The Value of Discretion: Price-Caps and Public Service Delivery

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JOB MARKET PAPER

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November 14, 2016

Abstract

It is often argued that price-caps – a ceiling on prices charged by monopolistic suppliers - are necessary to redistribute surplus and make essential goods and services affordable. I explore whether price-caps lead to welfare improvements through a field experiment in which I randomize whether public livestock extension agents are subject to a price-cap or not. This intervention has three effects. First, conditional on being served, the treatment increases the consumer surplus available to recipients: the price-cap reduces average prices by 17% and the within-agent standard deviation of prices by 42%. Second, the intervention increases the affordability of extension services: the price-cap increases the share of previously unserved and needy customers in the beneficiary pool by 15% and 9%, respectively. Third, the price-cap reduces the geographic coverage of services by decreasing the likelihood that agents will serve remote villages by 25%. This suggests that price-cap regulation creates a tension between making services affordable and providing incentives for agents to serve remote recipients. In light of this trade-off, I show that the marginal welfare effect of reducing discretion over prices can be expressed as a function of two sufficient statistics: the elasticity of the proportion of served villages in a service area with respect to the price-cap and the price elasticity of demand. Calculating the welfare effects, I find that any reduction of agents' discretion reduces social welfare.

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1 Introduction

A key function of governments is to assure that citizens are provided with essential services for welfare and economic development. When agents responsible for supplying these services have market power, decentralized market outcomes can generate an inefficient allocation of services and allow agents to extract surplus from recipients.[1] These undesirable outcomes threaten the ability of governments to provide for their citizens. Government imposed price-caps, which aim to reduce agents' discretion over prices, are one commonly proposed solution to this challenge.[2] Theoretical work suggests that such interventions may be welfare enhancing, as they can redistribute surplus and improve aggregate efficiency (Laffont and Tirole, 1993; Laffont, 2005). In addition, there is evidence that charging for essential services in a developing country setting can adversely affect peoples' willingness to use these services (Cohen and Dupas, 2010; Ashraf, Berry, and Shapiro, 2010). Those findings highlight a potential value of low price-caps and suggest that using price-caps as an instrument to reduce agents' discretion over prices may be welfare enhancing.

In many situations, unregulated monopolistic agents responsible for supplying public services do not, however, only hold decision power over prices. Rather, these agents also have discretion over which and how many recipients to serve.[3] In such situations, a theoretical literature argues that allocating discretion over pricing to agents can be welfare enhancing (Grossman and Hart, 1986; Aghion and Tirole, 1997). In particular, when governments are unable to impose and enforce universal service obligations, discretion over prices allows agents to extract surplus, creating an incentive for them to extend service coverage.[4] Price-caps reduce agents' discretion and, by limiting their ability to extract surplus, can reduce incentives to extend service coverage.[5] Capping prices for essential services can therefore also reduce welfare. The question of whether to cap prices or not hence has to solve a trade-off between making services affordable and providing incentives for agents to extend service coverage.

In light of this trade-off, evaluating whether reducing agents' discretion over prices improves welfare requires knowledge of two parameters: the price elasticity of demand (*"in-*

[1]For example, Sappington and Weisman (2010) in their review of evidence on regulatory policy state: *"When competition is unable to impose meaningful discipline on incumbent suppliers of essential services, regulation can be employed as an imperfect substitute for the missing market discipline."*

[2]Recent newspaper headlines include: *"Hillary Clinton Proposes Cap on Patients' Drug Costs"* (New York Times, 2015); *"FCA proposes price-cap for payday lenders"* (Financial Conduct Authority, 2014); *"Kenya to cap interest rates on bank loans"* (Financial Times, 2016).

[3]Chaudhury et al. (2006), for example, document an inability of governments to limit absenteeism among teachers and health workers in developing countries.

[4]In Tanzania, the setting of this study, the government cites the binding fiscal constraints and high monitoring costs to explain the absence of universal service obligations.

[5]The incentive value of discretion provides a possible explanation for an observed correlation between worker autonomy and project completion in the public sector (Rasul and Rogger, 2016). Duflo et al. (2014) also study how agents' discretion affect the effectiveness of public operations but, instead of focusing on incentives, highlight that discretion allows public agents to use private information.

tensive margin elasticity") and the impact of price-cap regulation on the number of markets served (*"extensive margin elasticity"*). While empirical estimates of demand elasticities are available for many goods and services, empirical evidence on the latter parameter is limited. As a result, little is known about whether price-cap regulation enhances or reduces welfare.

This paper provides experimental evidence to address this gap. In particular, I estimate the intensive and extensive margin elasticities and examine the trade-off between discretion and price-cap regulation for an essential public service provided in developing countries: agricultural extension.[6] As the basis of this investigation, I focus on the provision of I-2 poultry vaccines that protect against Newcastle Disease (ND). ND is highly prevalent in East Africa and is lethal for infected birds, therefore posing a substantial economic risk for populations dependent on agriculture.[7] I-2 is the primary ND vaccine used by rural farmers in Tanzania. It is exclusively produced by the government and distributed directly to farmers by specialized government service delivery agents. These agents receive the vaccine at a subsidized price from the government and distribute it by travelling to farmers in a geographically defined service area, where they perform veterinary examinations and apply the vaccine.[8] Each service area has exactly one agent tasked with providing vaccines.[9] Agents collect user charges from farmers in order to finance the vaccine delivery and application.[10] While there exist privately supplied imperfect substitutes for I-2, access to them is limited to urban areas and larger scale farmers, and trust in the effectiveness of privately supplied vaccines is low.[11] There is no formal service charge schedule in place that regulates prices for vaccinations. This lack of competition and price regulation thus gives substantial price discretion to agents.

To investigate whether capping prices for ND vaccinations enhances welfare, I collaborate with the Tanzanian government. Working with the Ministry of Agriculture, Livestock and Fisheries, I allocate 550 wards, the main administrative units at which agents are organized, to one of two experimental groups.[12] 274 of these wards are assigned to the control group

[6] On average, African countries spend 5% of their budget on agricultural extension, compared to 8% spent on primary health services.

[7] Poor farmers are particularly affected by ND, as their livelihoods and asset holdings are especially dependent on livestock.

[8] Private provision is widely viewed as sub-optimal to public provision, as effective ND vaccination requires adequate handling and application of the vaccine as well as the ability to detect preexisting poultry infections. A central concern is that private market provision would compromise service quality which risks reducing the acceptance and adoption of I-2 vaccinations. For example, the formal instruction sheet for I-2 reads: *Never vaccinate chickens which are already unhealthy. If they die, the owner will blame you.*

[9] Agents are, on average, responsible for 4 villages. The government's ability to introduce competition *within* the current system is constrained by a lack of qualified extension staff.

[10] Financing service delivery agents through user charges is common in developed and developing countries. See section 2.2 for an overview.

[11] 11% of recipients in my sample report having access to an imperfect substitute. Such recipients are predominantly located in urban areas. The absence of a quality control system for privately provided vaccines makes I-2 delivery through public agents the only available option to obtain access to a certified ND vaccine.

[12] While the Newcastle Disease program covers most of Tanzania, this study focuses on four regions:

that mirrors the Tanzanian status quo. Agents assigned to these wards have full discretion over pricing and typically charge recipients either 50 or 100 Shillings ($0.023 or $0.046) per vaccination. In contrast, 276 wards are assigned to the treatment group, in which agents face a price-cap of 80 Tanzanian Shillings ($0.035) per vaccination. Agents in both experimental groups have full discretion over which and how many recipients are served and which geographic areas are targeted.

The evaluation of this intervention yields three main results. First, the price-cap is effective in reducing agents' discretion over prices. I find that the intervention reduces average prices charged by agents by 17% and the within-agent standard deviation of these prices by 42%. I also show that farmers whose main source of livelihood is agriculture and smallholders were paying higher prices in the status quo and benefit particularly from the price-cap. Second, the price-cap affects the composition of the beneficiary pool: I find that the cap increases the share of previously unserved recipients in the beneficiary pool by 14%. In addition, the price-cap increases the share of farmers whose livelihood depends on agriculture and smallholders in the recipient pool by 8% and 9%, respectively. Third, the price-cap reduces the geographic coverage of services by reducing the proportion of villages that are served in each service area by 12%. This reduction is driven by a reduced likelihood of agents visiting remote villages: Agents are 25% less likely to visit the furthest half of villages and 26% less likely to visit the most remote village in their area of responsibility. Taken together, the evaluation suggests that the price-cap induced agents to shift towards comparatively closer but previously unserved recipients. These results therefore present direct evidence in support of the trade-off discussed previously: On the one hand, price-caps can enhance welfare by making services affordable for previously unserved recipients in markets that are comparatively cheaper to serve. In addition, price-caps can redistribute surplus to recipients. On the other hand, price-caps can adversely affect welfare by reducing agents' incentive to extend services to remote markets that are costlier to serve.

Given the countervailing effects induced by the price-cap, it is not clear whether it is welfare enhancing for the government to restrict the agents' discretion over prices and, if so, to what extent. To assess this, I develop a model of monopoly regulation that is consistent with the empirical results. Assuming that the government maximizes a weighted sum of consumer and producer surplus, this model allows me to express the aggregate social welfare effect of marginally reducing prices below the full discretion level as a function of two sufficient statistics: First, the elasticity of the proportion of villages served with respect to the price-cap, which acts as a sufficient statistic for the welfare loss incurred through the reduced number of markets served. Second, the price elasticity of demand, which captures the welfare benefits associated with reducing prices.

The relationship between price-caps and service allocation evaluated as part of the field

Dodoma, Iringa, Morogoro and Tanga. The sample includes all wards in those regions that were assigned a public service delivery agent at the time of the study.

experiment is sufficient to estimate the elasticity of the proportion of villages served with respect to the price-cap. However, the price-cap treatment is insufficient to estimate the elasticity of demand for I-2 vaccinations.[13] To address this, I induce additional experimental variation in costs. In particular, I independently assign agents in 273 wards to a second treatment that requires them to contribute a lump-sum participation fee of 25,000 Tanzanian Shillings (approximately $11.40) to cover the cost of the vaccine if they agree to participate in the program.[14] Agents in the remaining 277 wards receive the vaccine for free. This intervention generates variation in prices because, as I show in a lab-in-the-field experiment,[15] agents choose prices based on average instead of marginal costs. Using this variation, I obtain an estimate of the aggregate elasticity of demand of -1.22.

I then combine these estimates to evaluate the effect of marginally reducing agents' discretion over prices using the sufficient statistics formula. I find that marginally reducing prices below the monopoly level induces a social welfare *loss* of between 3% and 12% of total sales revenue per agent. In contrast, a counterfactual scenario that ignores the supply effects induced by the intervention suggests that price cap regulation would lead to moderate welfare increases of between 0.5% to 2.5% of total sales revenue per agent on the margin. Taken together, this result highlights the importance of studying the effect of price-interventions on service coverage: While price-caps can increase welfare by making services more affordable, reducing agents' discretion over prices can be counterproductive when agents also have discretion over which markets to serve.

This paper provides evidence on the welfare effects induced by imposing price-caps on monopolistically supplied services. By doing this, it complements a literature on pricing for public services. Theoretical work in this area has investigated how to optimally regulate providers of public services. Laffont and Tirole (1986), show that optimal level of regulation creates a tension between rent-extraction and efficiency. In deriving the optimal regulation mechanism, they show that price-caps can be optimal for highly efficient firms. While they also show that price-caps might allocate excessive rents to less efficient firms compared to alternative contracts, both Laffont (2005) and Alonso and Matouschek (2008) argue that in the absence of transfers and verifiable information on costs, price-caps remain the optimal regulatory policy.[16] This paper provides direct and causally identified empirical evidence on the effectiveness of price-cap regulation and the rent-extraction versus efficiency trade-off.

Empirical evidence on pricing for essential services in developing countries has focused primarily on demand effects induced by prices. For example, Cohen and Dupas (2010) in-

[13]This is because disentangling supply and demand responses using one source of variation is difficult.

[14]To avoid challenges associated with liquidity constraints, this fee was collected after the vaccination campaign had ended.

[15]The lab-in-the-field experiment is explained in detail in Appendix D.

[16]Empirical evidence that tests those theories to date has primarily focused on the comparison of various regulatory mechanisms with each other. For example, the review by Abel (2000) highlights that incentive regulation through price-caps in the telecommunication market reduces prices and provides incentives to invest in infrastructure compared to rate of return regulation.

vestigate how charging for insecticide treated bed nets affects demand and use. Similarly, Ashraf, Berry, and Shapiro (2010) show that prices for water-purifies can act as a screening device for high-use customers. This paper complements this work by highlighting the importance of considering the interaction between supply and demand decisions as the basis of setting prices for public service provision.

More broadly, this paper provides field-experimental evidence on the welfare effects induced by allocating discretion to agents. By doing this, it complements a new and rapidly growing literature that investigates the organization of the public sector in developing countries.[17] While previous empirical work has focused on understanding how contracts for public agents can be designed to encourage effort (e.g. Khan, Khwaja, and Olken, 2016; Muralidharan and Sundararaman, 2011 and Olken, Onishi, and Wong, 2014), a more recent stream of theoretical literature has begun to take into account that contracts in practice are rarely complete.[18] This poses a central question as to who should have decision rights in cases not covered by the employment contract. While theoretical work highlights that allocating discretion to agents in situations not covered by contracts can act as an incentive (Aghion and Tirole, 1997) empirical evidence on this mechanism is rare. Previous empirical work has primarily focused on documenting a correlation between employee autonomy and public project completion (Rasul and Rogger, 2016) as well as improved information use as a result of regulatory discretion (Duflo et al., 2014). This paper is, to the extent of my knowledge, the first to show the existence of incentive effects as a result of discretion.

The remainder of this article is structured as follows. Section 2 describes the relevant features of the setting in which the study takes place. Section 3 outlines the experimental design and section 4 presents the results. Section 5 theoretically conceptualizes the effects of the price-cap intervention and derives a sufficient statistics formula to evaluate the welfare impact of capping prices. Section 6 presents the welfare analysis. Section 8 concludes.

2 Setting

This project explores the effect of price-caps in the context of agricultural and livestock extension services in Tanzania, which is a public service delivery program administered by local governments and coordinated nationally by the Ministry of Agriculture, Livestock and Fisheries.[19] The service aims to subsidize animal health and production services to make them available to small-scale rural farmers who are excluded from private input markets. It hence provides crucial economic infrastructure in an economy in which over 60% of households depend on livestock for their livelihoods, and where livestock is the primary asset held by rural households.

[17]The review by Finan, Olken, and Pande (2015) provides a comprehensive overview of this literature.

[18]See Grossman and Hart (1986) for the theoretical foundations of this argument.

[19]This division of responsibilities is the common organizational form for extension in Africa (Crowder et al., 2002) and Latin America (Wilson, 1991).

In light of their importance, livestock and agricultural extension services are one of the key services provided in developing countries (Swanson, Farner, and Bahal, 1989; Feder, Willett, and Zijp, 1999). According to the United Nations Food and Agricultural Organization, governments in Africa spend on average 5% of their total annual expenditure on agricultural services, which is only slightly lower than their expenditure on health (8%), with a total global spending on extension services estimated at $31 billion in 2008. In total, 10,891 extension agents were employed in Tanzania in 2012, comprising approximately 5% of local government staff. This leaves extension workers as the third highest proportion of government employees, after education and health services.[20]

2.1 Agents

The agents responsible for delivering livestock extension services in Tanzania are para-veterinarians, who are employed by local governments. Agents have advanced professional qualifications and typically hold a diploma from specialized training institutes in animal health, animal production or general agriculture. They are responsible for 1 to 12 villages, averaging 4 villages per agent, and typically operate in areas where the private coverage of livestock services is low. Agents work by themselves and have their own geographically defined area of responsibility, in which they face no competition from other public providers. The main organizational unit of agents at the local level are wards, which are accumulations of roughly 8 villages. There are, on average, two agents per ward. Agents in the same ward interact on a daily basis and, while maintaining their own geographic areas, typically coordinate their work.[21] There was no entry or exit of agents during my study period.

Agents' primary task is to travel to farmers in order to provide services. Around 25% of agents have access to a government motorcycle, whereas the remainder travels by foot and uses public motorcycle taxis and buses to reach farmers.[22] The government does not provide any reimbursements of travel costs. Instead, service delivery is completely funded through user fees.

2.2 User Fees

A key component of agents' contract is their compensation structure. Delivery agents are compensated through a mix between government wages and user fees. Specifically, agents receive a flat compensation of around $200 per month.[23] In addition, agents can collect user fees from farmers, which cover delivery costs and act as performance pay. In the status quo

[20] Total numbers of local government staff in 2007 were 224'114, with 148'607 being teachers and 39'217 being health workers.

[21] To avoid spillovers and interaction, treatments are assigned at the ward level. See also section 3.1.

[22] This process of service delivery is the modus operandi for a number of key service delivery programs in developing countries. For example, Ashraf, Bandiera, and Lee (2016) study community health workers in Zambia. Community health workers are expected to devote 80% of their time to household visits and are hence required to incur similar costs as the agents in my setting.

[23] Wages vary across local government administrations.

local governments have allocated full discretion over pricing to the agents.

Existing evidence shows that the rationale for employing user fees in extension is three-fold. First, user fees are equivalent to commissions for private sellers, which provide high-powered incentives for agents to exert effort. This addresses one of the key challenges in public provision of extension services, which have traditionally suffered from a lack of mechanisms to induce providers to exert effort (Howell, 1986; Farrington et al., 2002). Second, high monitoring costs make performance pay schemes, in which payments from the government are linked to output, infeasible. User fees reduce monitoring costs as they delegate monitoring responsibilities to the recipients of services, who can directly observe output (Anderson and Feder, 2007; Kidd et al., 2000). In the presence of high monitoring costs, the alternative to user fees are therefore fixed wages, which provide no incentives to agents. Third, user fees are a cost-sharing device that reduce pressure on local governments' budgets to fund service delivery (e.g. Cary, 1998).

While cost-sharing schemes are common for agricultural and livestock extension programs (Rivera and Gustafson, 1991; Dancey, 1993), compensation schemes that partially rely on user payments are also present for other public service delivery schemes, such as health services and food distribution.[24] On the one hand, there is a substantial amount of evidence documenting bribes paid to public service delivery agents, which, while illicit, play a similar role to a user fee.[25] More formally, a number of countries and organizations have, either temporarily or permanently, switched to a system that relies on user fees to cover expenditure of health facilities.[26] For example, Deserranno (2016) studies community health workers recruited by BRAC, an international NGO, in Uganda. Those workers are tasked with providing basic health services to local residents and are compensated through medication sales to service recipients. Similarly, food distribution systems such as Solidaridad in the Dominican Republic and Raskin in Indonesia rely on co-payments to finance distributors and local government agents, respectively (Busso and Galiani, 2014; Banerjee, Hanna, Kyle, et al., 2015).

2.3 Services

Agents provide a range of services to recipients. This includes preventive animal health treatments such as vaccinations, deworming procedures and reactive treatment to address common livestock diseases. Unregulated user fees are charged for all animal health services.[27]

[24] Countries that have, among others, implemented such schemes include Cameroon, Chad, Mali, the Central African Republic, India, Kenya, Nicaragua, China, New Zealand, the Netherlands, Chile, Australia as well as most OECD countries. See the comprehensive reviews by Haan et al. (2001) and Anderson and Feder (2007) for details.

[25] See, for example, Deininger and Mpuga (2005) for an overview from Uganda.

[26] Examples include Burkina Faso, Kenya, Papua New Guinea , Uganda, South Africa, Colombia, Sudan and Lesotho. For an overview of those experiences, see the review paper by Lagarde and Palmer (2008).

[27] The program also aims to provide recipients with traditional extension services, such as advice regarding animal husbandry practices and information on optimal feed composition. As the extent to which agents

As part of this project I focus on the provision of I-2 vaccines as one dimension of service provision.[28] I-2 is a thermotolarent vaccine for poultry that protects against Newcastle Disease (ND), a viral disease that is transmitted between birds and leads to almost 100% mortality in affected and unvaccinated chicken. Estimates from Tanzania suggest that more than 30% of chicken die from ND every year, leading to an annual cost of up to $78 Million (Msami, 2007). As part of an I-2 vaccination program, agents receive subsidized vaccines from the government and then travel to recipients in order to apply vaccinations to farmers' livestock.

Four characteristics of I-2 service provision make it particularly suitable for my study. First, the Tanzanian government is the only producer and provider of I-2 vaccinations. This gives delivery agents market power and allows them to extract surplus from recipients.[29] In addition, this characteristic, together with the fact that there is no competition between public providers, simplifies the interpretation of my results as it alleviates concerns that the treatment shifts market shares between different providers. Second, the public provision of I-2 is based on a vaccination calendar which requires a coordinated vaccination effort on a four-monthly basis.[30] During such campaigns, agents' primary task is the provision of vaccinations. As this study focuses on vaccination periods, this reduces the concern that the intervention might affect the effort agents exert on alternative tasks. Third, in order to eradicate ND, vaccination levels in the poultry population need to be maintained at at least 85% (Boven et al., 2008). Given high turnover rates of flocks, the fact that an important transmission channel of ND is through non-domesticated birds, and the low coverage of vaccination programs, Tanzania's system is unlikely to eradicate ND in the near future. In light of this argument, I simplify the analysis by abstracting from externalities and focus on consumer surplus in my welfare analysis.[31] Finally, I-2 is the only public animal health service provided to poultry keepers. Although I do not observe prices for other services provided by agents to I-2 recipients, this property allows me to investigate potential cross-price effects by investigating whether the treatment induces agents to target more non-poultry farmers.

engage in providing those services and the prevalence of user fees for such services is more heterogeneous, the main focus of this investigation is on animal health services.

[28] By focusing only on I-2, I ignore possibly compounded adverse effects of the intervention induced by an additional reduction in other services provided to remote villages. Estimates are therefore a lower bound.

[29] Private provision is widely viewed as sub-optimal to public provision, as effective ND vaccination requires adequate handling and application of the vaccine as well as the ability to detect preexisting poultry infections. A central concern is that private market provision would compromise service quality which risks reducing the acceptance and adoption of I-2 vaccinations.

[30] Vaccination campaigns follow regional rainfall patterns and typically take place in January, May and September. Campaigns last three weeks before the lack of cooling renders the vaccine unusable.

[31] A complementary argument in favor of focussing on I-2 relates to the common criticism of public extension services in Africa that the services provided are largely ineffective and add little to farmer productivity (e.g. Dejene, 1989; Gautam, 2000). Focusing on a vaccine for which effectiveness has been medically proven alleviates this concern.

3 Experimental Design and Data

The experiment discussed in this paper examines how price-cap regulation affects public service delivery in the context of the public provision of I-2 Newcastle Disease vaccination. This section explains the experimental design before describing the data used as the basis of this evaluation.

3.1 Experimental Design

This paper examines the welfare effect of an intervention that imposes and enforces a maximum price ("price-cap treatment") for I-2 vaccinations. To avoid spillovers resulting from coincidental interactions between agents, treatment assignment was performed at the ward instead of at the individual level. I randomly and independently assign each of the 550 wards in the study to either the control or the treatment group. Table 1 displays the basic experimental design. Group allocation was stratified by 108 strata, where each stratum was defined by a district identifier and two binary variables, indicating whether all agents in the ward had specialized in general agriculture and whether only one agent was assigned to the ward.

This intervention was carried out during the first I-2 vaccination campaign of 2016.[32] The study covers the time period between January and February 2016 and enumerated the universe of agents in four of Tanzania's 30 regions (Dodoma, Iringa, Morogoro and Tanga). Figure 1 shows a map of the study regions and the wards included in this study. All 27 districts in the four regions were included in the study. The study area was chosen to include a wide variety of agricultural environments while assuring geographic proximity to the ministry headquarter in Dar Es Salaam. From each study district, I obtained administrative records of all employed agents, detailing their name, specialization, ward of responsibility and telephone number. In total, I collected this information for all 990 agents registered in the four regions, which forms the provisional sample of this study. 832 of those agents attended the training and participated in the vaccination campaign.

3.2 Implementation Procedures

All participants were invited to attend a 90-minute meeting at the district headquarter at the beginning of the campaign to collect the vaccine and receive instructions. Agents who attended this meeting received a show-up fee to cover their transport expenditure. Payments varied between 10,000 and 50,000 Tanzanian Shillings ($4.50 to $22), depending on the distance and available transport methods. The specific instructions were announced to participants only after they had arrived for vaccine collection at the district headquarters but before they departed to the field again. Thus the decision whether to attend the vaccine collection should be viewed as exogenous with respect to the experiments. Trainings and

[32] The timing is described in detail in section B.2.

surveys were conducted on different days for the different experimental groups and districts to avoid spillovers.

During this meeting, agents in the control group were reassured that they were allowed to collect fees from farmers which they could keep for themselves. Agents were specifically encouraged to profit financially from the transaction, stating that the government viewed user fees as a way to motivate employees and compensate them for good performance. In addition, the instructions reiterated that agents were allowed to charge farmers any price they chose and that it was acceptable to charge different prices to different farmers.

Agents were then informed that the ministry wanted to keep better records of how many chickens were vaccinated and that thus reporting procedures during this campaign would differ slightly from the status quo. In particular, a condition of participation in the vaccination campaign was that agents would issue formal receipts to every farmer served and submit the receipt information directly to a central headquarter using a phone based reporting system. Agents were specifically told that the ministry would contact farmers to verify that the information provided on receipts was correct. In order to assure compliance with this reporting system and encourage effort, the ministry offered a bonus payment of 60 Tanzanian Shillings (approximately $0.025) for every verified vaccination.

After the instructions, training staff collected data on demographics, work history and workplace characteristics of participants. Ministry staff then distributed the vaccines to agents, supplying agents with as many doses as they requested for their area of responsibility and informing them that more doses would be stored at the district headquarter where they could be picked up in case of additional demand.

To facilitate the experiment, procedures during this vaccination campaign differed slightly from the normal vaccination cycles with respect to vaccine distribution. During typical vaccination cycles, agents are required to purchase vaccines at subsidized prices when collecting supplies from local headquarters but are allowed to refinance themselves using user fee payments. As piloting suggested that liquidity constraints lead to low agent participation under this system, agents were provided with free vaccines from the central government during this campaign.

3.3 Price-Cap Treatment

Compared to the control group, the instructions given to agents in the price-cap treatment differed only with regards to the rules on pricing. In particular, ministry officials informed participants that they were free to set any price up to 80 Tanzanian Shillings (approximately $0.035) per vaccination. This cap was calibrated to balance two considerations. On the one hand, it had to be sufficiently low to be binding in order to affect agents' pricing and allocation behavior. On the other hand, it had to be sufficiently high to allow agents to cover their marginal costs. To achieve this balance, the maximum price of 80 Tanzanian Shillings was chosen after careful consultations with experts from the Tanzanian Veterinary Labo-

ratory Agency (the vaccine's main producer), MALF, local governments and international academics. In addition, this decision was also based on a mixed methods pilot study conducted by the author that analyzed pricing behavior during previous I-2 campaigns. To avoid setting a price-cap that would not allow agents to recover their marginal costs of applying the vaccine, the cap was conservatively set to bind only for comparatively high prices.

As price-caps are only effective if they can be enforced, I took the following measures to ensure compliance with the price-cap. First, the receipts that are normally employed during campaigns were amended to contain the national emblem of the United Republic of Tanzania, transforming them into official government documents. As receipts require the delivering agent's signature, forging them is equivalent to tempering with official government documents which is punishable by law and can lead to dismissal. Anecdotal evidence suggests that this incentive mechanism was taken serious: local government level supervisors requested detailed information on verified compliance behavior by their employees in the aftermath of the intervention to discipline non-compliant employees.

Second, MALF conditioned the bonus payment of 60 Tanzanian Shillings per vaccination on compliance with the price-cap. This scheme makes it incentive compatible to comply with the price-cap as long as deviation yields a price lower or equal to 140 Tanzanian Shillings per vaccination and the detection probability is sufficiently high. Given that 99% of transactions in the control group were conducted at user charges below this threshold, compliance was incentive compatible for the vast majority of transactions.

3.4 Data

Data used as the basis of this paper was collected from two different sources: administrative government receipts and a survey of service recipients. I designed and conducted the recipient survey specifically as part of this project. In addition, I implemented a new procedure of reporting service provision receipts via text message to increase accuracy and usability of the administrative data. I complement this data using information from a baseline survey of agents, described in detail in appendix section C.1.

The information provided on official government receipts and the number of receipts issued constitutes my provisional outcome data. The receipts detail each recipient's name, contact number, village, the date of the visit, the total user fee collected and the number of vaccinations applied. After issuing the receipt, agents electronically transmitted the receipt information to a government database using a text-message template.[33]

Using the receipt data, I construct two unverified, and therefore provisional, outcome measures. First, the total user fee collected divided by the number of vaccinations applied gives a direct measure of the per unit price charged to farmers. Second, the total number of farmers served can be measured through the total number of receipts submitted.

After the end of the vaccination campaign, I administered a survey to service recipients.

[33]In total, agents issued 31'657 valid receipts, accounting for 702'762 animals vaccinated.

The survey was conducted over a period of six weeks between March and April 2016 and sampled a randomly selected fraction of 15% of all receipts submitted, selected randomly and stratified by agent. This led to a total sample of 4'516 receipts selected for surveying and verification,[34] 80% of which were successfully contacted.[35] The farmer survey collected detailed information on the service provision and on recipient characteristics, thereby verifying that the service was actually provided and collecting verified information on user fees.

I use the information obtained from this survey to construct my main outcome measures. In order to arrive at a measure of the total number of farmers served, I multiply the number of verifiable receipts per agent by the agent-specific sampling weight of each receipt. I repeat the same procedure for the average price, total revenue collected and the total number of chickens vaccinated. In order to analyze outcome measures related to service allocation, I use farmer survey data on farmer demographics, asset holdings distance between farmers' home and the agent's headquarter and farmers' sources of livelihoods.[36]

4 Results

This section presents the empirical methodology and results from the evaluation of the price-cap treatment. Table 2 presents summary statistics and a balance check using baseline characteristics of agents participating in the campaign. All characteristics in the table were chosen prior to estimating the balance checks. These results suggest that experiment participants are similar across the treatment and the control group. Panel A considers agent level characteristics while panel B investigates differences in workstation characteristics. None of the 28 differences are statistically significant at the 5% level, which confirms balance at baseline.

The presentation of the results proceeds in two steps. Subsections 4.1 and 4.2 present results that investigate how the price-cap affects transaction prices. Subsection 4.3 then focuses on the allocation and extension of services to show the central trade-off between affordability and coverage necessary for welfare analysis.

4.1 Impact of the Price-Cap on Prices

I begin the evaluation by estimating the impact of the price-cap treatment on user fees charged over the course of the vaccination campaign. As treatment assignment was randomized, the empirical methodology is straightforward. I estimate Ordinary Least Squares (OLS) equations of the following form:

[34] Rounding errors induced by the stratification led to a sample that is slightly smaller than 15% of 31657.

[35] The procedures to contact farmers are described in detail in section C.3. Among the farmers not reached, enumerators were unable to reach 42% because of incorrect or invalid contact details. In total, phone survey procedures therefore were able to assess the validity of almost 90% of receipts sampled. I treat the remaining receipts as unverifiable and hence incorrect.

[36] While it would have been optimal to conduct a detailed consumption survey as part of this exercise in order to obtain a more precise measure of farmers' livelihoods, budget limitations rendered this option infeasible.

$$y_{iwd} = \beta_0 + \beta_1 PriceCap_{wd} + \beta_2 X_{wd} + \gamma_d + \epsilon_{iwd}$$

where y_{iwd} is the outcome of interest for participant i in ward w and district d, $PriceCap$ is a binary variable that indicates whether agents' wards were assigned to the price-cap or the participation-cost treatment, and X_{wd} denotes a vector of ward-level stratification variables.[37] The coefficient of interest is β_1. I also include district level fixed-effects (γ_d), as the assignment lottery was stratified by these strata. As the treatment is perfectly correlated within wards, every specification reports robust standard errors clustered at the ward level.

I first investigate the effect of the price-cap treatment on the distribution of prices. Panel A in figure 2 plots a histogram that visualizes the distribution of prices in the status quo, using farmer survey data from the control group.[38] Prices follow a bimodal distribution with peaks at 50 and 100.[39] Panel B overlays the distribution of prices in the treatment group over the histogram from the control group. Significant bunching at 80 suggests that the price-cap was binding and effectively reduced the level of prices.[40] Columns 1 and 2 in table 3 confirms this finding by showing that the price-cap reduced average prices by approximately 17%, which is statistically significant at the 1% level.[41] Column 5 confirms the visual impression of bunching at 80 by showing that this intervention increased the fraction of transactions per agent where a price of 80 Shillings was charged by a factor four, from around 5% to 20%.

I then investigate the impact of the price-cap on the within-agent variation of prices. To do this, I calculate the residuals of a regression of prices on agent fixed effects. Figure 3 presents a box-plot of the residuals, separated between treatment and control group, to visualize the effect of the treatment on price variation. The height of the box corresponds to the difference between the 25^{th} and 75^{th} percentile of residuals, and the whiskers correspond to the 10^{th} and 90^{th} percentile, respectively. I find that the intervention reduced price disparities between recipients. In particular, the figure shows that within-agent price variation

[37] X_{wd} also contains an indicator for whether a ward was assigned to a cross-cutting treatment ('Participation-Cost Treatment) explained in more detail in section 6. Note that, as I find no evidence of interaction effects between the price-cap and the cross-cutting treatment, I treat both as separate experiments. Given that the two treatments were assigned as part of a cross-cutting design, treatment effects of the price-cap intervention should therefore be interpreted conditional on 50% of the sample being assigned to the cross-cutting treatment.

[38] To improve the visualization, the histograms are truncated at 200 Tanzanian Shillings, which excludes less than 1% of all observed transaction.

[39] As expected when designing the intervention, the price-cap hence only binds for a subset of transactions. The histogram also shows that less than 1% of transactions in the control group occur at prices above 140 Tanzanian Shillings, which assures that complying with the price-cap is incentive compatible.

[40] Figure 2 also suggests that the price-cap intervention increased the mass of the price distribution for prices significantly *below* the cap. Anecdotal evidence suggests that this is driven by difficulties with calculating multiples of 80 and a tendency to round down to the nearest 1000 for the total price.

[41] The estimate using the farmer survey data is slightly lower than the estimate obtained from the receipt data, which is partially driven by (detected) under-reporting of prices on the receipts.

is substantially lower in the treatment group than in the control group.

Columns 3 and 4 in table 3 show that this reduction in variation is also statistically significant. Column 3 reports the estimate of the treatment effect on the within-agent standard deviation of prices using farmer survey data, whereas column 4 repeats the same analysis using receipt data. The results suggest that the treatment reduced the within agent variation of prices by 42 to 44%, which is statistically significant at the 1% level. The results don't differ substantially between the receipt and the farmer survey data.

4.2 Who benefits from the Price-Cap?

The bimodal distribution of prices in the control group and the evidence of within-agent price variation suggest that different farmers, even if they are served by the same agent, pay different prices. To gain an understanding of the impact of the price-cap on welfare, it is important to understand which farmers pay higher prices and thus are more likely to be affected by the cap. To do this, I present two pieces of evidence. First, table 4 presents the correlates of unit prices in the control group. Three results are worth noting. First, column 2 shows that agents offer lower per-unit prices for larger flocks. In particular, every additional vaccination applied by the agent is associated with a quantity discount of 0.12 Shillings. Second, indicators of education and asset holdings suggest that more disadvantaged recipients also, ceteris paribus, pay higher vaccination prices. Column 4 shows that recipients who have more than primary education pay 9 Shillings less per vaccination, and columns 5 to 7 suggest that increased holdings of non-poultry livestock assets are associated with lower per vaccination prices. The estimates of the partial correlations remain significant even after controlling for self-reported travel time to the recipient. Third, while there is some evidence that prices are related to travel and application costs, it is unlikely that this variation can conclusively explain the observed variation in prices. Column 1 in table 4 first shows that, while not statistically significant, an additional minute of walking to the recipient's village, as measured through self reported walking distance, is associated with marginally higher vaccination prices. I then investigate the extent to which measures of cost, in particular walking and motorcycle travel times and the number of vaccinations applied, can explain the within-agent price variation. I find that while individually the indicators show associations with prices, controlling for cost measures only explains 7% of the within-agent price variation. Taken together, this is suggestive that agents use discretion over prices in the status quo to extract surplus.

Second, in light of the previous results, it is instructive to investigate who benefits most from the price-cap. To investigate this, I run regressions at the transaction level that take the following form:

$$Price_{fwd} = \beta_0 + \beta_1 PriceCap_{wd} + \beta_2 K_{fwd} + \beta_4 PriceCap_{wd} \times K_{fwd} + \beta_5 Z_{fwd} + \beta_6 X_{wd} + \gamma_d + \epsilon_{fwd}$$

where $Price_{fwd}$ denotes outcome variables for recipient f in ward w and district d, K_{fwd} denotes a characteristic of the recipient and Z_{fwd} denotes control variables at the recipient level and X_{wd} controls at the agent level. Control variables at the farmer level include measures of travel distance. Control variables at the agent level contain stratification variables and indicators for the participation-cost treatment. Farmer level regressions are weighted to obtain equal weights for each service delivery agent. Standard errors are again clustered at the ward level.

Table 5 presents the results of this exercise. In column 1 I estimate the effect of the intervention on transaction prices and allow the treatment effect to vary depending on whether the recipients' livelihoods depend on agriculture. Approximately 80% of households in the control group match this definition. The results show that while the point estimate for the treatment effect on prices is negative for all farmers, it is small in absolute terms and statistically insignificant for non-agricultural households but approximately 50% larger and statistically significant at the 10% level for households whose main livelihood is derived from agriculture.

Column 2 shows that agents do not only price differentially based on farmers' livelihood characteristics but also on the number of chickens vaccinated per farmer. In particular, agents offer lower per-unit prices for larger flocks. Table 5, column 3 shows that farmers who own fewer than 11 chickens on average pay 14 Tanzanian Shillings (or 18%) more per vaccination than farmers with larger flocks.[42] Reducing discretion not only reduces average prices for all recipients by 12% but also eliminates this quantity discount. Taken together, the price-cap intervention appears to particularly benefit agriculturally dependent households and smallholders, who are likely to be poorer and hence more susceptible to shocks to livestock holdings.

4.3 Impact on Service Allocation

The previous section has shown that price-caps affect prices. While this directly affects the distribution of surplus, price-caps primary welfare implications operate through their effect on the allocation of services. This section highlights two channels through which reducing discretion over prices affects service allocation. First, price-caps increase the affordability of services. This increases the likelihood of agents extending services to previously unserved recipients and, in light of the evidence on differential pricing presented in the previous section, to agricultural households and smallholders. Second, price-caps reduce agents' expected profits from serving a given village, which in turn reduces their incentives to incur the travel costs associated with travelling to remote villages.

[42] When asked about the motivation for this pricing strategy agents mentioned that quantity discounts were needed to convince larger flock holders to bear the higher total cost of the service.

4.3.1 Impact of Price-Cap on Composition of Beneficiary Pool

I first investigate how price-caps affect the composition of the beneficiary pool. As the previous section has shown, price-caps reduce prices on average and do so in particular for agricultural households and smallholders. Price-caps therefore not only redistribute surplus but also increases the affordability of services. The intervention therefore should increase the likelihood that agents will provide services to previously unserved beneficiaries, conditional on the agent offering the service to them. Consistent with this, columns 1 and 2 in table 6 show that the price-cap indeed increases the proportion of previously unserved recipients in the beneficiary pool by 12% to 15%.

I then investigate how this price-cap affects the proportion of farmers in the recipient pool who benefited particularly from the reduction in discretion, namely those whose main source of income is derived from agricultural production and those with comparatively small chicken flocks. As discussed in section 4.2 and shown in table 5, reducing discretion reduces transaction prices more for such recipients. Columns 3 and 4 in table 6 suggest that this price-adjustment indeed leads to a positive demand effect, as households whose main source of income stems from agriculture are 6% more likely to be served in response to the price-cap treatment, conditional on being offered the service. While not statistically significant, the point estimate in columns 5 and 6 in table 6 suggest that smallholders are 9% more likely to be served in the price-cap group.

Taken together, the results presented in this section suggest that in the absence of price-caps agents use their discretion to extract rents from service recipients. Capping prices redistributes surplus to recipients, crowds in previously unserved farmers and makes services more affordable for recipients in need.

4.3.2 Price-Caps reduce Geographic Coverage

While the previous sections have shown that price-caps reduce prices and increase the proportion of new recipients and recipients with a high need for the service in the beneficiary pool, it is unclear how capping prices affects the aggregate coverage of services. In particular, it is possible that reducing discretion reduces agents' incentives to extend services to markets that are more costly to serve. For the setting studied in this paper, reducing agents' discretion might reduce their incentives to extend services to remote villages. Price-caps can therefore reduce welfare by discretely eliminating aggregate surplus obtained from serving a given market.

This section provides evidence in support of this mechanism. To do this, I merge information on villages and travel distances with the farmer survey and the receipt data. In particular, a list of all villages in their area of responsibility and the approximate travel time by foot from their headquarter to each village was collected from agents during the baseline survey. I use the data on travel times to rank the villages by their distance to the agent's headquarter. I then match the village information provided during the farmer survey or on

the receipt to the village list collected during the baseline survey, to obtain information on whether agents visited a given village.[43]

Table 7 shows how capping prices affects which villages agents visit. Column 1 shows that while agents in the status quo visit approximately 37% of villages that they are assigned to, the price-cap reduces this proportion by 4.5 percentage points. Columns 2 and 3 confirm that this reduction is driven by a reduced likelihood of agents visiting remote villages: they are 25% less likely to visit villages whose distance from their headquarter is above median, and 26% less likely to visit the furthest village in their area of responsibility. Taken together, this suggests that price-cap regulation reduces agents' incentives to serve more remote markets.

4.3.3 Impact on Remote Farmers in Need

I provide an additional piece of evidence on the aforementioned trade-off by focusing on the availability of I-2 substitutes in villages as a dimension of heterogeneity. In my sample, approximately 11% of farmers have access to an imperfect substitute for I-2 provided through private markets. Table 5 shows that while the price-cap was effective in reducing prices by roughly 15 Tanzanian Shillings, this effect is driven exclusively by transactions with farmers who don't have access to this substitute. In contrast, the treatment effect for farmers with access to the substitute is positive, small and statistically indistinguishable from zero. This suggests that competition induced by the substitute reduces the surplus available to agents, hence driving prices to a level where the price-cap does not bind. Price-caps therefore only affect the surplus available to the agent in areas where the substitute is unavailable.

As the substitute is also more likely to be absent in remote areas, this reduction in available surplus resulting from the price-cap substantially reduces agents' incentives to target villages without access to the substitute. To assess this intuition, column 2 in table 8 investigates how price-caps affect the likelihood that a farmer with no access to the substitute will be served. The results show that the price-cap treatment reduces the likelihood that farmers without access to the substitute are served by 3% to 4%. Taken together, these results further highlight the aforementioned tension: price-caps make services accessible to farmers in need. But when those farmers live far away, price-caps can be counter-productive, as caps reduce the likelihood that agents will travel to the remote farmers.

4.3.4 Impact on Total Number Served

Given the counterveiling forces discussed previously, it is unclear whether the price-cap will increase or decrease the total number of farmers served. Figure 4 investigates this question and shows little evidence of the price-cap affecting the total number of farmers served. The figure separately plots the daily number of farmers served for the treatment and the control group using receipt data. This shows that the difference between the daily number of farmers

[43] Approximately 11% of receipts were unmatchable to villages. This can either be because the information provided in the surveys or on the receipts was incorrect or because recipients live outside of the formal villages. Reassuringly, the likelihood of an agent visiting an "unmatched" recipient is uncorrelated with the treatment.

served is statistically indistinguishable from zero for 18 out of the 21 days of the campaign. Column 1 in table 9 confirms this impression: agents in the price-cap treatment only serve an average of 3.6 fewer farmers than agents in the control group. This difference is statistically insignificant.

I conduct two robustness checks to verify this result. First, a possible concern is that the result is a composite effect between a participation response on the extensive margin and an effort response on the intensive margin. To address this, I restrict the sample to agents who verifiably served at least one farmer, therefore ruling out responses on the extensive margin. Column 2 in table 9 confirms that the result is robust to this restriction: ruling out extensive margin responses, agents in the treatment group serve on average 5 fewer farmers than agents in the control group, which remains statistically insignificant. Second, I consider the impact of the treatment on the total number of vaccines applied. Column 3 in table 9 shows that while the point estimate for the treatment effect is negative for the number of farmers served, it flips sign for the total number of chickens vaccinated while remaining insignificant. Taken together, those results suggest that while the price-cap affected the *types* of farmers served, I am not able to detect an effect on the total number of recipients served.

5 Sufficient Statistics Model for Welfare Analysis

The previous section has shown that price-caps have three key effects on service provision. First, they reduce average prices and the within-agent variation of prices. Second, they *increase* the proportion of new recipients and recipients who were paying higher prices in the status quo in the recipient pool. Third, they *reduce* the likelihood of agents visiting remote villages. To conceptualize these effects and to understand their effect on welfare, this section develops a model that is consistent with the empirical results and allows for the estimation of welfare effects through a sufficient statistics formula.

I model I-2 provision as a slot assignment problem in which slots are assigned through two allocation mechanisms. First, suppliers choose which villages to visit. While agents are responsible for a given service area, the model takes into account that visiting a given village requires agents to pay a travel cost. Agents' willingness to pay this travel cost then determines service allocation *between* villages in a given service area. Second, agents choose prices that determine which recipients are served, conditional on the agent visiting their village.

5.1 Model Setup

This model considers a situation in which a monopolistic agent is supplying services to a population of potential customers, the size of which is normalized to 1. Customers are defined by their valuation of the service, which is denoted by v_i. I assume that v_i is a continuous random variable drawn from a distribution $F(v)$. I further assume that recipients' elasticity of demand is given by ϵ_D^i which can either be high or low: $\epsilon_D^i \in \{\epsilon_D^L; \epsilon_D^H\}$. Suppose that a

fraction μ of recipients has ϵ_D^L, whereas everyone else has ϵ_D^H. Customers do not only differ with regard to their valuation, but also in their location. Specifically, I assume that recipients live in a continuum of villages with differing travel distances to its agent's headquarter. I further assume that the distribution of valuations is the same for every village.

Agents in this model face two sequential choices. First, they decide which markets to serve by determining the allocation of services *between* villages. I assume that travelling to village j requires paying a cost of c_j which agents can choose to either pay or not. I assume that c_j is drawn from a distribution with c.d.f. $M(c)$ defined over $[0; c_{max}]$ with $c_{max} < \infty$. Second, agents decide on prices, which determine service allocation *within* a village, conditional on the village being served.[44] For simplicity, I model this by assuming that agents offer a take-it-or-leave-it price based on observable recipient characteristics. While I allow for price-discrimination, I assume that agents do not observe v_i and instead only learn about ϵ_D^i.[45] If customers accept the agent's offer, the agent receives the agreed sum and delivers the service at a constant cost τ. If the recipient rejects the price offer, no transaction takes place, but the agent still has to pay the travel cost to the village.

5.2 Effect of Price-Cap on Observables

To aid the interpretation of the empirical results, this section shows that the implications of the model are consistent with the three main empirical results. In particular, I show how agents' pricing decisions as well as the allocation of services are affected by the price-cap intervention. Section 5.3 then uses the empirical estimates to evaluate the marginal welfare effect of the price-cap.

Effect of Price-Cap on Prices

To begin the analysis of the model, note first that travel costs to villages are sunk at the time of price setting. This allows me to investigate the agent's two decisions separately. To understand the effect on prices, notice that agents face a monopoly trade-off: raising prices increases profit from a transaction but reduces the likelihood that farmers will accept the price. Formally, visiting a recipient of type i yields the following expected profit:

$$\pi_i = \left[1 - F\left(p_i | \epsilon_D^i\right)\right] [p_i - \tau]$$

[44]This implicitly assumes that travel costs within a village are 0. It is straightforward to relax this making travel costs farmer specific.

[45]Anecdotal evidence is consistent with this assumption. Agents report that negotiations with farmers regularly break down and that they are unable to charge similar farmers different prices. They do, however, mention that it is possible for them to give discounts based on observable characteristics, such as household wealth and on the number of chickens held by the household. While this description is in line with my model, it is inconsistent with alternative bargaining models, such as uniform pricing, first-degree price-discrimination and Nash bargaining. See also section B.3.

Pointwise maximization of the objective function yields the standard monopoly pricing solution:

$$p_{Dec}\left(\epsilon_D^i\right) = \tau\left(\frac{\epsilon_D^i}{1+\epsilon_D^i}\right) \tag{1}$$

Agents set prices based on a mark-up over marginal costs, with a low-elasticity of demand leading to high mark-ups. This model therefore gives rise to a bimodal price distribution in which the price-cap is more likely to bind for recipients with a low elasticity of demand. To explore the effect of the cap, assume that, consistent with the empirical design, the price cap only binds for recipients with a low elasticity of demand. Prices under a price-cap $\bar{p}$ are hence given by:

$$p^{Reg}\left(\epsilon_D^L\right) = \bar{p} \text{ and } p^{Reg}\left(\epsilon_D^H\right) = \tau\left(\frac{\epsilon_D^H}{1+\epsilon_D^H}\right)$$

This directly implies that the price-cap mechanically reduces the agent's ability to price-discriminate between recipients with a high and low elasticity of demand. Price-caps hence reduce the standard deviation and average of prices in equilibrium and particularly benefit recipients with a low demand elasticity.

Effect of Price-Cap on Beneficiary Pool

In order to understand how price-caps affect service allocation, I now investigate how price-caps affect the distribution of the different elasticity types in the recipient pool. This requires me to investigate recipients' acceptance decisions, conditional on their village being served. After receiving a price offer, recipients decide whether to accept or to reject it. In equilibrium, recipients accept every offer that does not exceed their willingness to pay. The distribution of types in the recipient pool is then:

$$\mu D\left(p\left(\epsilon_D^L\right)|\epsilon_D^L\right) + (1-\mu)\, D\left(p\left(\epsilon_D^H\right)|\epsilon_D^H\right) \tag{2}$$

where $D\left(p\left(\epsilon_D^i\right)|\epsilon_D^i\right)$ denotes the demand for customers of type i.

Note also that here the demand curve is downward sloping. If the price-cap binds only for recipients with a low elasticity of demand, it is hence straightforward to see that price-cap regulation will increase their demand. This increases the likelihood of serving previously unserved recipients and shifts the distribution of types in the direction of recipients with a low elasticity of demand.

Effect of Price-Cap on Village Choices

To understand the agent's coverage decision, I investigate the effect of the price-cap on the choice of villages conditional on a price vector $\mathbf{p}$. In the status quo, agents decide to visit village j if the expected profit exceeds the associated costs:

$$\mu\pi_{\epsilon_D^L}(\mathbf{p}) + (1-\mu)\,\pi_{\epsilon_D^H}(\mathbf{p}) \geq c_j$$

where $\pi_{\epsilon_D^i}(\mathbf{p})$ denotes the expected profit obtained from recipients with elasticity equal to ϵ_D^i. This defines a cut-off value for c_j, denoted by $c^*(\mathbf{p})$, which is the highest cost village visited by the agent. The proportion of villages visited in the status quo is hence given by:

$$\sigma = M(c^*(\mathbf{p})) \tag{3}$$

Regarding allocation between villages, agents are hence more likely to serve a larger proportion of villages when (i) the expected profit is higher and (ii) the proportion of recipients with a high elasticity of demand in each village is lower.

To understand how price-caps affect the allocation of services between villages, denote by π_j^{Dec} and π_j^{Reg} the agent's expected profit from visiting village j in the status quo and under the price-cap, respectively. As agents maximize their profits with respect to prices in the status quo, it follows that:

$$\pi_j^{Dec} \geq \pi_j^{Reg}$$

Profits play a dual role in this model. First, profits allow agents to extract surplus. Second, however, profits also compensate agents for the travel costs to remote villages incurred. This is necessary because travel costs are sunk when price offers are made. Pricing decisions therefore do not ensure that the agent breaks even in remote areas. Differentiating equation 3 with respect to prices shows how reducing discretion affects the targeting of remote areas:

$$\frac{\partial \sigma}{\partial p_{\epsilon_D^L}} = M'(c^*)\,\mu \frac{\partial \pi_{\epsilon_D^L}}{\partial p_{\epsilon_D^L}} < 0$$

This implies that the price-cap reduces the proportion of villages visited. This is because reducing discretion reduces the amount of surplus agents can extract from remote villages, which lowers the highest travel cost they can pay and still break even. Taken together, the model shows that price-cap regulation generates a tension between preventing surplus extraction to make services affordable for recipients with a low elasticity of demand and providing incentives to serve remote villages.

5.3 Effect of Price-Cap on Welfare

The previous section has shown that price-caps crowd-in previously unserved recipients and redistribute surplus at the expense of remote farmers. Given those countervailing forces, it is not clear whether it is welfare improving for the government to cap prices, and, if so, to what extent. To address this, this section first investigates the government's policy decision to motivate the choice of price-caps as a regulatory instrument. It then solves the government's objective function to derive an expression for the marginal effect of capping prices as a function of empirically estimatable sufficient statistics.

5.3.1 Regulatory Policy

The government's objective is to maximize social welfare. Denoting by g_{c_H}, g_{c_L} and g_a the government's welfare weight on high elasticity customers, low elasticity customers and the agent, social welfare for a generic price vector is given by:

$$
\begin{aligned}
&SWF(\mathbf{p}) = \\
&M(c^*(\mathbf{p}))\left[g_{c_L}\mu\int_{p(\epsilon_D^L)}^{\infty} v_i - p(\epsilon_D^L)\,dF(v|\epsilon_D^L) + g_{c_H}(1-\mu)\int_{p(\epsilon_D^H)}^{\infty} v_i - p(\epsilon_D^H)\,dF(v|\epsilon_D^H)\right] \\
&\quad + g_a M(c^*(\mathbf{p}))\left[\mu\left(p(\epsilon_D^L)-\tau\right)D(\mathbf{p}|\epsilon_D^L) + (1-\mu)\left(p(\epsilon_D^H)-\tau\right)D(\mathbf{p}|\epsilon_D^H)\right] \\
&\qquad - g_a\int_0^{c^*(\mathbf{p})} c_j\,dM(c) \qquad (4)
\end{aligned}
$$

Social welfare therefore consists of a weighted sum between consumer surplus and the agent's profit. To build intuition, it is instructive to define the first best regulatory policy for when the government cares equally about producers and consumers ($g_{c_H} = g_{c_L} = g_a = 1$). Suppose that in the first-best the government can make transfers to the agent and enforce which villages the agent serves. Denoting transfers by t, it is straightforward to see that the optimal regulatory contract implements the following policies:

$$
\begin{aligned}
\bar{p} &= \tau \\
c^* &= c^{max} \\
t &= \int_0^{c^*(\mathbf{p})} c_j\,dM(c)
\end{aligned}
$$

As denoted above, the optimal regulatory policy in the first best scenario sets prices equal to marginal costs, mandates the agent to serve all villages and uses transfers to reimburse the agent's travel costs.

In reality, fiscal constraints prevent governments from paying transfers to the agents. In addition, governments are constrained by moral hazard, which limits their ability to mandate which villages the agent visits. Under those circumstances, Laffont (2005) notes that the optimal regulatory policy includes price-caps that rule out agents' most opportunistic choices.

5.3.2 Sufficient Statistics Formula for Uniform Pricing

The central objective of this paper is to understand the effect of the price-cap on welfare. To achieve this, I take a sufficient statistics approach to determine the welfare effect of marginally lowering prices below the full discretion level. This approach has three key advantages. First, by expressing welfare effects as a function of reduced form parameters, it allows me to use

the empirical results to evaluate the optimal price-cap for the service I study. Second, my approach requires me to make no structural assumptions on agents and recipients behavior. Third, it uses estimates from the non-marginal experimental intervention to investigate the effect of *marginally* lowering prices below the full discretion level. This makes the welfare results less dependent on the chosen value of the price-cap.

For tractability, this section will derive the sufficient statistics formula for the case of uniform pricing and then postulate the appropriate extension to third-degree price-discrimination discussed previously. I present the derivation for this extension in the appendix. Governments choose the price-cap to maximize social welfare. Analogous to equation 4, social welfare for the uniform price case is given by a weighted sum between consumer and producer surplus:

$$SWF\left(p\right)=M\left(c^{*}\left(p\right)\right)\left[\int_{p}^{\infty}v_{i}-pdF\left(v_{i}\right)\right]+gM\left(c^{*}\left(\bar{p}\right)\right)\left(p-\tau\right)D\left(p\right)-g_{a}\int_{0}^{c^{*}\left(p\right)}c_{i}dM\left(c_{i}\right) \quad (5)$$

g denotes the welfare weight on agents relative to recipients. This formulation allows to consider the welfare effect for scenarios in which governments value only consumer surplus ($g=0$) and in which governments take into account aggregate surplus ($g=1$).

Starting from unregulated prices, the marginal welfare effect of lowering prices has three first order effects on welfare. First, on the extensive margin, marginally lowering prices reduces the fraction of villages served, which leads to a discrete loss in consumer surplus. Second, on the intensive margin, lowering prices reduces the monopoly distortions within a village, as it closes the gap between prices and marginal costs τ. Third, reducing prices redistributes surplus from agents to consumers, which has a direct effect on social welfare if the government values surplus accruing to recipients more than surplus accruing to agents.[46] Taking derivatives of equation 5 and using the definition of $c^{*}\left(p\right)$, the marginal effect on welfare is given by:

$$\begin{aligned}\frac{\partial SWF\left(p\right)}{\partial p} &= \frac{\partial M\left(c^{*}\left(p\right)\right)}{\partial p}\int_{p}^{\infty}v_{i}-pdF\left(v_{i}\right)\\ &- M\left(c^{*}\left(p\right)\right)g\left(p-\tau\right)\frac{\partial D\left(p\right)}{\partial p}\\ &- M\left(c^{*}\left(p\right)\right)D\left(p\right)\left(1-g\right)\end{aligned}$$

The first and second term capture the extensive and intensive margin effects, respectively.

[46] In addition to those effects, there are also two second-order effects. First, reducing prices increases demand, which has a second order effect on welfare because buyers on the margin were indifferent between purchasing and not-purchasing in the first place. Second, reducing prices reduces providers' profit from the villages that are no longer visited. This effect is second order because the expected profit from the marginal village was 0 in expectation during the status quo.

The third term captures the redistributive effect. To derive a formula based on sufficient statistics, it is useful to define two parameters. First, I denote by θ the extensive margin elasticity of village visits with respect to the price-cap. Formally:

$$\theta = \frac{\partial M\left(c^{*}\left(p\right)\right)}{\partial p}\frac{p}{M\left(c^{*}\left(p\right)\right)} \tag{6}$$

Second, I denote by ε_D the price elasticity of demand:

$$\varepsilon_D = \frac{\partial D\left(p\right)}{\partial p}\frac{p}{D\left(p\right)} \tag{7}$$

Finally, notice that consumer surplus at price p is given by:

$$CS\left(p\right) = \int_{p}^{\infty} v_i - p dF\left(v_i\right) \tag{8}$$

The above definitions, together with the fact that total number of farmers served is given by $N\left(p\right) = M\left(c^{*}\left(p\right)\right) D\left(p\right)$, yields the following proposition:

Proposition 1. *The welfare effect of marginally reducing prices below the uniform monopoly pricing level can be estimated using θ and ϵ_D as sufficient statistics:*

$$\frac{\partial SWF\left(p\right)}{\partial p} = \theta N\left(p\right)\frac{CS\left(p\right)}{pD\left(p\right)} + \varepsilon_D g N\left(p\right)\frac{p-\tau}{p} - \left(1-g\right)N\left(p\right) \tag{9}$$

To understand the intuition behind this formula, consider two scenarios. First, suppose there are no distortions associated with the exploitation of market power. In this case $p = \tau$ and the intensive margin benefit of capping prices disappears. Second, suppose the government puts equal weight on surplus accruing to agents and customers. In this case $g = 1$ and the last term, which captures the redistributive effect of the price-cap, disappears.

It is straightforward to extend this analysis to price-discrimination when there are two types of buyers in the market: One for whom the elasticity of demand is high and one for whom it is low. I denote by ε_D^L and ε_D^H the demand elasticities of the low and high elasticity customers, respectively. In addition, I denote by μ the share of low elasticity customers in the market. The following proposition then describes the sufficient statistics formula that allows for the estimation of welfare effects:

Proposition 2. *The welfare effect of marginally capping prices for consumers with a low-*

elasticity of demand under third-degree price-discrimination is given by:

$$\frac{\partial SWF(\mathbf{p})}{\partial p\left(\varepsilon_D^L\right)} = \theta N(\mathbf{p}) \frac{g_{c_L}\mu CS\left(p|\varepsilon_D^L\right) + g_{c_H}(1-\mu) CS\left(p|\varepsilon_D^H\right)}{p\left(\varepsilon_D^L\right)\left(\mu D^L(\mathbf{p}) + (1-\mu) D^H(\mathbf{p})\right)} \tag{10}$$

$$+ \varepsilon_D^L g_a \mu N^L(\mathbf{p}) \frac{p\left(\varepsilon_D^L\right) - \tau}{p\left(\varepsilon_D^L\right)} \tag{11}$$

$$- \left(g_{c_1} - g_A\right) N^L(\mathbf{p})\mu \tag{12}$$

Here $N(\mathbf{p})$ denotes the total number of services provided and $N^L(\mathbf{p})$ the number of services provided for recipients with a low elasticity of demand. Further, g_{c_L}, g_{c_H} and g_a denote the government's welfare weights on low elasticity customers, high elasticity customers and the agent, respectively.

6 Welfare Analysis

The previous discussion has shown that estimating the marginal welfare effect of price-cap regulation requires knowledge of two parameters: The extensive margin elasticity of village visits with respect to the price-cap and the intensive margin elasticity of demand. This section first discusses the estimation of the elasticity of demand before using those estimates to evaluate the welfare effect of capping prices.

6.1 Estimation

While the price-cap treatment allows me to estimate the extensive margin elasticity of village visits with respect to the price-cap,[47] I require additional variation in prices to estimate the latter. This is because the variation induced by the price-cap generates responses both on the extensive and the intensive margin. Traditionally estimating demand elasticities would require experimental variation in marginal costs. In my case, this would imply generating variation in vaccination costs. Introducing such variation in the context I study is challenging as it generates incentives for agents to report fewer vaccinations than were actually conducted.[48] To overcome this challenge, I leverage the findings from a lab-in-the-field experiment that shows that agents choose prices based on average instead of marginal costs. I describe this experiment in more detail in the appendix of this paper. The knowledge that agents choose prices on average costs allows me to induce price variation through a treatment that varies fixed costs, which does not generate any incentives to incorrectly report vaccination.

[47] Alternatively, one could allow travel costs to vary at the farmer level. In this case, the responses would not be calculatable directly from the price-cap treatment, but could be backed out using the elasticity of total recipients served with respect to the price-cap and the price elasticity of demand. My results are qualitatively robust to this alternative.

[48] As vaccines expire after 3 weeks without cooling, there is no formal system in place that requires agents to return unused vaccines to the headquarter.

6.1.1 Participation-Cost Treatment and Elasticity Estimates

To estimate the price-elasticity of demand, I induce additional experimental variation in costs. In particular, I independently assign agents in 273 wards to a cross-cutting treatment that requires them to contribute a lump-sum participation fee of 25,000 Tanzanian Shillings (approximately \$11.40) to cover a portion of the vaccine cost if they agree to participate in the program. To avoid concerns about liquidity constraints, MALF allowed agents to cover this fee through charges from farmers and collected the funds after the completion of the vaccination campaign. Agents were explicitly given the choice whether (i) to accept the cost, perform vaccinations and thereby collect user fees and bonus payments or (ii) to reject participation without any obligation to pay for the proportion of the vaccine costs. The ministry repeatedly emphasized that there would be no repercussions from refusing participation. Agents in the remaining 277 wards receive the vaccine for free. The randomization was designed so that the probability that each ward received a given treatment was always held constant, regardless of what stratum the village was in and whether an alternative treatment had been cross-randomized. The probability of being in the price-cap group is therefore orthogonal to having to pay a contribution to the vaccine cost.

The participation cost treatment successfully induced variation in prices. Table 10 presents the treatment effect estimates from the participation cost intervention. Column 1 shows that imposing a lump-sum cost raises average prices charged by around 11%.[49] Column 2 in table 10 shows that this increase in prices reduced the average number of farmers served by around 12%. As column 3 shows, the participation cost treatment therefore only reduced collected revenue by 3%, which is statistically insignificant.[50] I then use this variation to estimate the price elasticity of demand. To do this, I run agent level instrumental variable regressions in which I regress the log of the total number of farmers served on log prices. I instrument for prices using an indicator for an agent's assignment to the participation cost treatment. Column 4 in table 10 presents the results from this exercise. The estimate of the price elasticity of demand is -1.223, making ND vaccinations a fairly elastic good.

6.1.2 Welfare Wedges

In addition to knowledge of the two sufficient statistics, θ and ε_D, which were estimated through the field experiment, the welfare analysis requires estimates of the welfare wedges $\frac{CS(p)}{pD(p)}$, which depends on consumer surplus, and $\frac{p-\tau}{p}$, which depends on marginal application

[49] Column 2 in table 12 shows that this is not a selection effect, as the participation cost treatment did not affect participation in the vaccination campaign.

[50] One might be concerned that the participation cost treatment did not only affect agents' costs but also strengthened their bargaining position and therefore allowed them to extract higher profits from service recipients. To avoid a direct impact on bargaining, the agents were not given any documentation that formally stated the requirement to remit a participation cost. Agents in the treatment group also were not more likely to mention the need to cover vaccine costs during bargaining with farmers, as column 2 in table 14 shows.

costs.

To estimate consumer surplus, I assume that demand follows a constant elasticity demand function. When demand is $D(p) = D_0 p^{\varepsilon_D}$, consumer surplus is then:

$$CS(p) = \int_p^\infty D_0 x^{\varepsilon_D} dx$$

which can be calculated directly.

Regarding estimates of $\frac{p-\tau}{p}$, I take two approaches. First, I obtain an estimate of τ from the monopolists' pricing problem. In particular, when profit maximizing monopolists set uniform prices, they maximize $\pi = (p - \tau) D(p)$. The solution to this problem yields the first way to estimate mark-ups:

$$\frac{p-\tau}{p} = \frac{-1}{\varepsilon_D}$$

The evidence obtained through the separate lab-in-the-field experiment suggests, however, that agents set prices as mark-ups over average instead of marginal costs. In light of this, I also bound the estimates by setting $\tau = 0$.

One additional complication arises, as the price-cap reduces the prices *per vaccination* and recipients typically purchase more than one vaccination. Assuming, for simplicity, a constant number of vaccinations per recipient, $N(p)$ then refers to the total number of vaccinations applied.

For the case of price-discrimination, calibration of the sufficient statistics formula requires three further parameters. First, I require separate estimates of demand elasticities for recipients with a high and a low elasticity of demand. To estimate those, I define smallholders, farmers in areas without private providers and households whose livelihood depends on agriculture as low-elasticity households. I then obtain demand elasticities by separately estimating demand functions for the two populations, using the participation cost treatment as an instrumental variable. The results of this estimation are presented in column 5 and 6 in table 10. The estimated elasticities of demand are -0.41 for the low elasticity types and -3.83 for the high elasticity types. Second, I require knowledge of μ, the share of low elasticity households. As I cannot obtain this directly from the data, I bound my estimates by setting μ to either 0, 0.5, or 1.

6.2 Results

Table 11 presents the results from the calibration of the sufficient statistics formula. Panel A shows the calibrated marginal welfare effects, whereas panel B shows the welfare effects for a counter factual scenario in which extensive margin effects are absent. Three results are worth noting. First, the calibrated marginal welfare effects are negative across the board. Panel A shows that marginally reducing prices below the full discretion level leads to a welfare loss to the magnitude of between 3% and 11% of total sales revenue per agent.

This suggests that the adverse effects of price-cap regulation on the extensive margin are so strong that any deviation from full discretion leads to a welfare loss. This directly implies that, for the setting I study, any form of price-cap regulation will cause welfare to decrease, which makes no regulation the optimal policy for ND vaccinations. Second, the implied welfare losses are substantially larger for the price-discrimination case as compared to the no price-discrimination case. This is natural, because the benefits of price-cap regulation under price-discrimination only accrue to a subset of a given village market compared to uniform pricing, whereas the adverse extensive margin effects affect the whole village market. Finally, panel B shows that, in the absence of extensive margin responses, price-cap regulation can lead to moderate welfare increases ranging from 0.4% to 2.5% of total sales revenue per agent on the margin. Taken together, the results therefore conclusively show that price-cap regulation generates a tension between intensive margin demand effects that increase welfare, and extensive margin effects that reduce welfare. For ND vaccinations in Tanzania, extensive margin effects are sufficiently strong to lead to a net-welfare loss, making price-cap regulation counter-productive.

7 Robustness

While the experiment and data collection procedures were designed to estimate the channel of interest, some caveats to the analysis exist that make alternative explanations possible. First, while all available agents in the enumeration region were assigned the vaccination task, some of them failed to attend the necessary training. There were several reasons for this: some were on annual leave, sick, on professional training or were assigned other long-term duties. This attendance gap poses a challenge to experimental validity if attendance rates differ between treatment and control groups. Table 12 alleviates this concern by showing that on average 83% of agents attended training. This result does not differ significantly between treatment and control groups.

Similarly, while all agents who attended training also collected vaccines, some of them failed to serve any farmers. A concern is that this failure to participate is a response to the treatment. Table 12 again alleviates this concern by showing that the treatment did not affect the participation decisions: among the 832 agents who attended training, 82% submitted receipts in the control group, compared to 84% in the price-cap group and 82% in the participation-cost group. This difference is not statistically significant. I therefore conclude that the treatments did not induce any response on the participation margin.

Second, a concern is that the price-cap generated incentives for selective reporting. In particular, agents might be tempted to report only transactions that comply with the price-cap, while not reporting transactions whose value exceeds the price-cap. The experimental design addresses this concern through the bonus payment, which assures that it is always incentive compatible to report transactions, as only formal reporting generates eligibility for the bonus of 60 Tanzanian Shillings per vaccination. Consistent with this assertion, the

farmer survey detected limited non-compliance with the price-cap: for only 4.5% of participants in the treatment group did farmers report paying prices that exceed 80 Tanzanian Shillings per vaccination.[51] To further validate this point, I investigate whether vaccine loss differs between the experimental groups. While the logistics of the vaccine distribution and storage render it infeasible to track every dose, I can proxy for leakage using the ratio between confirmed number of vaccinations and the initially distributed amount of vaccine doses.[52] Evaluating this proxy suggests that leakage rates were generally low, as the average proxy value is 96% in the control group. More importantly, this figure does not differ systematically between treatment and control, as table 14 confirms. It is therefore unlikely that systematic leakage and misreporting is influential enough to drive my results.

Third, it is possible that the treatment induced agents to report receipts for which no service was provided in order to receive the bonus payment. To investigate this possibility, the third column of table 12 shows that on average 69% of transactions reported by agents could be verified. This figure does not differ significantly between treatment arms.[53]

Fourth, although the data verification procedures are reassuring in interpreting the observed price effects as a real transfer of surplus, one potential concern is that these impacts might be due to undetected misreporting. A particular concern is collusion between the agent and the farmer in generating inaccurate receipts. While it is not possible to conclusively rule out this possibility, the experimental design requires a high level of trust to make collusion profitable. To see this, notice that if agents decide to misreport, they face a lottery which pays the unconstrained revenue plus the bonus payment if they remain undetected and only the unconstrained revenue if the fraud is detected. If agents choose to report correctly, they receive the constrained revenue plus the bonus payment in every state of the world. Assuming risk-neutrality to obtain an upper bound, the largest possible detection probability agents are willing to accept is given by the expected increase in revenue from misreporting divided by the bonus payment. The experimental data suggests that non-compliance on average yields an additional revenue of around 11'000 Tanzanian Shillings (Table 9, column 5) while detection would lead to the loss of approximately 70'000 Tanzanian Shillings in bonus payments. Agents therefore decide to misreport if their detection probability is lower than 15% for all farmers. With 50 farmers served on average, this implies that collusion is profitable if the probability that every farmer honors the agreement is above 99%. Taken together, the experimental design therefore generates very small incentives for non-compliance

[51] While this figure is small, it is still key to notice that, even under lower compliance levels, rules can still improve outcomes by assuring that those with a high-cost of non-compliance comply (see also Banerjee, Hanna, and Mullainathan (2013) for a discussion of this).

[52] Notice that this measure allows for fractions that exceed 1, as agents might have collected additional vaccines from the storage locations at later stages of the vaccination campaign.

[53] A similar concern is that misreporting is distributed unevenly across agents, implying a heterogeneity between honest reporters and employees who misreport their performance. Figure 7 addresses this possibility by investigating how the fraction of verifiable receipts varies across individuals and showing that the inability to verify receipts is evenly distributed between respondents.

that are unlikely to justify large-scale misreporting.

Fifth, while agents complied with the price-cap for the vaccination service, they might have increased prices on other services in response to the treatment. While data on prices for such transactions is not available, two factors make it unlikely that this mechanism is driving my results. First, fewer than 1% of respondents in the farmer survey reported paying a transport and consultancy fee in addition to the vaccination charge, which suggests that transactions on top of the user fees are rare. Most importantly, this figure does not differ systematically between treatment and control group. Second, I-2 vaccinations are the only large-scale profitable service that agents provide for poultry farmers. Instead, their main profit raising activities accrue from services for large ruminants, especially cattle. Any cross-price effects would therefore have to raise prices for cattle-related services. On the one hand, this implies that in the presence of cross-price effects agents in the price-cap treatment should be more likely to serve poultry farmers that also hold cattle, as this allows them to mitigate the effect of the price-cap. Data from this study rejects this hypothesis. Column 1 in table 13 shows that 29% of service recipients report owning at least one cow, which does not differ between treatment and control group. Having ruled out selection effects, I then investigate whether excluding cattle owners, and therefore potential cross-price effects, qualitatively changes my main results. Columns 2 to 4 in table 13 show that this is not the case. Column 2 confirms that the treatment still reduced average prices, whereas columns 3 and 4 show that the aforementioned composition effects in the recipient pool remain even when excluding cattle owners, although the reduced sample size has made the estimates less precise. Taken together, this evidence makes it unlikely that cross-price effects substantially challenge the presented interpretation of the results.

8 Conclusion

In this paper I evaluate whether capping prices for public services increases or reduces welfare. I combine administrative government data with survey data to evaluate a field experiment which investigates how capping prices for public livestock vaccinations in Tanzania affects service delivery. The evaluation yields three main results. First, the price-cap reduces average prices charged by agents. Second, the price-cap affects the composition of the beneficiary pool by increasing the proportion of previously unserved recipients. Third, price-caps reduce the proportion of remote villages served.

I then employ a model of monopoly regulation to derive a sufficient statistics formula which allows me to evaluate and decompose the welfare effect of the intervention. This analysis yields two findings. The first finding is that the decision whether to introduce price-caps has to address a trade-off between demand and supply considerations. On the one hand, price-caps can enhance the provision of services by making services more affordable. This redistributes surplus to recipients and crowds in new recipients in markets that are comparatively cheaper to serve. On the other hand, price-caps can also harm social welfare,

as such interventions reduce agents' incentive to extend service to markets that are costlier to serve. The second finding is that for public livestock vaccine service provision in Tanzania, the introduction of any form of price-cap regulation reduces welfare compared to the status quo.

A central contribution of the paper is to highlight the importance of incentive effects induced by capping prices. One implication of this finding is that public regulation which mandates that essential services should be provided for free or at very low prices can be suboptimal when governments cannot control or incentivize the agent to maintain a sufficient coverage of services. More broadly, this paper shows that it can be optimal to allocate discretion over prices to agents when contracts are incomplete, even though this allows agents to extract surplus. In addition to being informative about pricing policies for public services, this findings therefore also has broader implications for organizational design.

One limitation of this paper is that the experimental setting does not allow me to study long-term effects of the price-cap, such as possible dynamic effects induced by changes to the competitive structure and demand responses. Another limitation is that the experimental design does not allow for the comparison between alternative regulatory contracts. I hope to address those shortcomings in future work.

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Tables

Table 1: Treatment Groups

Price Cap	
No	**Yes**
274 wards (410 agents)	276 wards (422 agents)

Table 2: Summary Statistics and Balance Table

	Price-Cap Experiment		
Panel A: Agent Level	Control	Treatment	P-Value of Difference
Tenure	12.466	12.198	0.743
	(0.602)	(0.556)	
Ward Level agent	0.663	0.642	0.531
	(0.127)	(0.144)	
Number of Villages	4.022	3.974	0.804
	0.040	0.019	
Animal Health Specialist	0.434	0.476	0.241
	(0.025)	(0.025)	
Main Income Earner	0.866	0.864	0.938
	(0.017)	(0.018)	
Uses Motorcycle	0.446	0.400	0.204
	(0.024)	(0.027)	
Secondary Income Source	0.659	0.604	0.129
	(0.026)	(0.025)	
Acting Village Leader	0.144	0.152	0.753
	(0.017)	(0.018)	
Raises Livestock	0.798	0.796	0.934
	(0.021)	(0.020)	
Panel B: Work Station			
Rural	0.844	0.820	0.443
	(0.021)	(0.023)	
Average Travel Time	80.015	90.842	0.366
	(4.757)	(10.990)	
Private Veterinarian	0.156	0.197	0.176
	(0.020)	(0.022)	
Private Drug Seller	0.029	0.033	0.763
	(0.008)	(0.010)	
Poultry Area	0.076	0.062	0.433
	(0.013)	(0.012)	
Observations	410	422	

Notes: The sample includes all agents who agreed to participate in the experiment. Standard errors (clustered at the ward level) are reported in brackets. Travel time is reported in walking minutes.

Table 3: Effect of Price Cap on Price Variation and Levels

Outcome:	**Mean Price**		**Within Agent Price Variation**		**% at Price-Cap**
	(1)	(2)	(3)	(4)	(5)
Price-Cap Treatment	-12.82***	-14.95***	-8.848***	-8.706***	0.200***
	(3.148)	(2.177)	(2.027)	(1.896)	(0.0212)
Observations	679	769	679	768	679
Data Source	Farmer Survey	Receipts	Farmer Survey	Receipts	Farmer Survey
District FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Control Mean	76.28	73.01	20.53	20.80	0.051
Control St. Dev.	50.29	39.16	31.96	35.96	0.16

Notes: Standard Errors are clustered at the ward level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. All specifications control for stratification variables and district fixed-effects. Columns 1 and 2 present coefficient estimates of a regression of the within-agent standard deviation of prices on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Columns 3 and 4 present coefficient estimates of a regression of average price per chicken charged per agent on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. The outcome variable for columns 3 and 4 is the average price charged by participants. Column 5 presents coefficient estimates of a regression of the fraction of all transaction at 80 Tanzanian Shillings on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Columns 1, 3, and 5 employ farmer survey data whereas columns 2 and 4 use the receipt data.

Table 4: Recipient level correlates of prices in the control group

	Outcome Variable: Price per vaccination						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Walking time to village	0.0712						
	(0.0463)						
# Vaccinations		-0.115***					
		(0.0298)					
Main livelihood is agriculture			5.386				
			(4.689)				
More than primary education				-9.033*			
				(4.607)			
# Cattle owned					-0.176*		
					(0.0907)		
# Sheep owned						-0.283***	
						(0.0976)	
# Goats owned							-0.196**
							(0.0970)
Observations	1,556	1,562	1,554	1,552	1,549	1,551	1,550
Data Source	Survey	Survey	Survey	Survey	Survey	Survey	Survey
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard Errors are clustered at the ward level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. All specifications control for district fixed-effects and employ farmer survey data from the control group. Column 1 presents coefficient estimates of a regression of the price per vaccination on a continuous variable measuring the walking distance from the agent's headquarter to the farmer's home. Column 2 presents coefficient estimates of a regression of the price per vaccination on the number of vaccinations applied. Column 3 presents coefficient estimates of a regression of the price per vaccination on a binary variable indicating whether a household's main livelihood is derived from agriculture. Column 4 presents coefficient estimates of a regression of the price per vaccination on a binary variable indicating whether the household head has received more than primary education. Column 5 presents coefficient estimates of a regression of the price per vaccination on a continuous variable for the number of cattle owned by the farmer. Column 6 presents coefficient estimates of a regression of the price per vaccination on a continuous variable for the number of sheep owned by the farmer. Column 7 presents coefficient estimates of a regression of the price per vaccination on a continuous variable for the number of goats owned by the farmer.

Table 5: Heterogeneous Effects on Prices by Elasticity

Interaction Variable:	**Main Livelihood is Agriculture**	**Farmer is a Smallholder**
Outcome: Price	(1)	(2)
Price-Cap Treatment	-6.347	-10.27***
	(4.223)	(3.589)
Interaction Var.	5.978	13.82***
	(4.414)	(5.008)
Price-Cap × Interaction Var.	-9.382*	-13.61**
	(5.546)	(5.690)
Observations	3,043	3,045
Data Source	Farmer Survey	Farmer Survey
District Fixed Effects	Yes	Yes
Agent Controls	Yes	Yes
Farmer Controls	Yes	Yes

Notes: Standard Errors are clustered at the ward level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. All columns present regressions of transaction level prices on an indicator variable for the treatment, district fixed effects, ward-level stratification and control variables, a proxy for farmers' elasticity of demand and the interaction of this proxy with the treatment indicator. The proxy variables are binary variables indicating whether the recipient's main source of income is agriculture and whether households are smallholders who own fewer than 11 chickens.

Table 6: Effect of Treatment on Composition of Recipients, by Elasticity

Recipient Characteristic:	**Not served before**		**Main Livelihood is Agriculture**		**Farmer is a Smallholder**	
	(1)	(2)	(3)	(4)	(5)	(6)
Price-Cap Treatment	0.0565**	0.0612**	0.0543**	0.0470**	0.0150	0.0248
	(0.0250)	(0.0260)	(0.0272)	(0.0215)	(0.0137)	(0.0205)
Observations	832	3,095	832	3,096	832	3,098
Observation Level	Officer	Transaction	Officer	Transaction	Officer	Transaction
Data Source	Survey	Survey	Survey	Survey	Survey	Survey
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Agent Controls	Yes	Yes	Yes	Yes	Yes	Yes
Farmer Controls	N.A.	Yes	N.A.	Yes	N.A.	Yes
Control Mean	0.38	0.51	0.59	0.79	0.14	0.28
Control St. Dev.	0.38	0.50	0.40	0.40	0.19	0.45

Notes: Standard Errors are clustered at the ward level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. Columns 1 presents coefficient estimates of a regression of the proportion of farmers who have not received services before on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Columns 2 presents coefficient estimates of a regression of a binary variable indicating whether a farmer has received services before on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Columns 3 presents coefficient estimates of a regression of the fraction of farmers served per agent whose main livelihood comes from agriculture on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Columns 4 presents coefficient estimates of a regression of a binary variable indicating whether a recipient's main source of income is from agriculture on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Columns 5 presents coefficient estimates of a regression of the fraction of farmers served that own fewer than 11 chickens on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Columns 2 presents coefficient estimates of a regression of a binary variable indicating whether a farmer owns fewer than 11 chickens on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables.

Table 7: Effect of Treatment on Village Choices

Village Level Outcome:	**Proportion Visited**	**Above Median Distance**	**Furthest Village**
	(1)	(2)	(3)
Price-Cap	-0.0445**	-0.0966***	-0.0598**
	(0.0219)	(0.0292)	(0.0303)
Observations	832	832	832
District Fixed Effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Control Mean	0.37	0.37	0.23
Control St. Dev.	0.25	0.48	0.42

Notes: Standard Errors are clustered at the ward level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. Column 1 presents coefficient estimates of the proportion of villages visited in the agent's area of responsibility on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Column 2 presents coefficient estimates of a regression of a binary variable indicating whether agents visited a village that was further than the median distance of all villages to their headquarter on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Column 3 presents coefficient estimates of a regression of a binary variable indicating whether agents visited the furthest away village in their area of responsibility on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables.

Table 8: Effect of Treatment on Likelihood of Serving Remote Farmers in Need

Outcome Variable:	**Price**	**No substitute available**
	(1)	(2)
Price-Cap Treatment	3.887	-0.0355**
	(8.379)	(0.0177)
No Substitute	5.702	
	(3.982)	
Price-Cap × No Substitute	-20.07**	
	(8.520)	
Observations	3,044	3,097
Data Source	Farmer Survey	Farmer Survey
District Fixed Effects	Yes	Yes
Agent Controls	Yes	Yes
Farmer Controls	Yes	Yes
Control Mean	76	0.89
Control St. Dev.	50	0.31

Notes: Standard Errors are clustered at the ward level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. Column 1 presents coefficient estimates of the transaction price on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables and an interaction with whether a recipient has access to a private substitute for I-2. Column 2 presents coefficient estimates of a regression of a binary variable indicating whether a recipient has access to a substitute for ND vaccines on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables.

Table 9: Effect of Price Cap on Quantities and Revenue

Outcome:	**# Farmers Served**		**# Vaccinations**	**Revenue**
	(1)	(2)	(3)	(4)
Price-Cap Treatment	-3.450	-4.759	13.54	-11,675*
	(3.708)	(4.209)	(121.6)	(6,372)
Observations	832	679	832	832
Data Source	Farmer Survey	Farmer Survey	Farmer Survey	Farmer Survey
District Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Cond. on Participation	No	Yes	No	No
Control Mean	50.66	62.62	1'154.47	76'118.21
Control St. Dev.	56.27	56.21	1'614.46	96'206.8

Notes: Standard Errors are clustered at the ward level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. All specifications control for stratification variables and district fixed-effects. Columns 1 and 2 present coefficient estimates of a regression of the number of farmers served on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Columns 3 presents coefficient estimates of a regression of the number of vaccinations applied on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Columns 4 presents coefficient estimates of a regression of total revenue collected on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. All specifications employ farmer survey data.

Table 10: Participation Cost Treatment Effects on Prices, Quantities and Revenue

Outcome:	**Price**	**# Farmers**	**Participation**	**Log(Q)**	**Log(Q)**	**Log(Q)**
	(1)	(2)	(3)	(4)	(5)	(6)
Participation-Cost	7.155**	-6.693*	-0.00405			
	(3.106)	(4.009)	(0.0267)			
Log(Price)				-1.223	-0.413	-3.834
				(0.907)	(0.607)	(38.663)
Observations	679	679	832	679	594	395
Recipients	All	All	All	All	Low ϵ_D	High ϵ_D
Estimation Method	OLS	OLS	OLS	IV	IV	IV
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard Errors are clustered at the ward level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. Column 1 presents coefficient estimates of a regression of the average price per vaccination on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Column 2 presents coefficient estimates of a regression of the total number of farmers served on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Column 3 presents coefficient estimates of a regression of the revenue collected on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Columns 4 to 6 presents coefficient estimates of an instrumental variable regression of the log number of farmers served on the log of average prices charged, using the participation-cost treatment as an instrument. Column 4 presents the coefficient estimates for the whole sample, whereas columns 5 and 6 present the results separately for high and low elasticity recipients.

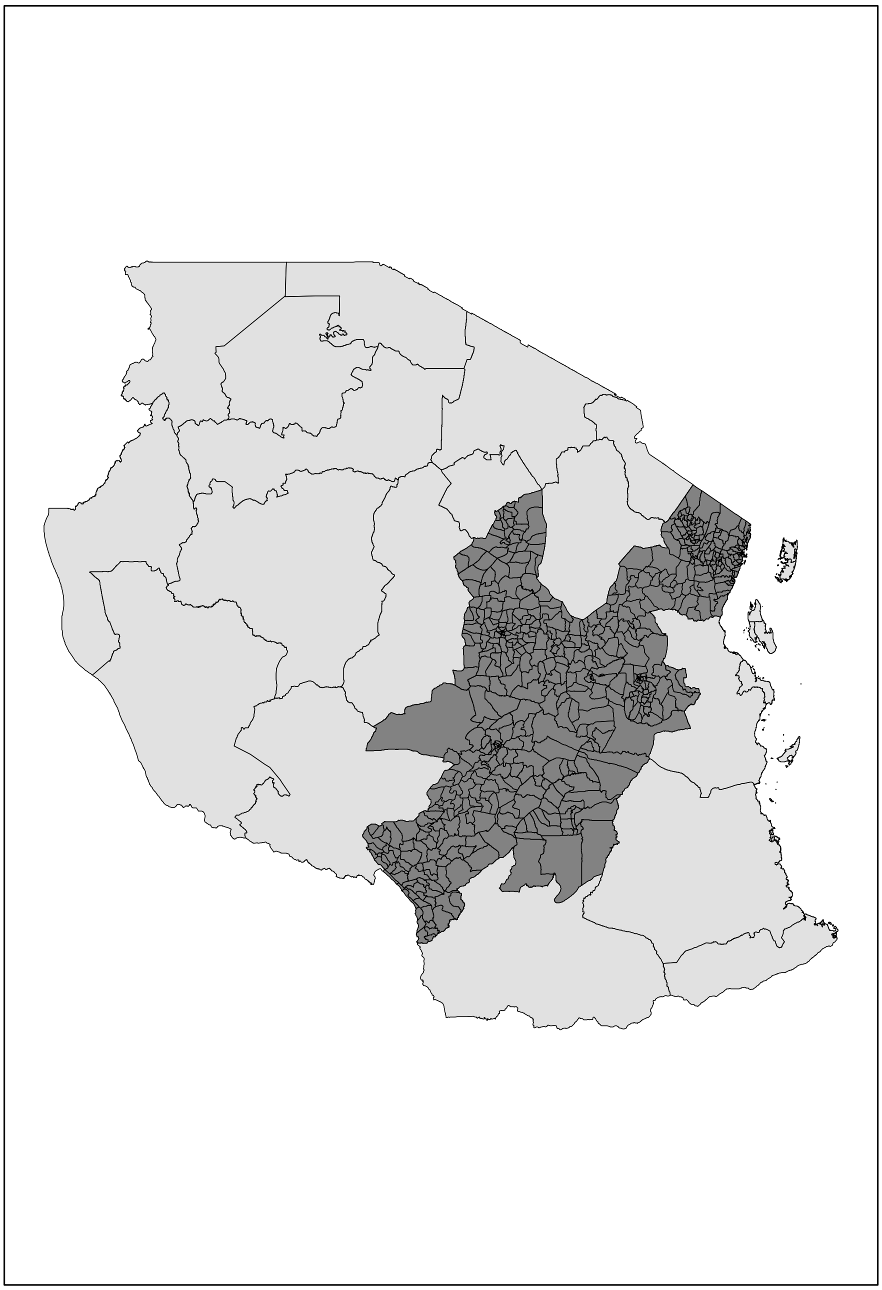
Table 11: Estimates of Welfare Effect of Marginally Reducing Prices

	Uniform Pricing		**Price Discrimination**					
	$g=0$	$g=1$	$g_a=0, g_{c_1}=g_{c_2}=1$			$g_a=g_{c_1}=g_{c_2}=1$		
			$\mu=0$	$\mu=0.5$	$\mu=1$	$\mu=0$	$\mu=0.5$	$\mu=1$
Panel A: Estimates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\tau=-\frac{1}{\epsilon_D}$	-4.62%	-4.62%	-3.33%	-10.55%	-17.77%	-3.33%	-11.57%	-19.82%
$\tau=0$	-4.62%	-4.17%	-3.33%	-10.47%	-17.62%	-3.33%	-11.50%	-19.67%
Panel B: Counterfactual								
$\tau=-\frac{1}{\epsilon_D}$	2.05%	2.05%	0%	1.37%	2.74%	0%	0.34%	0.69%
$\tau=0$	2.05%	2.50%	0%	1.45%	2.89%	0%	0.42%	0.84%

Notes: This table presents the results from the calibration of the sufficient statistics formulas. All estimates are expressed as percent of total sales revenue per agent. Columns 1 and 2 show the results for uniform pricing. Columns 3 to 8 show the welfare estimates measured using the sufficient statistics formula extended to third-degree price-discrimination. Panel A considers the aggregate welfare effects, whereas Panel B considers a counterfactual in which I ignore extensive margin responses.

Graphs

Figure 1: Map of Study Area



Notes: The figure shows a map of Tanzania. The study area, including the 550 sample wards, are shaded in darker grey.

Figure 2: Effect of Price Cap on Price Distribution

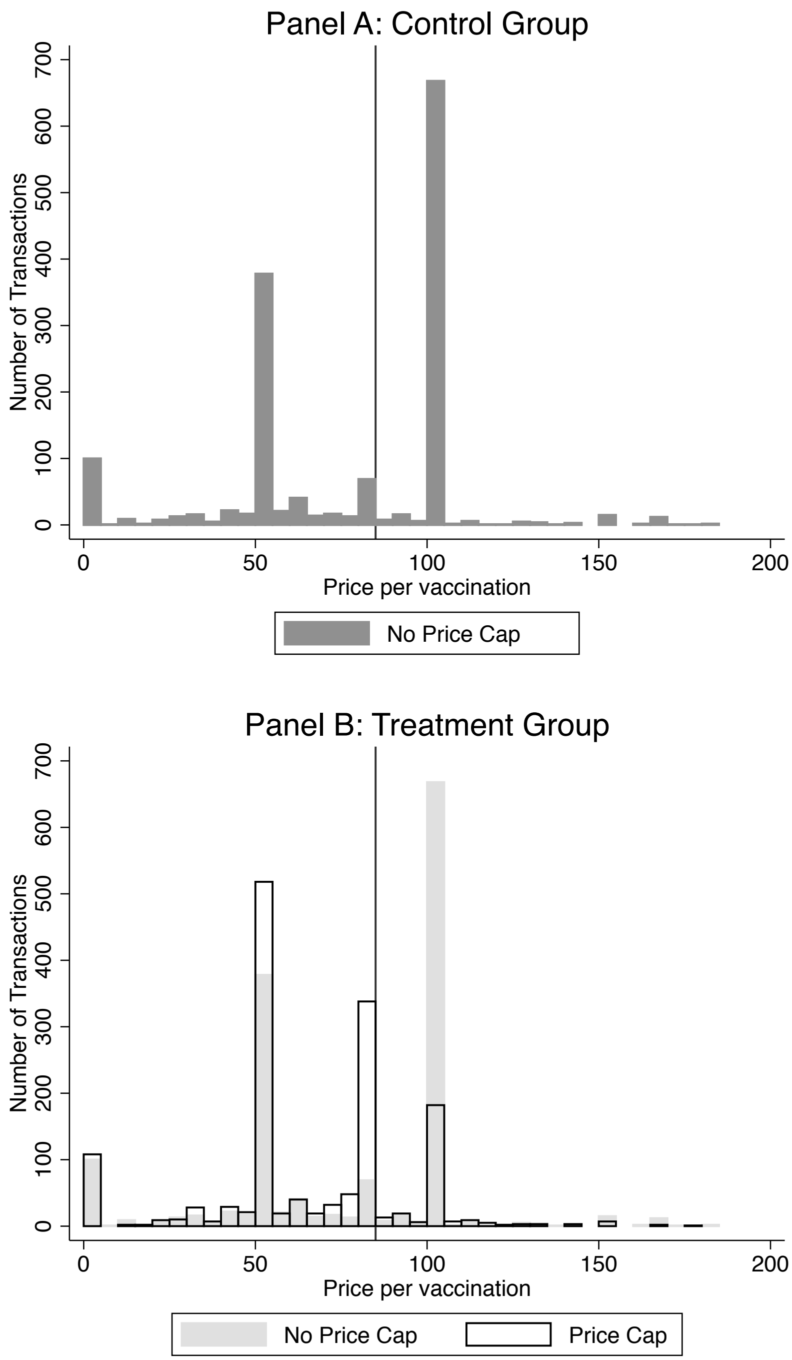
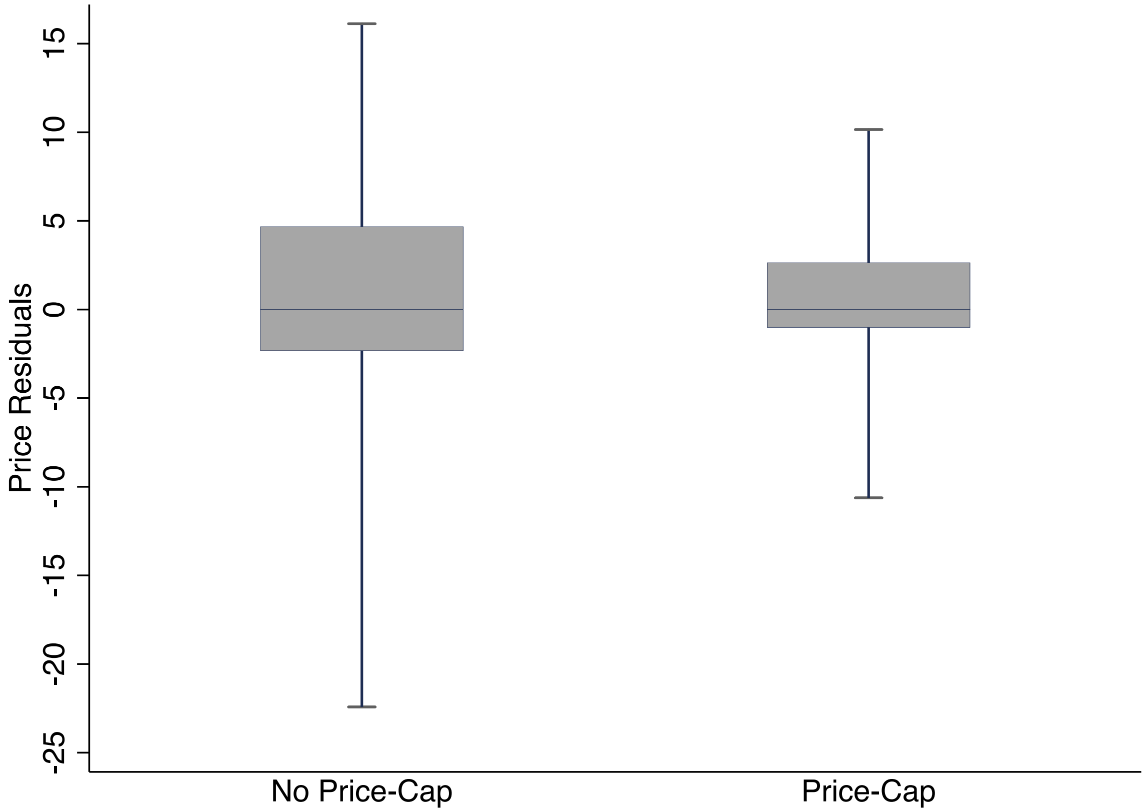
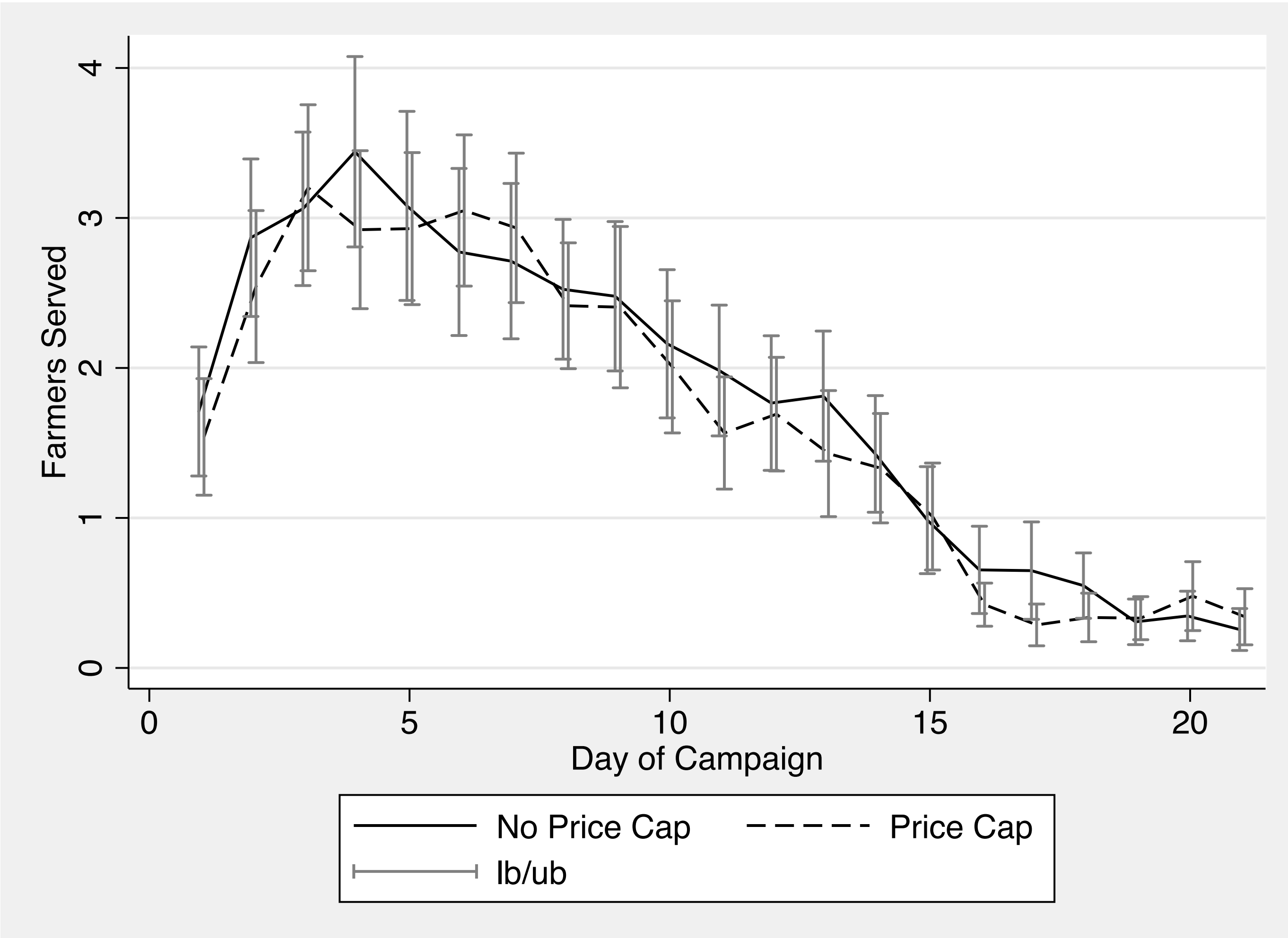


Figure 3: Box Plot of Price Variation



Notes: The figure shows box plots of residuals of a regression of prices on agent fixed effects. The regressions are estimated using receipt data. The box denotes the distribution of observations between the 25th and 75th percentile. The whiskers denote the length between the 10th and the 90th percentile of the price distribution. The vertical bar denotes the mean which, by construction of residuals, is at 0.

Figure 4: Effect of Price Cap on Number of Farmers Served



Notes: The figure shows the daily number of farmers served for every day of vaccination campaign, separated by treatment and control group. The error bars denote 95% confidence intervals. The figure uses receipt data.

A Appendix: Derivation of price discrimination formula

Recall that social welfare is given by:

$$SWF(\mathbf{p}) =$$
$$M(c^*(\mathbf{p}))\left[g_{c_1}\mu\int_{p(\varepsilon_D^L)}^{\infty} v_i - p(\varepsilon_D^L)\, dF(v_i|\varepsilon_D^L) + g_{c_2}(1-\mu)\int_{p(\varepsilon_D^H)}^{\infty} v_i - p(\varepsilon_D^H)\, dF(v_i|\varepsilon_D^H)\right]$$
$$+g_a M(c^*(\bar{p}))\left[\mu\left(p(\varepsilon_D^H)-\tau\right)D\left(p(\varepsilon_D^H)|\varepsilon_D^H\right) + (1-\mu)\left(p(\varepsilon_D^L)-\tau\right)D\left(p(\varepsilon_D^L)|\varepsilon_D^L\right)\right] - g_a\int_0^{c^*(\mathbf{p})} c_i dM(c_i)$$

Taking derivatives yields:

$$\begin{aligned}
\frac{\partial SWF(\mathbf{p})}{\partial p(\varepsilon_D^L)} &= \frac{\partial M(c^*(\mathbf{p}))}{\partial p(\varepsilon_D^L)}\left[g_{c_1}\mu\int_{p(\varepsilon_D^L)}^{\infty} v_i - p(\varepsilon_D^L)\, dF(v_i|\varepsilon_D^L) + g_{c_2}(1-\mu)\int_{p(\varepsilon_D^H)}^{\infty} v_i - p(\varepsilon_D^H)\, dF(v_i|\varepsilon_D^H)\right] \\
&+ \frac{\partial M(c^*(\mathbf{p}))}{\partial p(\varepsilon_D^L)} g_a\left[\left(\mu\left(p(\varepsilon_D^L)-\tau\right)D\left(p(\varepsilon_D^L)|\varepsilon_D^L\right) + (1-\mu)\left(p(\varepsilon_D^H)-\tau\right)D\left(p(\varepsilon_D^H)|\varepsilon_D^H\right)\right)\right] \\
&- M(c^*(\mathbf{p}))\, g_{c_1}\mu\left(1-F\left(p(\varepsilon_D^L)|\varepsilon_D^L\right)\right) \\
&+ M(c^*(\mathbf{p}))\, g_a\mu\left(1-F\left(p(\varepsilon_D^L)|p(\varepsilon_D^L)\right)\right) \\
&+ M(c^*(\mathbf{p}))\left[-g_a\mu\left(p(\varepsilon_D^L)-\tau\right)f\left(p(\varepsilon_D^L)|\varepsilon_D^L\right)\right] \\
&- g_a c'^*(\mathbf{p})\, c^*(\mathbf{p})\, m(\mathbf{p})
\end{aligned}$$

The number of low elasticity types served is given by: $N^L(\mathbf{p}) = M(c^*(\mathbf{p}))\, D^L(\mathbf{p})$. Using the definition of $c^*(\mathbf{p})$ as well as the definition of the elasticites and reordering yields the sufficient statistics formula:

$$\frac{\partial SWF(\mathbf{p})}{\partial p(\varepsilon_D^L)} = \theta N(\mathbf{p})\frac{g_{c_1}\mu CS\left(p|\varepsilon_D^L\right) + g_{c_2}(1-\mu)CS\left(p|\varepsilon_D^H\right)}{p(\varepsilon_D^L)\left(\mu D^L(\mathbf{p}) + (1-\mu)D^H(\mathbf{p})\right)} + \varepsilon_D^L N^L(\mathbf{p}) g_a\mu\frac{p(\varepsilon_D^L)-\tau}{p(\varepsilon_D^L)} - N^L(\mathbf{p})\mu\left(g_{c_1}-g_A\right)$$

B Appendix: Supplementary Information

B.1 Why Public Provision in the Absence of Externalities?

I-2 vaccinations primarily accrue private benefits to farmers and generate limited externalities. A natural question then is why such services should be provided by the government and not by the private sector. However, even in the absence of externalities private markets for livestock and agricultural extension services suffer from market failures that undermine service provision (Hanson and Just, 2001). For the case of animal health extension in Tanzania three particular failures motivate public provision of services.

First, private markets are unlikely to be competitive as geographical conditions and high operating costs of private providers raise concerns about local monopolization and extortion (Hanson and Just, 2001; Banerjee, Hanna, and Mullainathan, 2013). The main challenge is then how to assure that markets also develop for less commercial farmers who have a lower willingness or ability to pay for extension services (Anderson and Feder, 2007). The typical solution to address this challenge, also employed by the Tanzanian government, is to implement a stratified system of service provision, in which large scale commercial farmers are served by private markets and public providers are responsible for smallholders (Sulaiman and Sadamate, 2000). Second, a reliance on pure private provision raises concerns about deteriorating quality of goods and services provided. This is especially relevant for the provision of animal health services, as it is typically difficult for farmers to assess the quality of services or to differentiate between a good or a bad livestock drug (Umali and Schwartz, 1994). The decision to publicly provide I-2 in Tanzania has largely been driven by previous experiences of private vaccine provision which resulted in substantial reports of inadequate drug handling and application as well as the use of counterfeit or ineffective drugs. Finally, some stakeholders have also argued that public provision of animal health services is more efficient than private provision as the government can build the organization of service provision on its pre-existing infrastructure and network of frontline agents, therefore substantially reducing the cost of service provision (Ban, 2000).

B.2 Timing

The timing of the project proceeded as follows. From July to August 2015, the ministry collected background data on agents' work environment and activities. During this exercise, I conducted a pilot of the experiment. During November 2016 a workshop with senior central and local government officials introduced the experiment, finalized the design and secured political support at all administrative levels.

The intervention was then implemented in January and February 2016 by a mixed team of ministry staff and private enumerators. Both jointly communicated the campaign instructions to participants. To assure data confidentiality, the private enumerators then independently conducted the baseline survey with participants. After the survey, ministry staff was

responsible for the distribution of the vaccine and the communication of final technical instructions relating to the correct application and handling of the vaccine. agents started the vaccination campaign immediately after receiving the vaccines, and were given three weeks from vaccine distribution to complete the task. The last day of vaccination was February 24, 2016.

I then conducted a phone based follow-up survey with service recipients during March and April 2016. Finally, I conducted an in-person follow-up interview with 311 randomly selected experiment participants during May 2016.

B.3 Can Agents Perfectly Price-Discriminate?

A natural question is whether allocating discretion to agents enables them to extract the entire surplus from recipients. To investigate this question, I employ the variation in costs generated by the participation-cost treatment to formally test for third-degree price-discrimination. In particular, I investigate whether the distribution of prices in the participation-cost group first-order stochastically dominates the price distribution in the control group. Intuitively, shifts in costs should create a hole on the left tail of the distribution without increasing the mass in the right tail under perfect rent extraction. Testing whether the participation-cost treatment leads to mass increases in the right tales of the price distribution therefore tests for first-degree price discrimination.[54]

Figure 5 in the appendix investigates this graphically through a histogram using farmer survey data. Panel A plots the distribution of prices in the pure control group that neither had to pay participation-costs nor was constrained by a price-cap. Panel B overlays the distribution of prices in the pure participation-cost treatment group over the histogram from the control group. The graph visually rejects perfect rent extraction as the treatment substantially increases the number of transactions at 75 and 100 Tanzanian Shillings in the right tail of the distribution. In addition, a Kolmogorov-Smirnov test between the two distributions rejects the null hypothesis of first order stochastic dominance at all conventional significance levels. Taken together, the results therefore are inconsistent with a model of full surplus extraction.

[54]As I show below, agents pricing strategies imply that prices will respond to the participation cost even though it is fixed, making this a valid test.

C Appendix: Data

C.1 Baseline Data

The baseline survey was administered to every participant during the vaccination distribution and was completed before any vaccinations occurred. The survey included detailed questions on agents' demographics, education, work history and alternative income sources. It also collected data on agents' work environment, including information on travel times to villages, transport methods, private providers of veterinary services and agents' interaction with their supervisors.

As part of this survey I also administered two questions aimed at eliciting an incentive-compatible measure of pro-social motivation toward animal health causes. First, I designed a contextualized dictator game. Agents were told that they would receive a lunch allowance of 10'000 Tanzanian Shillings (approximately $4.50), which they could keep for themselves or donate, in part or in full, to TVLA to purchase subsidized vaccines for the next vaccination campaign. The amount donated is taken as a proxy for the agents' motivation for the cause. The median donation in the dictator game was 1000 Tanzanian Shillings.

Second, agents were given a map with 9 fields, each detailing a possible motivation for why they chose to work as a livestock field officer. Some stated motivations were intrinsic (e.g. *"my job allows me to help farmers when their animals are sick"*) while others reflected extrinsic sources of motivation (e.g. *"my job offers a stable income"*). Enumerators then gave participants 50 maize grains and asked them to distribute the grains between the different fields according to how important each reason was when they were making their career choice. The relative amount of beans allocated to fields that reflect intrinsic motivations then acts as a proxy for the agents' motivation for the cause.

Both measures were designed to increase the likelihood of being rank-preserving in order to assure that measures remain valid even if agents exaggerate their donation or grain allocation because of social pressure.

C.2 How accurate is the receipt data?

When assessing the validity of the receipt data it is important to remember that accurate reporting was financially incentivized, as verified receipts attracted a bonus payment of 60 Tanzanian Shillings per vaccination. Crucially, I don't consider receipts that were submitted without a contact phone number for farmers to be complete and therefore don't count them towards the total number of farmers served. Receipts without phone numbers are therefore also ineligible for the bonus payment. Agents were made aware of this rule during the roll-out and were encouraged to identify alternative contact numbers for farmers should they not own a phone, for example by providing the number of their neighbor or of the village leader. While this requirement might have incentivized employees to target farmers more likely to own cellphones, the need to provide phone numbers was present for all treatment groups

and is therefore unlikely to challenge the internal validity of the experiment. In addition, identifying farmers' contact numbers does not appear to have been a problem: Less than 4% of receipts were submitted without phone numbers and ministry staff tasked with supervising the campaign did not receive any complaints about challenges with identifying cellphone owners.

C.3 Farmer Survey Procedures

For cost reasons, the follow-up survey with farmers was implemented as a phone survey. The phone survey procedures were designed to maximize the likelihood of reaching service recipients. Enumerators were instructed to call each number on three different days, once in the morning and once in the afternoon. After every unsuccessful attempt, enumerators sent a text message to recipients informing them about the objective of the call and asking for an appointment to administer the survey.

D Appendix: Evidence for Average Cost Pricing

Standard economic theory suggests that monopolistic agents choose prices as mark-ups over marginal costs. While this assumption is consistent with profit maximization, it is not clear whether agents and firms are able to perfectly optimize prices. In particular, Liebman and Zeckhauser (2004) argue that when cost or price schedules are difficult to understand, individuals might base their decision on inaccurately perceived schedules, a practice they refer to as "*schmeduling*". It is hence possible that agents anchor their decisions on simplified heuristics that are easier to calculate than profit maximizing prices based on marginal costs.[55]
In this section, I present the results from a lab-in-the-field experiment, conducted with a randomly selected subset of study participants, to show that agents pricing choices respond to average instead of marginal costs.

D.1 Experimental Design

The lab-experimental design aims to simulate a situation that is similar to the service provision task that agents encounter during the field experiment. In particular, participants are told that they are delivering a service to four different customers. To incentivize profit maximization, the experiment instructions specifically emphasized the "private" nature of the task, therefore framing the service delivery as a for-profit interaction. Each customer is associated with an idiosyncratic delivery cost of 1,000, 4,000, 8,000 and 11,000 Experimental Shillings (ES), respectively. Participants were randomly divided in two groups. One group acted as a treatment group and was responsible for paying a fixed cost of 4'000 ES before commencing the simulation. The other group did not have a fixed cost requirement and therefore acted as a control.

I employ a multiple price list (MPL) mechanisms to elicit participants' reservation price for each of the customers.[56] In particular, respondents are shown ten hypothetical price offers between 1,000 and 10,000 ES for every customer. They are then asked to decide, independently for every offer, whether they would accept the price and pay the delivery cost or not serve the customer at the offered price. After making the ten choices, a piece of paper representing each choice is put in a bowl and agents draw one offer. The choice relating to this offer is then implemented.

Given this design, participants' main choice regards their reservation price, i.e. the smallest

[55] This assertion is consistent with a small literature that provides evidence on individuals and firms using simplified heuristics if identifying optimal choices is difficult. Ito (2014) shows that consumers respond to average rather than marginal electricity prices, as the former are easier to calculate. Wichman (2014) documents similar behavior for residential water demand. Feldman, Katuscak, and Kawano (2016) show that households reduce their reported wage income in response to a lump-sum tax liability increase because they perceive the change to affect their marginal tax rate. Altomonte, Barattieri, and Basu (2015) use survey evidence to document that over 75% of respondents in a large sample of European firms report setting mark-ups over total instead of marginal costs.

[56] The explanation protocol for the MPL mechanism is available from the author upon request.

price for which they would serve a customer instead of walking away from the deal.[57] The profit maximizing indifference point for every customer is the marginal delivery cost. It is therefore also optimal to reject serving the fourth customer. As the fixed cost of 4,000 ES has to be paid independent of whether any customers are served, it is sunk at the time of decision making and therefore does not affect the profit maximizing choice of reservation prices.

D.2 Implementation

I conduct the lab-in-the-field experiment with a total of 311 field agents from 14 randomly selected districts. All respondents had also participated in the field experiment. Subjects were individually paired with one enumerator, moved to visually isolated locations for the implementation of the experiment and were given a game sheet to make their choices.[58] In order to ensure independence across participants, subjects did not interact with one another during the experiment and were not informed of other participants' choices. Participants were first asked to play a simplified version of this game to practice. Agents then started the experiment. During the simulation, they made their choices sequentially for every customer, starting with the one with the lowest marginal cost. Every participant played two rounds of this experiment, one with and one without the fixed cost. The order of the two rounds was randomized.

Agents were paid out 20% of their experimental earnings in cash after the experiment, which further incentivized profit maximization. The average profit obtained was 10,271 ES.

D.3 Results

In order to estimate the effect of the fixed cost treatment on indifference points, I estimate variants of the following model:

$$y_{ir} = \beta_0 + \beta_1 FixedCost_{ir} + \gamma_r + \epsilon_{ir}$$

y_{ir} denotes the reservation price for individual i in round r, $FixedCost_{ir}$ is a binary variable indicating whether agents had to remit a fixed-cost and γ_r denotes round fixed effects. As randomization was performed at the individual level, I report robust standard errors clustered for every participant. Given the inclusion of round fixed-effects, the model employs within round-variation. As the allocation to the fixed cost treatment was randomized within rounds, this specification therefore causally estimates the effect of the treatment on the outcome variables of interest.

Table 15 presents the treatment effects of the fixed cost treatment on agents' choices of

[57] While the price offers appeared sequentially, participants weren't required to make consistent choices, i.e. choices that, for example, accepted a price offer of 1,000 ES but rejected an offer of 8,000 ES were not ruled out by the design. However, none of the participants decided to make inconsistent choices.

[58] Figure 8 shows an example of the game sheet.

indifference points, conditional on theoretically agreeing to serve a customer. Column 1 uses the sum of all indifference points as an outcome variable, whereas columns 2 to 5 investigate the choices for customers one to four separately. Note first that reservation prices in the control group correlate with marginal costs, but fall below marginal costs for the third and fourth customer whose marginal costs are highest. The fixed cost treatment significantly increases participants' reservation prices for all customers combined. The incidence of this cost increase falls primarily on the first and the second customer who have the lowest marginal cost. In contrast, the fixed cost treatment appears to reduce reservation prices for the two customers with the high marginal costs. Such responses to fixed costs are inconsistent with marginal cost pricing. Instead, the results suggest that agents base their choice of indifference point on average costs, which are affected by the fixed cost treatment.

E Appendix: Supplementary Tables

Table 12: Effects on Participation

Outcome Var.	**Training Att.**	**Participation**	**Overall Part.**	**% Verified**	**% Price Correct**
Treatment	(1)	(2)	(3)	(4)	(5)
Price-Cap	0.0130	0.0166	0.0224	0.00476	-0.0358*
	(0.0275)	(0.0265)	(0.0320)	(0.0219)	(0.0188)
Participation-Cost	-0.0102	-0.00405	-0.0100	0.0176	0.0182
	(0.0277)	(0.0267)	(0.0318)	(0.0220)	(0.0189)
Observations	990	832	990	740	675
District FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Control Mean	0.83	0.82	0.68	0.69	0.83
Control St. Dev.	0.38	0.38	0.47	0.30	0.25

Notes: Standard Errors are clustered at the ward level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. All specifications control for stratification variables and district fixed-effects. The sample for columns 1 and 3 contains the population of agents in all sample districts. The sample for columns 2 are all agents who attended the training and who performed vaccinations, respectively. "Overall Participation" refers to the likelihood of attending training and performing vaccinations after training. % Verified refers to the fraction of transactions reported through receipts that could be verified to have taken place. % Price Correct refers to the fractions of receipts that reported a price that could be verified through follow-ups with farmers.

Table 13: Robustness - Replication of Price-Cap Treatment Effects excluding Cattle Owners

Outcome: % of Farmers or Transactions with given Characteristic				
Characterstic:	**Owns Cattle**	**Price**	**Main Livelihood is Agriculture**	**Village has no Private Provider**
	(1)	(2)	(3)	(4)
Price-Cap	0.00526	-15.61***	0.0535**	-0.0421**
	(0.0222)	(3.711)	(0.0266)	(0.0205)
Observations	3,098	2,165	2,204	2,204
Observation Level	Transaction	Transaction	Transaction	Transaction
Data Source	Farmer Survey	Farmer Survey	Farmer Survey	Farmer Survey
District Fixed Effects	Yes	Yes	Yes	Yes
Agent Controls	Yes	Yes	Yes	Yes
Farmer Controls	Yes	Yes	Yes	Yes
Control Mean	0.29	79.57	0.76	0.89
Control St. Dev.	0.45	53.69	0.43	0.30

Notes: Standard Errors are clustered at the individual level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. All regressions apart from column 1 exclude cattle owners from the sample. Column 1 presents coefficient estimates of a regression of a binary variable indicating whether a farmer owns cattle or not on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Column 2 presents coefficient estimates of a regression of the transaction price on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Column 3 presents coefficient estimates of a regression of a binary variable indicating whether the recipient's main livelihood is from agriculture on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Column 4 presents coefficient estimates of a regression of a binary variable indicating whether the recipient's village has a private provider of veterinary services on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables.

Table 14: Robustness - Treatment Effects on Leakage and Transaction Behavior

Outcome Variable	**Vaccine Loss (Proxy)**	**Mentioned Vaccine Cost**
Treatment	(1)	(2)
Price-Cap	-0.0525	-.0047
	(0.0895)	(0.0344)
Participation-Cost	-0.0121	-0.0442
	(0.0979)	(0.0343)
Observations	819	832
Data Source	Farmer Survey	Farmer Survey
District Fixed Effects	Yes	Yes
Controls	Yes	Yes
Control Mean	0.96	0.65
Control St. Dev.	1.45	0.47

Notes: Standard Errors are clustered at the ward level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. Column 1 presents coefficient estimates of the fraction between confirmed vaccinations and the number of vaccine doses collected on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. Column 2 presents coefficient estimates of a binary variable indicating whether agents mentioned the vaccine cost during the service delivery process on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables. All columns employ farmer survey data.

Table 15: Lab Experiment - Effect of Fixed Cost on Indifference Point

Outcome Variable: Choice of Indifference Point					
Customer:	**All**	**First**	**Second**	**Third**	**Fourth**
	(1)	(2)	(3)	(4)	(5)
Fixed-Cost	3,678***	2,530***	831.6***	-98.17	-397.1**
	(294.0)	(96.30)	(113.0)	(122.9)	(185.6)
$\mathbb{1}(Round = 2)$	50.73	244.1**	-36.93	-104.9	-154.6
	(293.4)	(96.18)	(111.6)	(122.6)	(183.4)
Observations	621	621	610	575	413
Control Mean	23'248	2'199	4'458	7'179	7'178
Control St. Dev.	7'634	1'282	1'726	2'703	2'947

Notes: Standard Errors are clustered at the individual level. *** (**) (*) indicates significance at the 1 (5) (10) percent level. Column 1 presents coefficient estimates of a regression of the total of indifference points chosen during the experiment on a treatment indicator and round fixed effects. Columns 2 to 5 present coefficient estimates of a regression of the individual indifference points chosen for customers 1 to 4, respectively, on a treatment indicator and round fixed effects. All regression results are conditional on choosing to serve the customer.

F Appendix: Supplementary Figures

Figure 5: Effect of Participation Cost on Price Distribution

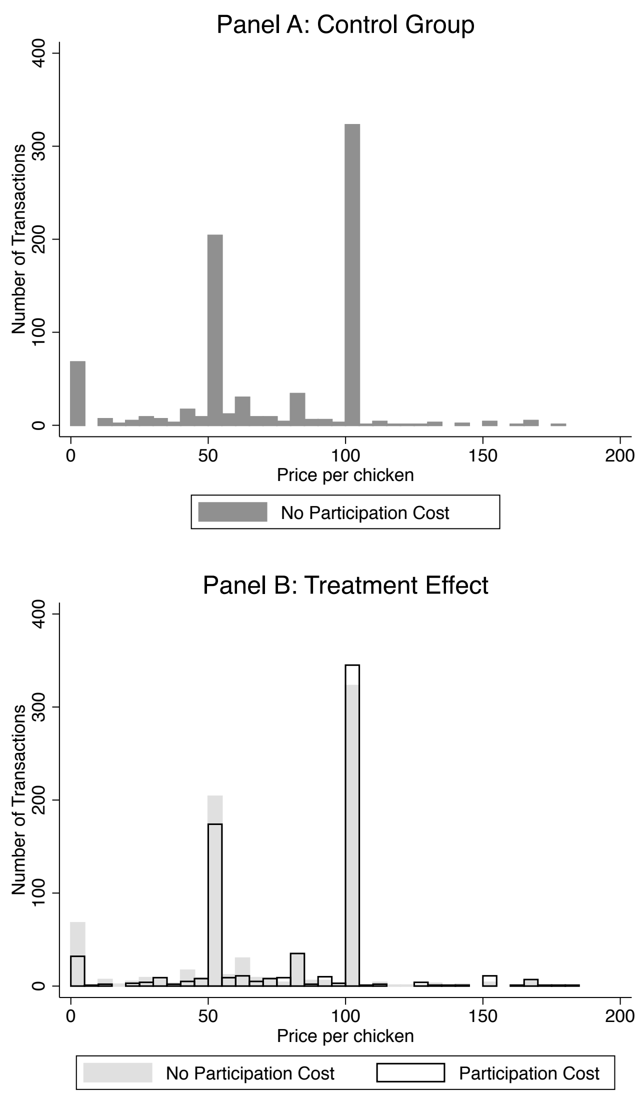
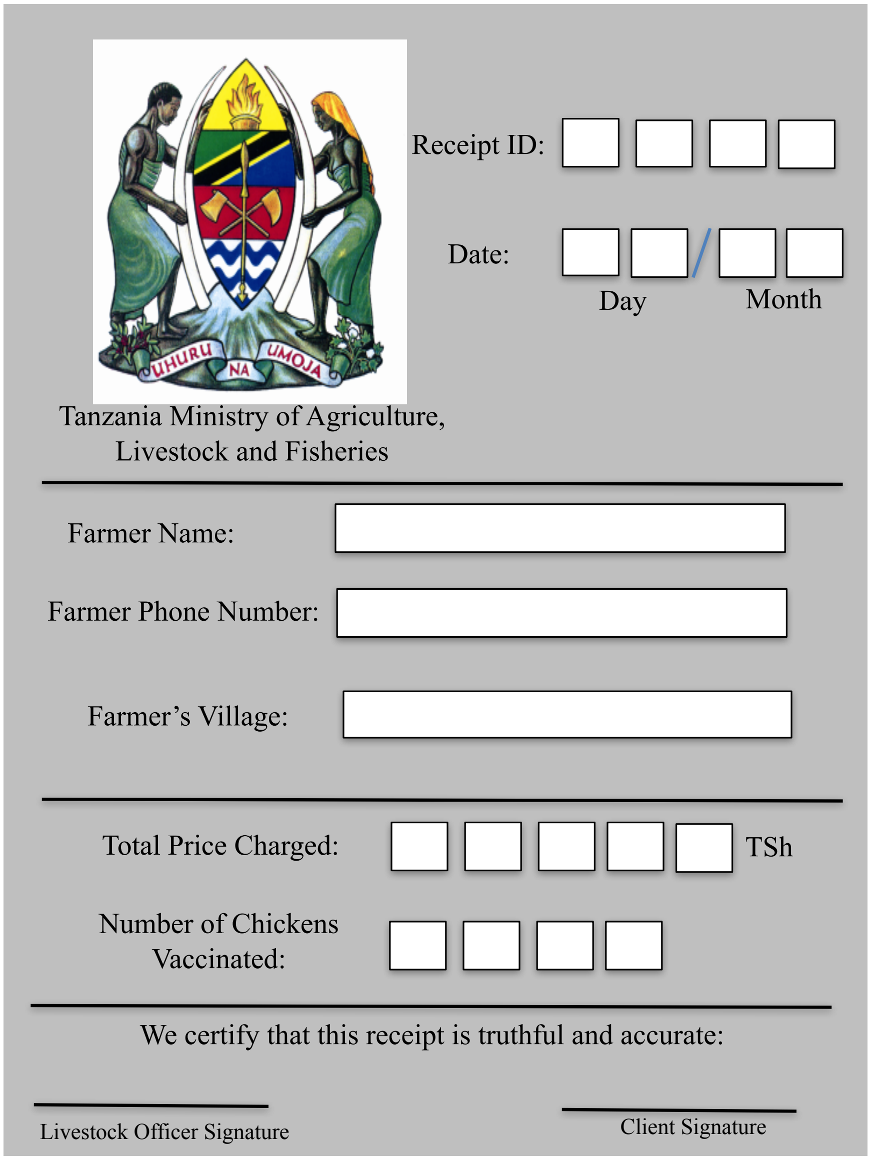


Figure 6: Receipt Format


Receipt ID:

Date: /
Day Month

Tanzania Ministry of Agriculture, Livestock and Fisheries

Farmer Name:

Farmer Phone Number:

Farmer's Village:

Total Price Charged: TSh

Number of Chickens Vaccinated:

We certify that this receipt is truthful and accurate:

Livestock Officer Signature

Client Signature

Figure 7: Distribution of Unverifiable Receipts

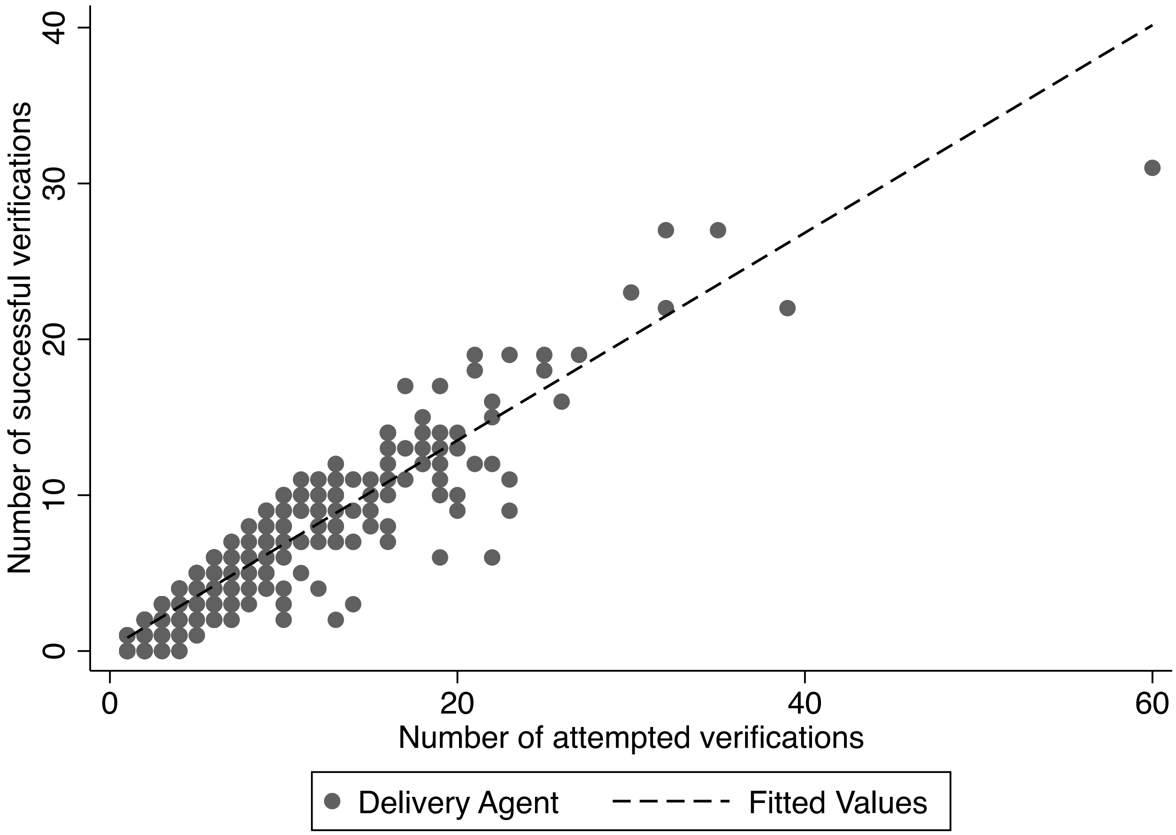


Figure 8: Information Sheet for Participants of the Lab-in-the-Field Experiment

Total cost if everyone is served: 24’000

Cumulative Revenue	Cumulative Cost
0	0

Customer Name	Served yes or no?	Price Offered
Mark		
Geoffrey		
Peter		
Karl		