Private Outsourcing and Competition: Subsidized Food Distribution in Indonesia

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We explore the impact of allowing for outsourcing service delivery to the private sector within Indonesia's largest targeted transfer program. In a field experiment across 572 municipalities, we find that allowing for outsourcing the last mile of food delivery reduced operating costs without sacrificing quality. However, the prices citizens paid were lower only where we modified the bidding rules to encourage more bidders. Higher rents are associated with greater entry despite elites' efforts to block reform. In this context, the option to outsource and sufficient

competition generated significant benefits relative to public distribution.

I. Introduction

The economics literature has highlighted trade-offs between governments providing services directly and outsourcing service delivery to private providers. For example, Hart, Shleifer, and Vishny (1997) show that private contractors may deliver better services because governments can provide stronger incentives to private contractors than to their own employees. This can lead to efficiency improvements but also comes with a risk that a contractor may cut costs by reducing quality along noncontractible dimensions.

Even if contracting out can improve efficiency in principle, it may fail to do so in practice. If there is limited competition for the contract, the gains may accrue entirely to the contractor rather than to the public. Frictions in the procurement process such as adverse selection (Spulber 1990) and moral hazard (Bajari and Tadelis 2001) may limit the gains even from outsourcing that is ex ante competitive. Vested interests may also seek to undermine or prevent an outsourcing process, especially since part of the point of outsourcing is to transfer rents from government insiders to consumers. This resistance may be particularly strong in places where the rents were initially large (Krusell and Rios-Rull 1996; Acemoglu and Robinson 2000).

This paper experimentally assesses the prospects for successful outsourcing in the context of local government in Indonesia, where there is limited on-the-ground expertise in how to do procurement and where corruption is high. We carried out a randomized control trial across 572 localities in which the central government offered localities the option to outsource a service previously provided by the village government. We first test whether offering localities the option to contract out leads

to lower costs and whether these gains come at a cost of lower quality. We then explore whether modifying the bidding rules in a subset of these villages to encourage more bids further improves outcomes. Finally, we examine how the process differs across areas 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. Under Raskin, eligible households receive a monthly allocation of subsidized rice. Even though Raskin 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. While this task is currently run by local governments in Indonesia, it is a function that is sometimes outsourced in other countries; in India, for example, the last mile of delivery of government-subsidized food is handled by privately owned "fair price shops."[1] More generally, this task is similar to other low-capital, high-labor activities, such as trash pickup, which are sometimes run by municipal governments and sometimes outsourced.

The last-mile Raskin distribution faces several challenges: while the local distributors report paying the central government logistics agency Rp. 1,617 per kilogram (kg) of rice—a markup of only 1 percent over the mandated copay of Rp. 1,600/kg—the average household pays a 41 percent markup to the distributor, or Rp. 652/kg above the official copay. While some of the markup may cover real local transport costs, Raskin is also an opportunity for rent extraction. Large amounts of rice disappear (Olken 2006; World Bank 2012). Moreover, citizens complain about the poor rice quality and inefficient distribution process. However, it is not clear that outsourcing would necessarily improve Raskin. Many elements of the service delivery, such as rice quality, are difficult to contract on (a common complaint is that the rice "smells bad," but this is subjective). And when problems do arise, it is hard for villagers to know if they are the fault of the local distributor (e.g., who substituted inferior rice) or the central government (e.g., which gave out bad rice to begin with). Moreover, procurement challenges 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 how to do so, or local leaders may try to sabotage the process.

To examine these questions, in 191 randomly selected localities out of the 572, the central government introduced a procedure that allowed

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[1] India and China also outsource the last mile of other major social transfer programs. For example, India's work program (NREGA) is centrally dictated and funded but locally run, as is China's urban Di Bao program (Gustafsson and Quheng 2011), which is among the world's largest transfer programs.

for competitive bidding for the right to distribute Raskin locally. Bids specified the markup to be charged, as well as other key aspects of the distribution process. A small, local committee examined the bids and selected the winner, who then held the contract for 6 months, at which point the committee could decide to renew the contract or return to the status quo. The incumbent local government distributor was also given the option to bid, providing the committee with the option to keep the status quo. Since the bidding process by its very nature increases transparency (since prospective bidders need information on the current process to prepare bids), 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 allows us to net out the effects of allowing bidding from the effects of increased transparency.

To probe the role of ex ante competition in the outsourcing process, in 96 randomly selected localities of the 191 that were assigned to the bidding process, we instituted an "extended bidding" treatment in which if at least three 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 extended bidding treatment to 2.74 in those with extended bidding. While this treatment changes the bidding treatment in multiple ways beyond just increasing the number of bidders (i.e., by changing the time that providers have to prepare bids), its large effect on ex ante competition makes it a useful treatment for studying the role of competition in the bidding process.

Overall, offering localities the opportunity to privatize increased efficiency with no detectable quality declines. However, only the extended bidding procedure led to substantially lower markups paid by households. Specifically, in areas without the extended bidding 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 relative to the wholesale price charged by the government. In contrast, in the areas with the extended bidding treatment, markups fall by 11 percent. Consistent with this, we find not only a reduction in reported transport costs but also a substantial reduction in "compensation" paid by the distributors. Neither bidding treatment induced a detectable decline in the quantity of rice received or in the quality of the distribution process, and rice quality may have even improved. One interpretation consistent with these results is that 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 elite capture limited the impacts of offering the option to outsource. As the municipal head (or someone he designates) is the incumbent supplier, he may put roadblocks in the contracting if he obtains substantial rents from the process. We test whether this type of blocking behavior is higher in areas with higher baseline Raskin prices, after partialing out the factors that predict actual distribution costs. We find that, in fact, areas with high baseline prices see more entry from private-sector bidders and fewer incumbents winning. Consistent with this, we also show that the outsourcing gains also came about primarily by eliminating the very highest prices. Thus, in our context, despite blocking by powerful elites, on net, high rents attract more bidders and lead to more outsourcing.

This paper builds on several literatures. First, it adds empirical evidence to the literature on outsourcing, building on papers such as Levin and Tadelis (2010), which focuses on which services are likely to be privatized and where.

Second, to the extent that the effects from the extended bidding treatment come through the increase in competition, our paper adds to the literature on the role of competition in procurement. There is an extensive theoretical literature on what happens in auctions when the number of potential or actual bidders goes up. There are standard arguments in the literature (e.g., Milgrom and Weber 1982; Bulow and Klemperer 1996) for why increased competition in this sense can increase efficiency and reduce rents going to the suppliers. That said, the literature emphasizes that improvements coming from more bidders are by no means inevitable,[2] and the theoretical reasons for caution, such as the presence of common values and endogenous and costly entry decisions, seem potentially relevant to our setting.[3]

Our paper is also related to the literature on ex ante versus ex post competition. As emphasized by Bajari, Houghton, and Tadelis (2014), while bidding generates ex ante competition, ex post the winner ends up in a

[2] 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. 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.

[3] For example, 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 potential suppliers other than the incumbent would show up for the job since most people who have the equipment to deliver rice (e.g., a truck) are also busy.

bilateral bargaining situation vis-à-vis the agency doing the procurement. This generates potential for ex post renegotiation, which might undermine the benefits of the bidding process. In this context, one advantage of attracting more bidders ex ante is that it creates a form of ex post competition: holdup is harder when the village knows that there are other suppliers who are willing to supply at relatively similar prices, since the contract could then be offered to someone else. Another advantage of having more bidders is that it allows the village to choose somebody they trust more—and therefore reduce the scope for ex post renegotiation. Consistent with these ideas, we show that the price eventually charged by the winner was not the price he or she bid, and most of the price reductions we see come from lower prices charged ex post rather than from lower bids ex ante.

Finally, our approach to assessing the benefits of increased ex ante competition in an auction setting (which may result in increased ex post competition as well) is based on a field experiment, in which we randomly change the bidding rules, observe changes in the level of competition, and trace out how this affects price, quality, quantity, and so on. To the best of our knowledge, this is the first time that a field experiment was carried out to answer this very basic question; the closest to our work in the experimental literature is the paper by 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 two bidders to mostly three bidders reduces the markup. By contrast, Hong and Shum find that going from two to three 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 three 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 extended bidding. Section IV explores the degree to which capture by vested interests reduced the impact of outsourcing. Section V presents conclusions.

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 copay 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 house-
hold's monthly consumption (Banerjee et al. 2018). It is Indonesia's larg-
est 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 (Government of Indonesia 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 a central distri-
bution point (either the warehouse itself or a central point located in the
subdistrict capital), located, on average, about 7 kilometers away. The lo-
cality 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.[4]

When picking up the rice at the warehouse, the local leaders have 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 not only are responsible for the time and effort re-
quired to distribute the rice but also assume the transportation costs,
which in control areas in our data cost an average of Rp. 244,161 (US$21)
each month.[5]

In practice, Raskin faces a number of challenges. Rice may go missing
at all stages in the distribution chain—from the central government to
the subdistrict distribution point to within hamlets. However, 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 receiv-
ing less than the full village quota in the last month from the govern-

[4] The village head is an appointed civil servant in urban *kelurahan* with a civil servant sal-
ary and is 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*).

[5] There is regional heterogeneity in these costs. In some areas, district governments
help subsidize these transport costs; in other areas, the Indonesian Bureau of Logistics
(BULOG) may deliver the rice directly to the village. Even when BULOG delivers directly
to the village, the local government is still responsible for distributing the rice to house-
holds, collecting their copayments, and remitting them back to BULOG.

ment logistics agency, household purchases reveal that a substantial share never reaches households (Olken [2006] estimates that at least 18 percent of rice goes missing; the World Bank [2012] estimates around 50 percent). Moreover, the rice that does arrive may be given to ineligible households rather than to eligible ones. On top of this, as shown in appendix table 1 (appendix is available online), households often pay a higher copay price (Rp. 660/kg, or about a 40 percent markup) than the central government intends.[6] All of these factors reduce the value of the intended transfer.

These facts do not necessarily 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 distribution costs. However, the distributors in our control group report transport costs that account for only about 12.4 percent of the price markup reported by households. Thus, it is possible that much of the higher price and missing rice is lost through corruption.

Beneficiaries also complain that the 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 rice. Quality problems, such as mold and pests, can render rice inedible.[7] These types of problems can reflect issues in the national procurement of rice or in warehouse storage, but they also likely reflect poor management and rent seeking at the local level.[8] 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. Moreover, it is also suggested that officials may sell official Raskin rice to private traders and replace it with lower-quality rice.

B. Sample

This project was carried out in six districts in Indonesia (two 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

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[6] There is much heterogeneity in the markup (app. fig. 1), with few households buying at the official rate.

[7] 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.

[8] 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-subdistrict variation rather than from between-subdistrict 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.

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. Using data from the 2011 Census of Villages, appendix table 2 shows that our chosen localities are broadly similar to Indonesia as a whole.

Note that because of a constrained time line 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, 2016). We also conducted a separate Raskin experiment on transparency (see Banerjee et al. 2018). 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 and control for strata dummies in all regressions.

C. Experimental Design

Stratifying by geographic location and the previous experiments, we randomly assigned the 572 locations to the different treatment groups, as shown in appendix table 3. Specifically, we randomly assigned 285 locations to a pure control group in which the status quo of the government running the Raskin distribution continued unchanged; 191 locations to the bidding treatment, where localities were told that they had the potential to outsource the Raskin delivery to a private provider through a bidding process; and 96 localities to an information-only placebo treatment that increased information about the distribution but did not explicitly change any of the distribution processes. Details of each of the treatments are as follows.

Pure control.—We randomly assigned 285 locations to the control group (see app. table 3). These locations reflect the status quo distribution process detailed above, where the local government primarily assumes responsibility for local pickup and distribution.

Bidding.—We randomly assigned 191 localities to a process in which private individuals or firms could bid for the right to become the official Raskin distributor, that is, 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 locality head that the location had been selected to have a procurement process

for Raskin distribution. The locality 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), 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 was not included on the forms. Bidders did not necessarily know the number of other bidders when they submitted the forms, 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 [LPM], Agency for Community Empowerment) 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 posted in the locality and the subdistrict 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 in which 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, pickup 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 its own priorities. At this point, the committee also had the option to reject nonserious proposals (11.8 percent of bids were rejected at this stage).

Next, each bidder presented his proposal to the bidding committee at a public meeting.[9] If more than five bids were submitted (which happened in only seven locations), only the best five were presented at the meeting to ensure sufficient time for discussion. Although the facilitator took notes at the meeting, the facilitator's participation was minimal and a committee representative led the meeting. Bidders were allowed to improve on their bids during the meeting in response to questions or in response to other bids.

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 (three or five) to ensure no ties. They also had the option of rejecting all 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 6 months; this letter was also provided to relevant subdistrict 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 6 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 members discussed their views on the process. They also decided whether to extend the distributor's time as Raskin distributor (if the distributor was not the original status quo), to choose a new distributor using either the same bidding process or another method of their choosing, or to revert to the old process.

"Extended bidding."—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. 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 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.

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[9] 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; this led to a 12 percentage point increase in female committee members. Appendix tables 4A and 4B provide results examining this treatment. Second, we randomized whether the facilitators suggested that the committee hold a follow-up meeting within 3 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, and so this treatment was essentially never followed.

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 nongovernment bidder, potentially putting greater pressure on lowering prices.

It is important to note that while the intended effect of this treatment was to increase the number of bidders, changing the bidding rules can have other effects. For example, this treatment extended the amount of time to bid, which may have changed the composition of bids. It could also potentially lead to complex strategic issues if, for example, the fact that bidding is extended signals to potential bidders that there was relatively little competition in the first round or that the process is a serious one and not a pro forma way of reappointing the existing distributor. Thus, while we treat the increase in the number of bidders as the primary effect of this treatment, it is important to keep these other effects in mind.

Information-only.—The bidding process naturally provides greater transparency: the government must provide information about the distribution process, so that potential bidders can decide whether to participate and, if so, 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 comprising 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 reevaluation 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.[10] We therefore use this treatment as a comparison group for the bidding

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[10] 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

treatment to isolate the pure effect of the option to outsource from increased transparency.

For the information-only meeting to be a real placebo, the meetings must be similar to the bidding meeting. A potential concern is that a bidding meeting might be more interesting, and hence draw more attention, than an information-only meeting. Appendix table 5 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). In short, on the basis of these observable characteristics, the meetings appeared similar across treatments.

D. Randomization Design, Timing, and Data

Appendix table 3 shows the number of locations randomly assigned to each treatment. We stratified by six geographic strata (districts) and the previous experimental treatments.

The time line was as follows (app. fig. 2): in April–July 2013, after the baseline survey was completed for the entire subdistrict, both treatments were conducted. During the following 6 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 subdistrict, 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 subunit 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. 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

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follow-up meeting at 3 months on their own (e.g., without any facilitators) to discuss the state of the distribution. Appendix tables 4A and 4B provide these results.

the baseline than in the endline as the baseline was used for other purposes (Banerjee et al. 2018). We surveyed the households on their background and their Raskin experiences. At this time, we additionally interviewed the village head.

In December 2013 and January 2014, just before the 6-month followup meetings were held in the treatment locations, an endline survey took place in which we interviewed six 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 thencurrent Raskin distributors. In the bidding and information locations, we also interviewed the old distributor (if different from the currently active distributor) as well as the winner in the bidding locations (if different from the current, which could occur, e.g., if the winner quit or was denied permission to distribute). 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) and 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.

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 (col. 5), information-only and pure control (col. 6), and bidding and information-only (col. 7). Of the 45 differences that we estimate between the groups, only five (11 percent) are significant at the 10 percent level, which is consistent with chance. The joint p-value across all 15 variables is .27, .50, and .20 in columns 5–7, respectively. In appendix table 6B, we also conduct a randomization check on extended bidding versus the regular 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 .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.
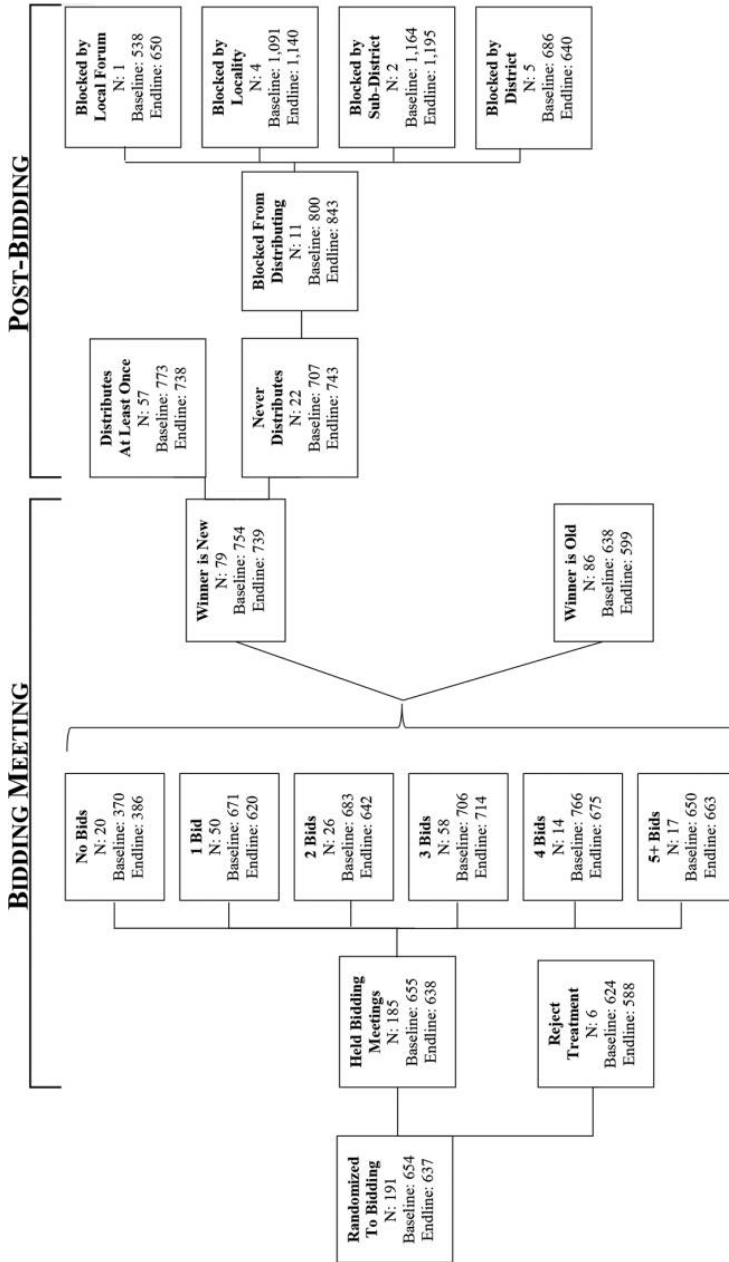
FIG. 1.—Flow of localities through the bidding process. In each box, we list the number of villages that fall in this category (N), as well as the average baseline and endline markups in these villages as collected in the household surveys.

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 to the status quo. However, of the 165 treatment locations that received at least one bid, 86 (52 percent) selected the original distributor. Of the 79 treatment locations that selected a new distributor, 60 of them (76 percent) elected to continue with the new distributor after the 6-month preliminary contract was over (not shown in fig. 1). Both the locations that chose a new distributor who actually takes effect ("Winner Is New—Distributes at Least Once") and those that elected to remain with the incumbent ("Winner Is Old") have similar reductions in price from baseline to endline of around Rp. 35–40/kg.

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 four 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 an average of Rp. 638. Interestingly, the six locations where the winner was blocked from distributing by the locality head or subdistrict 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. 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 to extended bidding process. In column 7, we present the p-value of the difference of means across the regular bidding process and extended bidding.

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.[11] Panel B of table 1 shows that, on average, about 22 individuals attended the bidding meetings (the average locality size is 1,299 households). Local leaders made up a fair share of the participants, with about

[11] In app. fig. 3, we present the reasons reported by the winners and losers, respectively, why they believe they won or lost the bidding process. The three biggest reasons to which 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).

TABLE 1
The Bidding Process (Conditional on Bidding Meeting Occurring)

	Overall		Regular Bidding		Extended Bidding		p-Value: Regular = Extended
	Mean (1)	Standard Deviation (2)	Mean (3)	Standard Deviation (4)	Mean (5)	Standard Deviation (6)	(7)
	A. Bids Submitted						
Number of bids	2.43	1.66	2.14	1.68	2.74	1.59	.01**
Number of bids, after initial screening	2.16	1.50	1.88	1.47	2.44	1.48	.01**
Old distributor wins	.52	.50	.49	.50	.55	.50	.49
	B. Meeting Attendance						
Attendees	21.69	9.13	21.09	10.31	22.31	7.75	.36
Raskin beneficiaries	8.28	8.32	8.78	8.95	7.77	7.63	.41
Local officials	9.42	5.83	8.80	5.95	10.06	5.66	.14
	C. Meeting Participation						
No discussion at meeting	.09	.29	.03	.18	.15	.36	.01***
<10% of people talk	.43	.50	.46	.50	.41	.49	.51
10%–50% of people talk	.45	.50	.50	.50	.40	.49	.16
>50% of people talk	.03	.16	.01	.11	.04	.21	.18

Note.—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 185 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 (94 localities).

* $p < .1$.
** $p < .05$.
*** $p < .01$.

nine of them attending, on average. About eight 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 the facilitator report that more than half of the crowd participated.

The extended bidding 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 extended bidding treatment as opposed to 2.14 without the requirement, about a 30 percent increase; this difference is significant with a p-value of .01. One worry is that the extended bid requirement would encourage more "unrealistic" or "ghost" bids, but this was not the case:

in the extended bidding areas, we observe an increase in bids that pass the screen (2.44 relative to 1.88; p-value .01). There were more meetings with no discussion (15 percent in the extended bidding treatment vs. 3 percent otherwise), but this may have been due to the fact that there were more proposals to present. On net, a new distributor won in 45 percent of the extended bidding areas as opposed to 51 percent in the regular; this difference, however, is not statistically significant (p-value .49).

III.   Effects of 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 extended bidding treatment), we estimate

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

where i represents a study location and s represents one of our district-by-previous-experiments strata (so α_s represents strata fixed effects). The dependent variable y_{is} in each column is a different characteristic of the distributor at endline (approximately 6 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 key coefficient of interest, γ, captures how the bidding locations differ from those that received the information-only (i.e., placebo) treatment. 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 = control. All p-values given in the tables are derived using randomization inference.

Second, we separately estimate the effect of bidding (regular vs. extended):

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

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 extended bidding 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 extended bidding (i.e., a p-value of the test that $\gamma = \omega$) and the p-value of bidding with extended bidding versus pure control (i.e., a p-value of the test of $\omega + \beta = 0$).

Table 2 shows that 6 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, col. 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 (col. 1) and about 20 percentage points more likely relative to the pure controls. A change was slightly more likely in the extended bidding treatment compared to the regular bidding treatment—20 compared with 14 percentage points, respectively (table 2, panel B, col. 1)—though this difference is not statistically significant (p-value .308).

The remaining columns explore the distributor's identity. In the pure control group, almost 85 percent of the distributors were a local official or hamlet official or were related to one (cols. 2, 3, and 4).[12] In the bidding group compared to the pure control group, local leaders were significantly less likely to be in charge (col. 2), but their spouses/relatives and hamlet-level leaders were then more likely to be in charge (cols. 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 in 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 groups (col. 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 changed the identity of those distributing Raskin, it largely redistributed the role within the existing local government elite. 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.

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[12] 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 official to be a member of their household, their nuclear family (brother, sister, mother, father), or their "large" family (cousin, nephew, niece, uncle, or aunt).

TABLE 2
WHO DISTRIBUTES RASKIN SIX MONTHS AFTER INTERVENTION?

	In Charge of Any Responsibilities before May 2013 (1)	Distributor/Spouse Is/Was Local Official (2)	Distributor/Spouse Is Related to a Local Official (3)	Distributor/Spouse Is/Was or Is Related to Hamlet Official (4)	Is a Trader (5)	Lives in Locality (6)
			A. Combined Effect of Outsourcing			
Info or bidding	−.035	−.059	.043	.001	.008	−.027
	(.048)	(.050)	(.044)	(.054)	(.027)	(.042)
	[.497]	[.243]	[.339]	[.990]	[.765]	[.529]
Bidding	−.165***	−.076	.046	−.002	.071**	−.019
	(.051)	(.053)	(.047)	(.058)	(.028)	(.045)
	[.003]	[.163]	[.331]	[.966]	[.014]	[.677]
p-value: bidding = control	.001	.001	.013	.972	.000	.173
Observations	586	586	586	586	587	587
Control mean	.803	.347	.160	.350	.034	.816

120

	B. Estimating Additional Effect of Competition					
Info or bidding	−.035	−.059	.043	.001	.008	−.027
	(.048)	(.050)	(.045)	(.054)	(.027)	(.042)
	[.498]	[.241]	[.337]	[.987]	[.766]	[.529]
Regular bidding	−.135**	−.110*	.053	.036	.068**	−.046
	(.059)	(.062)	(.055)	(.067)	(.033)	(.052)
	[.030]	[.079]	[.338]	[.579]	[.042]	[.381]
Extended bidding	−.197***	−.042	.038	−.041	.074**	.009
	(.059)	(.062)	(.055)	(.067)	(.033)	(.052)
	[.001]	[.502]	[.495]	[.529]	[.022]	[.858]
p-value:						
Regular = extended	.308	.267	.789	.235	.839	.281
Regular = control	.001	.001	.035	.482	.005	.085
Extended = control	.001	.043	.072	.444	.002	.681
Observations	586	586	586	586	587	587
Control mean	.803	.347	.160	.350	.034	.816

NOTE.—In this table, we explore the characteristics of bidders across the experimental groups, 6 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 (cols. 1–4 have 586 observations as there is one piece of missing data in each column); note this is more than the 572 total villages as 14 villages had multiple people listed as current distributors. Standard errors clustered by village are in parentheses; randomization-inference *p*-values are in brackets.

* *p* < .1.
** *p* < .05.
*** *p* < .01.

121

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 ordinary least squares (OLS) but now cluster the standard errors to account for the 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.[13]

Note two important aspects regarding the interpretation of the findings. First, we estimate the intent-to-treat effects rather than the instrumental variable impact on those locations where there was a new winner. The reason is that 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.[14]

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 (*p*-value .052): this constitutes about a 7.3 percent reduction in the markup charged (col. 2).

In table 3, panel B, we separately identify the price effect for the regular bidding treatment and that with extended bidding. Here, we find quite stark results: we see substantially larger price reductions in localities with extended bidding. Specifically, households in extended bidding localities pay Rp. 74/kg less than in the information placebo, about an 11 percent reduction in markup; this reduction is also statistically different from that in the pure control group (*p*-value .02). Households in localities with regular bidding pay a statistically insignificant Rp. 23/kg less than the information placebo and only Rp. 5/kg less than the pure controls. This implies that extended bidding led to a further reduction in price of Rp. 50/kg compared with regular bidding (*p*-value .09). 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 the central government had mandated that distributors were supposed to provide the correct

[13] Appendix table 7 replicates table 3 omitting the baseline controls. The results are qualitatively similar.

[14] In app. tables 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 with greater precision in estimates for eligible households in terms of price changes).

quantity of rice, so this was not a category in the application form for the bid and therefore not a criterion on which bidders were evaluated. However, we find no evidence that the overall quantity of rice bought changed (col. 3). Because the quantity data are much noisier than the price data, on net we cannot reject the null of no effect on subsidy received.[15]

The key concern articulated by the Hart et al. (1997) theory is that, as a result of outsourcing, private distributors may shirk and reduce noncontractible 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 noncontractible 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 (col. 4). We observe an increase in their assessments: about 3.7 percent higher compared to information-only (p-value .132) and about 4.9 percent higher than the pure control (p-value .01).[16] Interestingly, the quality improvements appear similar in both regular bidding and extended bidding (panel B), suggesting—against the Bajari, McMillan, and Tadelis (2009) hypothesis—that increased competition did not reduce quality.

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 (cols. 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 (col. 6). Finally, we examine changes in overall satisfaction with the Raskin process across the treatments (col. 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 extended bidding. We find no evidence of a decline in quality.

C. Impact on Distribution Costs

As Hart et al. (1997) point out, even when contracting out leads to efficiency gains, without sufficient competition, these efficiency gains may

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[15] We find that the information treatment increases the subsidy received by Rp. 426 per household, and the bidding increases it by a further Rp. 524 per household; but these are quite noisily estimated, and confidence intervals range from −1,700 to +2,600 (see app. table 9).

[16] 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.

TABLE 3
RASKIN DISTRIBUTION PROCESS

	Bought Raskin (1)	Price Markup (2)	Amount Purchased (3)	Satisfied with Rice Quality (4)	Distance to Purchase Point (Meters) (5)	Time to Purchase Point (Minutes) (6)	Paid for Rice in Advance (7)	Satisfied with Raskin Program (8)
				A. Combined Effect of Outsourcing				
Info or bidding	−.009	18.770	.151	.006	−1.080	.441*	.013	−.020*
	(.024)	(24.07)	(.23)	(.01)	(14.37)	(.27)	(.02)	(.01)
	[.728]	[.431]	[.511]	[.616]	[.940]	[.079]	[.571]	[.053]
Bidding	.021	−49.023*	−.002	.019	7.754	−.501*	−.009	.006
	(.03)	(24.91)	(.24)	(.01)	(15.15)	(.28)	(.03)	(.01)
	[.443]	[.052]	[.992]	[.132]	[.608]	[.066]	[.715]	[.621]
p-value: bidding = control	.56	.11	.42	.01	.56	.77	.84	.08
Observations	6,860	5,886	6,858	6,533	6,194	6,247	6,394	6,782
Control mean	.76	652.39	5.76	.51	190.96	5.94	.43	.59

			B. Estimating Additional Effect of Competition				
Info or bidding	−.009	18.842	.151	.006	−.999	.441*	.013	−.020*
	(.02)	(24.06)	(.23)	(.01)	(14.38)	(.27)	(.02)	(.01)
	[.728]	[.429]	[.510]	[.616]	[.945]	[.079]	[.571]	[.053]
Regular bidding	.014	−23.645	−.016	.016	25.315	−.430	.011	.001
	(.03)	(29.94)	(.28)	(.01)	(18.11)	(.33)	(.03)	(.01)
	[.658]	[.420]	[.954]	[.278]	[.155]	[.171]	[.710]	[.965]
Extended bidding	.027	−73.551**	.012	.022	−9.368*	−.569*	−.028	.011
	(.03)	(26.51)	(.27)	(.01)	(16.62)	(.31)	(.03)	(.01)
	[.372]	[.013]	[.963]	[.133]	[.597]	[.068]	[.324]	[.405]
p-value:								
Regular = extended	.67	.09	.92	.68	.05	.65	.18	.43
Regular = control	.85	.84	.55	.07	.10	.97	.31	.07
Extended = control	.48	.02	.49	.02	.47	.62	.53	.37
Observations	6,860	5,886	6,858	6,533	6,194	6,247	6,394	6,782
Control mean	.76	652.39	5.76	.51	190.96	5.94	.43	.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 6 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 extended bidding treatment. All regressions are estimated by OLS, and standard errors are clustered by the locality. In col. 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 four options on a scale of 0–1. Standard errors clustered by village are in parentheses; randomization-inference p-values are in brackets.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

be captured by vendors rather than enjoyed by citizens. To investigate these issues further, at the endline, we interviewed the current distributor and asked about his distribution costs. Note two aspects of the cost measures. First, they are self-reported; given the informal nature of the economy, one cannot track costs through credit card or bank transactions. Second, the reported costs often increase in the information treatment relative to the pure control. There are a number of possible reasons for this: perhaps it forces distributors to better compute their actual costs, perhaps because the greater scrutiny forces them to report their true costs so that they match the total markup they are charging, or perhaps because the information treatment encourages distributors to inflate their costs so as to make it look to the public that they are extracting fewer rents. Regardless of the reason, this effect stresses the importance of comparing 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 (col. 1). These reductions seem roughly similar in both regular bidding and extended bidding. This is consistent with the view suggested by Hart et al. (1997) 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 bidding tends to select traders, who are more experienced at moving rice around—it appears that the outsourcing treatment alone is enough to obtain these gains. Importantly, note that since these are intent-to-treat effects, these improvements could come from both those villages that outsource and those that were merely offered the choice to outsource but did not—which could occur if, for example, the threat of outsourcing induced incumbents to improve.

However, the extended bidding treatment also led to a reduction in compensation payments (col. 2), other costs (col. 3), and total costs (col. 4), whereas the regular bidding treatment without it did not. The differences between regular and extended bidding for both compensation to others and total costs are economically large and statistically significant (p-values of .018 and .023, respectively). The impact of extended bidding on reducing compensation mirrors the reduction in prices induced by extended bidding 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) and/or encouraged incumbents to become more efficient, but without the additional competitive pressure, the winning bidder was able to continue to pad his costs and avoid passing on the gains to the consumers. An alternative explanation, of course, is that the reduction in "payments to others" reflects fur-

TABLE 4
Endline Costs to Current Distributor

	Transportation Costs (1)	Compensation to Others (2)	Other Costs (3)	Total Costs (4)
	A. Combined Effect of Outsourcing			
Info or bidding	88,038*	121,875	40,716**	318,287
	(52,052)	(174,163)	(18,745)	(211,403)
	[.053]	[.488]	[.014]	[.104]
Bidding	−101,616**	−94,256	−30,531	−317,960
	(54,924)	(179,950)	(19,678)	(219,985)
	[.034]	[.606]	[.100]	[.122]
p-value: bidding = control	.720	.835	.448	.997
Observations	574	574	574	574
Control mean	244,161	961,974	84,166	1,315,030
	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)
	[.054]	[.481]	[.014]	[.101]
Regular bidding	−124,126**	154,902	−13,578	−51,869
	(59,089)	(222,663)	(22,454)	(263,152)
	[.026]	[.463]	[.526]	[.824]
Extended bidding	−79,174	−349,228	−47,098**	−590,262**
	(62,837)	(180,668)	(21,563)	(223,414)
	[.160]	[.102]	[.024]	[.013]
p-value:				
Regular = extended	.426	.018	.110	.023
Regular = control	.435	.104	.108	.164
Extended = control	.858	.194	.717	.164
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 rupiah). All data come from the endline distributor survey that we conducted about 6 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 extended bidding treatment. All regressions are estimated by OLS with robust standard errors in parentheses; randomization-inference p-values are in brackets. If we have data for at least one cost variable, we replace missing data with zeros for other cost categories. "Total costs" is the sum of cols. 1–3. The top 1 percent of values for each cost are dropped.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

ther efficiency gains induced by the extended bidding treatment. Either way, the net result is that the further reductions in distributor-reported costs from extended bidding mirror what we see happening in prices from the household perspective.

Interestingly, we find no evidence that the difference in prices from extended bidding comes from more aggressive bidding: the point esti-

mate on the effect of extended bidding on the realized minimum bid is almost exactly zero, though the standard errors are too large to rule a substantial negative effect (app. table 10). On the other hand, the effect on the gap between the actual endline price and the bid price is of the right magnitude and is closer to being statistically significant: in the regular bidding villages the endline price was Rp. 211 higher than the bid price, suggesting that ex post renegotiation is a major issue, but this gap shrunk by between Rp. 55 and Rp. 81 in the extended bidding treatment villages (*p*-values .184 and .112, respectively).

There are good theoretical reasons why in a setting like ours adding to the number of bidders may not make bidding more aggressive (see the discussion in n. 3 above). In addition, our setting was not strictly a first-price closed-bid auction: the committee had discretion to pick whomever they liked, so it may be that the bidders focused on dimensions other than the price. Indeed, we do 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 such as living in the locality or experience as a trader relative to price (see the appendix for more details). This might explain the fact mentioned above, that the actual price paid is substantially closer to the promised price in the extended bidding treatment than in the regular bidding treatment (though the difference is not significant at conventional levels), if we think that the increased number of bidders allowed the committee to select more reliable candidates. Alternatively, the extended bidding treatment may have allowed the village to discover more potential suppliers, leading to greater pressure on the chosen supplier to stick to his promise.

On net, the results in this subsection confirm the results from the household survey: the extended bidding treatment led to a substantial lowering of reported costs above and beyond that which occurred in regular outsourcing, which were in turn passed on to consumers in the form of lower prices.

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 why 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.

To study these questions, we focus on the Raskin copay price at baseline as a measure of rents in the system. 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, particularly once we control for other, observable determinants of distribution costs. Indeed, we find that if we regress the Raskin copay price on a subjective measure of the local leader's corruption level, we find a strong association—both alone and when we control for local characteristics that proxy for these actual distribution costs (e.g., distance to the subdistrict, log population, number of hamlets, and distance to the distribution point where the local distributor receives the rice from the BULOG; see app. table 11).[17] This suggests that the baseline price may indeed capture not just operating costs but also the amount of rents in the system.

Table 5 then examines whether failure of the process ex ante or ex post appears correlated with baseline prices. We focus on failure of the outsourcing process at various steps: whether no meeting was actually held, whether the incumbent won the bid, or whether the incumbent was still distributing despite the fact that someone else won the bid. To the extent that failure of the process is positively correlated with baseline prices, this suggests 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 5 begins by investigating whether privatization occurs or not; that is, of the 191 locations randomized to bidding, in which types of areas did bidding actually occur? We regress a dummy variable that equals one 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; column 2 reports the results from a single regression. In addition to the variables shown, we control in all regressions for the proxies for distribution costs discussed above (app. table 12 shows the analogous version without these controls).

We find that higher prices substantially predict the occurrence of a meeting with at least one bidder: a one standard deviation increase in

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[17] Following Fischbacher and Föllmi-Heusi (2013) and Hanna and Wang (2017), we also elicited 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. See app. table 11.

TABLE 5
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 (1)	Joint (2)	1-by-1 (3)	Joint (4)	1-by-1 (5)	Joint (6)
Average price markup (Rp./kg)	−.00299*** (.00089)	−.00354*** (.00123)	−.00115** (.00056)	−.00129* (.06217)	.00017 (.00079)	.00056 (.00109)
Household bought Raskin in last 2 months	.369 (.894)	.358 (1.297)	1.598** (.651)	.98783 (.799)	−.206 (.868)	.537 (1.173)
Average amount of Raskin purchased (kg)	.02395 (.06829)	−.04059 (.09653)	.06064 (.07064)	−.00757 (.06217)	−.08141 (.07088)	−.03390 (.06979)
Average satisfaction with program quality (0–1 scale)	3.887* (2.245)	.599 (2.978)	4.082** (1.766)	4.272** (1.972)	.524 (2.274)	−.786 (2.843)
Average distance to purchase point (meters)	.00078 (.00133)	−.00093 (.00147)	−.00181* (.00099)	−.002* (.001)	.00163 (.00131)	.00196 (.00143)
Household purchased Raskin in advance	1.470*** (.488)	1.160** (.546)	.803** (.377)	.264 (.428)	−1.101* (.625)	−.955 (.815)
Old distributor provides credit if recipient cannot afford Raskin	−.688 (.645)	.125 (.744)	−.255 (.387)	.161 (.429)	.341 (.561)	.221 (.650)

130

Costs of rental vehicle and/or fuel to old distributor	−.00620	−.01191	−.00203	.00059	−.00246
	(.00874)	(.00762)	(.00255)	(.00305)	(.00729)
Nontransportation costs to old distributor	−.00031	.00280	−.00076	.00051	−.00214
	(.00174)	(.00172)	(.00106)	(.00115)	(.00187)
Joint p-value		.264		.397	.785
Observations		184		159	74
Mean		.14		.53	.32

Note.—In this table, we explore what characteristics predict that the locality will have bidders present at a meeting, what characteristics predict that the 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 relevant outcome listed in each column on baseline characteristics from the household survey and/or from the baseline information forms on the process. We include controls for baseline characteristics: log locality population, number of hamlets in locality, distance from purchase point to BULOG's distribution point, and distance to subdistrict. The cell in cols. 1, 3, and 5 present coefficients from separate regressions; the cells in cols. 2, 4, and 6 present regressions from multivariate regressions. All regressions are estimated as a logit; coefficients are interpreted as the impact of the independent variable on the log-odds ratio. The top 1 percent of transportation and other costs are dropped; costs are reported in Rp. 10,000.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

131

baseline markup (Rp. 390) would increase the log odds by 1.31, that is, increasing the odds of having the meeting with at least one bidder by over 300 percent. Table 5 shows that locations with low baseline satisfaction are also more likely to hold a contested meeting, even conditional on price.

Part of the reason the bidding process was successfully held is that higher baseline rents attract more entrants. To examine this, table 6 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. On net, the results suggest that high baseline rents may encourage new entrants and increase demand for outsourcing.

The remaining columns of table 5 examine other points in the bidding process where vested interests might exercise some control. Columns 3 and 4 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 its not being blocked in the first stage). We find similar results 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

TABLE 6
Baseline Markup and Number of Bids

	Number of Bids	
	(1)	(2)
Baseline price markup	.000895**	.00109**
	(.000367)	(.000444)
Extended bidding	.624**	.623**
	(.243)	(.246)
Number of hamlets		−.000767
		(.000586)
Log number of households		−.0528
		(.2064)
Distance to subdistrict		−.0247**
		(.0119)
Distance to distribution point		−.00738
		(.0127)
Observations	182	182

Note.—In this table, we explore the relationship between the number of bids submitted and assignment to extended bidding in treatment localities. In col. 1 we control for baseline price markup, and in col. 2 we add baseline locality characteristics (number of hamlets, log number of households, distance to subdistrict, distance from purchase point to BULOG's distribution point) as controls. All regressions are estimated by OLS, using villages as the unit of observation, with robust standard errors in parentheses.
  * $p < .1$.
  ** $p < .05$.
  *** $p < .01$.

the previous columns. The incumbent is also more likely to be chosen when baseline household satisfaction is high.

The final set of results in table 5 examines whether the incumbent distributor is still distributing 6 months later, conditional on his not having won the bidding. This variable captures ex post capture. It is not strongly statistically related to any of the predictors we examine.

On net, the results from tables 5 and 6 show that high baseline rents appear to lead to more entry. While there is some evidence that blocking did occur—figure 1 showed that the places where the winning bidder was blocked from distributing had much higher baseline prices than others—in general, the increase in entry due to the high rents seems to have been the dominant force. In turn, the experimental estimates from Section III suggest that the additional entry induced by the high baseline rents should lead to more substantial price reductions. For example, the estimates from table 6 imply that moving from the 10th to the 90th percentile in baseline price markup (i.e., from Rp. 129 to Rp. 1,359) would lead to an additional 1.37 bidders, which is more than twice as large as the effect of the extended bidding treatment. If one were to assume that the entire effect of extended bidding came from its increase in the number of bidders, then 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 7 reestimates the price effects in table 3 as quantile treatment effects, for the 10th, 25th, 50th, 75th, and 90th quantiles. Figure 2 displays this information graphically by plotting the cumulative distribution functions of price for the extended bidding 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 markups. Table 7 shows almost no effects at quantiles up to about the median and clear negative effects—about Rp. 90 overall and Rp. 119 for the extended bidding treatment—at the 90th percentile of the distribution.

The difference we estimate using the quantile treatment effects between the 10th and 90th percentiles—about Rp. 80/kg overall and about Rp. 110/kg for the extended bidding treatment—is almost precisely what one would predict on the basis of the additional entry induced by the difference in baseline prices. This suggests that the additional entry in-

TABLE 7
QUANTILE TREATMENT EFFECTS ON PRICE MARKUP

	QUANTILE				
	.1 (1)	.25 (2)	.5 (3)	.75 (4)	.9 (5)
A. Combined Effect of Outsourcing					
Info or bidding	.000	7.705	.000	53.333	39.394
	(15.52)	(15.84)	(11.94)	(33.70)	(41.26)
Bidding	−.000	−7.705	−.000	−53.333	−90.505**
	(16.71)	(15.84)	(12.53)	(34.06)	(35.33)
p-value: bidding = control	1.00	1.00	1.00	1.00	.13
Observations	5,886	5,886	5,886	5,886	5,886
Control mean	150	400	600	900	1,114.29
B. Estimating Additional Effect of Competition					
Info or bidding	.000	7.705	.000	52.954*	45.722
	(15.62)	(15.85)	(11.73)	(30.73)	(42.89)
Regular bidding	−7.843	−7.705	.000	−47.709	−81.355*
	(17.56)	(18.09)	(15.72)	(34.07)	(42.48)
Extended bidding	−.000	−7.705	−.000	−56.751*	−118.645***
	(18.77)	(19.63)	(13.40)	(30.75)	(41.28)
p-value:					
Regular = extended	.65	1.00	1.00	.59	.27
Regular = control	.54	1.00	1.00	.69	.25
Extended = control	1.00	1.00	1.00	.76	.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 6 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 with standard errors clustered by locality.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

duced by the high prices was important to the heterogeneous treatment effects we observe.

V.   Conclusion

In this paper, we examine whether allowing local governments to outsource delivery to the private sector improves distribution. Focusing on a subsidized food distribution program, we show that giving localities the option to outsource last-mile delivery of subsidized food reduced its price, without sacrificing other aspects of quality. The price declines appear to come in part from greater efficiency in the form of lower transport costs and in part from payments that may reflect rents. These re-
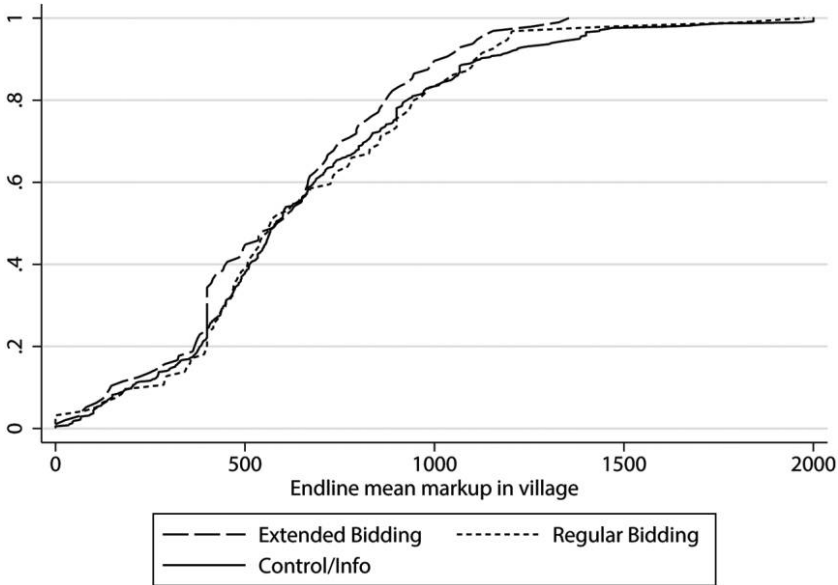
Fig. 2.—This graph plots the cumulative distribution function of the mean endline Raskin markup (in rupiah) in each village from the household survey, by treatment group. Control and "info-only" treatment groups are pooled for greater statistical power. $N = 568$ villages. Color version available as an online enhancement.

sults, of course, come from a single bidding cycle and are based only on data from 6 months, so it is possible that consistent use of bidding over the long term might have different effects. Nevertheless, they do show the potential for outsourcing to improve outcomes, even in a village society.

We find that the option to outsource itself was insufficient: outsourcing with limited competition resulted in some efficiency reductions, but no cost savings passed on to the public at large. Only when outsourcing was combined with extended bidding provisions that resulted in more bidders did we see substantial reductions in markups charged to the public, even though the change in the number of bidders between regular and extended bidding was modest—from 2.14 to 2.74. There are, however, other instances in the oligopoly literature showing that moving from two to three competitors can result in substantial changes in prices (e.g., Bresnahan and Reiss 1991), though these are cases in which the competition is purely ex post whereas in our setting it is primarily ex ante (though as pointed out before, greater ex ante competition could also enhance ex post competition).

We also investigate the tension created by the presence of rents at baseline. When rents are high, incumbents may fight to block the out-

sourcing 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, it is possible that the power of vested interests to block may have muted the effect of outsourcing to some extent.

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