

No bulls: Experimental evidence on the impact of veterinarian ratings in Pakistan*

Ali Hasanain[†] Muhammad Yasir Khan[‡] Arman Rezaee[§]

September 20, 2019

Abstract

We design and implement an information clearinghouse to measure, through crowdsourcing, veterinarian success at artificial insemination service provision and prices charged and to reveal back to consumers these ratings in Punjab, Pakistan. We measure the impact of clearinghouse ratings using a randomized controlled trial. We find that, compared to control farmers, farmers receiving ratings enjoy 25 percent higher insemination success and no higher prices. We find that these effects are due to increased veterinarian effort, rather than farmers switching veterinarians. These results help us understand the nature of asymmetric information in an important market and suggest large welfare benefits from a low-cost information intervention.

*We thank Eli Berman, Michael Callen, Julie Cullen, Clark Gibson, Craig McIntosh, Edward Miguel, Karthik Muralidharan, Monica Singhal, and faculty at UC San Diego for their support at all stages of this project. We also thank Saad Gulzar, the International Growth Centre Pakistan office, the Punjab Livestock and Dairy Development Department, and the World Bank Pakistan office for help designing and implementing the project. Excellent research assistance was provided by Amanullah Haneef, Umair Khawaja, Zia Mehmood, and Zarak Sohail. We thank Sarojini Hirshleifer and Janna Rezaee for their excellent feedback. This research was supported by the University of California Office of the President UC Lab Fees Research Program Grant ID No. 23855, by funding from the Abdul Latif Jameel Poverty Action Lab and the Center for Effective Global Action through the Agricultural Technology Adoption Initiative, and by the International Growth Centre. Support for Rezaee's time was provided by AFOSR # FA9550-09-1-0314 and ONR # N00014-14-1-0843.

[†]Lahore University of Management Sciences. email: hasanain@lums.edu.pk

[‡]University of California, Berkeley. email: yasir.khan@berkeley.edu

[§]University of California, Davis. email: abrezaee@ucdavis.edu

1 Introduction

Asymmetric information is ubiquitous across the developing world and often leads to sub-optimal outcomes for the rural poor there (World Bank, 2004; Wild et al., 2012). This asymmetric information can be leveraged by rent seeking government agents (Ferraz and Finan, 2011; Bandiera et al., 2009; Reinikka and Svensson, 2004; Chaudhury et al., 2006) or private agents (Jensen, 2007; Svensson and Yanagizawa, 2009; Aker, 2010). This asymmetric information also means potential gains from trade go untapped, and may lead to sub-optimal outcomes for both poor consumers and agents in steady state. And while the contexts and players vary wildly, this asymmetric information can often be modeled following canonical theories of moral hazard and/or adverse selection (Hölmstrom, 1979; Akerlof, 1970).

We study asymmetric information in an important context across the developing world: agricultural service provision. More specifically, we study the market for artificial insemination (AI) of livestock in rural Punjab, Pakistan.¹ The market for AI is fraught with asymmetric information about veterinarian effort, leading to AI success rates lower than what is possible given the technology, costing farmers potential income in calves and milk and veterinarians a possible share of this income.

We measure and reveal to consumers veterinarian success at AI service provision through an information clearinghouse similar to a yelp.com or angieslist.com. The clearinghouse provides households with government veterinarians' average success rates at artificially inseminating livestock, an objective measure of veterinarian effort, and the average price charged for each AI service in one district of Punjab, Pakistan. It gathers and disseminates locally relevant effort and price information from a large base of farmers automatically, in real time, using a call center.

We measure the impact of decreased asymmetric information via this clearinghouse using

¹AI is crucial to renewing livestock. Most households only keep female cows because of the dual advantage of producing milk and calves, both of which require cows be pregnant. Livestock agriculture accounts for 12 percent of GDP in Pakistan, and is a key growth sector for the rural poor (Pakistan Economic Survey 2013-14).

a randomized controlled trial. We find that farmers treated with information on local government veterinarians' AI success rates and prices have a 25 percent higher AI success rate than controls and they do not pay any higher prices.² In addition, treatment farmers who specifically return to a government veterinarian for AI rather than seeking a private provider after treatment selection³ see 61 percent higher AI success rates. While these results use a representative sample of farmers that we surveyed before and after treatment, we also find comparable results using data generated by our clearinghouse itself.

And, crucially, we find that treatment farmers enjoy these increased AI success rates and decreased prices *without switching government veterinarians*. Thus the effect cannot be driven by farmers simply switching to the 'best vet' in terms of AI success and/or price. And an additional result suggests that the effect cannot be driven purely by changes in farmer behavior.⁴ These results rule out a pure adverse selection model and support one of moral hazard. These results also suggest that, despite high transaction costs in this market, including the cost of switching veterinarians, veterinarians choose to pay a higher effort cost when providing AI rather than potentially losing a customer. This is not a surprise as, in addition to giving out information on success rates and prices, our clearinghouse also gave households the phone numbers of the three top-rated local veterinarians if they were interested. This presumably lowered farmer switching costs and gave farmers a credible outside option.⁵

Our results fit the context—artificial insemination requires unobserved effort in at least

²The estimated treatment effect on log AI price is negative but insignificant.

³In this setting, there are government and private veterinarians but only government veterinarians were rated by our clearinghouse, and only farmers originally contracting government veterinarians were considered for treatment selection.

⁴It is also possible that learning something about AI success rates in general causes farmers to take better care of their livestock and that this in turn increases AI success rates. However, we find that treatment farmers who subsequently switch to private providers do not have increased AI success rates. If our treatment effects were driven by changes in livestock care, we would expect to see effects regardless of which provider farmers subsequently choose.

⁵Others have addressed the impact of improving farmers' credible outside options in their negotiating with service providers. See, e.g., Dreze and Sen (1989); Basu et al. (2009); Muralidharan et al. (2017). This can also be thought of as an increase in contestability, as in Baumol (1986). Note that in the long run increased contestability could even lead to welfare improvements for veterinarians, though such is outside the scope of this paper.

two ways. First, veterinarians must keep semen straws properly frozen in liquid nitrogen canisters from the time when they are delivered to AI centers until right before insemination. Second, veterinarians must then precisely insert these straws during insemination. At the same time, farmers cannot infer a veterinarian's effort from outcomes alone. Even when executed properly, AI will not be successful 100 percent of the time, and success rates may vary based on animal health and nutrition.

In addition, while government veterinarians collect a salary and are protected from punishment for poor performance, they are legally allowed to charge a 'show-up' fee to farmers for their services on top of the fixed cost of AI. Therefore, in response to their low unobserved effort being revealed to farmers, government veterinarians may prefer to exert more effort and continue to collect a fee than to lose a customer. In other words, they may internalize the benefits of their marginal effort, a characteristic more common to private than public markets. This ensures they respond when the market becomes more contested.

Several additional results support a standard agency model of asymmetric information. First, we find that farmers' baseline expectations about the average AI success rate of their own government veterinarians do not correlate with actual average AI success rates. This suggests the existence of asymmetric information *ex ante*. Second, treatment causes farmers' endline expectations about their veterinarian to become strongly correlated with the truth. This suggests that farmers indeed update their beliefs. Third, farmers who received more negative information relative to their expectations saw larger treatment effects. This suggests that the amount of information farmers receive determines their benefit. None of these facts in and of themselves distinguish between moral hazard or adverse selection, however—doing so requires measures of farmer and veterinarian behavior as well.

More generally, the market for AI in rural Punjab is one in which informationally disadvantaged consumers pay more than the marginal cost of AI provision through two channels—prices and veterinarian effort. In this market, treatment-induced veterinarian effort implies consumer welfare gains so long as there are no compensating price increases or negative

spillovers onto control farmers, which we do not find. Furthermore, this implies overall social welfare gains so long as the cost to veterinarians' increased effort is not too great.⁶

Our study differs from previous evaluations of the effect of information on markets with only a price channel, where changes in prices are pure transfers and any social welfare gains must come from increased market efficiency (Jensen, 2007; Svensson and Yanagizawa, 2009; Aker, 2010). Many other markets have multiple channels for rents and thus expect similar social welfare gains, including education (Andrabi et al., 2014), elections (Ferraz and Finan, 2011), and markets for private restaurants (Jin and Leslie, 2003).

In such related studies, with the exception of previous clearinghouses evaluated in Fafchamps and Minten (2012) and Mitra et al. (2014) (in both cases, the authors find no treatment effects),⁷ interventions to reduce asymmetric information are costly, static, and/or do not lead to clear social welfare gains. Our clearinghouse, on the other hand, relies on crowdsourcing technology that is cost-effective, self-sustaining, and scalable. Conservative estimates suggest a 25 percent higher AI success rate translates into nearly an additional half of one month's median income per AI provided, a 275 percent return on the cost of the intervention. These effects hold out hope for improved government accountability as cellular technology improves and becomes cheaper.⁸

These differences between our clearinghouse and past interventions to reduce asymmetric information are highlighted in the literature on market-based learning. Supporting consumers in market-based learning, rather than engaging in more hands-on policies such as

⁶We do not believe the marginal cost to veterinarians' increased effort induced by treatment to be very large in this setting, as travel costs are paid either way. Government veterinarians also do not spend any more time visiting treatment farmers. Any costs must be in terms of concentration, etc.

⁷Fafchamps and Minten (2012) cite a low take-up rate as the reason for the failure of an sms-based clearinghouse for crop and weather information. While the rate at which farmers answered the phone was nowhere near 100 percent, we had no problem generating sufficient data for our clearinghouse estimates to be meaningful. Mitra et al. (2014) cite a lack of an outside option as the reason farmers are not able to better leverage information on crop prices. Our clearinghouse directly provided information on outside options. Of course this required contestability could be increased to begin with, which was true in our market (i.e. veterinarians are not monopolists). This will not be the case everywhere.

⁸To further understand the value of the clearinghouse, we investigate whether the data generated by the clearinghouse is biased by either veterinarians (they have to first report providing a service) and/or farmers (they have to answer the phone several times to provide and receive information). We do not find evidence of bias, suggesting the clearinghouse was able to capture and transmit information representative of the truth.

regulating quality directly (Björkman-Nyqvist et al., 2013), may be a less burdensome and more effective intervention for governments to undertake, because it may entail a lower administrative burden and create less potential political choke points. Consumers already learn from each others' aggregate experiences without intervention (Hubbard, 2002). Of course, market-based learning is limited by the ability of consumers to experience quality. Learning happens rapidly in markets with low switching costs and high turnover, such as packaged yogurt (Akerberg, 2003), but more slowly when the converse is true, as is the case with car insurance (Israel, 2005). In our case, quality is fairly easy to experience as it entails more milk and more calves.

Our study also relates to a growing literature on monitoring to improve government service provision. This literature has found mixed results, with research suggesting monitoring may not be effective without complimentary financial incentives (Duflo et al., 2012) and that monitoring's effects attenuate as agents find alternative strategies to pursue rents (Olken and Pande, 2012).⁹ While we cannot speak to the latter given the time frame of this paper, our results are consistent with the former as veterinarians have a financial incentive to maintain customers. In Pakistan specifically, a literature on health monitoring suggests that the mean zero impacts of smartphone monitoring on the performance of doctors may mask important heterogeneity driven by political competitiveness (Callen et al., 2017b) and individual characteristics (Callen et al., 2017a).

Our study also relates to a connected literature focused on community monitoring specifically. This literature has also found mixed results when citizens take collective action to monitor the performance of their public servants (Olken, 2007; Björkman and Svensson, 2009; Banerjee et al., 2010). While households in our study do not act collectively, it did require a sufficient number of households providing information into a collective information system for it to be useful for anyone.

The paper proceeds as follows: Section 2 provides background on our study district and

⁹See Finan et al. (2015) for a review of monitoring efforts as apart of a larger review of the growing literature dubbed the personnel economics of the state.

government AI service provision there, Section 3 outlines our research design, including providing more information on the clearinghouse and the randomized controlled trial embedded within it, Section 4 provides results, Section 5 discusses the interpretation and social welfare implications of these results, and Section 6 concludes.

2 Background

2.1 The market for AI in Sahiwal, Punjab, Pakistan

We implemented our clearinghouse in the Sahiwal district of Punjab province, Pakistan. While we selected Sahiwal based on several logistical constraints, we view it as representative of the whole of Punjab, and of similar agricultural districts across the country, though with a slightly higher prevalence of livestock.¹⁰

Sahiwal has a vibrant market for artificial insemination for at least two reasons. First, almost all livestock in the district are female. Second, artificial insemination decreases the costs of selectively breeding to increase milk yields, as only the semen from high-yielding bulls needs to be transported and not the bulls themselves.¹¹

The government is the largest supplier in this market, offering low-cost AI services by veterinarians who have required AI training. The official cost of government AI is 50 PKR per insemination (approximately 0.5 USD), but government veterinarians are legally allowed to charge a ‘show-up’ fee to cover the cost of their gasoline, as well as any other costs or risks. This results in average costs of approximately 200 PKR per visit. The government has 92 one-room artificial insemination centers or veterinary offices spread throughout the district, staffed by roughly 70 active veterinarians.¹² These veterinarians’ sole job is to

¹⁰According to the 2010 Punjab’s Multiple Indicator Cluster Survey, households in Sahiwal on average have 1.4 fewer acres of agricultural land and .24 more cattle than households in other districts in Punjab. Sahiwal’s average wealth, labor force participation rates, and child mortality rates are representative of Punjab.

¹¹The provincial government selectively breeds livestock in two main centers in Punjab. It then distributes the semen produced to government veterinarians across the province, including in Sahiwal.

¹²Throughout our study period, a total of 77 veterinarians were active in Sahiwal for any amount of time.

provide artificial insemination.¹³

The only other organized supplier in this market is Nestle, but they have far fewer active veterinarians providing AI services in Sahiwal. Most private veterinarians are self-employed, buying semen from large private suppliers and providing AI services without any training. At baseline, these private veterinarians collectively provide approximately 57 percent of AI services across Sahiwal, with government veterinarians making up the remainder.

2.2 Asymmetric information in the market for AI

On a single visit, a farmer can never fully observe veterinarian effort. However, even before our intervention, farmers could have decreased asymmetries by aggregating information about their veterinarians' success rates across visits and across households. Our data suggests that they do not. At baseline, farmers' estimates of their current government veterinarian's AI success rate are uncorrelated with the truth. This can be seen in Figure 5, Panel A.

This asymmetric information contributes to AI success rates that are lower than what veterinarians can achieve. At baseline, AI success rates average approximately 70 percent, while success rates of 85-90 percent are possible with the training and equipment in Sahiwal.

3 Research design

3.1 The clearinghouse

To measure veterinarian prices and effort and to subsequently disseminate that information to consumers, we developed a novel cellular-based information clearinghouse. Figure 1 diagrams the four components of our intervention.

Pre-treatment: During the study, government veterinarians in Sahiwal were required to collect real time information on all AI service provisions using an Android smartphone

Only a handful of veterinarians transferred in or out of Sahiwal.

¹³In some cases they may provide vaccinations during AI service provision, but this occurs very rarely. A smaller, distinct group of veterinarians care for sick animals.

equipped with an Open Data Kit-based application.¹⁴ The data was immediately sent to the clearinghouse. We denote this data collection as $t = 0$ in Figure 1.

Data collection and aggregation: Each service provision generated two subsequent phone calls. First, one day later (denoted $t = +1$ day in Figure 1), a representative from the clearinghouse call center called the farmer to verify that the veterinarian had provided service and to ask what price he had charged. Then, sixty days later ($t = +60$ days), they called again to ask if the artificially inseminated livestock were pregnant. The clearinghouse continuously aggregated this price and AI success rate data for each veterinarian.

Treatment: The clearinghouse collected and aggregated information from January to September, 2014. Treatment began in October 2014, once we had sufficient data on veterinarians to have meaningful measures of price and AI success rates. Treatment took place during the second call (at $t = +60$). Only this time a randomized group of farmers was provided information on local veterinarians' prices and AI success rates. The uninformed farmers became the control group.

Post-treatment: The clearinghouse allowed us to link farmers over time, so we observe post-treatment government AI provision for both treatment and control farmers (if they return; Figure 1 depicts the return of a treatment farmer but not a control farmer). These post-treatment observations also generate two follow-up phone calls.¹⁵

3.2 Information provision

In the treatment group, the clearinghouse representative presented farmers with information on the top three veterinarians within three kilometers of their household in terms of AI success rates for cows, and the top three veterinarians in terms of AI success rates for buffalo.¹⁶

¹⁴In practice, veterinarians did not always comply. See Section 4.3.1 for discussion.

¹⁵Note, however, that treatment selection is carried forward in time. See Section 3.2.

¹⁶When we had fewer than 25 observations for a veterinarian, we weighted success by $\sqrt{n}/5$, where n was the number of observations. By design, almost every veterinarian had more than 25 observations each for cows and buffalo once the treatment began. The exceptions were two veterinarians hired after our treatment began in October 2014.

We gave treatment farmers AI success rates for these three to six veterinarians, and the average price of the service, during the second follow-up call.¹⁷ The clearinghouse then sent a follow-up SMS with the same information. We also gave farmers veterinarians' phone numbers and, if they requested it, information on average farmer-reported satisfaction with veterinarians on a 1-5 scale, and information on any other veterinarian in our system.

The clearinghouse administered treatment at the farmer level through a coin-flip stratified on the nearest government veterinary clinic to a farmer's household. Farmers who returned for service provision after treatment assignment retained their initial assignment. Note that treatment occurred at a different time for each farmer, 60 days after they first entered our clearinghouse. This means that the post-treatment period differs for each farmer.¹⁸

3.3 Representative survey

In addition to the clearinghouse data, we independently surveyed a representative sample of farmers from across Sahiwal. We did so because the clearinghouse sample is not representative: to enter the clearinghouse, farmers first selected government AI over private, then their government veterinarian complied to record their service provision, then we were able to reach them on the phone to collect price and AI success information; and then we only observed post-treatment outcomes for clearinghouse farmers who subsequently returned to a government veterinarian for AI (as opposed to a private provider). See Section 4.3 for a discussion of selection into the clearinghouse by veterinarians and farmers.

For these surveys, we sampled 90 of Sahiwal's approximately 500 villages from a district village census.¹⁹ Within each village, we selected ten households using the Expanded Pro-

¹⁷There can be overlap in the most successful veterinarians in terms of cows and buffalo.

¹⁸Unfortunately, the coin used for randomization was shaved, due to a glitch in the clearinghouse algorithm. This resulted in 52 percent of farmers being treated. However, the probability of treatment remained fixed across farmers across time.

¹⁹We stratified the sample by whether or not a government veterinarian center was in each village and on whether each village bordered an irrigation canal. The sample is representative of Sahiwal in terms of: area, settled area, cultivated area, area of wheat, rice, cotton, sugar cane, pulses, orchards, and vegetables, having a river, distance to the nearest veterinarian center, number of livestock in the village, literacy rates, religion, age, and standard wealth index characteristics. Results available upon request.

gram on Immunization (EPI) cluster sampling method (Henderson and Sundaresan, 1982). We selected households that reported owning at least two livestock (cows and/or buffalo) and having regular access to a cellular phone.

We manually entered survey farmers’ phone numbers into our clearinghouse to generate treatment or control follow-up calls. These calls were near identical to those to farmers that entered our clearinghouse on their own, and the treatment information provision component was identical.²⁰ We also only selected farmers that had reported using a government veterinarian for their last AI, in-line with the clearinghouse.

Sample villages can be seen in Figure A.1. Figure 2 presents a timeline of the clearinghouse and survey data collection. The baseline survey occurred prior to our clearinghouse implementation, and the endline survey occurred immediately prior to the clearinghouse being shut down.²¹

Balance Tables: Tables 1, and 2 report the balance of our clearinghouse and representative survey samples between treatment and control farmers. Appendix table A.1 further tests the balance on characteristics of treatment and control farmers. The study is fairly balanced on the pretreatment co-variates.

3.4 Empirical specifications

We use the following specification for our primary analysis:

$$outcome_{ft} = \alpha + \beta T_f + \Gamma_{ft} + \epsilon_{ft} \tag{1}$$

where $outcome_{ft}$ is an outcome for farmer f from post-treatment AI visit t . T_f is a treatment indicator, Γ_{ft} are treatment strata and other baseline controls to improve precision, and

²⁰The only difference was that instead of asking questions about a specific recorded service provision from 60 days ago as is the case with clearinghouse calls, we asked about farmers’ last AI service.

²¹We conducted a purely technical survey at midline to collect new phone numbers for those households that changed numbers between the baseline and the first round of treatment phone calls. This allowed us to treat as many independently surveyed farmers as possible.

ϵ_{ft} is an idiosyncratic error term. While we administered treatment at the farmer level, treatment information provision was localized at the village-cluster level. We cluster standard errors at this village-cluster level to allow for correlation in outcomes between farmers in the same village-cluster. Village-clusters are groups of villages that share the same government veterinarians within a three kilometer radius. There are roughly two villages per village-cluster.

We define post-treatment for control farmers as all observations after the phone call in which they were selected into control rather than treatment. This ensures balance in the length of the post period between treatment and control farmers.

We have four primary outcomes:

Switched veterinarians_{ft}: a dummy variable equal to one if a farmer’s veterinarian at visit t differed from the farmer’s veterinarian at visit $t - 1$.

Log price_{ft}: the log price paid for AI at visit t , as reported by the farmer when called the next day.

AI success rate_{ft}: a dummy for the success of the AI provided at visit t , as reported by the farmer when called 60 days later.

Returned_f: a dummy variable equal to one if a farmer returned for government AI after treatment by the end of the project.²²

4 Results

In this section, we present results. First, we present treatment effects (Section 4.1). Second, we show that treatment does not induce veterinarian reporting bias (Section 4.3.1) or farmer reporting or selection biases (Section 4.3.2). Third, we explore the primary mechanism for our treatment effects, decreased moral hazard or increased effort by veterinarians for the

²²We pre-specified our empirical specification in our pre-analysis plan, registered in the AEA RCT registry. We did not pre-specify *Returned_f*. We did pre-specify *Switched veterinarians_{ft}*, *Log price_{ft}*, and *AI success rate_{ft}*. We pre-specified the latter two outcomes conditional on veterinarian switching, but we have made them unconditional since we do not observe veterinarian switching.

treated, through heterogeneity analyses (Sections 4.4.1 and 4.4.2).

4.1 Treatment effects

Table 3 presents our primary intent-to-treat effects on prices and AI success rates. Columns (1) and (2) present results for the entire sample of farmers that had been using a government veterinarian at baseline (a requirement for treatment selection) and that returned for AI service provision after treatment selection (i.e. have at least one post-treatment outcome).²³ We find no significant impact on prices paid, though the direction of the effect is negative. We do, however, find a large, significant (at 10 percent) impact of treatment on AI success rates of 17 percentage points, or 25 percent. In fact, treated farmers see their AI success rates raise to 84.2 percent on average, near the theoretical maximum.

Columns (3) through (6) break these treatment effects into those for farmers who either returned to a government veterinarian post-treatment selection (columns (3) and (4)) or instead returned to a private veterinarian (columns (5) and (6)). We see that the impact on AI success rates is being driven entirely by those that returned to government veterinarians after treatment. In this sample, AI success rates increase by 36 percentage points, or 63 percent.

The results in table 3 columns (1) and (2) are the true, unconditional intent-to-treat effect of information provision on AI success rates. As such, we will use this for farmer welfare calculations. We believe, however, that the effect of information provision conditional on farmers returning to a government veterinarian is policy relevant and intellectually quite interesting.²⁴ These conditional results speak toward the ability for our clearinghouse to increase the effort put in by veterinarians who are being ranked by the system (private veterinarians were not apart of the clearinghouse rankings), and could be closer to the

²³In this case we can only use our representative survey data as our clearinghouse data does not follow farmers who seek private veterinarians.

²⁴We will also show in section 4.2 that those who returned to a government veterinarian and those who switched to a private veterinarian do not differ on baseline observables, including baseline AI success rates, prices, and satisfaction.

impacts we might expect if all veterinarians were included in the clearinghouse as would be the case in many markets. As we will discuss in the mechanisms sections below, the fact that treated farmers see improved AI success rates only when returning to government vets will also allow us to rule out changes in farmers' behavior as leading to our results.

Once conditioning on farmers returning to a government veterinarian after treatment selection, we can also measure treatment effects using our clearinghouse sample.²⁵ Table 4 presents treatment effects using this data, in Panel B. In Panel A, it presents results using our survey sample, where columns (4) and (5) are identical to the previous table to allow for comparisons. In Panel B column (4) we see a much larger negative treatment effect on price, though it is also not significant. In Panel B column (5) we find a large impact of treatment on subsequent AI success rate of 20.9 percentage points, or 37 percent. We cannot reject that this effect is the same in magnitude to the effect we find in the survey sample. The fact that we find similar results in two independently drawn samples is particularly reassuring given our small sample sizes.

Table 4 also presents two additional results. In columns (1) and (2), it presents the effects of treatment on a farmer returning for any AI before the end of the study (including private) and for government AI specifically, respectively. In the case of returning at all in column (1), we find no significant impact though the coefficient is positive. We find more mixed results in the case of returning specifically to a government veterinarian for AI. In Panel A column (2), we find a treatment effect of 5.1 percentage points. In Panel B, we find a treatment effect of 2.9 percentage points, which is 41 percent of the control mean in the clearinghouse sample. However, the effect in Panel A is insignificant while the effect in Panel B is significant at 5 percent.²⁶ In column (3), we present the effect of treatment on a

²⁵We acknowledge that clearinghouse data is susceptible to additional selection issues but argue in section 4.3 that selection into the clearinghouse at various stages of the experiment is not the main driver of the patterns that we see in this section.

²⁶The low overall return rate is likely because the average time for farmers between treatment and the end of our study period is five months and AI is only required roughly once a year per animal. As we see in Table 6 as well, only roughly 20 percent of return visits were recorded by veterinarians, so even in five months the true return rate is likely around 40 percent. Also, the fact that the representative survey does not suffer from recording error could explain the difference between panels A and B.

farmer switching which government veterinarian he/she used for AI conditional on returning to a government veterinarian post-treatment selection. In this case, we find a consistent zero impact across both panels. We will discuss this result below as this means the improvement in AI success rates for returning farmers came through specific veterinarians improving rather than farmers switching to better veterinarians.

In Figure 3, we present the treatment effect on AI success rates in real time (as opposed to in pre/post time, where post begins at a different time for each farmer) using the clear-house data. The top panel illustrates that treatment farmers have higher AI success rates throughout most of the post period, while the bottom panel traces the size and significance of this treatment effect over the post period with bootstrapped standard errors. In addition to confirming the results from our regression analysis, these results suggest that there may have been some initial spillovers between treatment and control farmers that then disappeared. This is consistent with anecdotes that suggested that veterinarians initially responded to the onset of treatment phone calls by improving service for everyone but then soon realized only certain people were more informed. Of course, we did not design our study to measure spillovers so we cannot formally test for them. If this bump in control farmer AI success rates was due to spillovers, it would attenuate our results.

The most likely cause of the across-the-board downward trend in AI success rates beginning in March 2014 is changes in leadership of the Punjab Livestock and Dairy Development Department at both the provincial and Sahiwal district levels—the new regime was less focused on veterinarian performance than the last had been.

In Figure 4, we present the treatment effect on log AI prices in real time. We find that the same visual trends hold for prices, and that when we bootstrap standard errors, the treatment effect is significant over a several-month period. This serves to strengthen our insignificant price effect result in Table 4.

Of course, all results beyond the initial ITTs in Table 3 columns (1) and (2) are conditional on selection. In the case of the representative survey, they are conditional on farmers

returning to a government veterinarian after treatment selection. In the case of the clearinghouse data, they are conditional on the same return by farmers as well as conditional on veterinarian and farmer reporting to the clearinghouse itself. So we will now seek to understand these margins of selection.

4.2 Farmers' choice to return to a government veterinarians after treatment selection

Table 5 compares those farmers that chose to return to a government veterinarian after treatment selection with those that chose to instead shift to a private veterinarian (all farmers considered for treatment had been contracting government veterinarians at baseline). Across 23 variables, the means between the two groups of farmers are insignificantly different from each other in 19 cases. In four cases, the groups differ at the 10 percent significance level, though in three out of four of these cases the magnitudes across two groups are still very similar (i.e. 4.59 vs 4.47 for risk willingness on a 0-10 scale). We would argue this is consistent with sampling error and that the two groups look the same on observables. Importantly, farmers baseline AI success rate, visit charges, farmer satisfaction, and farmers' estimates of their own government veterinarian's average AI success rate are all balanced between the two groups. So we find no evidence that farmers that are unhappy with their veterinarians are switching. This is consistent with the results we find in the clearinghouse which we will now turn to.

4.3 Selection into the clearinghouse

In this section, we test for a number of ways in which clearinghouse data can be biased by treatment. We first explore whether treatment induced veterinarian reporting bias (section 4.3.1) and we then study if farmers selection and reporting is biased (section 4.3.2).

Note the goal of this section is twofold. First, we seek to establish that our clearinghouse

data is not subject to selection (on observables) and thus treatment effects estimated using this data can be believed. Second, and more importantly given that we have the representative survey to ensure unbiased ITTs, we seek to understand farmer and veterinarian interaction with the clearinghouse system, which we believe is quite interesting in and of itself.

4.3.1 Treatment does not induce a veterinarian reporting bias

In Table 6, we find that that treatment does not induce a reporting bias among government veterinarians. We measure reporting bias by comparing farmer reports of service provision from our representative survey with entries in the clearinghouse. While government veterinarians only comply by reporting AI approximately 20 percent of the time, they are equally likely to report for treatment and control farmers. And there is not a statistically significant difference in the likelihood that farmers that had their service provision data submitted to the call center report receiving a phone call to verify that service. These results suggest that veterinarians are not biasing the clearinghouse data by choosing only to report when they are sure they were successful. Though of course they are often not reporting at all. Anecdotally many veterinarians struggled to maintain the phone charge and/or often left it at home with their family as this was the first smartphone they had ever had.

4.3.2 Ruling out farmer selection and reporting biases in the clearinghouse sample

We might imagine that farmers that have experienced higher AI success rates and/or lower prices with a government veterinarian could be more likely to select back into government AI after treatment selection, biasing the clearinghouse sample. Similarly, even after farmers return for government AI, they have to answer the phone multiple times to report to the clearinghouse on the prices they paid and whether the AI was successful. We could imagine certain farmers may be more or less likely to report—e.g. those that were less happy might

be more likely to answer and to complain, or vice versa.

In Table 7, we show that the characteristics of control farmers change little with each additional selection margin, the exception being that there is a drop off in response rates by farmers who live farther away from the closest AI center both in answering the phone for the first time and then in subsequently returning for AI (and having their return service recorded by their government veterinarian). This is not surprising, as farmers living far away from AI centers may not use the public AI as frequently as the ones living nearby.

We, of course, might also worry that treatment itself biases the returning clearinghouse sample. In Table 8 we show treatment balance on all measured pre-treatment outcomes for those farmers who return for government AI and answer subsequent phone calls. While this does not rule out selection on unobservables, we believe that it does rule out the most likely type of selection that could drive such a large increase in AI success rates in our post-treatment sample—selection back into government AI by farmers who have younger, healthier livestock more likely to get pregnant. If this selection were occurring, such younger and healthier animals should have then been more likely to get pregnant in the pre-periods as well, yet we do not see this. We also do not see any differences in past prices paid, past veterinarian switching, or other administrative variables.

4.4 Heterogeneous Effects

In order to explore the mechanism for our treatment effects, we present a series of heterogeneous treatment results that support a standard moral hazard model as being explanatory in this setting.

4.4.1 Treatment effects by government veterinarian rank

First, in Table 9, we present treatment effects for two important sub-populations, separated according to the ranking of the last government veterinarian who served them—those for whom this veterinarian was ranked in the top three in their village-cluster, and those for

whom he was not. This aligns with those veterinarians on whom treatment farmers received information regarding AI success rate and price. We separate control farmers based on what they would have been told, had they been treated.²⁷

We find suggestive evidence that our main results are localized to farmers whose past veterinarian was not ranked in the top three in their area at the time of treatment.²⁸ Again, this is in line with a standard moral hazard model. The more a farmer learns a veterinarian can increase unobserved effort, the more s/he is able to then bargain away rents from the veterinarian.²⁹

Perhaps the most surprising result in Table 9 is that farmers whose past veterinarian was not ranked in the top three are more likely to return. To investigate this, we show in Table A.2 that farmers in Table 9 Panel B tend to live almost twice as far away from their closest veterinary center. This is consistent with farmers living in more remote areas settling for lower effort veterinarians because of higher switching costs. And it is exactly these farmers with higher switching costs that receive the largest benefits from treatment.³⁰

4.4.2 Results using farmer expectations from the representative survey sample

If we are to believe that our results are in line with a standard moral hazard model, we should expect the level of asymmetric information between farmers and veterinarians at baseline to be important. We present three results in this vein, in this case using farmers' stated expectations. These expectations come from our representative survey sample, in which we asked farmers what they expect the average AI success rate of their past veterinarians to be.

²⁷Note that at the beginning of our treatment phone calls we verify farmers' villages as they were automatically generated by GPS. This verification is not done with control farmers. To avoid measurement error correlated with treatment, we separate treatment farmers based on what they would have been told had we not verified their village. This hypothetical information set correlates with the truth at over 90 percent.

²⁸These results are suggestive because, while the point estimates are qualitatively different, we cannot reject this difference with significance.

²⁹We should also expect heterogeneous treatment effects based on whether or not a farmer's past government veterinarian was ranked top in their village-cluster versus second best, or second best versus third best, etc. We do not have power to accurately detect these differences, but results are consistent with the same simple model. Results available upon request.

³⁰In addition, these farmers have more buffalo. They also pay slightly more on average, which is consistent with larger 'show-up' fees due to higher veterinarian travel costs.

In Figure 5, we compare farmers' expected average AI success rate for their veterinarian prior to treatment with the actual average AI success rate of that veterinarian. Actual average AI success rates are drawn from our clearinghouse data prior to October 2014 when treatment calls began.

Our first result is in Panel A of the figure—at baseline there is no correlation between farmer expectations and the true AI success rate of their veterinarian. This suggests there is room to improve service delivery by relieving asymmetric information.

Our second result is in Panel B of the figure—at endline there is a strong correlation between expectations and the truth for treatment farmers. In other words, treatment changes expectations. This is a crucial test that information was passed on through our treatment. Panel C presents the endline correlation for control farmers—while much smaller than with treatment farmers and insignificant, there is a positive correlation. This suggests potential information spillovers between treatment and control farmers, which would attenuate our treatment results above and is consistent with visual trends.

Point estimates for these two results are reported in Table 10. The null hypothesis that the coefficients in columns (2) and (3) are equal is almost rejected, with a p-value of 0.115.

Third, using farmer expectations we can also separate treatment effects by the level of asymmetric information between farmers and veterinarians at baseline. To do so, we difference farmers' expected average AI success rate with the truth. We then split our sample according to whether farmers had above or below the median in this difference. Positive values in this difference occur when farmers are told that their veterinarian is better than they expected; negative values occur when farmers are told their veterinarian is worse than they expected. The median is .012.

Table 11 presents results from this heterogeneity analysis. We find that, as with treatment effects by government veterinarian rank, the more unexpectedly negative the information a farmer receives about their veterinarian, the more s/he is able to then bargain away rents from the veterinarian. Of course these results rely on few observations so we consider them

speculative.

5 Discussion

5.1 Interpretation: Unobserved effort or inherent ability?

Several results suggest that the treatment effect on AI success rates is entirely due to increased veterinarian effort for the treated. To illustrate this, we can walk through the process by which farmers select a veterinarian and negotiate prices and effort. First, farmers decide whether to get AI at all when a cow is in heat. Next, they decide whether to stick with their previous veterinarian. If farmers switch, they then decide whether to call a government or private veterinarian. Finally, they decide how to engage with this veterinarian in pre-visit negotiations over the phone as well as during the AI visit (and veterinarians have to decide how to respond).

In our setting, farmers almost always choose to inseminate their livestock in heat, so we would not expect any changes in this decision. Next, we show in Table ?? that treatment farmers are no more likely than control farmers to switch veterinarians after treatment. Thus the treatment effect cannot be driven by farmers simply switching to the ‘best vet’.

We do see changes in whether farmers call a government or private veterinarian, however. Importantly, we show in Table 3 that treatment farmers who subsequently switch to private providers do not have increased AI success rates. If our treatment effect is driven by changes in farmer behavior towards their livestock, we would expect effects regardless of which veterinarian the farmer selects after treatment. The same argument can be applied to the results from Section 4.4.1. If our treatment effect is driven by changes in farmer behavior, farmers’ past veterinarian ranking should not matter.

Thus, we can turn to the final part of the decision process as the likely mechanism—farmers’ engagement with veterinarians. Our results are consistent with farmers using the information we provide to them to negotiate reductions in government veterinarians’ infor-

mational rents through higher effort and (potentially) lower prices.

If we are to view increased veterinarian effort as the driver of our results, then that effort must be easily varied across visits. Anecdotes suggest that this is true. One commonly cited example of low veterinarian effort is the way in which veterinarians treat semen straws. As mentioned above, the provincial government delivers these straws to veterinary centers in liquid nitrogen canisters, and they must be kept frozen until just before use. Veterinarians sometimes take straws out before leaving on a visit rather than transporting the canister to the farm. This likely results in the semen spoiling, though the veterinarian still performs AI and charges the farmer. And because farmers call veterinarians before AI to negotiate a time and price, treatment farmers could pressure them to take better care transporting semen. Veterinarians would have to exert more effort but farmers would likely still pay them positive rents rather than having to pay the cost to find a new veterinarian.

5.2 Social welfare implications

To understand the social welfare implications of this intervention, we consider benefits and costs to farmers and to veterinarians as well as the cost of the intervention itself.³¹

Benefit to farmers: if the treatment effect of 25 percent on AI success rates (.170 over a control mean of .672) translates into just 2.8 percent more calves born per year per farmer (i.e., if farmers with a failed AI attempt are able to successfully impregnate their animal two months later), and the expected value of a calf is roughly 107,500 PKR (approximately 1075 USD) at the market, then treatment farmers would earn an additional 3,010 PKR (30 USD) per year, equal to almost half of one month's median income.³² This is a conservative estimate. It does not count the additional net value of two months of milk nor the cumulative net present value effect of an increased future stream of livestock.

³¹We do not consider changes in price as such is a transfer with no net social welfare implications.

³²This calf value is the average of male and female calf prices reported at <http://www.pakdairyinfo.com/feasibility.htm>, accessed 10/8/2015. The monthly median income of households in Paksitan, according to the World Bank, is 73.26 USD per month, accessed 10/8/2015.

Cost to farmers: we showed that farmer treatment effects are not due to changes in farmer behavior, we do not consider there to be costs to farmers of this intervention.

Benefit to veterinarians: farmers do not switch veterinarians more as a result of treatment, which suggests no change in veterinarian market shares that could impact social welfare. However, treatment farmers are more likely to return for government AI. Thus, if anything, government veterinarians benefit from this intervention. This would be at the cost of private veterinarians, however, so we will not consider it.

Cost to veterinarians: we do not believe the marginal cost to veterinarians' increased effort induced by treatment to be very large in this setting, as travel costs are paid either way. Government veterinarians also do not spend any more time visiting treatment farmers (our smartphone application data collects this information). Any costs must be in terms of concentration, etc.

Cost of the intervention: including one-time fixed costs to develop our clearinghouse technology, this intervention cost approximately 50,000 USD to reach over 6,000 farmers for treatment or control calls, or approximately 8 USD per farmer.

Adding it up, we find benefits of 30 USD per farmer from an intervention that cost 8 USD per farmer. This suggests a large, 275 percent return.

6 Conclusion

In this paper, we present results from the randomized controlled trial of a novel solution to a common government accountability failure: shirking by government agents in a setting of moral hazard. Our solution is novel not only in that it leverages the cost-effective, self-sustaining nature of crowdsourcing to help the poorest, but also in that it does so in a tough setting. In rural Punjab, the market for artificial insemination is thin, literacy rates are low, and cellular networks are very limited—yet we were able to employ an information clearinghouse with success.

The very fact that our clearinghouse was successful purely through providing information confirms the existence of asymmetric information in this setting. And the fact that veterinarians respond with increased effort confirms that this asymmetric information is about unobserved effort as opposed to unobserved inherent ability. While these confirmations are neither novel nor heartening in and of themselves, they allow us to fit the livestock sector in Punjab into a context that is much more general. Moral hazard has been documented in numerous sectors, public and private, across the developing world. We might expect our clearinghouse to help citizens in any of these sectors, so long as they answer the phone.

And given the low cost of our clearinghouse, we might expect similarly large returns in other sectors. Conservative estimates suggest a 275 percent return to farmers on the cost of the intervention. This is driven by a 25 percent increase in AI success rates for treatment farmers. In other words, thousands of poor, rural Pakistanis who were treated are now more likely to have milk to drink and calves to raise or to sell for substantial income. This is heartening.

These results suggest the importance of distinguishing between forms of asymmetric information when developing new policies. In addition, we hope this paper and other new studies will improve our understanding of how technology can be leveraged to improve the feasibility and impact of already tried-and-true interventions, such as monitoring to reduce asymmetric information. As cellular networks improve and as technology to collect, aggregate, and disseminate information advances, our results suggest we may see improved outcomes for citizens across the rural developing world.

References

- Ackerberg, Daniel A**, “Advertising, learning, and consumer choice in experience good markets: an empirical examination,” *International Economic Review*, 2003, 44 (3), 1007–1040.
- Aker, Jenny C**, “Information from markets near and far: Mobile phones and agricultural markets in Niger,” *American Economic Journal: Applied Economics*, 2010, 2 (3), 46–59.
- Akerlof, George A**, “The market for” lemons”: Quality uncertainty and the market mechanism,” *The quarterly journal of economics*, 1970, pp. 488–500.
- Andrabi, Tahir, Jishnu Das, and Asim Ijaz Khwaja**, “Report cards: The impact of providing school and child test scores on educational markets,” 2014.
- Bandiera, Oriana, Andrea Prat, and Tommaso Valletti**, “Active and Passive Waste in Government Spending: Evidence from a Policy Experiment,” *American Economic Review*, 2009, 99 (4), 1278–1308.
- Banerjee, Abhijit V, Rukmini Banerji, Esther Duflo, Rachel Glennerster, and Stuti Khemani**, “Pitfalls of participatory programs: Evidence from a randomized evaluation in education in India,” *American Economic Journal: Economic Policy*, 2010, 2 (1), 1–30.
- Basu, Arnab K, Nancy H Chau, and Ravi Kanbur**, “A theory of employment guarantees: Contestability, credibility and distributional concerns,” *Journal of Public Economics*, 2009, 93 (3), 482–497.
- Baumol, William J**, “Contestable markets: an uprising in the theory of industry structure,” *Microtheory: applications and origins*, 1986, pp. 40–54.
- Björkman, Martina and Jakob Svensson**, “Power to the people: evidence from a randomized field experiment on community-based monitoring in Uganda,” *The Quarterly Journal of Economics*, 2009, 124 (2), 735–769.
- Björkman-Nyqvist, Martina, Jakob Svensson, and David Yanagizawa-Drott**, “The market for (fake) antimalarial medicine: Evidence from uganda,” Technical Report, Abdul Latif Jameel Poverty Action Lab 2013.
- Callen, Michael, Saad Gulzar, Ali Hasanain, Muhammad Yasir Khan, and Arman Rezaee**, “Personalities and Public Sector Performance: Evidence from a Health Experiment in Pakistan,” Working paper 2017.
- , – , – , – , and – , “The Political Economy of Public Sector Absence: Experimental Evidence from Pakistan,” Working paper 2017.
- Chaudhury, Nazmul, Jeffrey Hammer, Michael Kremer, Karthik Muralidharan, and F Halsey Rogers**, “Missing in action: teacher and health worker absence in developing countries,” *The Journal of Economic Perspectives*, 2006, 20 (1), 91–116.

- Dreze, Jean and Amartya Sen**, *Hunger and public action*, Oxford University Press on Demand, 1989.
- Duflo, Esther, Rema Hanna, and Stephen P Ryan**, “Incentives work: Getting teachers to come to school,” *The American Economic Review*, 2012, pp. 1241–1278.
- Fafchamps, Marcel and Bart Minten**, “Impact of sms-based agricultural information on indian farmers,” *The World Bank Economic Review*, 2012, 26 (3), 383–414.
- Ferraz, Claudio and Frederico Finan**, “Electoral accountability and corruption: Evidence from the audits of local governments,” *American Economic Review*, June 2011, 101 (4), 1274–1311.
- Finan, Frederico, Benjamin A Olken, and Rohini Pande**, “The personnel economics of the state,” Technical Report, National Bureau of Economic Research 2015.
- Henderson, Ralph H and T Sundaresan**, “Cluster sampling to assess immunization coverage: a review of experience with a simplified sampling method.,” *Bulletin of the World Health Organization*, 1982, 60 (2), 253.
- Hölmstrom, Bengt**, “Moral hazard and observability,” *The Bell journal of economics*, 1979, pp. 74–91.
- Hubbard, Thomas N**, “How do consumers motivate experts? Reputational incentives in an auto repair market,” *The Journal of Law and Economics*, 2002, 45 (2), 437–468.
- Israel, Mark**, “Services as experience goods: An empirical examination of consumer learning in automobile insurance,” *American Economic Review*, 2005, 95 (5), 1444–1463.
- Jensen, Robert**, “The digital divide: Information (technology), market performance, and welfare in the South Indian fisheries sector,” *The Quarterly Journal of Economics*, 2007, pp. 879–924.
- Jin, Ginger Zhe and Phillip Leslie**, “The effect of information on product quality: Evidence from restaurant hygiene grade cards,” *The Quarterly Journal of Economics*, May 2003.
- Mitra, Sandip, Dilip Mookherjee, Maximo Torero, and Sujata Visaria**, “Asymmetric information and middleman margins: An experiment with west bengal potato farmers,” 2014. Working paper.
- Muralidharan, Karthik, Paul Niehaus, and Sandip Sukhtankar**, “General equilibrium effects of (improving) public employment programs: Experimental evidence from india,” Technical Report, National Bureau of Economic Research 2017.
- Olken, Benjamin A**, “Monitoring corruption: evidence from a field experiment in Indonesia,” *Journal of political Economy*, 2007, 115 (2), 200–249.
- Olken, Benjamin A. and Rohini Pande**, “Corruption in Developing Countries,” *Annual Review of Economics*, 2012, 4, 479–509.

Reinikka, Ritva and Jakob Svensson, “Local Capture: Evidence from a Central Government Transfer Program in Uganda,” *The Quarterly Journal of Economics*, 2004, 119 (2), 679–705.

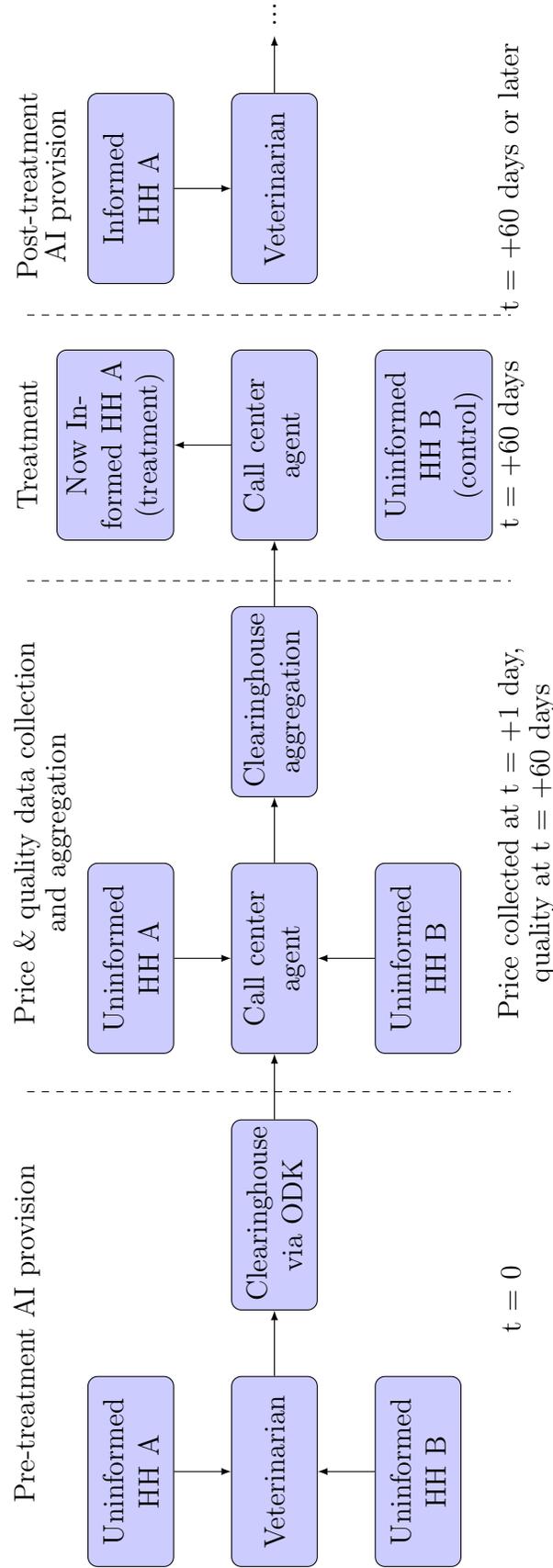
Svensson, Jakob and David Yanagizawa, “Getting prices right: the impact of the market information service in Uganda,” *Journal of the European Economic Association*, 2009, 7 (2-3), 435–445.

Wild, Lena, Vikki Chambers, Maia King, and Daniel Harris, “Common Constraints and Incentive Problems in Service Delivery,” Technical Report, Overseas Development Institute 2012.

World Bank, *World Development Report 2004: Making Services Work for the Poor*, World Bank, 2004.

7 Tables and figures

Figure 1: Clearinghouse flowchart



Notes: Arrows indicate the flow of information. The collection of quality data and treatment occur during the same follow-up phonecall 60 days after service provision. Beginning in October 2014, treatment farmers received information about the AI success rates of their local government veterinarians.

Figure 2: Clearinghouse and representative survey timelines

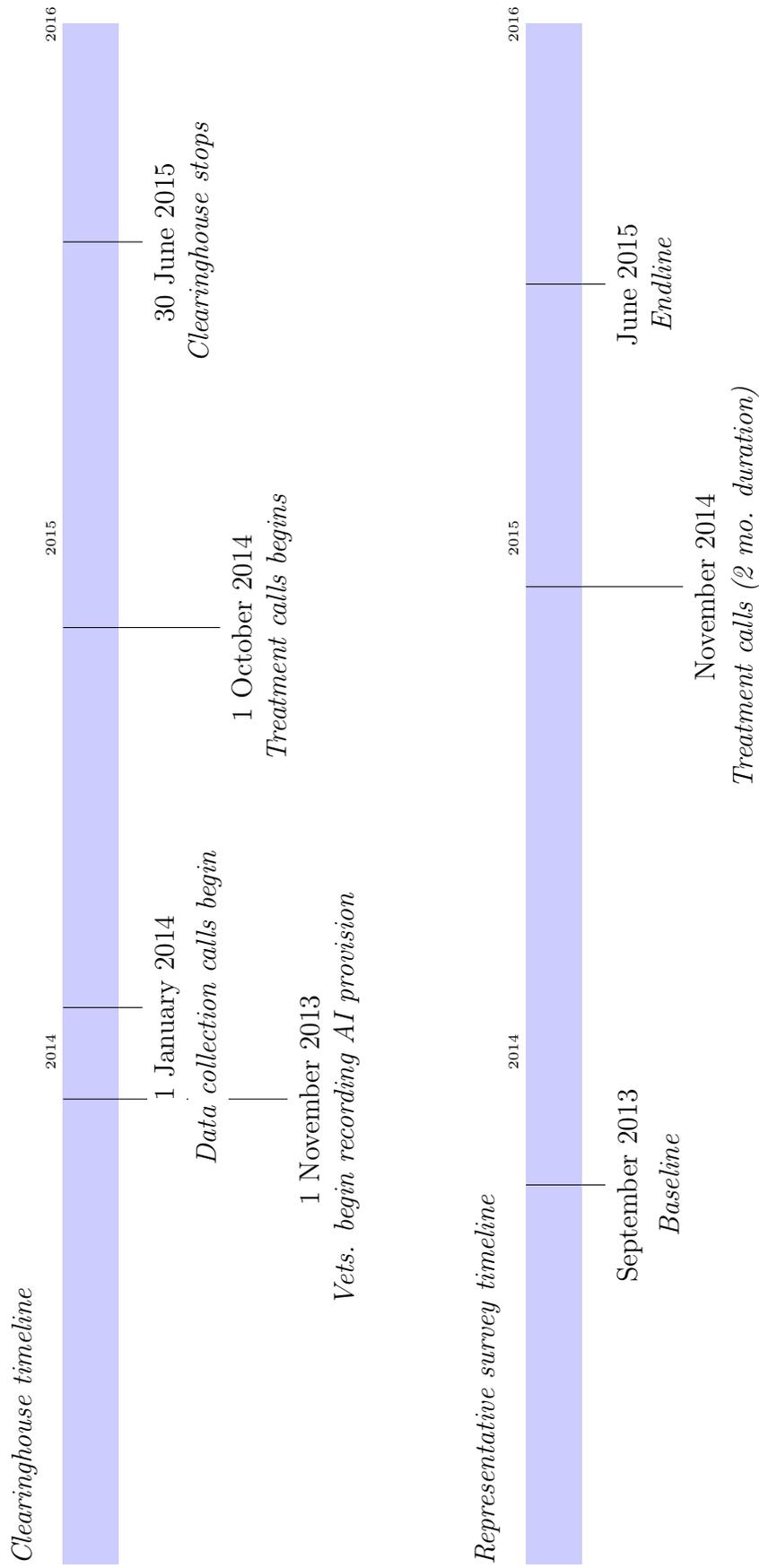


Table 1: Treatment balance—representative survey sample

	Treatment	Control	Difference	P-value
Farmer-level baseline variables—190 observations across 61 village-clusters				
Livestock is primary source of HH’s income (=1)	0.085 [0.281]	0.097 [0.297]	-0.012 (0.042)	0.748
1-10 effort household puts into selecting veterinarian	6.200 [2.361]	5.575 [2.049]	0.625 (0.537)	0.491
Farmer attrited from in-person endline	0.021 [0.145]	0.011 [0.104]	0.011 (0.018)	0.812
Farmer-visit-level variables—356 pre-treatment observations from 190 farmers across 61 village-clusters				
Farmer switched vets since last recorded AI visit (=1)	0.179 [0.385]	0.190 [0.393]	-0.011 (0.055)	0.879
AI visit charges	367 [373]	356 [361]	10 (48)	0.771
AI visit success rate	0.703 [0.447]	0.750 [0.431]	-0.047 (0.049)	0.159
1-10 AI visit farmer satisfaction	7.694 [2.184]	9.302 [22.333]	-1.608 (1.754)	0.290
1-10 farmer estimated AI visit veterinarian success rate	6.636 [1.739]	6.315 [1.981]	0.321 (0.276)	0.606

Notes: Standard deviations reported in brackets. Standard errors reported in parentheses. Means and differences are unconditional. P-values are from OLS regressions with randomization strata fixed effects and standard errors clustered at the village-cluster level. Some regressions have fewer observations due to missing data. All data come from baseline surveys fielded in August and September 2013, with the exception of “Farmer attrited from endline survey”. This variable is a dummy equal to one if a farmer was present during our baseline survey and not our endline survey. The sample of farmers was selected to be geographically representative of Sahiwal and is drawn from 90 different villages. The sample is limited to farmers that report receiving services from a government veterinarian at baseline. Treatment farmers received information about the AI success rates of their local government veterinarians. Treatment calls were conducted in November 2014 and January 2015.

Table 2: Treatment balance—clearinghouse data

	Treatment	Control	Difference	P-value
Satisfaction with AI service provision (1-5)	4.186 [0.736]	4.138 [0.759]	0.048 (0.029)	0.146
Farmer switched vets since last AI visit	0.055 [0.228]	0.050 [0.218]	0.005 (0.010)	0.164
AI visit charges (PKR)	197 [182]	203 [244]	-6 (9)	0.574
AI visit success rate (pregnancy / AI attempts)	0.684 [0.458]	0.688 [0.457]	-0.004 (0.016)	0.346
No of cows owned by farmer	2.545 [3.441]	2.446 [3.055]	0.099 (0.156)	0.294
No of buffalo owned by farmer	3.125 [3.779]	3.318 [6.354]	-0.193 (0.366)	0.782
Distance to closest AI center (km)	2.169 [2.253]	2.267 [2.257]	-0.098 (0.114)	0.821

Notes: Standard deviations reported in brackets. Standard errors reported in parentheses. Means and differences are unconditional. P-values are from OLS regressions with randomization strata fixed effects and standard errors clustered at the village-cluster level. The sample consists of 6,462 pre-treatment farmer-visit-level observations from 3,088 unique farmers across 202 village-clusters. Some regressions have fewer observations due to missing data. Beginning in October 2014, treatment farmers received information about the AI success rates of their local government veterinarians. Satisfaction, AI visit charges, and numbers of cows and buffalo are reported by farmers on the phone one day after AI service provision. AI visit success rate is reported by farmers on the phone 60 days after AI service provision. Farmer switched vets and distance to closest AI center are automatically generated administrative data.

Table 3: Treatment effects—representative survey sample

Outcome:	Log price	AI success rate	Log price	AI success rate	Log price	AI success rate
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment farmer (=1)	-0.064 (0.171)	0.170* (0.101)	-0.005 (0.364)	0.357** (0.146)	-0.179 (0.205)	-0.005 (0.195)
Mean of dependent variable	5.874	0.672	5.856	0.567	5.888	0.765
# Observations	156	142	69	63	87	79
# Village-clusters	53	51	27	29	39	35
R-Squared	0.526	0.265	0.631	0.451	0.582	0.242
Sample	All	All	Returned gov't	Returned gov't	Left gov't	Left gov't

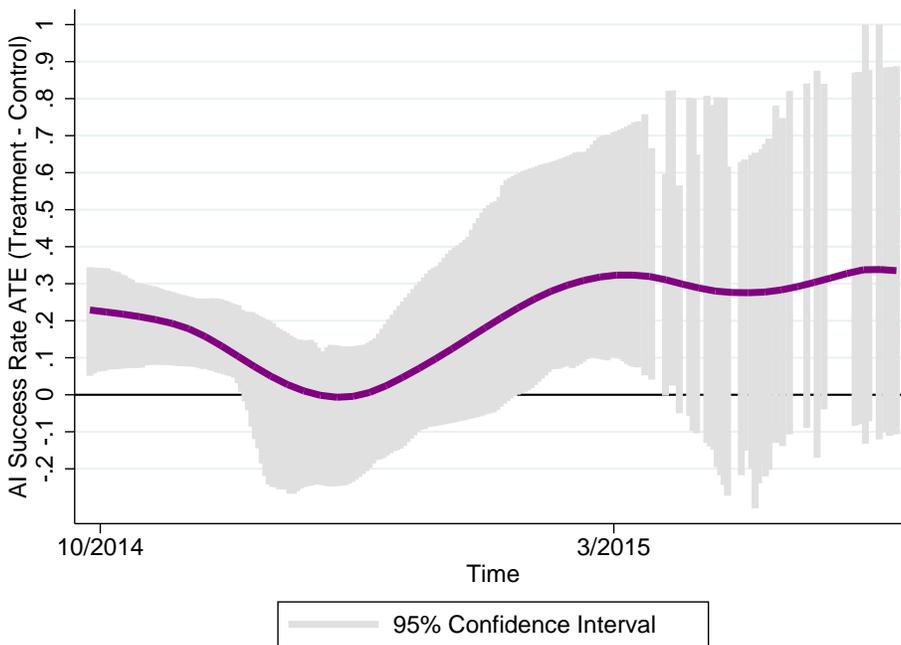
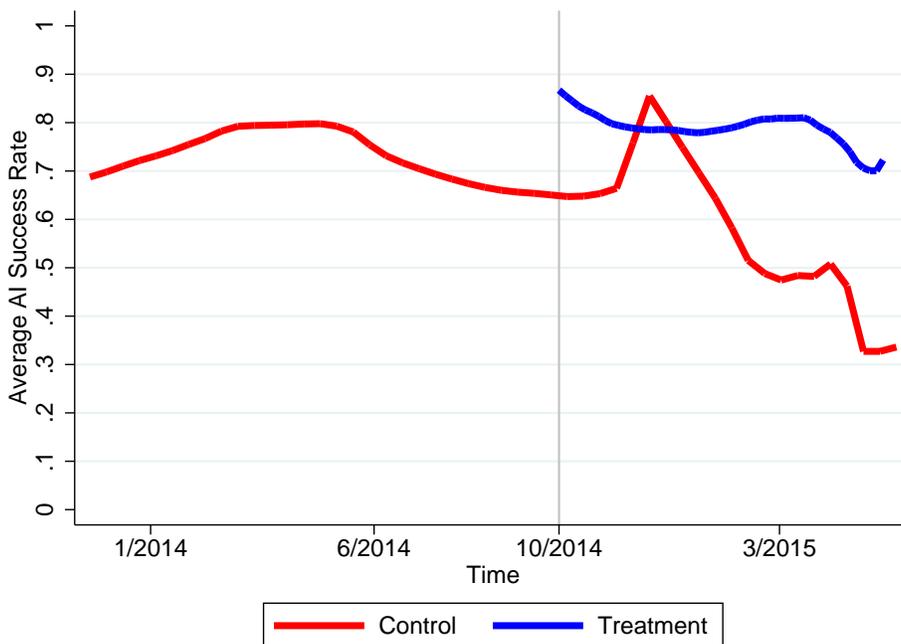
Notes: Standard errors clustered at the village-cluster level reported in parentheses. All regressions include randomization strata fixed effects, survey wave fixed effects, and controls for baseline mean outcomes of the dependent variable. The sample is limited to post treatment reports of AI service provision from farmers during our endline survey, conducted in June 2015. Treatment farmers received information about the AI success rates of their local government veterinarians. Treatment calls were conducted in November 2014 and January 2015. All indicates farmers that received government AI before treatment and subsequently returned for either government or private AI by the end of the project. Returned gov't indicates farmers that received government AI before treatment and subsequently returned for government AI after treatment by the end of the project. Left gov't indicates farmers who received government AI before treatment and instead subsequently received private AI by the end of the project. Price and AI success rates are recalled by farmers from service provisions two to seven months ago.

Table 4: Treatment effects for farmers who return for gov't AI—representative survey sample

Outcome:	Returned at all	Returned to gov't	Switched veterinarians	Log price	AI success rate
	(1)	(2)	(3)	(4)	(5)
Panel A: Representative Survey					
Treatment farmer (=1)	0.087 (0.075)	0.051 (0.065)	-0.055 (0.157)	-0.005 (0.366)	0.357** (0.147)
Mean of dependent variable	0.468	0.222	0.152	5.852	0.581
# Observations	251	251	69	70	64
# Village-clusters	72	72	27	28	30
R-Squared	0.199	0.230	0.430	0.631	0.456
Panel B: Clearinghouse Data					
Treatment farmer (=1)		0.029*** (0.010)	0.006 (0.031)	-0.249 (0.307)	0.209** (0.102)
Mean of dependent variable		0.070	0.091	5.083	0.563
# Observations		3112	564	213	174
# Village-clusters		202	100	78	72
R-Squared		0.095	0.291	0.714	0.610
Sample	Pre	Pre	Post	Post	Post

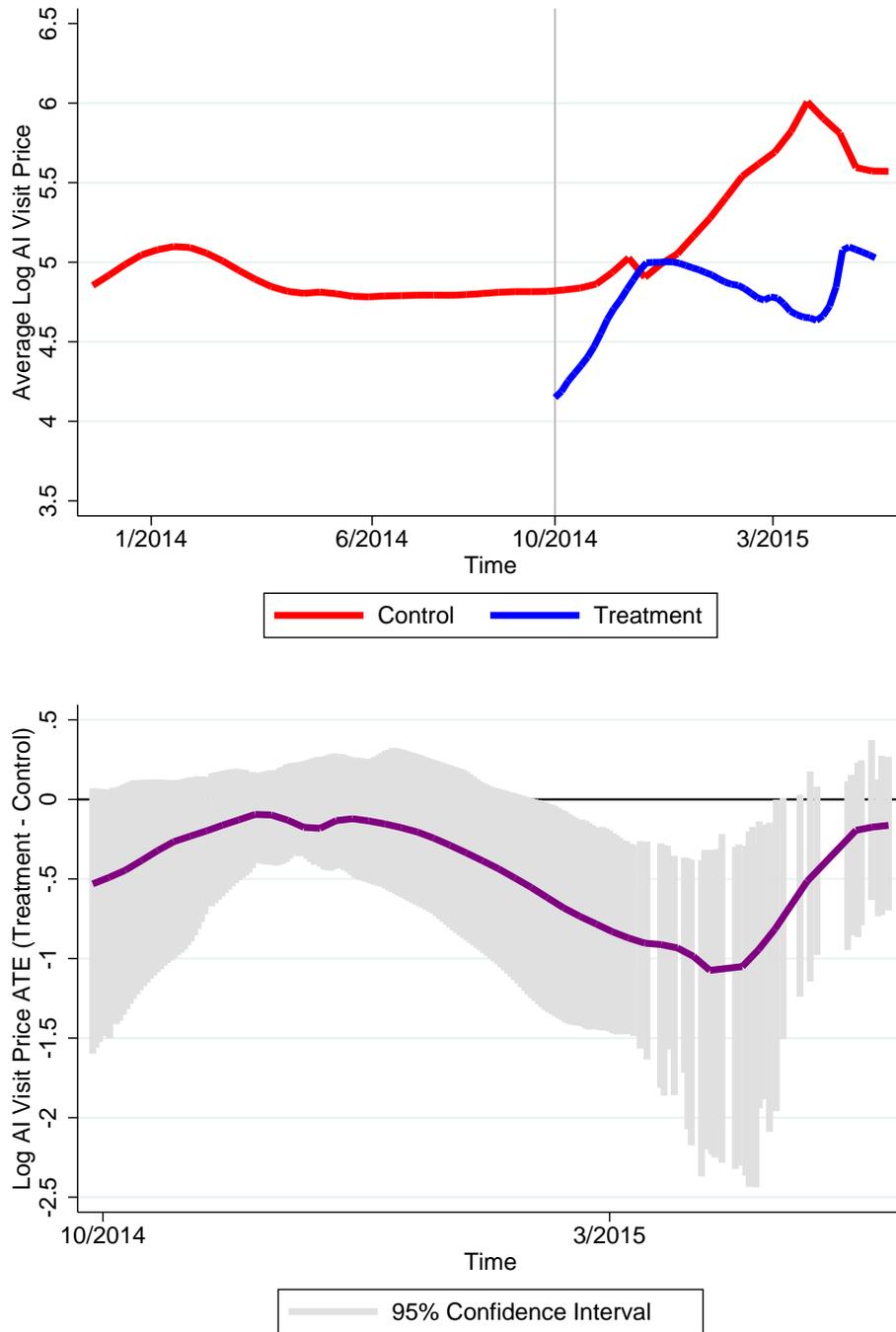
Notes: Standard errors clustered at the village-cluster level reported in parentheses. All regressions include randomization strata fixed effects and controls for baseline mean outcomes (switched veterinarians, log price, AI success rate) as well as dummies for whether the given observation is missing each baseline mean outcome. Returned is a dummy variable equal to one if a farmer that received government AI before treatment subsequently returned for government AI after treatment by the end of the project. Switched veterinarians is a dummy variable coded as one if the veterinarian a farmer saw for a service provision was different than the last veterinarian seen. Log price and AI success rates are recalled by farmers from service provisions two to seven months ago. In Panel A, columns (2) through (4) include survey wave fixed effects and restricts the sample to those farmers that returned. The sample is limited to post treatment reports of AI service provision from farmers during our endline survey, conducted in June 2015. In Panel B, columns (2) through (4) include a time trend control. The sample for column (1) is farmers that received a government AI service and were subsequently treated, regardless of whether they then returned. The sample for columns (2) through (4) are farmers that returned after treatment. Note the differences in observations across columns are due to the fact that veterinarian switching can be detected without any successful phone calls, where as log price requires one successful phone call and AI success rate requires two successful phone calls to a farmer. Beginning in October 2014 treatment farmers received information about the AI success rates of their local government veterinarians.

Figure 3: AI success rates in real time—clearinghouse data



Notes: The sample is farmers that received a government AI service and then answered the phone and reported AI success 60 days later. Beginning in October 2014 treatment farmers received information about the AI success rates of their local government veterinarians. Lines are smoothed using a kernel-weighted local polynomial regression with the Epanechnikov kernel and a rule-of-thumb bandwidth estimator. Confidence intervals are bootstrapped.

Figure 4: Log price per AI visit in real time—clearinghouse data



Notes: The sample is farmers that received a government AI service and then answered the phone and reported price paid one day later. Beginning in October 2014 treatment farmers received information about the AI success rates of their local government veterinarians. Lines are smoothed using a kernel-weighted local polynomial regression with the Epanechnikov kernel and bandwidth one. Confidence intervals are bootstrapped.

Table 5: Comparing farmers that returned to a government veterinarian with those that returned to a private veterinarian—representative survey sample

	Returned to gov't	Returned to private	Difference	P-value
<hr/>				
Farmer-level baseline variables—124 observations across 56 village-clusters				
HoH education = None (=1)	0.424 [0.498]	0.338 [0.477]	0.085 (0.092)	0.896
A child in the HH attends public school (=1)	0.441 [0.501]	0.662 [0.477]	-0.221 (0.086)	0.062
HH has used govt health services in past 2 yrs (=1)	0.593 [0.495]	0.538 [0.502]	0.055 (0.091)	0.670
Amount of land HH owns and rents for livestock	1.278 [2.719]	1.310 [2.667]	-0.032 (0.536)	0.249
HH owns the house that they live in (=1)	0.983 [0.130]	0.923 [0.269]	0.060 (0.037)	0.168
Hours of electricity per day	10.322 [3.466]	10.354 [3.511]	-0.032 (0.642)	0.376
HH has a cooking stove/range (=1)	0.138 [0.348]	0.123 [0.331]	0.015 (0.062)	0.306
HH made less than 100k PKR last year (=1)	0.220 [0.418]	0.283 [0.454]	-0.063 (0.085)	0.201
Any member of HH has bank account (=1)	0.339 [0.477]	0.369 [0.486]	-0.030 (0.085)	0.622
Believed it was likely that last vote was not secret (=1)	0.659 [0.479]	0.488 [0.506]	0.171 (0.106)	0.582
Is likely to believe information given by govt employee (=1)	0.830 [0.380]	0.771 [0.425]	0.059 (0.083)	0.715
Average number of digits recalled	3.587 [0.985]	3.162 [1.092]	0.425 (0.284)	0.860
On a scale fo 0-10, how willing are you to take risks?	4.591 [2.518]	4.469 [3.267]	0.122 (0.603)	0.094
Agreeableness	4.284 [0.550]	4.031 [0.716]	0.253 (0.115)	0.330
Conscientiousness	4.225 [0.508]	4.185 [0.603]	0.040 (0.099)	0.766
Extroversion	4.220 [0.700]	4.112 [0.692]	0.109 (0.126)	0.099
Neuroticism	2.229 [0.687]	2.354 [0.792]	-0.125 (0.134)	0.247
Openness	3.746 [0.625]	3.615 [0.709]	0.130 (0.121)	0.230
1-10 effort HH puts into selecting a vet. off.	6.308 [1.463]	6.034 [2.485]	0.273 (0.543)	0.534
<hr/>				
Farmer-visit-level variables—239 pre-treatment observations from 124 farmers across 56 village-clusters				
AI visit success rate	0.763 [0.411]	0.764 [0.417]	-0.001 (0.056)	0.629
AI visit charges	460.833 [529.396]	404.038 [356.176]	56.795 (67.339)	0.193
AI visit farmer satisfaction	7.925 [2.091]	7.464 [2.220]	0.461 (0.363)	0.094
Farmer estimated AI visit vet success rate	6.641 [1.768]	6.214 [1.959]	0.427 (0.345)	0.215

Notes: Standard deviations reported in brackets. Standard errors reported in parentheses. Means and differences are unconditional. P-values are from OLS regressions with randomization strata fixed effects and standard errors clustered at the village-cluster level. Some regressions have fewer observations due to missing data. All data come from baseline surveys fielded in August and September 2013. The sample of farmers was selected to be geographically representative of Sahiwal and is drawn from 90 different villages. The sample is limited to farmers that report receiving services from a government veterinarian at baseline.

Table 6: Does treatment induce a veterinarian reporting bias?

	Treatment	Control	Difference	P-value
Farmer reported AI and vet submitted data to call center (=1)	0.181	0.196	-0.015	0.397
	0.386	0.398	0.046	.
Farmer reported receiving a call verifying AI service (=1)	0.784	0.688	0.096	0.746
	0.417	0.471	0.103	.

Notes: Standard deviations reported in brackets. Standard errors reported in parentheses. Means and differences are unconditional. P-values are from OLS regressions with randomization strata fixed effects and standard errors clustered at the village-cluster level. The sample consists of 367 farmer-visit-level observations from 225 unique farmers across 65 village-clusters from our endline survey, conducted in June 2015. Treatment farmers received information about the AI success rates of their local government veterinarians. Treatment calls were conducted in November 2014 and January 2015. “Farmer reported AI and vet submitted data to call center” is a dummy equal to one if a government AI service provision reported in our endline survey was subsequently submitted to the clearinghouse by the veterinarian that performed the service. This is done by verifying survey data with clearinghouse data directly. “Farmer reported receiving a call verifying AI service” is conditional on a vet submitting data to the call center.

Table 7: Farmer selection in to clearinghouse at various stages

	All control	Answered once	Answered twice	Returned for AI	Returned & answered once	Returned & answered twice
No of cows owned by farmer	2.04	2.09	2.09	2.39	2.39	2.37
	[2.40]	[2.39]	[2.42]	[2.60]	[2.77]	[2.78]
No of buffalo owned by farmer	2.64	2.75	2.77	3.24	3.11	3.10
	[3.71]	[3.83]	[3.76]	[4.29]	[3.94]	[3.81]
Distance to closest AI center [km]	5.74	3.30	3.47	2.27	2.28	2.24
	[132.96]	[52.03]	[56.84]	[2.34]	[2.40]	[2.44]
Satisfaction with AI service provision [1-5]		4.18	4.19	4.23	4.20	4.20
		[0.76]	[0.75]	[0.72]	[0.74]	[0.75]
Log AI visit charges [PKR]		4.83	4.84	4.79	4.85	4.85
		[1.52]	[1.50]	[1.49]	[1.40]	[1.37]
AI visit success rate [pregnancy / AI attempts]			0.69	0.73	0.72	0.73
			[0.46]	[0.44]	[0.44]	[0.44]
# Observations	16,907	4,781	4,005	1,198	661	544

Notes: Sample: Non-treatment farmers from the clearinghouse sample. Standard deviations in parenthesis.

Table 8: Treatment balance of returning sample—clearinghouse data

	Treatment	Control	Difference	P-value
Pre-treatment mean satisfaction with AI service provision (1-5)	4.191 [0.686]	4.260 [0.709]	-0.069 (0.087)	0.459
Pre-treatment mean veterinarian switching rate	0.064 [0.233]	0.033 [0.218]	0.030 (0.025)	0.165
Pre-treatment mean log AI visit charges	4.825 [1.358]	4.805 [1.397]	0.019 (0.168)	0.713
Pre-treatment mean AI success rate	0.685 [0.446]	0.674 [0.440]	0.011 (0.056)	0.710
Pre-treatment mean no. of cows	2.758 [2.788]	3.483 [2.360]	-0.726 (0.443)	0.150
Pre-treatment mean no. of buffalo	3.505 [3.370]	3.725 [4.100]	-0.221 (0.515)	0.393
Pre-treatment mean distance to closest AI center (km)	2.317 [2.170]	1.942 [2.177]	0.374 (0.246)	0.918

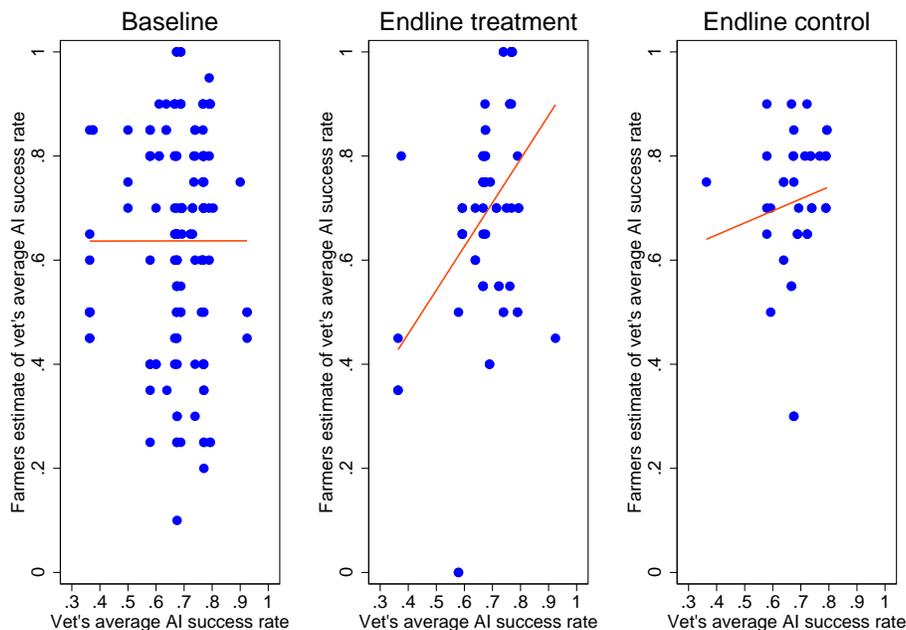
Notes: Standard deviations reported in brackets. Standard errors reported in parentheses. Means and difference are unconditional. P-values are from OLS regressions with randomization strata fixed effects and standard errors clustered at the village-cluster. The sample consists of 262 farmer-level observations across 102 village-clusters of those farmers who received government AI service provisions both before and after receiving a treatment or control phone call. Some regressions have fewer observations due to missing data. Beginning in October 2014 treatment farmers received information about the AI success rates of their local government veterinarians. Satisfaction, AI visit charges, and numbers of cows and buffalo are reported by farmers on the phone one day after AI service provision. AI visit success rate is reported by farmers on the phone 60 days after AI service provision. Farmer switched vets and distance to closest AI center are automatically generated administrative data.

Table 9: Treatment effects by veterinarian ranking—clearinghouse data

Outcome:	Returned	Switched veterinarians	Log price	AI success rate
	(1)	(2)	(3)	(4)
Panel A: Farmers told vet. was in top three in area				
Treatment farmer (=1)	0.016 (0.012)	-0.006 (0.037)	0.061 (0.089)	0.088 (0.137)
Mean of dependent variable	0.073	0.108	4.895	0.661
# Observations	1973	404	132	105
# Village-clusters	174	68	52	45
R-Squared	0.086	0.347	0.932	0.698
Panel B: Farmers told vet. was not in top three in area				
Treatment farmer (=1)	0.038* (0.020)	0.001 (0.076)	-0.816 (1.696)	0.217*** (0.074)
Mean of dependent variable	0.058	0.038	5.690	0.273
# Observations	1084	151	73	62
# Village-clusters	160	51	35	31
R-Squared	0.112	0.524	0.770	0.890
Sample	Pre	Post	Post	Post

Notes: Standard errors clustered at the village-cluster level reported in parentheses. All regressions include randomization strata fixed effects and controls for baseline mean outcomes (switched veterinarians, log price, AI success rate) as well as dummies for whether the given observation is missing each baseline mean outcome. In addition, columns (2) through (4) include a time trend control. The sample for column (1) is farmers that received a government AI service and were subsequently treated, regardless of whether they then returned. The sample for columns (2) through (4) are farmers that returned after treatment. Note the differences in observations across columns are due to the fact that veterinarian switching can be detected without any successful phone calls, where as log price requires one successful phone call and AI success rate requires two successful phone calls to a farmer. Beginning in October 2014 treatment farmers received information about the AI success rates of their local government veterinarians. Returned is a dummy variable equal to one if a farmer that received government AI before treatment subsequently returned for government AI after treatment by the end of the project. Switched veterinarians is a dummy variable equal to one if the veterinarian that a farmer saw for a service provision was different than the last veterinarian seen. Log price is the log price paid for the service provision, as reported by the farmer when called to verify service provision. AI success rate is the rate of success of the AI services provided at a specific service provision upon follow up 60 days later. Panels are divided by whether a farmer was told when treated that his/her veterinarian from the last visit was in the top three or not, or would have been if s/he was not selected for control.

Figure 5: Treatment effect on farmer expectations—representative survey sample



Notes: The sample is farmers that received AI from a reported veterinarian that could be matched to our clearinghouse veterinarians. Farmer’s estimates of vet’s average AI success rate reported by farmers in baseline and endline surveys. Vet’s actual average AI success rate is from clearinghouse data before October 2014. Beginning in October 2014 treatment farmers received information about the AI success rates of their local government veterinarians.

Table 10: Change in farmer expectations—representative survey sample

	Farmer’s estimate of vet’s average AI success rate		
	(1)	(2)	(3)
Vet’s actual average AI success rate	-0.000 (0.178)	0.838** (0.385)	0.232 (0.229)
# Observations	145	66	37
# Village-clusters	34	21	20
R-Squared	0.000	0.162	0.020
Sample	Baseline	Endline T	Endline C

Notes: Standard errors clustered at the village-cluster level reported in parentheses. The sample is farmers that received AI from a reported veterinarian that could be matched to our clearinghouse veterinarians. Farmer’s estimates of vet’s average AI success rate reported by farmers in baseline and endline surveys. Column (1) limits to baseline responses by eventual treatment and control farmers. Column (2) limits to endline responses by treatment farmers. Column (3) limits to endline responses by control farmers. Vet’s actual average AI success rate is from clearinghouse data before October 2014. Beginning in October 2014 treatment farmers received information about the AI success rates of their local government veterinarians. The null hypothesis that the coefficients in columns (2) and (3) are equal is rejected with a p-value of 0.115 from a regression interacting Vet’s actual average AI success rate with a treatment indicator in the Endline sample.

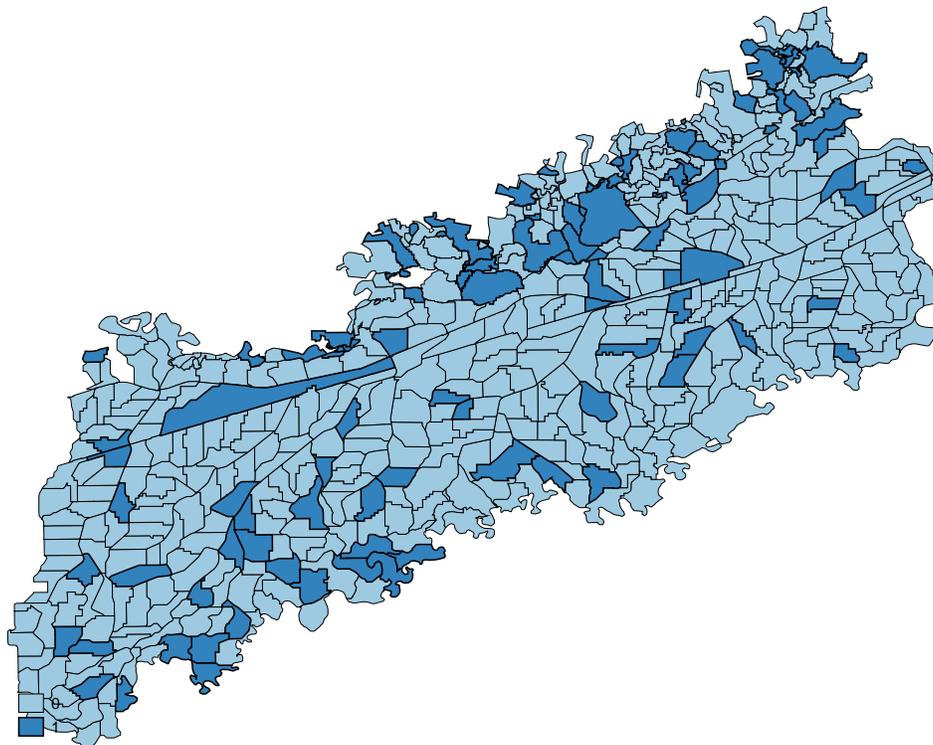
Table 11: Treatment effects by farmer expectations—representative survey sample

Outcome:	Returned	Switched veterinarians	Log price	AI success rate
	(1)	(2)	(3)	(4)
Panel A: Farmers with below median expected-actual AI success				
Treatment farmer (=1)	-0.083 (0.135)	0.049 (0.055)	0.294 (0.493)	0.318 (0.412)
Mean of dependent variable	0.370	0.231	5.688	0.500
# Observations	60	29	29	20
# Village-clusters	28	12	12	9
R-Squared	0.536	0.589	0.738	0.514
Panel B: Farmers with above median expected-actual AI success				
Treatment farmer (=1)	0.113 (0.274)	0.369 (0.329)	-1.399*** (0.385)	0.749* (0.370)
Mean of dependent variable	0.419	0.118	5.939	0.563
# Observations	53	32	28	28
# Village-clusters	29	16	14	16
R-Squared	0.468	0.756	0.898	0.588
Sample	Pre	Post	Post	Post

Notes: Standard errors clustered at the village-cluster level reported in parentheses. All regressions include randomization strata fixed effects and controls for baseline mean outcomes (switched veterinarians, log price, AI success rate) as well as dummies for whether the given observation is missing each baseline mean outcome. In addition, columns (2) through (4) include survey wave fixed effects and restricts the sample to those farmers that returned. The sample is limited to post treatment reports of AI service provision from farmers during our endline survey, conducted in June 2015. Returned is a dummy variable equal to one if a farmer that received government AI before treatment subsequently returned for government AI after treatment by the end of the project. Switched veterinarians is a dummy variable coded as one if the veterinarian a farmer saw for a service provision was different than the last veterinarian seen. Log price and AI success rates are recalled by farmers from service provisions two to seven months ago. Panels are divided above and below the median of farmers' estimate of their veterinarian's average AI success rate minus veterinarian's actual average AI success rate from clearinghouse data before October 2014. Negative values in this difference occur when farmers are told their veterinarian is better than they expected. Positive values occur when farmers are told their veterinarian is worse than they expected. The median is .012.

A Appendix tables and figures

Figure A.1: Representative Survey sample villages



Notes: Sampled villages are dark blue. The sample was stratified by whether or not a government veterinarian center was in the village and on whether the village was a canal colony. It is balanced along the following variables: area, settled area, cultivated area, area of wheat, rice, cotton, sugar cane, pulses, orchards, and vegetables, having a river, distance to the nearest veterinarian center, number of livestock in the village, literacy rates, religion, age, and standard wealth index characteristics. Results available upon request. Within each village, we selected ten households using the well-documented EPI cluster sampling method. In order to be surveyed, households had to report owning at least two livestock (cows and/or buffalo) and having regular access to a cellular phone.

Table A.1: Treatment balance—representative survey sample, additional covariates

	Treatment	Control	Difference	P-value
Head of household education = None (=1)	0.394 [0.491]	0.473 [0.502]	-0.080 (0.072)	0.542
A child in the HH attends public school (=1)	0.543 [0.501]	0.516 [0.502]	0.026 (0.073)	0.602
HH has used govt health services in past 2 yrs (=1)	0.511 [0.503]	0.505 [0.503]	0.005 (0.074)	0.782
Amount of land HH owns and rents for livestock	1.862 [3.860]	1.075 [2.017]	0.787 (0.534)	0.261
HH owns the house that they live in (=1)	0.979 [0.145]	0.968 [0.178]	0.011 (0.024)	0.320
Hours of electricity per day	11.348 [3.160]	10.087 [3.624]	1.261 (0.515)	0.011
HH has a cooking stove/range (=1)	0.053 [0.226]	0.118 [0.325]	-0.065 (0.041)	0.093
HH made less than 100k PKR last year (=1)	0.258 [0.440]	0.371 [0.486]	-0.113 (0.070)	0.326
Any member of HH has hank account (=1)	0.277 [0.450]	0.280 [0.451]	-0.003 (0.066)	0.263
Believed it was likely that last vote was not secret (=1)	0.542 [0.501]	0.598 [0.493]	-0.055 (0.076)	0.283
Is likely to believe information given by govt employee (=1)	0.796 [0.405]	0.774 [0.420]	0.022 (0.061)	0.789
Average number of digits recalled	3.400 [0.938]	3.245 [1.078]	0.155 (0.224)	0.846
On a scale fo 0-10, how willing are you to take risks?	4.400 [2.951]	3.972 [2.793]	0.428 (0.481)	0.783
Agreeableness	4.112 [0.716]	3.986 [0.743]	0.125 (0.107)	0.264
Conscientiousness	4.077 [0.588]	4.155 [0.589]	-0.078 (0.087)	0.359
Extroversion	4.197 [0.679]	4.079 [0.652]	0.118 (0.098)	0.589
Neuroticism	2.351 [0.793]	2.410 [0.762]	-0.059 (0.112)	0.549
Openness	3.731 [0.688]	3.614 [0.626]	0.117 (0.096)	0.654

Notes: Standard deviations reported in brackets. Standard errors reported in parentheses. Means and differences are unconditional. P-values are from OLS regressions with randomization strata fixed effects and standard errors clustered at the village-cluster. The sample consists of 190 baseline farmer-level observations across 61 village-clusters. Some regressions have fewer observations due to missing data. All data come from baseline surveys fielded in August and September 2013. This sample of farmers was selected to be geographically representative of Sahiwal and is drawn from 90 different villages. The sample is limited to farmers that report receiving services from a government veterinarian at baseline. Treatment farmers received information about the AI success rates of their local government veterinarians. Treatment calls were conducted in November 2014 and January 2015. Agreeableness, conscientiousness, extroversion, neuroticism, and openness are all measures from the Big 5 Personality Index. These traits are each mean responses to statements that represent the trait on a five point likert scale, in which 1 corresponds to disagree strongly, 2 to disagree a little, 3 to neutral, 4 to agree a little, and 5 to agree strongly. Likert responses are given the same direction (5 always being more agreeable, for example, never less).

Table A.2: Comparing farmers by pre-treatment veterinarian ranking—clearinghouse data

	Vet. in top three	Vet. not top three	Difference	P-value
Satisfaction with AI service provision (1-5)	4.170 [0.736]	4.147 [0.769]	0.024 (0.030)	0.996
Farmer switched vets since last AI visit	0.055 [0.227]	0.075 [0.263]	-0.020 (0.013)	0.927
AI visit charges (PKR)	192 [172]	211 [268]	-19 (10)	0.027
AI visit success rate (pregnancy / AI attempts)	0.627 [0.478]	0.636 [0.476]	-0.009 (0.017)	0.449
No of cows owned by farmer	2.381 [3.155]	2.669 [3.662]	-0.288 (0.156)	0.435
No of buffalo owned by farmer	2.817 [3.166]	3.525 [5.957]	-0.708 (0.259)	0.025
Distance to closest AI center (km)	1.709 [1.573]	3.249 [2.948]	-1.539 (0.120)	0.000

Notes: Standard deviations reported in brackets. The sample consists of 4,784 pre-treatment farmer-visit-level observations from 2,978 unique farmers that received government AI service provision. Some regressions have fewer observations due to missing data. Beginning in October 2014 treatment farmers received information about the AI success rates of their local government veterinarians. Satisfaction, AI visit charges, and numbers of cows and buffalo are reported by farmers on the phone one day after AI service provision. AI visit success rate is reported by farmers on the phone 60 days after AI service provision. Farmer switched vets and distance to closest AI center are automatically generated administrative data. Columns are divided by whether a farmer was told when treatment that his/her veterinarian from the last visit was in the top three or not, or would have been if s/he was not selected for control.