the incumbents will undermine or block outsourcing, especially in places where the rents were initially large (Krusell and Rios Rull 1996; Acemoglu and Robinson 2000).[4] On the other hand, competitive mechanisms have a dynamic of their own: it is possible that the same rents that the incumbents want to protect will attract entrants, and the rents will get competed away.

This paper is an attempt to assess the relevance of these competing forces and the overall benefits of outsourcing, in a context, rural Indonesia, where there is limited on the ground expertise in how to do procurement and, as we will see, corruption is rife. To this end, we carry out a randomized control trial of outsourcing across 572 localities in rural Indonesia. We first test whether contracting out leads to efficiency gains and whether these gains come at a cost of lower quality. We then explore whether the level of competitiveness of the bidding process affects the outcomes by randomly varying the degree of competition among areas selected for outsourcing. Finally, to examine the potentially conflicting effects of baseline rents, we examine how the outsourcing process, entry decisions, and ultimately the outcomes for the consumers differs across areas that we associate with high and low levels of rents.

We study these questions in the context of the last-mile delivery of rice in Raskin, Indonesia's largest targeted transfer program (with an annual budget of over US$1.5 billion). Under Raskin, eligible households receive a monthly allocation of subsidized rice. As is typical in most developing countries, even though this is a central government program, the process of transferring rice from central government warehouses to beneficiaries –the "last mile" – is administered locally by either the locality head or someone he designates as social welfare coordinator.[5]

[4] One can easily imagine this happening in a developing country context, where the goal is to outsource to eliminate corruption by a local official, but this also happens in developed ones: public sector unions, for example, vociferously oppose privatization, with substantial success (Hirsch 1995; McEntee 1987).

[5] This is often the case: India's work program (NREGA) is centrally dictated, but locally run, as is China's urban Di Bao program (Gustafsson and Quheng, 2011), which is among the world's largest transfer programs.

The distribution of rice in Raskin is plagued by several challenges in this "last mile." While the local distributors report paying the central government logistics agency an average of Rp. 1,617/kg of rice—a markup of only 1 percent over the officially-mandated copay of Rp. 1,600/kg—the average *household* pays a markup of 41 percent to the distributor, or Rp. 652/kg above the official copay. While some of the mark-up may cover real transport costs, it is also an opportunity for rent extraction. Large shares of rice never reach households at all (Olken 2006 estimates a lower bound of 18 percent missing; World Bank 2012 estimates about 50 percent). Moreover, citizens readily complain about the poor rice quality and inefficient distribution process: lazy or incompetent distributors may accept low quality rice from the government warehouse without protest; nefarious distributors may sell good rice from the government to a private trader and substitute inferior rice instead; and distributors without proper equipment (e.g. a proper sized truck) can cause distribution delays.

It is not clear that outsourcing would necessarily improve Raskin. As in HSV, many elements of the service delivery are difficult to contract on. Rice quality, for example, is hard to enforce (a common complaint is that the rice 'smells bad', for example, but this is subjective). And, for many of the quality and delay problems, it is hard for the villagers to know if the problem is due to the local distributor (e.g. who substituted inferior rice, or delayed picking it up), or if it originated in the central government warehouse (e.g. which gave out bad rice to begin with, or was out of stock). Moreover, real procurement challenges may exist: there may be inadequate competition for a job of this size from people competent to do it, those administering the procurement procedures may have limited experience or understanding of procurement procedures, and/or local leaders who get rents from status quo may try to sabotage the process or steer the bidding process to their favored bidders.

To examine these questions, in 191 randomly selected localities out of the 572, the central government introduced a procedure that allowed for competitive bidding for the right to distribute Raskin locally. Bids specified the markup to be charged, as well as other aspects of the distribution process (e.g. where rice would be distributed, when copays would be collected). The selection rule used to choose among bidders was not imposed externally, but instead a small, local committee examined the bids and

selected the winner. The incumbent local government distributor was also given the option to bid, providing the committee with the option to keep the status quo. In short, this created a process that allowed citizens to compete with the government provider for the job.

The bidding process by its very nature increases transparency since people need information on how the current process works in order to decide whether and how much to bid. Thus, we also randomly assigned an additional 96 localities (out of the 572) to have the same set of meetings to describe the current processes, but not the actual bidding. This information-only treatment serves as a placebo comparison group that allows us to disentangle whether any observed effects are driven by allowing for private distributors to enter or simply arise from increased transparency.

To isolate the role of competition in the outsourcing process, in 96 randomly selected localities of the 191 that were assigned to the bidding process, we instituted an 'enhanced competition' treatment encouraging a minimum of 3 bids: if at least 3 bids were not received by the end of the tendering process, the process was extended by 10 days. This treatment increased the number of bidders by about 30 percent, from 2.14 in localities without the 'enhanced competition' treatment to 2.74 in those with. This allowed us to understand whether outsourcing was more or less effective under an exogenous increase in competition.

Overall, we find that offering localities the opportunity to privatize led to increases in efficiency with no detectable declines in quality. However, encouraging sufficient competition was critical to ensuring that the efficiency gains were translated into lower markups. Specifically, in areas without the 'enhanced competition' treatment, we find distributors reporting transportation costs 37 percent lower than in the information placebo – yet, we find no statistically significant declines in the overall costs of distribution or in markups, as measured by the actual prices households pay for the rice. In contrast, in the areas with the 'enhanced competition' treatment, markups *fall* by 11 percent. Consistent with this, we find a 36 percent reduction in overall distribution costs, driven by a reduction in both transport costs and 'compensation' paid by the distributors. We find no declines on other dimensions in either bidding treatment: 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, and rice quality may have even improved. In short, outsourcing has the potential to improve outcomes, but only with sufficient competition in procurement does the public share in its gains.

We then investigate whether capture by elites was an important factor limiting the impact of the program. As the municipal head (or someone he designates) is the incumbent supplier, he may put road-blocks in the contracting if he obtains substantial rents from the process. Local officials could do this *ex-ante* by preventing the bidding process from occurring at all or by discouraging people from bidding, thus limiting or eliminating competition in the process. Or, they could allow a competitive bidding process to occur but block the winning bidder *ex-post* from actually assuming responsibility for distribution.

We test whether this type of blocking behavior is higher in cases when the incumbent was enjoying higher rents from the program. While it is challenging to measure the rents directly, two pieces of evidence suggest that higher baseline Raskin prices are consistent with larger rents, and not just with higher transportation or other costs. First, high baseline markups are strongly correlated to households' perceptions of the level of corruption of the municipal head and the incumbent Raskin distributor. Second, this appears to be more than just dissatisfaction with being charged a high price. Following Fischbacher and Follmi-Heusi (2013) and Hanna and Wang (2015), we elicit an experimental measure of dishonesty from the distributors: we gave each of them a die, asked them to privately roll it 42 times and then report the outcomes in order to receive a payment that was a multiple of the points rolled. In areas where the baseline markup was higher, baseline distributors reported higher than median dice points, which is indicative of cheating on the task.

On net, we find evidence of two offsetting effects from a high baseline price. On the one hand, high baseline prices see more entry from private sector bidders (consistent with an upward sloping supply curve) and fewer incumbents winning, even conditional on the outcomes from the dishonesty task. On the other hand, there is some evidence that corrupt elites tried to block the process to protect their rents: localities in which the incumbent distributor scored highly on the dice-based cheating task (which is more likely in area with high baseline rents) are more likely to have the bidding process fail (either because it

was blocked or because nobody bid), and conditional on the bidding process actually occurring, are more likely to choose the incumbent distributor. These results suggest that the presence of high rents leads to two partially offsetting effects: greater competition and more demand from the community to switch, but also entrenched elites fighting harder to protect their rents. On net, we show that the outsourcing gains were indeed highest in areas with high baseline rents – but that the pushback from local elites may be a reason why the effects were not quantitatively larger.

In short, giving localities the option to contract out delivery of government services works, but only when there is sufficient competition among potential providers. This paper builds on several literatures, notably the literature on outsourcing government services and that on the role of competition in procurement. With respect to outsourcing, Levin and Tadelis (2010) focus on which services are likely to be privatized and where, focusing on the tradeoffs between contracting costs, the potential efficiency gains from outsourcing, and the potential political reasons not to privatize. Our results on the tension between protecting rents and efficiency gains from protecting rents echo these findings.

There is an extensive theoretical literature on the role of competition in procurement auctions.[6] One important point from this literature is that while there are many situations where competition reduces rents going to the suppliers, this is by no means inevitable. For example, Bulow and Klemperer (2002) and Hong and Shum (2002) observe that in common value auctions the presence of more bidders can worsen the winner's curse and lead to more defensive bidding by all participants. Compte and Jehiel (2002) make a similar point about affiliated value auctions and more recently, Li and Zheng (2009) show that even in first price private value auctions the presence of a higher number of potential bidders can increase the rents going to the suppliers when there is endogenous costly entry because a higher number of potential bidders can reduce the probability that any given bidder will enter by so much that the total number of actual bidders goes down.

[6] Bajari, Houghton, and Tadelis (2014) suggest that there may be over-emphasis on competition, and not enough attention paid to important practical issues like ex post renegotiation.

It is plausible that our setting has both private values and common values. Each potential supplier knows more about his or her own cost of time and ability to transport rice than the rest of the population. On the other hand, the incumbent probably knows more about just how much work it is to distribute rice than the potential entrants, and this information is relevant for all of them in assessing their costs. Moreover, entry was clearly endogenous and somewhat costly; there was in fact a great deal of uncertainty about whether in fact potential suppliers other than the incumbent would show up for the job since most people who have the equipment to deliver rice (a truck, etc.) are also busy. In this sense all the caveats to the intuitive beneficial effects of competition mentioned above are potentially relevant for us.

Our approach to assessing the benefits of competition is based on a field experiment. We randomly assign the level of competition and observe what it does to rice distribution in terms of the price, quality, quantity, timeliness, etc. To the best of our knowledge this is first time that a field experiment was carried to answer this very basic question—the closest to our work in the experimental literature is Busso and Galiani (2014), who study the impact on prices of randomly selecting villages to open an additional retail store.

By contrast, Hong and Shum (2002) and Li and Zheng (2009), who also empirically examine the effect of competition in an auction setting, use the observational variation in competition across different auctions combined with a structural model and the assumption of equilibrium behavior to identify the effect. Interestingly our results suggest that the increase from mostly 2 bidders to mostly 3 bidders reduces the markup. By contrast Hong and Shum find going from 2 to 3 bidders actually increases rents going to bidders in many of the auctions they study; Li and Zheng, on the other hand, find negative effects of competition only when there are more than 3 bidders.

The remainder of the paper proceeds as follows. Section II describes the setting and research design. Section III explores the bidding process impact under both regular and enhanced competition. Section IV explores the degree to which capture by vested interests reduced the impact of outsourcing. 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 17.5 million low-income households to purchase 15 kg of rice per month at a co-pay of Rp. 1,600 per kg (US$0.15), or about one-fifth of the market price. The intended subsidy is substantial, about 4 percent of a beneficiary households' monthly consumption. It is Indonesia's largest permanent, targeted social assistance program, with an annual budget of over US$1.5 billion intended to distribute 3.41 million tons of rice each year (Indonesian Budget 2012).

Although it is a national program, much of the day-to-day logistics for the "last mile" delivery to beneficiaries are handled at the local level, by local governments known as *kelurahan* in urban areas and *desa* (village) in rural areas (we refer to both as "localities"). The central governmental logistics agency procures the rice and delivers it to its warehouses located (typically) in district capitals. Locality governments are responsible for picking up their allotment of rice --on average, 5,550 kg of rice each month to be distributed to about 375 households – from the a central distribution point (either the warehouse itself or a central point located in the sub-district capital), located, on average, about 7 kilometers away. The locality head, known as the *lurah* in *kelurahan* and as the *kepala desa* in *desa* (hereafter, "village head" for simplicity), typically appoints someone in the local government to run the distribution, usually either himself or someone he designates as social welfare coordinator.[7]

While picking up the rice at the warehouse, the local leader has to remit the copayment for the rice to the central government. Once they transport the rice back to their locality, there is substantial heterogeneity in where they distribute it—at the village head's office, at the homes of hamlet or neighborhood heads, or even directly to beneficiaries' houses. Local governments are not only

[7] The village head is an appointed civil servant in urban *kelurahan* with a civil servant salary and an elected private citizen in rural *desa*. During the period of our study, *kepala desa* were largely compensated in the form of usufruct rights over village lands (in Java: *tanah bengkok*).

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.[8]

In practice, Raskin faces a number of challenges, many of which occur in the last mile of service delivery. Rice may go missing at all stages in the distribution chain—from the central government to the sub-district distribution point to within hamlets. Evidence suggests, however, that many of the issues with missing rice crop up in the last mile of service delivery: While only 1 percent of Raskin distributors in the sample report receiving less than the full village quota in the last month from the government logistics agency, household purchases reveal that a substantial share of the quota never reaches households at all (Olken 2006 estimates that at least 18 percent of rice goes missing; World Bank 2012 estimates around 50 percent). Moreover, the rice that does arrive may be given to ineligible households rather than the eligible ones. On top of this, as shown in Appendix Table 1, households often have to pay a higher copay price (Rp. 660 per kg, or about a 40 percent markup) than the central government intends.[9] All of these factors reduce the value of the intended transfer.

It is important to note that these facts do not necessary imply malfeasance: local governments may be diverting rice to deserving, but ineligible households, or they may charge a higher copay for legitimate reasons, for example, to cover the transportation costs of distributing the rice. However, the distributors in our control group report transport costs that only account for about 12.4 percent of the price markup reported by households. Thus, it is likely that much of the higher price and missing rice is lost through corruption.

Beneficiaries also complain that quality of rice is low, with 93 percent of eligible households reporting that the quality of rice in the market is higher than that of Raskin. Quality problems, such as

[8] There is regional heterogeneity in these costs. In some areas, district governments help subsidize these transport costs; in other areas, the government logistics agency may deliver the rice directly to the village. Even when the government logistics agency delivers directly to the village, the local government is still responsible for distributing the rice to households, collecting their copayments, and remitting them back to the government logistics agency.
[9] There is much heterogeneity in the markup (Appendix Figure 1), with few households buying at the official rate.

mold and pests, which households may only discover later, can render rice inedible.[10] These types of problems can reflect issues in the national procurement of rice or in warehouse storage, but it also reflects poor management and rent-seeking at the local level.[11] For example, poor quality rice can indicate that local distributors accepted bad rice from the government warehouse without protest or that they waited too long before picking it up. Anecdotally, people complain that Raskin rice is often crushed and mixed with small stones, which is one way corrupt local officials disguise the weight of sold rice, or that nefarious officials sell official Raskin rice to private traders and replace it with lower quality rice.

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 across Indonesia—specifically, on and off Java—in order to capture important heterogeneity in culture and institutions (Dearden and Ravallion, 1988). To further capture heterogeneity across institutions, we ensured that the sample consisted of about 40 percent urban and 60 percent rural locations. Within these districts, we had originally randomly sampled 600 locations. Prior to conducting the randomization, we dropped 28 localities that were deemed too unsafe to send survey teams. Thus, the final sample comprised 572 localities.[12]

[10] Among households in the control group that purchased Raskin in the past 2 months, about 54 percent report issues with quality overall, including mold, pests, smell, discoloration, and brokenness.

[11] Corresponding to the idea that many issues with rice quality stem from local distribution factors, 85 percent of the variation in rice quality reported by households in control villages is from within-sub-district variation rather than from between-sub-district variation. If the quality issues were caused solely by higher level distribution problems, then we would expect quality problems to be similar across areas that receive rice from the same warehouse.

[12] 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 (see Alatas et al. (2012) and Alatas et al. (forthcoming). We also conducted a separate Raskin experiment on transparency (see Banerjee et al 2016). 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.

C. Experimental Design

Stratifying by geographic location and the previous experiments, we randomly assigned the 572 locations to one of three treatment assignments—bidding, bidding with enhanced competition, and information-only—or to a control group, as follows:

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

Bidding: We randomly assigned 191 localities to a process where private individuals or firms could bid for the right to become the official Raskin distributor, i.e. to purchase the rice from the national logistics agency at the distribution point, transport it to the locality, and sell the Raskin rice to households. The bidding process proceeded as follows: a facilitator from the district would arrive in the locality, accompanied by an official letter from the central government, to explain to the village head that the location had been selected to have a procurement process for Raskin distribution. The village head 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 locality—could bid for the right to distribute Raskin by submitting a bidding form within 10 days. The bidding form was a standard one that was provided to the local government, which included, but was not limited to, the price that the prospective bidder would charge citizens, the process (e.g. where the rice would be distributed, whether the households would have to pay upfront), and the bidder's qualifications (e.g. access to credit, owning a truck). The central government insisted that households should receive their full allotment of rice, 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 when they submitted and the bids remained sealed until the bidding meeting. Individuals were told that the winner would have the right to

distribute Raskin for 6 months, with another meeting held at that time in which the committee would decide whether to continue with him, revert to the previous 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 to monitor its outcomes. The committee included members of the independent local monitoring committee (the *Lembaga Pemberdayaan Masyarakat*, Agency for Community Empowerment, "LPM") charged with overseeing community development and improving the quality of local public services, neighborhood heads, informal community leaders, and Raskin beneficiaries. To avoid conflicts of interest, current distributors were excluded from being on this committee.

Note several important details. First, in addition to spreading information about the bidding process via word of mouth, informational posters were strategically posted in the locality and the sub-district capital in order to advertise both inside and outside the locality. Second, the current distributor—generally, the village head or another local government staff member—was also allowed to bid. In fact, the current distributor bid in 66 percent of the cases where there was at least one bid.

After the window to submit bids, but before 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 the 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 bidding committee at a public meeting.[13] If more than five bids were submitted (which only happened in 7 locations), only the best five were to be presented at the meeting to ensure sufficient time for discussion. Although the facilitator took notes at the meeting, their participation

[13] Note that we also randomized two aspects of the committee formation and function. First, we randomized whether we required that a third of the committee 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 4 and 5, respectively, provide results examining these changes.

was minimal and a committee representative led the meeting. During each presentation, the key proposal information was written on a large notepad to facilitate discussion. Bidders were allowed to improve upon their bids during the meeting in response to questions or in response to other bids.[14]

After the presentations, the committee members privately scored each proposal according to their criteria and summed the scores to determine the winner. Each bid was scored with a 1-10 qualitative score on each dimension, 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, 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 locality about six months later. At this time, 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 to extend the new winner's time as Raskin distributor (if there was one), to choose a new distributor either using the same bidding process or another method of their choosing, or to revert back to the old process.

Bidding with Enhanced Competition: In half the villages assigned to bidding, we introduced an additional rule designed to encourage additional competition in the bidding process. Specifically, it was announced at the start that at least three bids must be received before the bidding meeting took place (no such requirement was given in the other bidding locations). If three bids were not submitted by the deadline, the bidding period was extended by 10 days to continue advertising the procurement process. If, after the extension, there were still not enough bids, the process continued with the realized number of

[14] The fact that bidders were allowed to improve upon their bids means that this process is more complicated than a standard, sealed-bid auction, and has elements of negotiation as well. Bajari, McMillan, and Tadelis (2009) discuss the costs and benefits of allowing elements of negotiation into a procurement process rather than focusing on a strict auction, and argue that these types of iterative discussions facilitate the exchange of information may be important when quality is a concern.

bidders. The extension of the bidding window allowed for more time for the information to reach possible Raskin distributors in the locality and to prepare bids.

In this context, three bidders versus two bidders can potentially change the overall composition of bidders in important ways. When there are only two bidders and one of the bidders is the potentially inefficient incumbent government distributor, then the challenger needs only to beat the government distributor's price. When there are three bidders, however, then the second best bidder may be another "efficient" (i.e. non-government) bidder, potentially putting greater pressure on lowering prices. This treatment thus served as a randomized increase in the number of bidders, though it did not necessarily require three bidders if indeed three bidders could not be found.

Information-Only: The bidding process naturally provides greater transparency: one must provide information about the distribution process, so that potential bidders can decide whether to participate and, if so, can prepare realistic bids. But, the act of simply being forced to publicly itemize costs might lead distributors to lower markups if they could not provide adequate justification for their costs or if citizens notice a discrepancy between reported costs and price markups. Thus, any observed effects could be driven by greater transparency.

To control for the information effects of the bidding treatment, we also randomly selected 96 locations for an information-only treatment, where a community facilitator coordinated with the village head to set up the organizational meeting. This meeting exactly mimicked the meetings held in the bidding villages, following the same procedures for setting up a committee comprised of LPM members, informal community leaders, neighborhood heads, and Raskin beneficiaries tasked with discussing and monitoring the distribution process. The Raskin distributor was asked to present the same specific information as in bidding villages, including distribution costs, distribution location and processes, and retail prices for households. A follow-up 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

information provision, but did not include the bidding.[15] We therefore use this treatment as a comparison group for the bidding treatment to isolate the pure effect of the potential to outsource from increased transparency.[16]

D. Randomization Design, Timing, and Data

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

The timeline was as follows (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; only 17 calls were ever received. In January-February 2014, after the endline survey was completed in that sub-district, the facilitators returned to hold the follow-up meetings.

E. Data Collection

An established, independent survey organization (SurveyMeter) conducted the surveys. 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 sub-unit (RW) within the locality. In total, across both survey waves, we randomly sampled between 15 and 19 households in each locality, for a total of 10,277 households.[17] We surveyed the households on their background and their Raskin experiences. At this time, we additionally interviewed the village head.

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[15] 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.

[16] A potential concern is that a bidding meeting might be more interesting, and hence draw more attention, than an information-only meeting. Appendix Table 21 compares what happened at the information only and bidding meetings, and shows that while the meetings were 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 participants (28.5 vs 21.7) and slightly more questions/comments (6.5 questions in information meetings vs. 4.3 in bidding meetings).

[17] 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. 2016).

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

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

As part of this distributor survey, we also conducted a modified version of the dice-based dishonesty 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 (US$0.01) for each die point that they record. The idea is that any given person can cheat without being detected, but that one can detect cheating statistically by looking for scores that are higher than would be predicted by chance. Hanna and Wang (2015) show that this task is correlated with real-world corruption: they show that a high score is correlated with fraudulent absenteeism by government nurses in India.

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

F. Experimental Validity

Appendix Table 6A provides a check on the randomization of locations to the control, bidding and information treatments. 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 0.7, 0.50, and 0.20 in Columns 5-7, respectively. In Appendix Table 6B, we also conduct a randomization check on enhanced competition versus the open bidding process. Again, the two treatment groups appear balanced with none of the individual differences statistically significant at the 10 percent level and with a p-value for a joint significance test of 0.68.

G. Descriptive Statistics on the Bidding Process

In Figure 1, we document the flow of the 191 bidding locations through the process. We also provide the average Raskin price markup reported in both the baseline and endline household surveys at each step.

The flowchart highlights two key descriptive facts: First, almost all – 185 out of 191 – of the locations randomized to the bidding treatment conducted the procurement processes, though 20 received no bids and reverted back to the status quo. However, of the 165 treatment locations that received at least 1 bid, 86 (52 percent) selected the original distributor.

Second, the baseline markup seems to be an important predictor of the bidding process outcomes. 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. 766. New individuals won in places with an average baseline markup of Rp. 754, while incumbents won in places with Rp. 638. However, there is some evidence that local leaders ex-post block when there are greater rents: the 6 locations where the winner was blocked from distributing by the locality head or sub-district had a baseline price almost double the average. The fact that the baseline price 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 high rents. These descriptive statistics are suggestive but do not control for regional differences, other characteristics, etc., 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 locations randomly assigned either to the regular or enhanced competition bidding process. In Column 7, we present the p-value of the difference of means across the regular bidding process and enhanced competition.

Citizens did bid for the distribution rights (Panel A). On average, we observed 2.43 bids placed, with 2.16 passing the initial screening process by the local committee and thus considered at the meeting. However, the process may have been dominated by the opinions of a few, namely the elites (Panel B of Table 1).[18] On average, about 22 individuals attended the bidding meetings (the average locality size is 1,299 households). Local leaders comprised a fair share of the participants, with about 9 of them attending, on average. About 8 of the meeting participants claimed to be Raskin beneficiaries. The facilitators reported that relatively few people spoke at the meetings, with no discussion from the crowd in 9 percent of the meetings and with less than 10 percent of attendees talking at 43 percent of them (Panel C). In only 3 percent of the meetings did they report that more than half of the crowd participated.

The enhanced competition treatment 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 locations randomized to the enhanced competition treatment as opposed to 2.14 without the requirement, about a 30 percent increase; this difference is significant with a p-value of 0.01. One worry is that to fulfill the requirement, we would observe more "unrealistic" or "ghost" bids, but this was not the case: in the enhanced competition areas, we observe an increase in bids that pass the screen (2.44 relative to 1.88; p-value 0.01). There were more meetings with no discussion (15 percent in the minimum bid versus 3 percent otherwise), but this may have been due to the fact that there were more proposals to present. On

[18] In Appendix Figures 3, we present the reasons reported by the winners and losers, respectively, that they believe they won or lost the bidding process. 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.

net, a new distributor won in 45 percent of the enhanced competition areas as opposed to 51 percent in the regular; this difference, however, is not statistically significant (p-value 0.49).

III. CONTRACTING OUT AND THE LEVEL OF COMPETITION

A. Who is in charge of distribution?

In Table 2, we examine whether the Raskin distributor characteristics changed as a result of the bidding treatment. We estimate two regressions. First, to estimate the overall effect of bidding (pooling the regular treatment with the bidding treatment with enhanced competition), we estimate:

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

where *i* represents a study location and *s* represents one of our geographic strata. The dependent variable y_{is} in each column is a different characteristic of the distributor at endline (approximately six months after the intervention); this specification, thus, captures the net intent-to-treat effect of the treatment, including the fact that bidding may not always have occurred, that distributors may naturally change over time, and that the winning bidders may be blocked, resign, or be otherwise forced out. We include an indicator variable for whether there was 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 locations differ from those that received the information-only (i.e. placebo) treatment and is the key coefficient of interest. We also report the p-value of the difference of the bidding treatment against the pure control group (i.e. a test of $\gamma + \beta = 0$) in the row labeled "Bidding = Ctl").

Second, we separately estimate the effect of bidding with and without enhanced competition:

$$y_{is} = \alpha_s + \beta(BIDDING\ or\ INFO)_{is} + \gamma REGULAR\_BIDDING + \omega ENHANCED\_BIDDING + \epsilon_{is}$$

In this regression, γ estimates the impact of the regular bidding procedure relative to the information placebo, and ω estimates the impact of the bidding procedure with the enhanced competition treatment (i.e. where a minimum of three bids were encouraged), relative to the information placebo. In this

specification, we also report p-values of the difference between regular and enhanced bidding (i.e. a p-value of the test that $\gamma = \omega$) and the p-value of bidding with enhanced competition vs. pure control (i.e. a p-value of the test of $\omega + \beta = 0$).

Table 2 shows that six months after the bidding process, locations that were assigned to the bidding treatment were substantially more likely to have a new distributor relative to the other groups (Table 2, Panel A, Column 1). Specifically, the distributor in the bidding areas was 17 percentage points—or 21 percent—less likely to have had Raskin responsibilities prior to the intervention than the information-only group (Column 1), and about 20 percentage points more likely relative to the pure controls. A change was slightly more likely in the enhanced competition treatment compared to the regular bidding treatment – 20 compared with 14 percentage points, respectively (Table 2, Panel B, Column 1)– though this difference is not statistically significant (p-value 0.297).

The remaining columns explore the distributor's identity. In the pure control group, almost 85 percent of the distributors were a local official, hamlet official, or related to one (Columns 2, 3, and 4).[19] In the bidding group compared to the pure control group, local 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 areas, relative to both the information-only and pure control group (Column 5). Traders are likely to have skills and assets relevant to distributing Raskin, though they are perhaps more likely to be a part of the "elites." In short, while the bidding treatment

[19] In assessing whether the distributor is related to a local official or hamlet official, we count whether the distributor himself or his spouse considers a local or hamlet officials to be a member of their household, their nuclear family (brother, sister, mother, father), or their "large" family (cousin, nephew, niece, uncle, or aunt).

changed the identity of those distributing Raskin, it largely redistributed the role within the existing local government elite. However, within the elite, it reallocated the job to people with the relevant experience as a trader. Both bidding treatments produced broadly similar results on these dimensions.

B. Impact on program outcomes

Did the bidding process change actual program outcomes and satisfaction? In Table 3, we focus on outcomes from the household survey data. We estimate the same equations as in Table 2 using OLS, but now cluster the standard errors to account for fact that the randomization was conducted by locality. We also control for the baseline value of the outcome variable in all regressions except rice quality in Column 4, for which we lack baseline data.[20]

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 locations where there was a new winner. This is because the very act of having to compete for the distribution rights may have changed the outcomes, even if the incumbent still won. Second, as 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 bought, we pool eligible and ineligible households. Thus, the regressions provide results for all citizens, regardless of eligibility status.[21]

As shown in Table 3 Panel A, overall, the bidding treatment led to a reduction in the Raskin copay 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 a 7.3 percent reduction in the markup charged (Column 2).

In Table 3 Panel B, we separately identify the price effect for the regular bidding treatment and that with enhanced competition. Here, we find quite stark results: we *only* see price reductions in localities with enhanced competition. Specifically, households in enhanced competition localities pay Rp.

[20] Appendix Table 7 replicates Table 3 omitting the baseline controls. The results are qualitatively similar.
[21] In Appendix Table 8A and 8B, 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).

74 less than in the information placebo, about an 11 percent reduction in markup; this reduction is also statistically different from the pure control group. Households in localities with regular bidding pay a statistically insignificant Rp. 23 less than the information placebo, and only Rp. 5 – less than 1 percent – less than the pure controls. These results suggest that competition helps achieve price reductions – the opportunity to outsource itself is not enough. We return to this finding in more detail below.

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 only to eligible households—so this was not a category in the application form for the bid and therefore not a criterion on which bidders were evaluated, even though correct distribution is important in practice. Put another way, since all distributors were in theory supposed to distribute all the rice, it was not possible for bidders to compete on this dimension. In any case, the overall quantity of rice bought did not change (Column 3).

The key concern articulated by the HSV theory is that, as a result of outsourcing, private distributors may shirk and reduce non-contractible dimensions of quality. In our case, a key dimension is rice quality. Distributors can increase quality by refusing to accept low quality deliveries from the warehouse or by stopping a practice of selling high quality rice on the market and substituting lower quality rice for Raskin. Quality is non-contractible in this context: measurement of quality is fairly subjective (i.e., does the rice smell bad?) and distributors can blame quality problems on the central government warehouse. Thus, we asked households to subjectively assess the rice quality (Column 4). We observe an increase in their assessments – about 3.7 percent higher compared to information-only (p-value 0.096) and about 4.9 percent higher than the pure control (p-value 0.005).[22] Interestingly, the quality improvements appear similar in both regular bidding and enhanced competition (Panel B), suggesting that – unlike the Bajari, McMillan, and Tadelis (2009) hypothesis – increased competition did not reduce quality.

[22] In fact, in the bidding locations, households reported that the rice had fewer stones, an act of malfeasance by distributors to make the rice appear heavier than it really is.

Looking at other dimensions of quality, such as physical distance to purchase point, time needed to get there (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 worsened to compensate for the price change. If anything, households 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 treatments (Column 8). Overall satisfaction actually fell in the information treatment as citizens learned more about how the process should really look, with no additional difference for just the bidding process.

Overall, we observe a decrease in the price markup, but only in areas with enhanced competition. We find no evidence of a decline in quality.

C. Impact on Distribution Costs

As HSV point out, the theory tells us that even when contracting out leads to efficiency gains, without sufficient competition, these efficiency gains may be captured by vendors rather than enjoyed by the public at large. To investigate these issues further, at the endline, we interviewed the distributor (whomever it was) and asked about their distribution costs. Note two aspects of the cost measures. First, they are self-reported; given the informal nature of the economy, one cannot track them through credit card or bank transactions. Nevertheless, they may shed light on how the distributers functioned. Second, the reported costs often *increase* in the information treatment relative to the pure control, likely because it forces distributors to better compute their actual costs (and because they may be constrained to make sure the costs add up to the total markup) 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 this transparency effect 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). These reductions seem roughly similar in both regular bidding and bidding with enhanced competition. This is consistent with the view suggested by HSV that contracting out government services can lead to efficiency improvements and the overall view

that privatization can improve performance (see Megginson and Netter 2001 for a review). To the extent that these transportation cost reductions represent an efficiency gain – perhaps because they select traders, who are more experienced at moving rice around -- it appears that the outsourcing treatment alone is enough to obtain these gains.

However, the enhanced competition treatment also led to a reduction in compensation payments (Column 2) and other costs (Column 3), whereas the regular bidding treatment without it did not. These differences between bidding with and without enhanced competition are both economically large and statistically significant (p-values of 0.009 and 0.093, respectively). The impact of enhanced competition on reducing compensation mirrors the reduction in prices induced by enhanced competition discussed above. One 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 additional competitive pressure, 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.

We find that most of the difference in prices from increased competition comes from differences in behavior of distributors ex-post, rather than coming from lower-priced bids being submitted ex-ante. While standard auction theory suggests that bidders in a sealed-price first-price auction should bid more aggressively if they expect more bidders (e.g. Milgrom and Weber 1982), we find no evidence of this in our context (Appendix Table 22). We do, however, find that bid committees choose differently among bidders when they have more choice. Specifically, with more choice, bidding committees appear to place more weight on factors like living in the locality or experience as a trader relative to price (see the Appendix for more details.) And, although the estimates are somewhat noisy, the data also suggest that the actual price paid is closer to the promised price in the enhanced competition treatment than in the regular bidding treatment (Appendix Table 24). This could be because the winners are more reliable, or perhaps the enhanced competition treatment allowed the village to discover more potential suppliers, leading to greater pressure on the chosen supplier to stick to their promise.

On net, the experimental results presented in this section point to the importance of sufficient ex-ante competition for ensuring the success of outsourcing: outsourcing without sufficient competition can yield efficiency improvements, but sufficient competition is needed to ensure that the gains are passed on to the government.

IV. THE ROLE OF EXISTING RENTS: THE RACE BETWEEN VESTED INTERESTS AND ENTRY

A. Cross-sectional evidence on blocking and entry

An important concern about outsourcing is that it can be often be blocked by political or vested interests seeking to protect their rents. This is not just an issue in developing countries: Levin and Tadelis (2010) hypothesize, for example, that blocking by public sector unions in developed countries may be one reason they find that there is less privatization in older cities, which may have a larger union presence. On the other hand, the very presence of these rents may also encourage entry from the private sector. As shown in the previous section, outsourcing is more effective when there is more competition, so the additional entry spurred on by these rents improves the effectiveness of outsourcing.

Our setting allows us to study these issues carefully, both because we can proxy for rents in the system by using cross-sectional variation in the baseline prices, and because we can directly measure likely corruption among incumbents using our experimental measures. To measure rents in the system, we focus on the Raskin copay price at baseline, as well as other program metrics. 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. To see if this is the case, Table 5 examines the correlation of the Raskin price at baseline (Columns 1 and 2) and the Raskin price at endline in our control areas (Column 3-5) various corruption metrics, controlling for local characteristics that proxy for actual transportation costs (e.g. distance to the sub-district, log population, and number of hamlets).[23] Higher Raskin prices are not only strongly

[23] Results are similar without controls; see Appendix Table 13.

correlated with citizen perceptions of corruption, but are also positively correlated with the distributors scoring higher than median on the experimental dice-based dishonesty task. This suggests that the baseline price may indeed capture not just operating costs, but also the amount of rents in the system.

Table 6 then examines whether failure of the process ex-ante or ex-post appears correlated with baseline prices and corruption of the incumbent Raskin distributor, as proxied by their score on the dice task. To the extent that failure of the process (e.g. no bidding meeting held) is positively correlated with baseline prices, this suggest blocking by elites seeking to protect vested interests; to the extent that it is negatively correlated with baseline prices, this suggests the system is responding appropriately, with villagers not bothering to outsource when the system is working well.

Table 6 begins by investigating whether privatization occurs or not– i.e. of the 191 locations randomized to bidding, in which types of areas did bidding actually occur? We regress a dummy variable that equals 1 if there was no bidding meeting or no bids at the meeting on local characteristics. Each cell in Column 1 comes from a separate regression; Columns 2 – 4 report the results from a single regression in each column.[24] Higher prices substantially predict the occurrence of a meeting with at least one bidder: a one standard deviation increase in baseline markup (Rp. 390) would increase the log-odds by 1.31 i.e. increasing the odds of having the meeting with at least one bidder by over 300 percent.

Part of the reason the bidding process was successfully held is that higher baseline rents attract more entrants. To examine this, Table 7 shows the relationship between the number of bids received and the baseline markup. The results show that a one-standard deviation increase in baseline markup increases the number of bidders by about 0.5.

In addition to the effect that entry in response to rents may make the process more likely to succeed, there may be other forces at play. For example, Table 6 shows that locations with low baseline satisfaction are more likely to hold a contested meeting, even conditional on price. On the other hand, Table 6 shows that locations where the baseline distributor had a high dishonesty score (measured by the dice task) were less likely to have a contested meeting. On net, the results suggest the presence of

[24] Appendix Table 15 shows that the Table 6 results are robust to OLS estimation.

offsetting effects: high baseline rents may encourage new entrants and increase demand for outsourcing, but corrupt incumbents may also seek to obstruct the process.

The remaining columns of Table 6 examine other points in the bidding process where vested interests might exercise some control. Columns 5-8 investigate the selection stage by examining the probability that the incumbent distributor was chosen as the winner, conditional on the bidding process occurring (i.e. conditional on it not being blocked in the first stage; results defined for all 191 treatment locations are available in Appendix Table 14). We find similar countervailing forces at play at the selection stage as well. The incumbent is less likely to be chosen when baseline prices were high, though the results are about a third of the magnitude as in the previous table. The incumbent is also more likely to be chosen when baseline household satisfaction is high.[25] However, this is also offset by the fact that dishonest incumbents (as measured by dice score) are more likely to win.

The final set of results in Table 6 examines whether the incumbent distributor is still distributing six months later, conditional on him *not* having won the bidding. This variable captures ex-post capture. Here, very little predicts action at this stage, suggesting that on average at least, most of the tussle over rents happens before and during the bidding, not after.

Tables 6 and 7 show countervailing forces at play during the bidding process: high rents leads to more entry, but corrupt officials are more likely to block the process, either by preventing the bidding from occurring or by selecting the incumbent from among the bids provided. The experimental estimates from Section 3 suggest that the additional entry induced by the high baseline rents should lead to more substantial price reductions. For example, the estimates from Table 7 imply that moving from the 10th to the 90th percentile in baseline price markup (i.e. from Rp. 129 to Rp. 1359) would lead to an additional 1.37 bidders, which is more than twice as large as the effect of the enhanced competition treatment.

[25] Appendix Table 16 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, such as the number of hamlets in the locality, log population, and distance to the sub-district (which is where the rice is often dropped off by the local government). Appendix Table 17 replicates Appendix Table 16, but for all treatment locations.

Applying the experimental estimates from Table 3 suggests that this additional entry would lead to an additional reduction in prices of about Rp 100/kg.

B. Quantile treatment effects of contracting out

To explore the net effects more directly, we examine whether, on net, the program was more effective at eliminating the very high markups. To examine this, Table 8 re-estimates the price effects in Table 8 as quantile treatment effects, for the 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} quantiles. Figure 2 displays this information graphically by plotting the CDFs of price for the enhanced competition treatment, the regular bidding treatment, and the control group (we combined the pure control and information treatment in the graph for ease of presentation, though they are separated in the table).

The key result is that the price reductions indeed occur by eliminating the very high rents. Table 8 shows almost no effects at quantiles up to about the median, but then much higher results – about Rp. 90 overall, and RP. 119 for the enhanced competition treatment – at the 90^{th} percentile of the distribution. This suggests that on net the treatment was most effective in more problematic areas, despite the fact that vested interests may have had more rents to protect.

The difference we estimate using the quantile treatment effects between the 10^{th} and 90^{th} percentile – about Rp. 80/kg overall, and about Rp. 110/kg for the enhanced competition treatment – is almost precisely what one would predict based on the additional entry induced by the difference in baseline prices. This suggests that the additional entry induced by the high prices may have been critical to the heterogeneous treatment effects we observe.

All told, the evidence presented suggests offsetting effects. On the one hand, we do see evidence that in areas with high rents, corrupt elites appear to have been working to protect their rents either ex-ante by preventing people from bidding or during the process by maneuvering to have themselves selected as the winner of the bidding. On the other hand, we see results consistent with upward sloping supply curves: the bidding was more likely to be held and there were more bidders in areas where there were more rents to begin with. And, as shown in Section 3, adding additional bidders seems to reduce

prices substantially. On net, the entry effect and demand effect dominates, and the opportunity to outsource had the largest effects in areas where baseline rents were highest.

V. CONCLUSION

In this paper, we examine whether allowing local governments to outsource the delivery to the private sector improves the distribution. Focusing on a subsidized food distribution program, we show that allowing localities the ability to outsource last-mile delivery of subsidized food reduced its price, without sacrificing other aspects of the distribution quality to "pay" for these price reductions (if anything, it also improved rice quality). The price declines appear to come from both from greater efficiency in the form of lower transport costs and lower rents.

Critically, we find that outsourcing by itself was insufficient: outsourcing with limited competition resulted in efficiency reductions, but no cost savings passed on to the public at large. Only when outsourcing was combined with provisions to increase competition did we see substantial reductions in markups charged to the public. While the change in the number of bidders between regular and enhanced competition was not enormous – from 2.14 to 2.74 – the results are consistent with other arguments in the oligopoly literature that moving from 2 to 3 competitors can result in substantial changes in prices (e.g. Bresnahan and Reiss 1991).

We also document a tension created by the presence of rents at baseline. When rents are high, incumbents may fight to block the outsourcing process. On the other hand, the high rents also attract entrants, so that when the process occurs, the results are more substantial. While in our setting the entry effect dominated, so that on net we found the largest effects of being offered the opportunity to outsource in the areas with high baseline rents, the power of vested interests to block may have muted the effect of outsourcing to some extent. More generally, this suggests while there can be gains from outsourcing, it is important to think about how we design these processes in contexts with strong vested interests.

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

	Overall		Regular Bids		Enhanced Comp.		P-Value
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Regular = Enhanced
	(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
Local 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 localities where a bidding meeting was held and then we disaggregate the data by whether the locality was randomly assigned to the minimum bid requirement (91 localities) or it was left open (93 localities). *** p<0.01 ** p<0.05 * p<0.1.

Table 2: Who Distributes Raskin Six Months After Intervention?

	In charge of any responsibilities before May 2013	Distributor/spouse is/was local official	Distributor/spouse is related to a local official	Distributor/spouse is/was or is related to hamlet official	Is a trader	Lives in locality
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Combined Effect of Outsourcing			
Info or Bidding	-0.035	-0.059	0.043	0.001	0.008	-0.027
	(0.048)	(0.050)	(0.044)	(0.054)	(0.027)	(0.042)
Bidding	-0.165***	-0.076	0.046	-0.002	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	0.970	0.000	0.175
Control Mean	0.803	0.347	0.160	0.350	0.034	0.816
			Panel B: Estimating Additional Effect of Competition			
Info or Bidding	-0.035	-0.059	0.043	0.001	0.008	-0.027
	(0.048)	(0.050)	(0.045)	(0.054)	(0.027)	(0.042)
Regular Bids	-0.135**	-0.110*	0.053	0.036	0.068**	-0.046
	(0.059)	(0.062)	(0.055)	(0.067)	(0.033)	(0.052)
Enhanced	-0.197***	-0.042	0.038	-0.041	0.074**	0.009
Competition	(0.059)	(0.062)	(0.055)	(0.067)	(0.033)	(0.052)
P-Value						
Regular = Enh	0.297	0.272	0.783	0.248	0.839	0.288
Regular = Ctl	0.001	0.001	0.033	0.499	0.005	0.085
Enh = Ctl	0.000	0.046	0.073	0.460	0.002	0.671
Control Mean	0.803	0.347	0.160	0.350	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. In Panel B, we disaggregate the bidding effect by whether the locality was randomized into the minimum number of bids requirement. All regressions are estimated by OLS. Each column in each panel 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)	Paid for rice in advance	Satisfied with Raskin program
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Combined Effect of Outsourcing								
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
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
Panel B: Estimating Additional Effect of Competition								
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)
Regular 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)
Enhanced	0.027	-73.551***	0.012	0.022	-9.368	-0.569*	-0.028	0.011
Competition	(0.03)	(26.51)	(0.27)	(0.01)	(16.62)	(0.31)	(0.03)	(0.01)
P-Value								
Regular = Enh	0.66	0.06	0.92	0.66	0.04	0.63	0.17	0.38
Regular = Ctl	0.84	0.84	0.55	0.05	0.10	0.96	0.29	0.06
Enh = Ctl	0.46	0.01	0.44	0.01	0.44	0.57	0.52	0.29
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. In Panel B, we disaggregate the bidding effect by whether the locality was randomized into the minimum number of bids requirement. All regressions are estimated by OLS and standard errors are clustered by the locality. 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

	Transportation costs	Compensation to others	Other costs	Total costs
	(1)	(2)	(3)	(4)
	Panel A: Combined Effect of Outsourcing			
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
Observations	574	574	574	574
Control Mean	244,161	961,974	84,166	1,315,030
	Panel B: Estimating Additional Effect of Competition			
Info or Bidding	87,943*	123,544	40,726**	320,069
	(52,102)	(174,286)	(18,740)	(211,657)
Regular Bids	-124,126**	154,902	-13,578	-51,869
	(59,089)	(222,663)	(22,454)	(263,152)
Enhanced	-79,174	-349,228*	-47,098**	-590,262***
Competition	(62,837)	(180,668)	(21,563)	(223,414)
P-Value				
Regular = Enh	0.396	0.009	0.093	0.013
Regular = Ctl	0.373	0.137	0.113	0.189
Enh = Ctl	0.850	0.095	0.693	0.084
Observations	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 (in Rp). 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. In Panel B, we disaggregate the bidding effect by whether the locality was randomized into the minimum number of bids requirement. 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" 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: Corruption on Locality Price Markup

	Baseline Raskin Markup		Endline Raskin Markup		
	(1)	(2)	(3)	(4)	(5)
Raskin Distributor's Dice Score Above Median	61.029*		81.821**		
	(31.315)		(40.940)		
Perception of Local Head's Corruption		598.602***		632.064***	
		(123.065)		(179.431)	
Perception of Raskin Distributor's Corruption					647.773***
					(210.777)
Observations	454	455	282	283	273

Note: In this table, we explore the relationship between corruption and Raskin price markup. In each column, we regress average price markup reported by households in a locality on a measure of corruption, controlling for baseline locality characteristics (number of hamlets, log number of households, distance to subdistrict). Our measures of corruption, by column, are: 1. Dummy for baseline distributor's dice score above median; 2. Average perception of locality head's corruption at baseline; 3. Dummy for endline distributor's dice score above median; 4. Average perception of locality head's corruption at endline; 5. Average perception of Raskin distributor's corruption at endline. Measures of corruption consist of 4 categories, scaled 0-1, where 0 equals "no possibility of corruption" and 1 equals "very high possibility of corruption." Columns 1 and 2 examine baseline Raskin markup and include all localities; Columns 3, 4, and 5 examine endline Raskin price and include control localities only. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: When Did Original Distributor Win and Continue Distributing?

	Where Was No Bidding Meeting Held, or Meeting Had No Bids?				Where Did Original Distributor Win? (Conditional on Bidding Held with 1+ Bid)				Where is Original Distributor Still Distributing? (Conditional on Not Winning)			
	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)	-0.00299***	-0.00291**		-0.00337***	-0.00115**	-0.00111*		-0.00141	0.00017	-0.00011		0.00062
	(0.00089)	(0.00115)		(0.00122)	(0.00056)	(0.00065)		(0.00099)	(0.00079)	(0.00090)		(0.00144)
HH Bought Raskin in Last 2	0.369	0.517		0.285	1.598**	0.995		0.698	-0.206	0.392		1.493
Months	(0.89)	(1.24)		(1.46)	(0.65)	(0.80)		(1.05)	(0.87)	(1.13)		(1.79)
Avg Amount of Raskin	0.0240	-0.0429		-0.0615	0.0606	-0.0031		0.0223	-0.0814	-0.0694		-0.121
Purchased (kg)	(0.068)	(0.091)		(0.120)	(0.071)	(0.061)		(0.064)	(0.071)	(0.088)		(0.15)
Avg Satisfaction with Program	3.887*	1.408		0.766	4.082**	3.987**		5.981**	0.524	-0.042		0.405
Quality (0-1 scale)	(2.24)	(2.99)		(3.37)	(1.77)	(1.9402)		(2.35)	(2.27)	(2.7635)		(4.23)
Avg Distance to Purchase Point	0.00078	-0.00070		-0.00030	-0.00181*	-0.00234*		-0.00300**	0.00163	0.00182		0.00104
(meters)	(0.0013)	(0.0015)		(0.0016)	(0.0010)	(0.0012)		(0.0014)	(0.0013)	(0.0014)		(0.0018)
HH purchased Raskin in	1.47***	1.10**		1.20**	0.80**	0.24		-0.11	-1.10*	-1.00		-1.28
advance	(0.49)	(0.53)		(0.61)	(0.38)	(0.42)		(0.51)	(0.63)	(0.77)		(1.15)
Panel B: From Facilitation Forms												
Raw dice score above median	0.81*		0.95**	1.01**	0.98***		0.98**	0.87**	-0.22		-0.42	-0.66
	(0.46)		(0.48)	(0.49)	(0.37)		(0.39)	(0.43)	(0.60)		(0.62)	(0.66)
Old Distributor Provides Credit	-0.69		-0.59	0.36	-0.26		-0.11	0.24	0.34		0.22	-0.44
if Recipient Cannot Afford	(0.65)		(0.73)	(0.80)	(0.39)		(0.56)	(0.66)	(0.56)		(0.83)	(0.96)
Costs of Rental Vehicle and/or	-0.0062		-0.0065	-0.0120*	0.0011		-0.0023	-0.0000	-0.0025		0.0033	0.0047
Fuel to Old Distributor	(0.0087)		(0.0081)	(0.0070)	(0.0036)		(0.0046)	(0.0048)	(0.0073)		(0.0071)	(0.0076)
Non-Transportation Costs to	-0.0003		0.0004	0.0025	-0.0013		-0.0008	-0.0000	-0.0021		0.0003	-0.0004
Old Distributor	(0.0017)		(0.0014)	(0.0016)	(0.0012)		(0.0014)	(0.0013)	(0.0019)		(0.0027)	(0.0033)
Joint P-Value		0.004	0.294	0.006		0.020	0.135	0.041		0.546	0.962	0.540
Observations		187	149	147		162	123	122		76	55	54
Mean		0.13	0.17	0.17		0.53	0.55	0.56		0.32	0.33	0.31

Note: In this table, we explore what characteristics predict that the locality 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 7: Baseline Markup and Number of Bids

	Number of Bids	
	(1)	(2)
Baseline price markup	0.000895**	0.00111**
	(0.000367)	(0.000436)
Enhanced Competition	0.624**	0.615**
	(0.243)	(0.244)
Number of Hamlets		-0.000764
		(0.000584)
Log number of HH		-0.0447
		(0.204)
Distance to subdistrict		-0.0249**
		(0.0119)
Observations	182	182

Note: In this table, we explore the relationship between number of bids submitted and assignment to enhanced competition in treatment localities. In Column 1 we control for baseline price markup, in Column 2 we add baseline locality characteristics (number of hamlets, log number of households, distance to subdistrict) as controls. *** p<0.01 ** p<0.05 * p<0.1

Table 8: Quantile Treatment Effects on Price Markup

	Quantile 0.1	Quantile 0.25	Quantile 0.5	Quantile 0.75	Quantile 0.9
	(1)	(2)	(3)	(4)	(5)
	Panel A: Combined Effect of Outsourcing				
Info or Bidding	0.000	7.705	-0.000	53.333	39.394
	(15.52)	(15.78)	(12.07)	(33.70)	(41.26)
Bidding	-0.000	-7.705	0.000	-53.333	-90.505**
	(16.71)	(15.78)	(12.57)	(34.06)	(35.33)
P-Value					
Bidding = Ctl	1.00	1.00	1.00	1.00	0.13
Observations	5,886	5,886	5,886	5,886	5,886
Control Mean	150	400	600	900	1,114.29
	Panel B: Estimating Additional Effect of Competition				
Info or Bidding	0.000	7.705	0.000	52.954*	45.722
	(15.62)	(15.85)	(11.73)	(30.73)	(42.89)
Regular Bids	-7.843	-7.705	0.000	-47.709	-81.355*
	(17.56)	(18.09)	(15.72)	(34.07)	(42.48)
Enhanced	-0.000	-7.705	-0.000	-56.751*	-118.645***
Competition	(18.77)	(19.63)	(13.40)	(30.75)	(41.28)
P-Value					
Regular = Enh	0.65	1.00	1.00	0.59	0.27
Regular = Ctl	0.54	1.00	1.00	0.69	0.25
Enh = Ctl	1.00	1.00	1.00	0.76	0.05
Observations	5,886	5,886	5,886	5,886	5,886
Control Mean	150	400	600	900	1,114.29

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. In Panel B, we disaggregate the bidding effect by whether the locality was randomized into the minimum number of bids requirement. All regressions are quantile treatment effects clustered by the locality. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1: Flow of Localities through Bidding Process

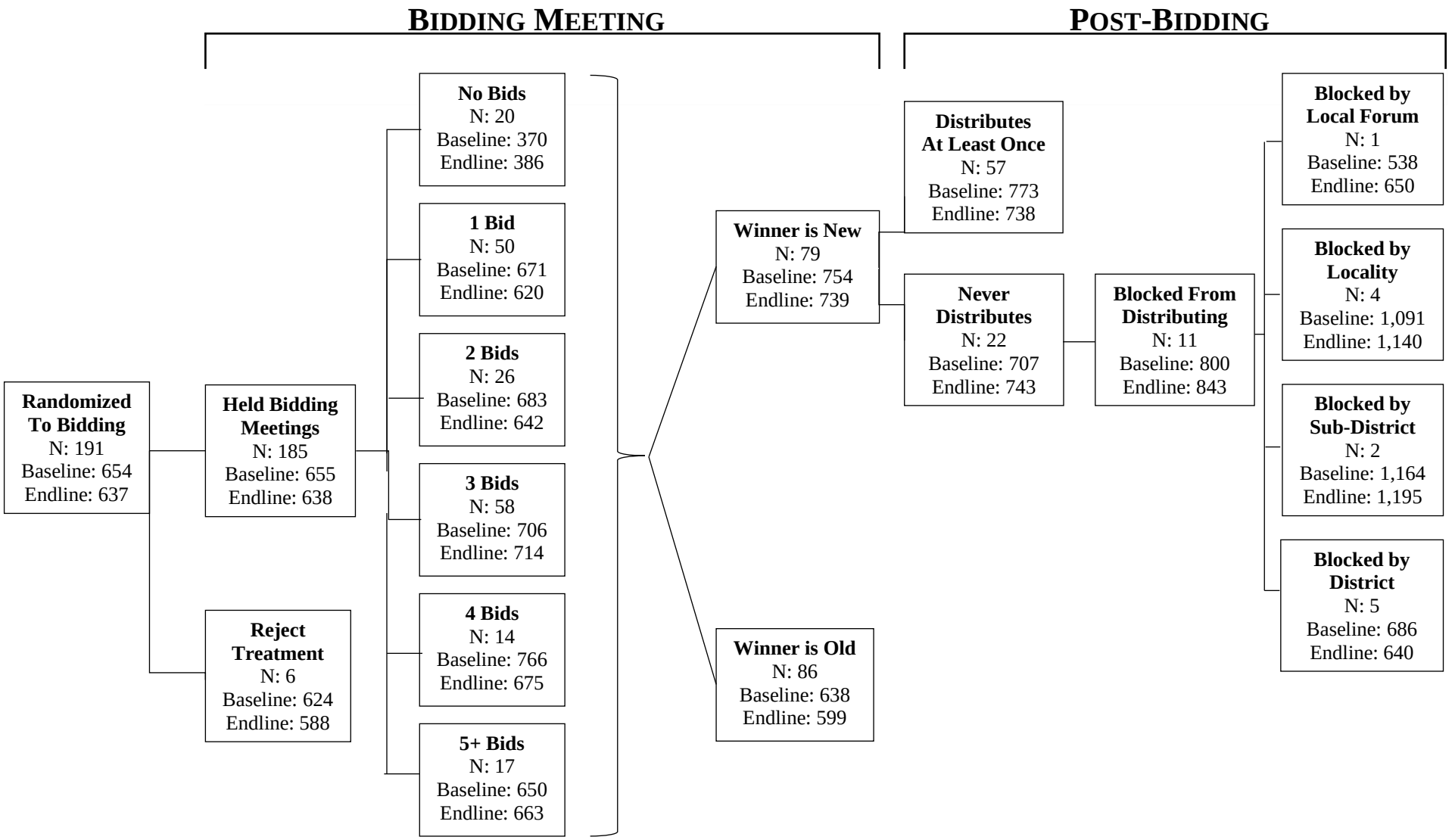

Figure 2: Endline Markup