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# Price-caps, discretion, and service delivery

## Evidence from a field experiment

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# Price-Caps, Discretion, and Service Delivery: Evidence from a Field Experiment

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**Abstract**

It is often argued that price-caps – a ceiling on prices that monopolistic suppliers are allowed to charge - are necessary to redistribute surplus and make essential goods and services accessible to recipients in need. I explore whether price-caps lead to welfare improvements through a field experiment, implemented in cooperation with the Tanzanian government, in which I randomize whether monopolistic veterinary service providers are subject to a price-cap or not. This intervention has three effects. First, conditional on being served, the treatment increases the 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 services: the price cap increases the likelihood of agents serving farmers with a high need for the service by 3 to 6 percentage points. Third, the price-cap reduces the geographic coverage of services: the intervention decreases the likelihood of agents serving remote villages by 6 to 10 percentage points. This suggests that price-cap regulation faces a tension between making services affordable to close farmers in need and serving remote recipients. In light of those opposing forces, I show that the optimal regulatory policy can be expressed as a function of two sufficient statistics: The elasticity of the proportion of villages served with respect to fees and the price elasticity of demand. Calculating the welfare effects, I find that marginally reducing prices below the full-discretion level induces a social welfare *loss* to the magnitude of 3% to 12% of total sales revenue per agent. This suggests that leaving prices uncapped is the optimal policy for Tanzania.

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# 1 Introduction

A central objective of governments is to assure that citizens have access to services that are essential for human welfare and economic development. When suppliers of essential services have market power, this objective is at risk, as decentralized market outcomes can generate an inefficient allocation of services and an undesirable distribution of surplus.[1] To overcome this challenge, it is often argued that governments need to impose price-caps to reduce suppliers' discretion over prices.[2] Three rationales underlie this argument. First, compared to more complicated forms of regulation, price-caps are comparatively easy to implement and enforce.[3] Second, price-caps can generate savings for existing customers and limit suppliers' ability to extract surplus. Third, price-caps can make it possible for needy customers to afford the services.

Previous empirical work provides evidence that charging for essential services can discourage buyers in need, which suggests that price-caps can be welfare enhancing (Cohen and Dupas (2010); Ashraf, Berry, and Shapiro (2010)). But charges do not only screen between recipients, they also fund service delivery. Capping prices can hence reduce suppliers' incentive to invest in extending supply to new recipients and markets. This effect is likely especially important in developing countries, where budget constraints limit governments' ability to subsidize suppliers and where delivering services to remote areas is costly. The question of whether to cap prices or not therefore faces a trade-off between making services affordable for recipients in need and serving remote areas.

Empirically evaluating this trade-off requires knowledge of two parameters: First, the price elasticity of demand. Second, the impact of price-cap regulation on the likelihood that remote areas are served. To date, empirical evidence on the latter parameters has been constrained by the absence of exogenous sources of variation in price-caps. As a result, little is known about whether price-cap regulation enhances or reduces welfare.

In this article, I provide experimental evidence that overcomes this challenge. In particular, I estimate the two key parameters and examine the trade-off between discretion and price-cap regulation for an essential economic service provided in developing countries: Livestock vaccinations. I focus on the provision of I-2 poultry vaccines that protect against Newcastle Disease (ND). ND is highly prevalent in East Africa and, due to its lethal effect on infected birds, causes substantial economic damage. Poorer farmers are particularly affected by ND, as their livelihoods and asset holdings are especially dependent on livestock. In Tanzania, I-2

[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]Price-caps also provide stronger incentives to improve efficiency than other forms of regulation. See, for example, Laffont and Tirole (1993).

is supplied to farmers by para-veterinarians. specialized frontline service delivery agents, who receive the vaccine at subsidized prices from the government.[4] Agents fund the distribution and application of the vaccine through user fees, which also indirectly provide performance incentives. In the status quo, agents are local monopolists and have full discretion over prices.

Working with the Tanzanian Ministry of Agriculture, Livestock and Fisheries, I allocate 550 administrative wards to one of two experimental groups.[5] 274 wards are assigned to the control group that mirrors the Tanzanian status quo. Agents in those wards have full discretion over pricing and typically charge recipients either 50 or 100 Shillings per vaccination. In contrast, 276 wards are assigned to the treatment group, in which agents face a price-cap of 80 Tanzanian Shillings (approximately $0.035) per vaccination. Agents in both experimental groups have full discretion over the types and number of recipients and geographic areas targeted.

To evaluate the intervention, I compare outcomes in the treatment and control groups across three dimensions. First, I investigate how the treatment affects prices. I find that the intervention reduces average prices by 17% and the within-agent standard deviation of prices by 42%. This reduction in prices is driven primarily by reduced fees for farmers with a high need for the service and limited outside options, as measured by indicators for whether recipients main source of livelihood is agriculture, whether recipients are smallholders and whether there are no private providers in the recipients' village. This suggests that agents indeed use discretion in the status quo to redistribute surplus to themselves. They do so through a system of price-discrimination, in which they charge higher prices to recipients who require the service more. The price-cap is effective in limiting agents' ability to extract surplus and generates substantial savings for served farmers.

Price-caps are not, however, only intended to redistribute surplus but also aim to make services affordable for recipients in need. The second part of my investigation therefore examines how the intervention affects the likelihood of agents extending services to new recipients and farmers with a high need for the vaccine. I find that that the treatment makes agents 14% more likely to serve previously unserved recipients. In addition, agents in the treatment group are 8% and 9% more likely to serve farmers whose livelihood depends on agriculture and smallholders, respectively. Taken together, those results suggest that price-caps can make service accessible to previously unserved recipients and farmers with a high need for the service.

The results presented so far show that price-caps can enhance the provision of services by redistributing surplus to recipients and by crowding in new recipients with a high need for

[4]Reason for public provision. As the effectiveness of I-2 relies crucially on adequate handling and application, there exists no secondary private market for I-2.

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

the service. At the same time, a price-cap could also harm service delivery if it reduces agents' incentive to extend service to farmers who are costlier to serve. Consistent with this idea, the third part of my investigation finds that the intervention reduces the proportion of villages served by 12%. This reduction is driven by a reduced likelihood of agents visiting remote villages: They 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 results therefore suggest that governments indeed face a trade-off when deciding whether to regulate prices: Price-caps can crowd-in previously unserved recipients with a high need for the service at the expense of remote farmers.

The relationship between price-caps and service allocation discussed so far combines the effect of the intervention on the extensive village margin and the intensive demand margin. To disentangle those channels, 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.[6] 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, agents choose prices based on average instead of marginal costs.

I use the variation in regulation and prices generated by the two interventions to evaluate the optimal regulatory policy. In particular, given the countervailing effects induced by the price-cap treatment, a crowd-in of previously unserved recipients and a reduction in the geographic coverage of services, it is not clear whether it is optimal for the government to restrict the agent's discretion and, if so, to what extent. To address this, I develop a simple model of monopoly regulation that is consistent with the empirical results. The model expresses the aggregate 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. This acts as a sufficient statistic for the welfare loss incurred through the reduced geographic coverage. Second, the price elasticity of demand. This captures the welfare benefits associated with reducing prices. Using the results of the evaluation, I find that marginally reducing prices below the monopoly level induces a social welfare *loss* to the magnitude of 3% to 12% of total sales revenue per agent. This suggests that leaving prices uncapped is the optimal regulatory policy for veterinary services in Tanzania.

To the extent of my knowledge, this paper is the first to provide causal field-experimental evidence on the trade-offs associated with imposing price-caps on monopolistically supplied essential services. By doing this, it complements a literature that investigates pricing for essential services. Theoretical work in this area has investigated how to optimally regulate

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

providers of public services. Laffont and Tirole (1986), show that optimal level of regulation faces 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, Laffont (2005) as well as Alonso and Matouschek (2008) argue that in the absence of transfers and verifiable information on costs, price-caps remain the optimal regulatory policy.[7] My 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) investigate how charging for insecticide treated bed nets affects demand and use. Similarly, Ashraf, Berry, and Shapiro (2010) show that prices for water-purifies can screen for high-use customers. My paper complements this work by highlighting the importance of considering the interaction between supply and demand decisions when deciding whether and how much to charge for service provision.

More broadly, this papers fits into a new and rapidly growing literature that investigates the organization of the public sector.[8] While previous empirical work has focussed on understanding how bonus payments and career incentives should be designed to encourage effort (e.g. Khan, Khwaja, and Olken (2016); Muralidharan and Sundararaman (2011) and Olken, Onishi, and Wong (2014)), a recent stream of this literature has started taking into account that contracts in practice are rarely complete.[9] A central question then is whether agents should have discretion in situations not covered by contracts. Existing work has documented a correlation between employee autonomy and public project completion in Nigeria (Rasul and Rogger, 2016) as well as improved environmental inspection targeting as a result of regulatory discretion (Duflo et al. (2014)). Complementary to this, my paper shows that, while generating opportunity for surplus extraction, giving agents' discretion can also provide performance incentives.

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 theoretically the possible impacts of price-cap intervention. Section 4 outlines the experimental design and section 5 presents the results. Section 6 derives a sufficient statistics formula to evaluate the welfare impact of capping prices. Section 7 describes the estimation of the demand elasticity, which is used to estimate the welfare effect in section 8. Section 9 concludes.

[7] 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.

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

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

# 2 Setting

This project explores the effect of price-caps in the context of livestock extension in Tanzania, a service delivery program administered by local governments and coordinated by the Ministry of Agriculture, Livestock and Fisheries.[10] The program 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.

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 expenditure on agriculture, compared to 8% on health, which led to a global spending on extension of $31 billion in 2008. In Tanzania, extension services rank third in terms of staffing numbers after education and human health services. In total, 10'891 extension agents were employed in Tanzania in 2012, comprising approximately 5% of local government staff.[11]

An important feature of extension, in contrast to health and education, is that the returns to receiving services are primarily private. Services are therefore part of the country's economic infrastructure, which aims to support citizens' economic activity.[12] This is key for my research question, as it allows me to abstract from potential externalities when interpreting demand responses.

## 2.1 Suppliers

The agents responsible for delivering livestock extension services in Tanzania are paraveterinarians, 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 4 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

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

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

[12] In this sense, extension is more comparable to electricity than to health services.

coordinate their work.[13] There is no entry or exit of agents during my study period.
Agents primary task is to travel to farmers in order to provide services to them. 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.[14] 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 paid wages and user fees. Specifically, agents receive a flat compensation of around $200 per month.[15] In addition, they are allowed to collect user fees from farmers, which cover delivery costs and act as performance pay. In the status quo, the absence of capacity to monitor compliance with a regulated user fee schedule induced local governments to allocate full discretion over pricing to the agents.
The rationale for employing user fees in extension is threefold. First, user fees are equivalent to commissions for private sellers. They hence provide high-powered incentives for agents to exert effort. This solves one of the key challenges in public provision of extension services, which have traditionally suffered from a lack of mechanisms to induce providers' accountability to farmers (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 solve the problem of high 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 and adequately fund service delivery (e.g. Cary, 1998).
Cost-sharing schemes are common for agricultural and livestock extension programs (Rivera and Gustafson, 1991; Dancey, 1993; ).[16] Compensation schemes that partially rely on user payments are, however, also present for other public service delivery schemes, such as health services and food distribution. 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

[13] To avoid spillovers and interaction, treatments are assigned at the ward level. See also section 4.1.

[14] 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.

[15] Wages vary across local government administrations.

[16] 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.

role to a user fee.[17] More formally, a number of countries and organizations have, either temporarily or permanently, switched to a system that employs user fees to cover expenditure of health facilities.[18] 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 and deworming procedures as well as reactive treatment to address common livestock diseases. Unregulated user fees are charged for all animal health services.[19] As part of this project I focus on the provision of I-2 vaccines as one dimension of service provision. I-2 is a thermotolarent vaccine for poultry that protects against Newcastle Disease (ND), a viral disease that is transmitted through birds' bodily discharge and air and leads to almost 100% mortality in affected and unvaccinated chicken. Estimates from Tanzania suggest that 30% to 80% of chicken die from Newcastle Disease 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.[20] 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-

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

[18] 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).

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

[20] While the private sector provides an effective vaccine against Newcastle Disease, *La Sota*, cooling requirements and high per-vaccination-costs exclude small-scale farmers from accessing it. An additional source of market power is that I-2 is an eye-drop vaccination and requires adequate handling and application for it to be successful. While no professional veterinary degree is required, I-2 cannot be applied by the farmers themselves but requires an agent who has received basic training in its application.

monthly basis.[21] During such campaigns, agents primary task is the provision of vaccinations. This alleviates 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. The main benefit of the program and the government's stated objective, therefore is private, as it assures that livestock is protected from disease outbreaks. This property makes consumer surplus an adequate metric of service delivery performance.[22] 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.

# 3 Model

In this section I develop a model to illustrate how price-caps affect the distribution of surplus and program targeting. 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. 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 *across* villages. Second, agents choose prices which determine the number and types of recipients served *within* a village.

## 3.1 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 for the service, which is denoted by $v_i$. Following Niehaus et al. (2013), I assume that $v_i$ is a continuous random variable drawn from an exponential distribution, $F(v)$, with rate parameter $\frac{1}{\eta_i}$.[23] For simplicity, I assume that $\eta_i$ can either be high or low: $\eta_i \in \{\overline{\eta}; \underline{\eta}\}$. Suppose that a fraction $\mu$ of recipients has $\eta_i = \overline{\eta}$, whereas everyone else has $\eta_i = \underline{\eta}$. Customers do not only differ with regards to their valuation, but also in their location. In particular, I assume that recipients live in a continuum of villages that differ with regards to the travel distance to the agent's headquarter. I further assume that the

[21] 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.

[22] 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.

[23] Notice that this implies that the elasticity of demand is given by $\frac{p_i}{\eta_i}$.

distribution of valuations is homogeneous across villages.

Given this set-up, agents face two sequential choices. First, agents determine the allocation of services *across* villages. I assume that travelling to village $j$ requires paying a cost of $c_j$ which agents decide whether to 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. For simplicity, I model this by assuming that agents offer a take-it-or-leave-it price based on observable recipient characteristics when visiting a recipient. Crucially, while I allow for price-discrimination, I assume that agents do not observe $v_i$ and instead only learn about $\eta_i$.[24] If customers accept the agent's offer, the agent receives the agreed price and delivers the service at a constant cost $\tau$. If the recipient rejects the price offer, no transaction takes place.

### 3.2 Decentralized Market Outcomes

To guide the empirical analysis, this section describes agents' behavior in the absence of regulation. In particular, I show how agents allocate services *across* and *within* areas under the full discretion policy currently in place. Section 3.3 then investigates the policy options available to the government.

Notice first that travel costs to villages are sunk at the time of price setting. This allows me to investigate the agent's two problems separately. I first investigate the agent's 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:

$$\mu \pi_{\overline{\eta}}(\mathbf{p}) + (1-\mu)\, \pi_{\underline{\eta}}(\mathbf{p}) \geq c_j$$

where $\pi_{\overline{\eta}}(\mathbf{p})$ denotes the expected profit obtained from recipients with $\eta_i = \overline{\eta}$ and $\pi_{\underline{\eta}}(\mathbf{p})$ is defined analogously. 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:

$$\sigma = M(c^*(\mathbf{p})) \tag{1}$$

Regarding allocation *across* villages, agents are hence more likely to serve a larger proportion of villages when (i) the expected profit obtained from recipients is higher (ii) the proportion of recipients with a high elasticity of demand is lower.

I then turn to the price-based allocation of services *within* a village. To understand how

[24] 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 and 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.

agents choose prices, notice that they face a monopoly trade-off: Raising prices increases profit from the transaction but reduces the likelihood that farmers will accept the offer. Formally, visiting a recipient of type $i$ yields the following expected profit:

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

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

$$p_{Dec}\left(\eta_i\right) = \tau + \eta_i \tag{2}$$

Agents set prices based on a mark-up over marginal costs, with a low-elasticity of demand leading to high mark-ups.[25] After receiving a price offer, recipients decide whether to accept or to reject it. In equilibrium, they accept every offer that does not exceed their willingness to pay. Combining the agent's allocation choices *across* and *within* villages, the total number of recipients served is then given by:

$$N_{Dec}\left(\mathbf{p}_{Dec}\right) = M\left(c^*_{Dec}\right)\left[\mu D\left(\mathbf{p}|\overline{\eta}\right) + (1-\mu)\, D\left(\mathbf{p}|\underline{\eta}\right)\right] \tag{3}$$

where $D\left(\mathbf{p}|\eta_i\right)$ denotes the within village demand for customers of type $i$.

## 3.3 Regulatory Policy

The principal'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 and low elasticity customers as well as the agent, social welfare for a generic price vector is given by:

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

Social welfare therefore consists of a weighted sum between consumer surplus and the agent's profit.

To build intuition, it can be instructive to define the first best regulatory policy for the case 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 costless transfers to the agent and

[25] Notice that the assumption that $v_i$ is exponentially distributed implies that the elasticity of demand is given by $\frac{p_i}{\eta_i}$, which makes the size of the mark-up independent of marginal costs. This can be relaxed using a more general distributional assumption.

enforce which villages the agent serves. Denoting transfers by $t$, it is straightforward to see that the optimal regulatory contract implements the following policies:

$$\bar{p} = \tau$$
$$c^* = c^{max}$$
$$t = \int_0^{c^*(\mathbf{p})} c_j dM(c)$$

In words, the optimal regulatory policy in the first best 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 are price-caps that rule out agents' most opportunistic choices. I denote the price cap by $\bar{p}$ and, in the next section, analyze its effect on the distribution of surplus and the allocation of services.

## 3.4 Effect of Price-Cap Regulation

This section analyzes how price-caps affect service allocation. In particular, I will show that price-caps have two opposing effects on service provision. First, price-caps increase the likelihood that needy customers within a village are served. Second, price-caps reduce the likelihood of the agent visiting remote villages. For expositional purposes, I assume that the price cap is such that:

$$\tau + \underline{\eta} < \bar{p} < \tau + \overline{\eta}$$

This implies that the price-cap will only bind for recipients with a low elasticity of demand. Prices under reduced discretion are hence given by:

$$p^{Reg}\left(\overline{\eta}\right) = \bar{p} \text{ and } p^{Reg}\left(\underline{\eta}\right) = c + \underline{\eta}$$

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. It will hence reduce the standard deviation and average of prices in equilibrium.

### Effect on Service Allocation within a Village

I first investigate how price-caps affect the allocation of services *within* a village, conditional on it being served. Recall that the number of recipients served in a given village is given by:

$$\mu D\left(\mathbf{p}|\overline{\eta}\right) + (1-\mu) D\left(\mathbf{p}|\underline{\eta}\right)$$

Notice also that the demand curve is downward sloping. If the price-cap binds, it is hence straightforward to see that price-cap regulation will increase demand in a village. The price-cap is more likely to bind for farmers with a higher need for the service, as those have a lower elasticity of demand and therefore pay higher prices in the status quo. As such, the price cap will increase the likelihood that recipients with a high need for the service will be served.

**Effect on Service Allocation across Villages**

I now turn to investigating how price-caps affect the allocation of services *across* villages. Denote by $\pi_j^{Dec}$ and $\pi_j^{Reg}$ the agent's expected profit from visiting village $j$ in the status quo and under the price-cap, respectively. Agents maximize their profits with respect to prices in the status quo, it hence must be that:

$$\pi_{\overline{\eta}}^{Dec} \geq \pi_{\overline{\eta}}^{Reg}$$

Notice that profits play a dual role in my setting. First, they allow agents to extract surplus. Second, however, they also compensate agents for the incurred travelling costs to remote villages. This becomes necessary because travel costs are sunk when price offers are made. Pricing decisions therefore don't assure that the agent breaks even in remote areas. Differentiating equation 1 with respect to prices shows how reducing discretion affects the targeting of remote areas:

$$\frac{\partial \sigma}{\partial p_{\overline{\eta}}} = M'\left(c^*\right)\mu \frac{\partial \pi_{\overline{\eta}}}{\partial p_{\overline{\eta}}} < 0$$

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 faces a tension between making services affordable for needy recipients within a village and serving remote villages. The next section outlines the empirical design to test this prediction.

# 4 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 to evaluate the intervention.

## 4.1 Experimental Design

I examine two interventions that jointly address my research questions: implementing and enforcing a maximum price ("price-cap treatment") and increasing agents' costs ("participation-cost treatment"). To avoid spillovers, treatment assignment was performed at the ward level.

I randomly and independently assign each of the 550 wards in the study to either the control or the treatment group (see table 1). 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. 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 receiving a given treatment is therefore orthogonal to other treatments.
This project was carried out during the first I-2 vaccination campaign of 2016.[26] The study covers the time period between January and February 2016 and enumerated agents in four of Tanzania's 30 regions (Dodoma, Iringa, Morogoro and Tanga). All 27 districts in the enumeration 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 990 agents, which forms the provisional sample of this study. 832 of those agents attended the training and participated in the vaccination campaign.

## 4.2 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 on procedures. 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 treatments 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 surveys were conducted on different days for the different treatment groups and districts to avoid spillovers.
During this meeting, agents in the control group were informed 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 seminar 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

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

chickens were vaccinated and that hence 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 the ministry 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, the ministry offered a bonus payment of 60 Tanzanian Shillings (approximately $0.025) for every verified vaccination.

After the seminar, training staff administered a survey to participants that collected data on demographics, work history and workplace characteristics. Ministry staff then handed out 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 participation under this system, agents were provided with free vaccines from the central government during this campaign.

### 4.2.1 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 choose any price they wanted but weren't allowed to charge more than 80 Tanzanian Shillings (approximately $0.035) per vaccination.

This cap was calibrated to balance two considerations. On the one hand, it had to be low enough 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 was chosen after careful consultations with experts from TVLA, MALF, local governments and international academics. In addition, it was based on a qualitative pre-study, conducted by the author, that elicited pricing behavior during previous I-2 campaigns. This suggested that agents in the control group would charge either 50 or 100 Tanzanian Shillings per vaccination. To avoid setting a price-cap that would not allow agents to recover their marginal costs, the cap was conservatively set to bind only for the comparatively high prices.

Price-caps are only effective if they can be enforced. I took the following measures to assure 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. This transformed 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 worked: 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, the ministry 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. 99% of transactions in the control group were conducted at user charges below this threshold, suggesting that compliance was incentive compatible for the vast majority of participants.

### 4.2.2 Participation-Cost Treatment

Compared to the control group, the instructions given to agents in the participation-cost treatment differed only with regards to vaccine distribution. In particular, ministry officials informed participants that vaccines would not be provided for free and instead participation in the vaccination campaign required a fixed payment of 25'000 Tanzanian Shillings (approximately $11.40) to cover parts of the vaccine production cost.

To avoid concerns about liquidity constraints, the ministry 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 to accept the cost, perform vaccinations and collect user fees and bonus payments or to reject participation without any obligation to pay. The ministry repeatedly emphasized that there would be no repercussions from refusing participation.

The participation cost is small relative to the possible expected revenue from participation. On average, revenue in the control group was 70'000 Tanzanian Shillings. However, 25% of total earnings (revenue plus bonus payments) fall below the participation cost. For agents with low revenue potential it may hence make sense to reject this proposal, but for those with sufficient business potential the expected return from accepting the participation cost appear substantial.

## 4.3 Data

The main data used in this paper were 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 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 information on the receipts details the 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 ministry database using a text-message template.[27]

Using the receipt data, I can 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. The survey was conducted over a period of 6 weeks, between March and April 2016, and sampled a randomly selected fraction of 15% of all receipts submitted, stratified by agent. This led to a total sample of 4'516 receipts selected for surveying and verification.[28] The survey was able to contact 3'580 farmers which equates to 80% of receipts sampled.[29] 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 the 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 with 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, distance between farmers' home and the agent's headquarter, farmers' sources of livelihoods, and the availability of private animal health service providers.[30]

# 5 Results

This section presents the empirical methodology and results from the evaluation of the price-cap treatment. Table 2 presents summary statistics and a randomization check using baseline characteristics of agents choosing to participate in the campaign. All characteristics in the table were chosen prior to estimating the balance checks. The results suggest that experiment participants are similar across the two treatments and the control groups. Columns 1 to 4

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

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

[29] 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.

[30] 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.

contrast control and treatment characteristics for the price-cap treatment, whereas columns 5 to 8 compare agents in the participation-cost treatment to the respective control group. In addition, panel A considers agent level characteristics, while panel B investigate balance based on workstation characteristics. Only 3 of the 28 differences are statistically significant at the 10% level, which confirms balance at baseline.

## 5.1 Impact of Price-Cap on Prices

I begin the investigation 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:

$$y_{iwd} = \beta_0 + \beta_1 PriceCap_{wd} + \beta_2 ParticipationCost_{wd} + \beta_3 X_{wd} + \gamma_d + \epsilon_{iwd}$$

where $y_{iwd}$ is the outcome of interest for participant $i$ in ward $w$ and district $d$, $PriceCap$ and $ParticipationCost$ are binary variables that indicate whether agents' wards were assigned to the price-cap or the participation-cost treatment, and $X_{wd}$ denotes ward-level stratification variables.[31] I also include district level fixed-effects ($\gamma_d$), 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 1 plots a histogram that visualizes the distribution of prices in the status quo, using farmer survey data from the control group.[32] Prices follow a bimodal distribution with peaks at 50 and 100.[33] 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.[34] Columns 1 and 2 in table 3 confirms this by showing that the intervention reduced average prices by approximately 17%, which is statistically significant at the 1% level.[35] Column 5 confirms the visual impression of bunching at 80 by showing that the intervention increased the fraction of transactions per

[31] As I find no evidence of interaction effects between the price-cap and the participation-cost 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 responsible to pay participation costs.

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

[33] As expected when designing the intervention, the price-cap hence only binds for a fraction of prices. 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.

[34] Figure 1 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.

[35] 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.

agent for which 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 estimate the effectiveness of the treatment in reducing price discrimination. To do this, I calculate the residuals of a regression of prices on agent fixed effects to obtain indicators of within-agent price variation. Figure 2 presents a box-plot of the residuals, separated between treatment and control group, to visualize the effect of the treatment on price discrimination. 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 was successful in reducing price discrimination. The figure shows that within-agent price variation 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.

A central question is whether the observed variation in prices under discretion indeed reflects price-discrimination, or whether it is generated by differential service delivery costs. In particular, it could be that the observed variation in prices reflects differences in travel and application costs associated with visiting farmers. To provide supporting evidence of price-discrimination, I show that measures of such costs only explain a relatively small fraction of the within agent price-variation. To do this, I run regressions at the transaction level that take the following form:

$$Price_{fiv} = \beta_0 + \beta_1 WalkingTime_{fiv} + \beta_3 MotorcycleTime_{fiv} + \beta_4 NumberVaccinations_{fiv} + \mu_i + \gamma_v + \epsilon_{fi} \quad (5)$$

where $Price_{fiv}$ denotes the price paid by recipient $f$ served by agent $i$ in village $v$. $WalkingTime$ and $MotorcycleTime$ measure the time it takes to walk or take a motorcycle (in minutes) to travel between the recipient's home and the agent's headquarter. $NumberVaccinations$ is a continuous variable measuring the total number of vaccinations applied for recipient $f$. I also employ village level fixed effects that control for cross-village service delivery costs. To measure the variation explained by such cost proxies, I contrast the residual sum of squares from estimating equation 5 when including only the agent fixed-effects $\mu_i$, with the residual sum of squares from the full specification. I find that controlling for cost measures is only able to explain 7% of the within-agent price variation. Taken together, the results presented so far therefore show that the price-cap treatment was effective in reducing average prices and price discrimination.

## 5.2 Who benefits from the Price-Cap?

In light of the previous results on price discrimination, it is instructive to investigate who benefits most from the price-cap. Consistent with the model presented in section 3, this section shows that the price-cap especially benefits recipients with a high need for the service. To show 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 proxy for the recipient's elasticity of demand 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 and 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.

I consider two proxies for recipients with a high need. First, I investigate whether recipients whose main source of income is from agriculture benefit more from the price-cap. This characteristic is likely positively related to valuation of the service as ND vaccinations directly affect the household's main income source. Approximately 80% of households in the control group match this definition. Second, I investigate prices for smallholders owning fewer than 11 chickens. Such households are likely to be poorer and hence more susceptible to shocks to livestock holdings. Approximately 30% of households in the control group are characterized as smallholders.

Table 4 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. 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 household but approximately 50% larger and statistically significant at the 10% level for agricultural households.

Column 2 shows that agents do not only price-discriminate 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 4, 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.[36] Reducing discretion doesn't only reduce average prices for all recipients by 12% but also eliminates this quantity discount. Taken together, the price-cap intervention appears to particularly benefit agricultural households and smallholders.

[36] 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.

## 5.3 Impact on Service Allocation

When considering the delivery of essential services, governments typically are not only interested in the distribution of surplus, but also in how many and what kind of recipients are served. While the previous section has shown that price-caps reduce agents' ability to extract surplus, it is not clear how this affects service allocation. This section highlights two channels through which reducing discretion affects service allocation. First, price-caps increase the affordability of services. In light of the evidence on price discrimination presented in the previous section, this effect is likely to be especially pronounced for farmers with a high need for the service. Second, price-caps reduce agents' expected profits from serving a given village, which reduces their incentives to incur the travel costs associated with travelling to remote villages.

### 5.3.1 Price-Caps increase Affordability

I first investigate how price-caps affect the likelihood that needy farmers are served. As the previous section and the model have shown, if agents are not able to perfectly price discriminate, they will charge higher prices for recipients with a lower elasticity of demand, conditional on marginal cost. By removing this kind of price discrimination, reducing discretion therefore doesn't only redistribute surplus but can also increase the likelihood that recipients with a low elasticity of demand are served.

In order to provide evidence for this mechanism I first show that the price-cap indeed crowds in new recipients. In particular, columns 1 and 2 in table 5 show that agents in the price-cap group are 12% to 15% more likely to extend services to previously unserved farmers than agents in the control group.

I then focus on the two proxies for a low elasticity of demand discussed previously. In particular, I investigate how the price-cap affects the likelihood of serving farmers whose main source of income is derived from agricultural production and the likelihood of serving smallholders. As discussed in section 5.2 and shown in table 4, reducing discretion reduces transaction prices more for such recipients. Columns 3 and 4 in table 5 show 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. While not statistically significant, the point estimate in columns 5 and 6 in table 5 suggest that smallholders are 9% more likely to be served in the price-cap group.

Taken together, the results presented in this section are consistent with the theoretical framework: In the absence of price-caps, agents use their discretion to extract rents from service recipients. Capping prices redistributes surplus to recipients and makes services more affordable for recipients in need.

### 5.3.2 Price-Caps reduce Geographic Coverage

The model in section 3 illustrates how the price-cap treatment can reduce agents' incentive to serve remote villages. 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, agents provided a list of all villages in their area of responsibility and the approximate travel time by foot to each village 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 and on the receipts to the village list collected during the baseline survey, to obtain information on whether agents visited a given village.[37]

Table 6 shows how capping prices affects agents' choice of villages. 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 crowds in close farmers in need at the expense of service provision to remote areas.

### 5.3.3 Impact on Remote Farmers in Need

I provide an additional piece of evidence on the the aforementioned trade-off by focusing on the presence of private animal health service providers in villages as a dimension of heterogeneity. Private providers are agents' only competitors as they offer an alternative ND vaccine aimed at larger scale farmers (*"La Sota"*). As such, their presence increases farmers outside option from rejecting the providers' price offer. In my sample, approximately 11% of farmers in the control group have access to a private provider.

Table 4 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 private providers. In contrast, the treatment effect for farmers with access to private providers is positive, small and statistically indistinguishable from zero. This suggests that competition induced by private providers reduce 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 unserved by private providers.

As a lack of private providers is also more likely to be present in remote areas, the reduction in available surplus as a result of the price-cap substantially reduces incentives to target villages without private providers. To assess this intuition, column 2 in table 7 investigates

[37] 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.

how price-caps affect the likelihood that a farmer with no access to a private provider will be served. The results show that the price-cap treatment reduces the likelihood that farmers without access to private providers are served by 3% to 4%. Taken together, the results therefore 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 they reduce the likelihood that agents will travel to them.

#### 5.3.4 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 3 investigates this questions 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, and shows that the difference between the daily number of farmers served is statistically indistinguishable from zero for 18 out of the 21 days of the campaign. Column 1 in table 8 confirms this impression: agents in the price-cap treatment serve on average 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 8 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 8 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.

### 5.4 Caveats and Alternative Explanations

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 can challenge 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, which doesn't differ significantly between treatment and control group.

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.[38] 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.[39] 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 access to 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.[40]

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 will never be possible to conclusively rule out the possibility of collusion in reporting, the experimental design requires a high level

[38] 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).

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

[40] A similar concern is that misreporting is distributed unevenly across agents, implying a heterogeneity between honest reporters and employees who misreport their performance. Figure 6 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.

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 8, 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 that are unlikely to justify large-scale misreporting.

Fifth, one might be concerned that 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 I do not have data on prices for such transactions, 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. My data 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.

# 6 Sufficient Statistics for Welfare Analysis

The previous section has shown that price-caps crowd-in previously unserved recipients at the expense of remote farmers. Given those countervailing forces, it is not clear whether it is optimal for the government to regulate prices, and, if so, to what extent. To address this question, this section develops a simple model of monopoly regulation. The model expresses the optimal price-cap as a function of two sufficient statistics, which allows me to use the empirical results to evaluate the optimal price-cap for the service I study. 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. I present the derivation for this extension in the appendix.

## 6.1 Sufficient Statistics Formula for Uniform Pricing

Governments choose the price-cap to maximize social welfare which, for a generic uniform price, is given by (see also equation 4):

$$SWF\left(p\right)=M\left(c^{*}\left(p\right)\right)\left[\int_{p}^{\infty}v_{i}-pdF\left(v_{i}\right)\right]+gM\left(c^{*}\left(\bar{p}\right)\right)\left(p-\tau\right)D\left(p\right)-g_{a}\int\limits_{0}^{c^{*}\left(p\right)}c_{i}dM\left(c_{i}\right) \quad (6)$$

$g$ denotes the welfare weight on agents relative to recipients. Intuitively, 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 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 $\tau$. 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.[41] Taking derivatives of equation 6 and using the definition of $c^{*}\left(p\right)$, the marginal effect on welfare is given by:

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

[41] 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 first and second term capture the extensive and intensive margin effects, respectively. 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 $\theta$ the extensive margin elasticity of village visits with respect to the price-cap. Formally:

$$\theta = \frac{\partial M\left(c^{*}\left(p\right)\right)}{\partial p}\frac{p}{M\left(c^{*}\left(p\right)\right)} \tag{7}$$

Second, I denote by $\varepsilon_D$ the price elasticity of demand:

$$\varepsilon_D = \frac{\partial D\left(p\right)}{\partial p}\frac{p}{D\left(p\right)} \tag{8}$$

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

$$CS\left(p\right) = \int_{p}^{\infty} v_i - pdF\left(v_i\right) \tag{9}$$

Using those 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 $\theta$ and $\eta$ as sufficient statistics:*

$$\frac{\partial SWF\left(p\right)}{\partial p} = \theta N\left(p\right)\frac{CS\left(p\right)}{pD\left(p\right)} + \varepsilon_D g N\left(p\right)\frac{p-\tau}{p} - \left(1-g\right)N\left(p\right) \tag{10}$$

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, that captures the redistributive effect of the price-cap, disappears.

## 6.2 Extension to Price-Discrimination

It is straightforward to extend this analysis to price-discrimination when there are two types of buyers in the market: One with a high elasticity and one with a low elasticity of demand. Notice that this, given the bimodal distribution of prices, is the likely scenario that applies to this study. Denote by $\varepsilon_D^L$ and $\varepsilon_D^H$ the demand elasticities of the low and high elasticity customers, respectively. In addition, denote by $\mu$ 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:*

$$\frac{\partial SWF(\mathbf{p})}{\partial p(\varepsilon_D^L)} = \theta N(\mathbf{p}) \frac{g_{c1}\mu CS\left(p|\varepsilon_D^L\right) + g_{c2}(1-\mu) CS\left(p|\varepsilon_D^H\right)}{p(\varepsilon_D^L)\left(\mu D^L(\mathbf{p}) + (1-\mu) D^H(\mathbf{p})\right)} \tag{11}$$

$$+ \quad \varepsilon_D^L g_a \mu N^L(\mathbf{p}) \frac{p\left(\varepsilon_D^L\right) - \tau}{p\left(\varepsilon_D^L\right)} \tag{12}$$

$$- \quad \left(g_{c_1} - g_A\right) N^L(\mathbf{p})\mu \tag{13}$$

$N(\mathbf{p})$ denotes the total number of vaccinations applied and $N^L(\mathbf{p})$ the number of vaccinations applied for recipients with a low elasticity of demand. Further, $g_{c_1}$, $g_{c_2}$ and $g_a$ denote the government's welfare weights on low elasticity customers, high elasticity customers and the agent, respectively.

# 7 Estimating the Elasticity of Demand

The discussion has shown that welfare analysis requires knowledge of 2 parameters: The extensive margin elasticity of village visits with respect to the price-cap and the intensive margin elasticity of demand. While the price-cap treatment allows me to estimate the former, 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. The traditional approach to estimating demand elasticities would be to generate 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, however, challenging as it generates incentives for agents to report fewer vaccinations than were actually conducted.[42] 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. This allows me to induce price variation through a treatment that varies fixed costs, which does not generate any incentives to incorrectly report vaccination. This section first presents a brief overview of the lab-in-the-field experiment that documents agents' average cost pricing behavior before presenting the estimates of the elasticity of demand.

## 7.1 Aside: 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

[42]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.

heuristics that are easier to calculate than profit maximizing prices based on marginal costs.[43] 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.

### 7.1.1 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.[44] 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 price for which they would serve a customer instead of walking away from the deal.[45] 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.

[43] 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.

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

[45] 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.

### 7.1.2 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.[46] 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.

### 7.1.3 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 $\gamma_r$ 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 9 presents the treatment effects of the fixed cost treatment on agents' choices of 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 investigates 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

[46] Figure 7 shows an example of the game sheet.

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.

### 7.2 Elasticity Estimates

Consistent with the evidence from the lab-in-the-field experiment, I now show that the participation cost treatment indeed 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%.[47] Inconsistent with a theory based on marginal cost pricing, agents therefore again appear to consider fixed costs when making pricing decisions.

Second, column 2 in table 10 shows that the rise 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.[48] 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 whether an agent was in a participation cost treatment ward or not. 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.

## 8 Welfare Analysis

Sections 5 and 7 presented my estimates for the two sufficient statistics needed to estimate the welfare effect of marginally reducing prices below the full discretion level. This section uses those estimates to calculate the marginal welfare effects to determine the optimal price-cap policy.

### 8.1 Calibration

In addition to knowledge of the two sufficient statistics, $\theta$ and $\varepsilon_D$, 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 costs. To estimate consumer surplus, I assume that demand follows a constant elasticity demand func-

[47] 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.

[48] One might be concerned that the participation cost treatment didn't 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 weren't 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.

tion. 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 $\tau$ 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 presented previously has, however, suggested that agents set prices as mark-ups over average instead of marginal costs. In light of this, I also bound the estimates by assuming that $\tau = 0$.

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, estimation 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 for this 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 $\mu$, the share of low elasticity households. As I cannot obtain this directly from the data, I bound my estimates by setting $\mu$ to either 0, 0.5, or 1.

## 8.2 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 3% to 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 lead to a welfare decrease, which makes no regulation the optimal policy for ND vaccinations. Second, the implied welfare losses are substantially larger for the price-discrimination case 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 to the magnitude of 0.4% to 2.50% of total sales revenue per agent on the margin. Taken together, the results therefore conclusively show that price-cap regulation faces a tension between intensive margin demand effects, which increase welfare, and extensive margin effects, which 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.

# 9 Conclusion

This article examines how price-cap regulation affects the delivery of essential services. It does so in the context of veterinary service provision in Tanzania. Theoretically, the effect of price-caps is ambiguous. On the one hand, the absence of price-caps gives suppliers access to rents, which provide incentives to extend services to new markets. On the other hand, the process of rent extraction in itself can lead to distributional concerns and reduce the likelihood that recipients with a small elasticity of demand are served. This paper provides, to the best of my knowledge, the first within-organizational empirical evidence on the costs and benefits of price-cap regulation.

To identify the effect of price-caps, I conduct a field experiment that creates exogenous variation in regulation. I find that price-caps indeed faces a trade-off between making services available to close farmers in need and serving remote areas. Using a sufficient statistics model, I then show that price-cap regulation leads to a welfare loss on the margin, making no regulation the optimal policy for Tanzania.

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# Tables

Table 1: Treatment Groups

| | | Price Cap | |
|---|---|---|---|
| | | **No** | **Yes** |
| **Fixed Cost** | **No** | 137 wards (210 agents) | 140 wards (212 agents) |
| | **Yes** | 137 wards (200 agents) | 136 wards (210 agents) |

Table 2: Summary Statistics and Balance Table

| | Price-Cap Experiment | | | Participation-Cost Experiment | | |
|---|---|---|---|---|---|---|
| **Panel A: Agent Level** | Control | Treatment | P-Value of Difference | Control | Treatment | P-Value of Difference |
| Tenure | 12.466 | 12.198 | 0.743 | 11.947 | 12.734 | 0.336 |
| | (0.602) | (0.556) | | (0.564) | (0.592) | |
| Ward Level agent | 0.663 | 0.642 | 0.531 | 0.682 | 0.622 | 0.075 |
| | (0.127) | (0.144) | | (0.023) | (0.025) | |
| Number of Villages | 4.022 | 3.974 | 0.804 | 4.043 | 3.951 | 0.639 |
| | 0.040 | 0.019 | | (0.132) | (0.141) | |
| Animal Health Specialist | 0.434 | 0.476 | 0.241 | 0.486 | 0.424 | 0.086 |
| | (0.025) | (0.025) | | (0.025) | (0.025) | |
| Main Income Earner | 0.866 | 0.864 | 0.938 | 0.869 | 0.860 | 0.708 |
| | (0.017) | (0.018) | | (0.017) | (0.018) | |
| Uses Motorcycle | 0.446 | 0.400 | 0.204 | 0.445 | 0.400 | 0.215 |
| | (0.024) | (0.027) | | (0.025) | (0.026) | |
| Secondary Income Source | 0.659 | 0.604 | 0.129 | 0.647 | 0.615 | 0.365 |
| | (0.026) | (0.025) | | (0.025) | (0.026) | |
| Acting Village Leader | 0.144 | 0.152 | 0.753 | 0.133 | 0.163 | 0.213 |
| | (0.017) | (0.018) | | (0.017) | (0.018) | |
| Raises Livestock | 0.798 | 0.796 | 0.934 | 0.816 | 0.777 | 0.193 |
| | (0.021) | (0.020) | | (0.020) | (0.021) | |
| **Panel B: Work Station** | | | | | | |
| Rural | 0.844 | 0.820 | 0.443 | 0.836 | 0.827 | 0.759 |
| | (0.021) | (0.023) | | (0.022) | (0.022) | |
| Average Travel Time | 80.015 | 90.842 | 0.366 | 88.781 | 81.933 | 0.562 |
| | (4.757) | (10.990) | | (10.579) | (5.228) | |
| Private Veterinarian | 0.156 | 0.197 | 0.176 | 0.149 | 0.205 | 0.065 |
| | (0.020) | (0.022) | | (0.019) | (0.023) | |
| Private Drug Seller | 0.029 | 0.033 | 0.763 | 0.031 | 0.032 | 0.945 |
| | (0.008) | (0.010) | | (0.009) | (0.009) | |
| Poultry Area | 0.076 | 0.062 | 0.433 | 0.062 | 0.076 | 0.434 |
| | (0.013) | (0.012) | | (0.012) | (0.013) | |
| Observations | 410 | 422 | | 422 | 410 | |

**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: 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 $\times$ 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 5: 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 6: 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 |
| Data Source | Farmer Survey | Farmer Survey | Farmer Survey |
| 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 7: Effect of Treatment on Likelihood of Serving Remote Farmers in Need

| **Outcome Variable:** | **Price** | **Village has no private provider** |
|---|---|---|
| | (1) | (2) |
| Price-Cap Treatment | 3.887 | -0.0355** |
| | (8.379) | (0.0177) |
| No Private Provider | 5.702 | |
| | (3.982) | |
| Price-Cap × No Private Provider | -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 village has a private provider of substitute ND vaccines. Column 2 presents coefficient estimates of a regression of a binary variable indicating whether a village has a private provider of substitute ND vaccines on an indicator variable for the treatment, district fixed effects as well as ward-level stratification and control variables.

Table 8: 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 9: 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.

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 $\epsilon_D$ | High $\epsilon_D$ |
| 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: Effect of Price Cap on Price Distribution

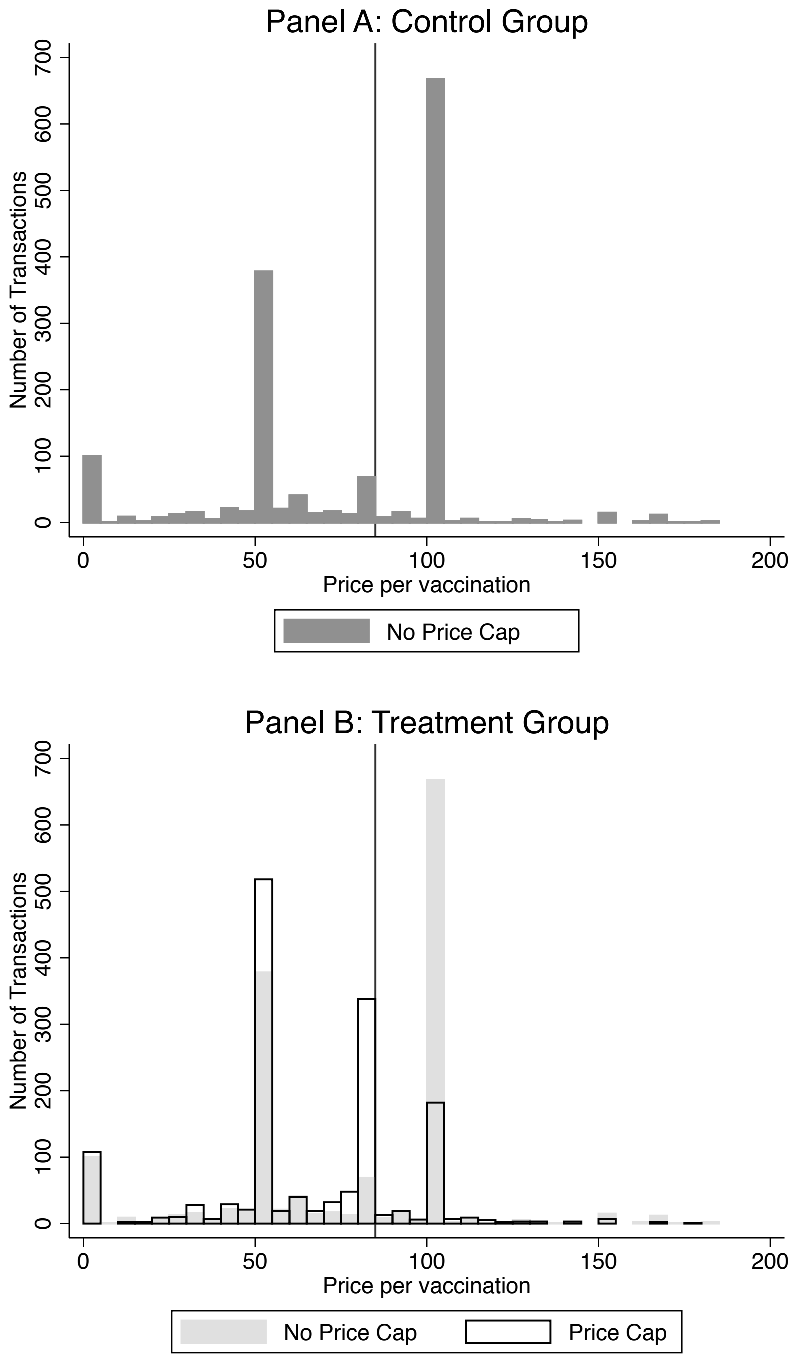
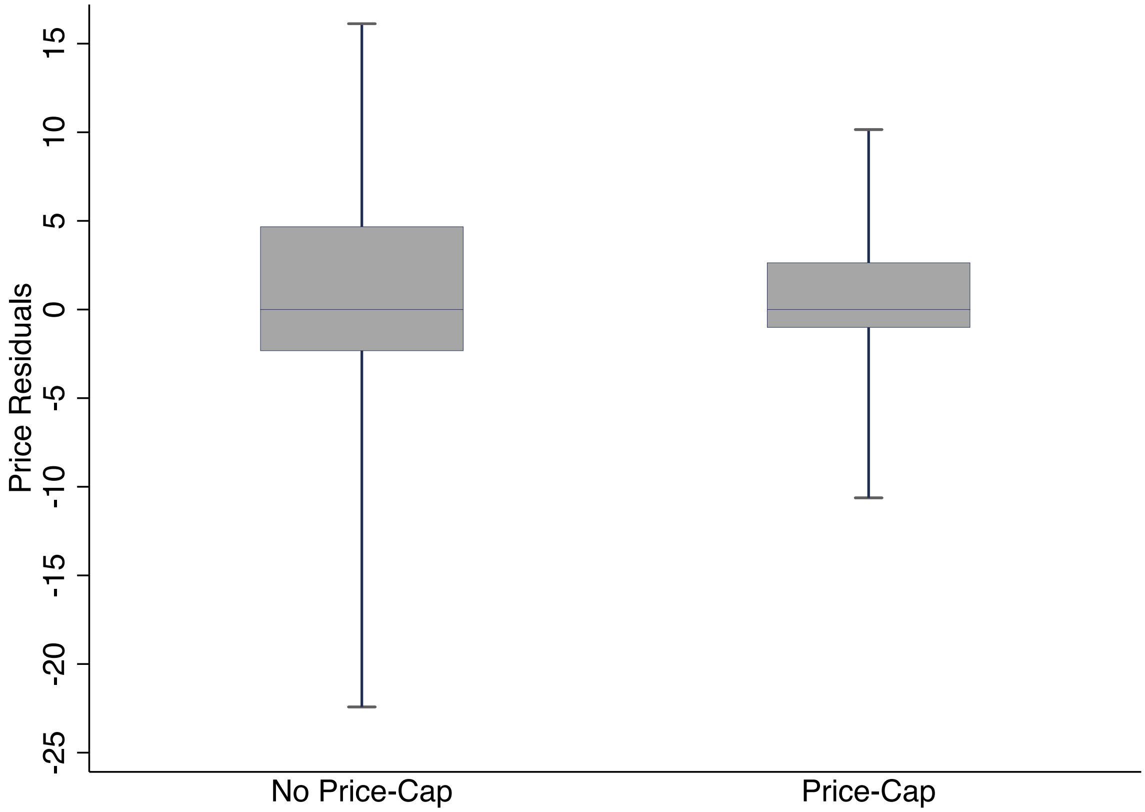
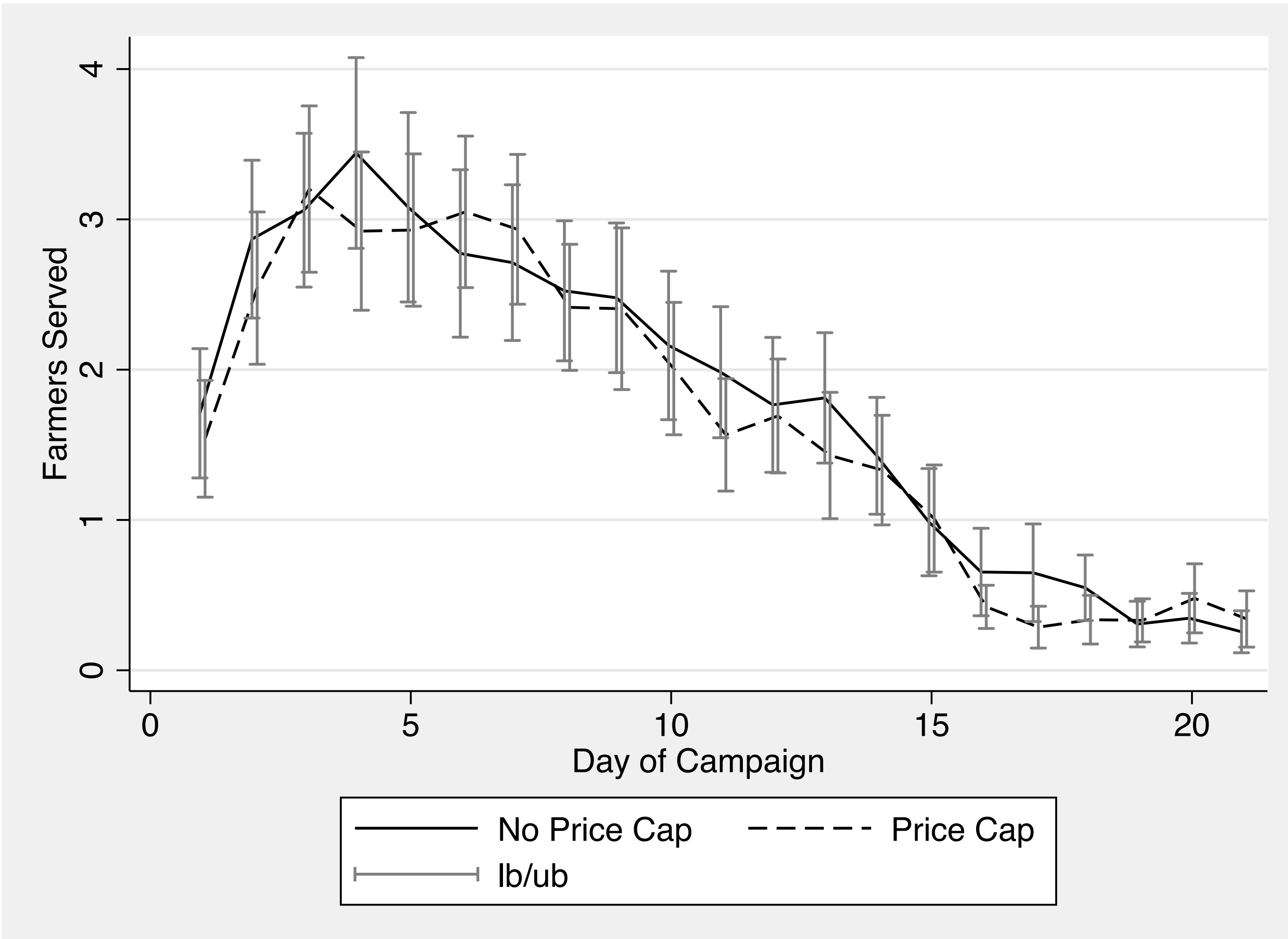


Figure 2: Box Plot of Price Variation



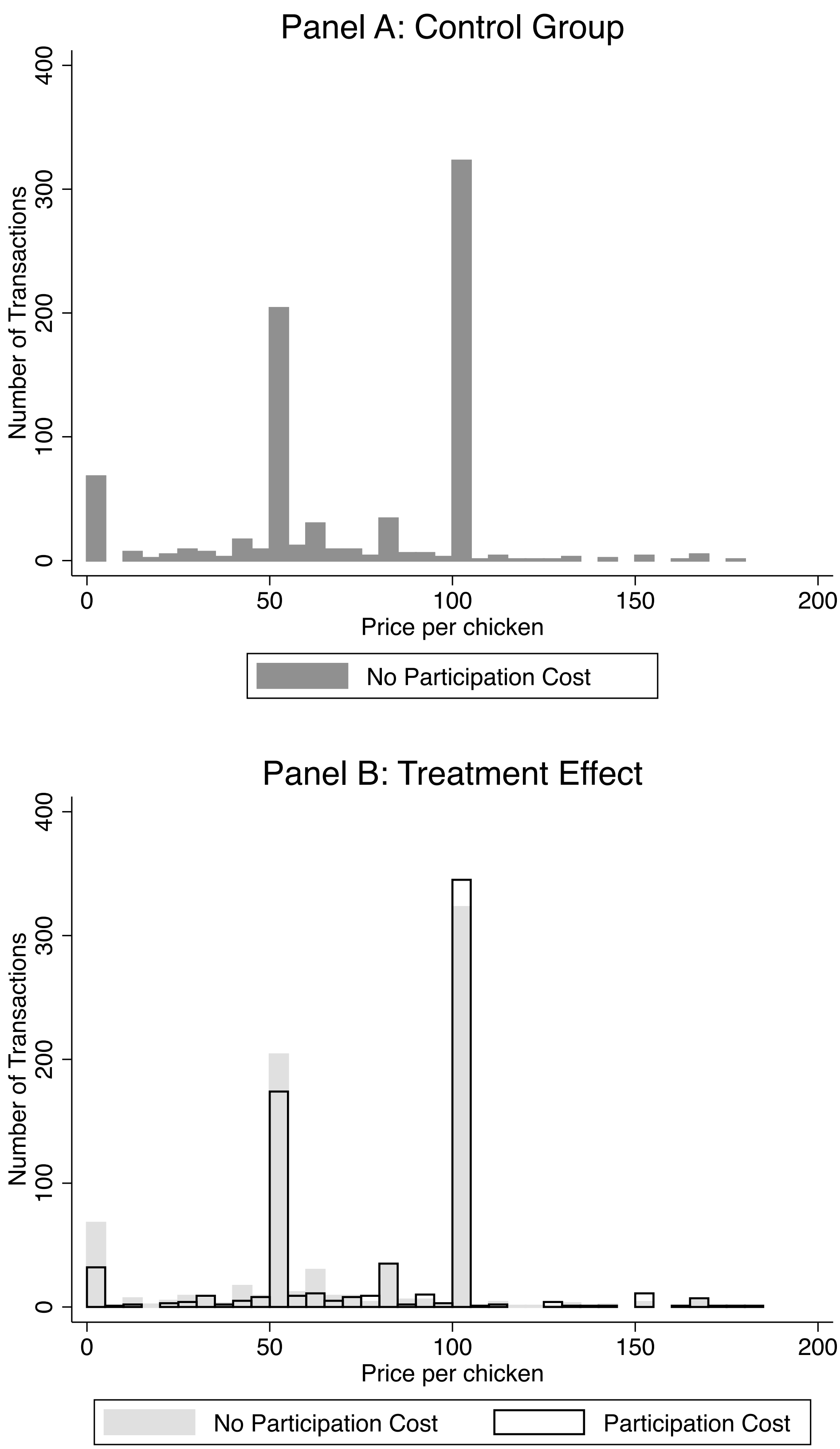

**Notes:** The figure shows box plots of residuals of a regression of prices on agent fixed effects. The regressions are estimated using receipt data. The box denotes the distribution of observations between the 25th and 75th percentile. The whiskers denote the length between the 10th and the 90th percentile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

Figure 3: 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.

Figure 4: Effect of Participation Cost on Price Distribution



# 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-Deininger 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?

I begin by investigating whether allocating discretion to agents enables them to extract the entire surplus from recipients. This is key for two reasons. First, if agents are able to extract the entire surplus then redistributive policies become necessary, as public service delivery effectively provides no benefit to recipients. Second, while perfect rent extraction gives rise to distributional concerns, the allocation of services is efficient in the status quo, which alleviates concerns about distortions resulting from rent extraction. 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.[49]
Figure 4 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 rent extraction.

# 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

[49] 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.

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: 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.

# E Appendix: Supplementary Figures

Figure 5: 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 6: Distribution of Unverifiable Receipts

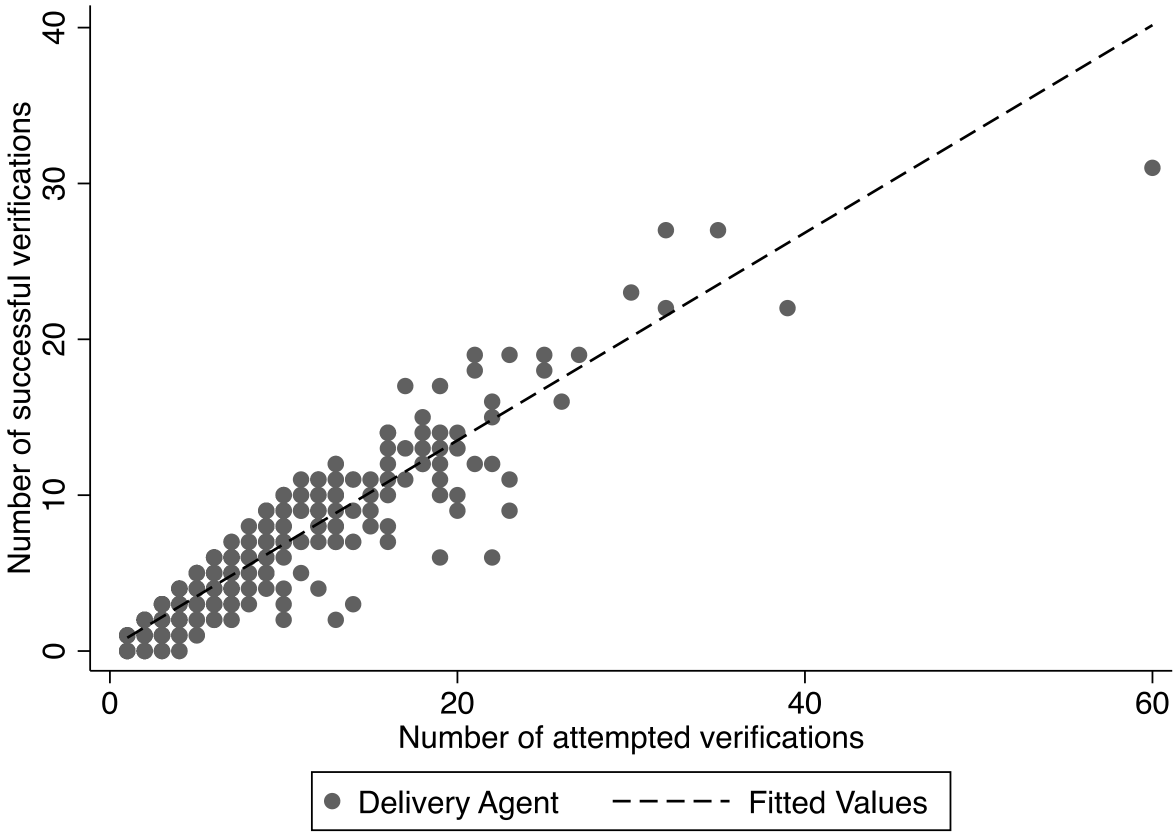


Figure 7: Information Sheet for Participants of the Lab-in-the-Field Experiment


**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 | | |

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