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Crowdsourcing Government Accountability

Experimental Evidence from Pakistan

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Crowdsourcing government accountability: Experimental evidence from Pakistan*

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Abstract

We develop and implement a novel, mobile phone-based information clearinghouse, and experimentally evaluate its ability to overcome information asymmetries and improve public service delivery to farmers in Punjab, Pakistan. Like many crowdsourcing websites, our clearinghouse collects and disseminates ratings—here, on the success of government veterinarians in inseminating livestock. We find that, compared to control, farmers receiving ratings enjoy 27 percent higher insemination success. This effect is entirely due to increased veterinarian effort, rather than farmers switching veterinarians. Treatment farmers are also 33 percent more likely to return to a government veterinarian rather than seeking a private provider. These results suggest large welfare benefits from a low-cost information intervention, which holds out hope for improved government accountability for the poor using basic mobile technology.

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1 Introduction

Asymmetric information between citizen principals and service-providing agents often leads to sub-optimal outcomes for the rural poor across the developing world (World Bank, 2004; Wild et al., 2012). In the case of *government agents*, asymmetric information has led to corruption in elected officials (Ferraz and Finan, 2011), waste in government processes (Bandiera et al., 2009), leakage between public service allocations and expenditures (Reinikka and Svensson, 2004), and more generally poor public service delivery across sectors, countries, and even continents (Chaudhury et al., 2006). In the case of *private agents*, asymmetric information has led to inefficient market allocations and rent capture at the expense of consumers (Jensen, 2007; Svensson and Yanagizawa, 2009; Aker, 2010).

Monitoring can decrease asymmetric information, but it is particularly costly to implement monitoring schemes in rural developing settings. This is because poor infrastructure makes information collection and transmission expensive in these contexts. In addition, research shows monitoring may not be effective without complimentary financial incentives (Duflo et al., 2012) and its effects attenuate as agents find alternative strategies to pursue rents (Olken and Pande, 2012).

Information clearinghouses, such as yelp.com, angieslist.com, and amazon.com, decrease asymmetries inexpensively. These crowdsourcing websites collect, aggregate, and disseminate masses of ratings at costs much lower than traditional reviewers such as the New York Times, though to date, their application has been limited to commercial settings. Furthermore, such sites have yet to take hold in the rural developing world, characterized by thin markets, low literacy rates, and 2G wireless networks.

We design and implement an information clearinghouse to reduce government agent shirking in a context fraught with asymmetric information: agricultural service provision in the developing world. Our clearinghouse provides citizens in rural Punjab, Pakistan with government veterinarians' success rates at artificially inseminating livestock, an objective measure of veterinarian effort. It gathers and disseminates locally relevant information from a large base of farmers automatically, in real time, using a call center.

Our clearinghouse model stands in contrast to government monitoring schemes that provide information to agents' superiors, relying on the "long route" of accountability in which citizens must influence policymakers to improve service provision (Callen et al., 2015). It approaches the problem more directly; it strengthens the "short route" of accountability by increasing citizens' direct power over government agents (World Bank, 2004).

And our clearinghouse strengthens government agent accountability in providing a service that is important for the livelihood of people across the developing world—renewing livestock through artificial insemination (AI). 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). AI 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. But government veterinarian shirking leads to AI success rates lower than what is possible given the technology, costing farmers potential income.

We evaluate this clearinghouse using a randomized controlled trial. Using data generated by the clearinghouse, we find that farmers treated with information on local government veterinarians' AI success rates have a 27 percent higher AI success rate than controls when they subsequently return for government services. In addition, treatment farmers are 33 percent more likely to return to a government veterinarian for AI rather than to seek a private provider.

Multiple mechanisms could explain this treatment effect on AI success rates, including treatment farmers selecting better veterinarians and/or veterinarians exerting more effort for treatment farmers. Several of our results suggest the latter—that government agents work harder when the ratings system is in place. First and foremost, treatment farmers are no more likely than control farmers to switch veterinarians after treatment. Thus the effect cannot be driven by farmers simply switching to the 'best vet' in terms of AI success and/or price. Second, treatment farmers pay lower prices after treatment.[1] While farmers may be able to improve AI success rates through their behavior alone, a change in prices requires a change in veterinarian behavior.[2]

Our estimated treatment effects on AI success are potentially subject to both selection and reporting biases since they use data from the clearinghouse. In this data, we only observe farmers who return for government AI after treatment and not those who switch to private providers, as these are not part of our clearinghouse. Returning farmers must then also choose to answer the phone and to report AI success to the clearinghouse. Importantly, we find analogous results using a representative in-person survey not subject to selection or reporting biases but with lower precision. We find an overall 26 percent treatment effect in this representative sample, which averages a treatment effect of 83 percent for farmers that select back into government AI after treatment and a treatment effect of 4 percent for attritors.[3]

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

[1] Note the estimated treatment effect on log AI price has a p-value of 0.12 in our primary specification.

[2] It 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.

[3] Note the estimated overall treatment effect has a p-value of 0.12 in our primary specification. The treatment effect for farmers that select back into government AI, analogous to the AI success rate result using clearinghouse data, is significant at 5 percent.

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.

In a standard agency model with a stochastic outcome and inability to contract on this outcome, either unobserved agent effort (moral hazard) or unobserved inherent agent ability (adverse selection) a priori predicts both sub-optimal outcomes at baseline and that outcomes will improve as unobserved effort is revealed. We find both of these predictions to be true. However, because treatment farmers see increased AI success rates without switching veterinarians, our results rule out a pure adverse selection model and support one of moral hazard.

Several additional results from our representative in-person survey support a standard agency model. 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 the amount of information farmers receive determines their benefit.

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.[4]

[4]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

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), 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 27 percent higher AI success rate translates into nearly an additional half of one month's median income per AI provided, a 300 percent return on the cost of the intervention. These effects hold out hope for improved government accountability as cellular technology improves and become cheaper.

The paper proceeds as follows: Section 2 provides background on our study district and 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, and Section 5 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

more time visiting treatment farmers. Any costs must be in terms of concentration, etc.

a slightly higher prevalence of livestock.[5]

Sahiwal has a vibrant market for artificial insemination for at least two reasons. First, Sahiwal is representative of poor, dairy-producing areas in that 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.[6]

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 at a time.[7] These veterinarians' sole job is to provide artificial insemination.[8]

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.

[5]According 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. However, Sahiwal's average wealth, labor force participation rates, and child mortality rates are no different from the rest of Punjab.

[6]The provincial government selectively breeds livestock in two main centers in Punjab. It then distributes the semen produced by this program for use by government veterinarians across the province, including in Sahiwal.

[7]Throughout our study period, a total of 77 veterinarians were active in Sahiwal for any amount of time. Only a handful of veterinarians transferred in or out of Sahiwal.

[8]In some cases they may provide vaccinations during AI service provision, but this occurs very rarely. A smaller, distinct group of veterinarians is tasked with providing care to animals for sickness.

2.2 Asymmetric information in the market for AI

It is clear that asymmetric information exists between veterinarians and farmers about unobserved effort during each single visit. However, even before our intervention, farmers could decrease asymmetries by aggregating information about their veterinarians' success rates across visits and across households. Our data suggests that they do not do so. At baseline, farmers' estimates of their current government veterinarian's AI success rate are uncorrelated with the truth. This can be seen in Figure 6, Panel A.

Because of this lack of ex-ante information aggregation, AI success rates average approximately 70 percent in Sahiwal, while success rates of 85-90 percent are technologically possible. This gap is consistent with studies showing farmers' underinvestment in fertilizer, due to present bias Duflo et al. (2011).

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 the clearinghouse.

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 equipped with an Open Data Kit-based application.[9] 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 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

[9] In practice, veterinarians did not always comply with this requirement. See Section 4.1 for discussion.

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 then 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 already-occurring second calls, 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 across time, so we could observe post-treatment government AI provision for both treatment and control farmers (if and when they return, Figure 1 depicts the return of a treatment farmer but not a control farmer). From the perspective of the clearinghouse, these post-treatment observations are treated as a new t = 0 observation, generating follow-up phone calls.[10]

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 weighted AI success rates for cows, and the top three veterinarians in terms of weighted AI success rates for buffalo. When we had fewer than 25 observations for a veterinarian, weighted success was $q_v * \sqrt{n}$, where q_v was observed average success and n was the number of observations. For more than 25 observations, weighted success was $5 * q_v$.[11]

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.[12] The clearinghouse then sent a follow-up SMS. We also gave farmers veterinarians' phone numbers, information on average farmer-reported satisfaction with veterinarians on a 1-5 scale, and information on any other

[10]Note, however, that treatment selection is carried forward in time. See Section 3.2.

[11]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.

[12]There can be overlap in the most successful veterinarians in terms of cows and buffalo.

veterinarian in our system, if they requested it.

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 this coin-flip occured at different times for each farmer, 60 days after they first entered our clearinghouse. This means that the post-treatment period differs for each farmer; however, treatment and control farmers were randomized at the same rate across time.[13]

3.3 Representative survey

We use administrative data from the clearinghouse for our primary analysis. We also independently collected data from a representative sample of farmers from across Sahiwal through baseline and endline surveys. This was to fill any information gaps due to the farmers selecting into the clearinghouse (they first chose 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 because we only observed post-treatment outcomes for clearinghouse farmers who subsequently returned to a government veterinarian for AI (as opposed to a private provider).

For these independent surveys, we sampled 90 of Sahiwal's approximately 500 villages from a district village census.[14] Within each village, we selected ten households using the well-documented EPI cluster sampling method. We selected only households that reported owning at least two livestock (cows and/or buffalo) and having regular access to a cellular phone.

Between survey rounds, we manually entered survey farmers' phone numbers into our

[13]Unfortunately, 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.

[14]We 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.

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.

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.[15]

We report use of representative survey data in table and figure titles. Tables 1, 2, and A.1 report the balance of our clearinghouse and representative survey samples between treatment and control farmers.

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 ϵ_{ft} is an idiosyncratic error term. While we administered treatment at the farmers 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

[15]Note the midline depicted on the figure was used only 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.

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 time t differed from the farmer's veterinarian at time $t-1$.

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

$AI\ success\ rate_{ft}$: the rate of success of the AI provided at time t, as reported by the farmer when called 60 days later.

$Returned_f$: 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.[16]

4 Results

In this section, we present several sets of results that fall into three broad groups. First, we verify the internal validity of our data. To do this, we show treatment does not induce veterinarian reporting bias (Section 4.1) or farmer reporting or selection biases (Section 4.3). Second, we present our treatment effects. This includes those on our primary outcomes (Section 4.2) as well as two sets of heterogeneous treatment effects that support a moral hazard model (Sections 4.4 and 4.5). Lastly, we summarize our treatment effects to first understand the primary mechanism for our treatment effects, increased effort by veterinarians for the treated (Section 4.6), and second the social welfare implactions of our intervention (Section 4.7).

[16]We 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.

4.1 Does treatment induce a veterinarian reporting bias?

Before we present results using our clearinghouse data, it is important to note in Table 3 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 30 percent of the time, they are equally likely to report for treatment and control farmers. This allows us to trust the internal validity of our clearinghouse sample.

4.2 Treatment effects on primary outcomes

Table 4 presents treatment effects of information provision on our primary outcomes using clearinghouse data. On the extensive margin, in column (1), treatment farmers are 3.2 percentage points, or 33 percent, more likely than control farmers to return for government AI after treatment.[17] As a visualization, we present an added-variable plot of this result in Figure 3.

In columns (2) through (4), we present intensive margin treatment effects on those farmers that return after treatment selection. In columns (2) and (3) we find that there are no statistically significant effects on veterinarian switching or on log prices, though the coefficient on log price is nearly significant with a p-value of 0.12. In column (4), we find that treatment farmers have a 17 percentage point, or 27 percent, higher AI success rate after treatment.

In Figure 4, 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). We show that treatment farmers have higher AI success rates consistently across time. This suggests that any information spillovers between treatment and control farmers are either small or fixed

[17] The 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 3 as well, only 30 percent of return visits were recorded by veterinarians, so even in five months the true return rate is likely 40 to 50 percent.

throughout time. The latter is unlikely given the rolling nature of treatment. If anything, there is a small bump up in AI success rates for control farmers in the first month of the treatment, which suggests positive information spillovers. This would attenuate our results. The figure also suggests that there are no negative spillovers onto control farmers from veterinarian effort constraints.[18]

In Figure 5, 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 in six of eight months.

We reproduce our primary treatment effects on our representative survey sample in Table A.2. While we have less precision in this smaller sample, the point estimates are of a similar magnitude. And we find that the treatment effect on AI success in this sample is significant and has an even larger magnitude.[19]

4.3 Ruling out farmer selection and reporting biases

To argue that the treatment effect on AI success rates from our clearinghouse data represents a change in farmer and veterinarian behavior, we must rule out farmer selection and reporting biases. Our estimates would include a farmer selection bias if farmers that would otherwise see higher success rates are those that select back into government AI as apart of the large extensive margin treatment effect. Our estimates would include a farmer reporting bias if treatment farmers are more or less likely to answer the phone when we call to ask about AI success.

Table 5 presents evidence against both farmer selection and response biases. In our representative survey sample, we surveyed both farmers who returned for government AI

[18]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.

[19]Note that the mean return rate of control farmers is higher in this sample, but not three times that of the clearinghouse sample. This is consistent with the facts that we do not rely on veterinarian reporting for this data but also that these farmers had less time after treatment to return to our sample on average.

after treatment and those who attrited to a private provider. Accounting for attriters removes possible selection bias. In addition, the representative survey had a successful follow-up rate of 96 percent which greatly limits response bias. In column (6) we find that, while it is not quite significant, overall AI success rates are large and positive even when including those farmers that attrited.[20]

As an additional check for farmer selection bias, in Table 6 we show balance on all measured pre-treatment outcomes, including AI success rates, between returning treatment and control farmers in the clearinghouse data. 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 Treatment effects by government veterinarian rank

In Table 7, 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.[21]

We find that our main results on both the extensive and intensive margins are localized

[20] The p-value of this estimate is 0.12.

[21] Note 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.

to farmers whose past veterinarian was not ranked in the top three in their area at the time of treatment. 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.[22]

As with any farmer characteristic, past government veterinarian effort is not randomly assigned, and is in fact correlated with several other farmer characteristics. We show in Table A.3 that farmers whose past government veterinarian was not ranked in the top three in their village-cluster 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. In addition, these farmers have more buffalo. We control for baseline means of both of these variables in Table 7, but we cannot rule out that heterogeneous effects we attribute to information are actually driven by some other unobserved difference.

4.5 Results using farmer expectations from the representative survey sample

We present three additional results that are consistent with a standard moral hazard model, 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.

In Figure 6, 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.

[22]We 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.

Our first result is in Panel A of the figure—at baseline there is no correlation between farmer expectations and the truth. 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. Point estimates for these first two results, as well as the complimentary result that we do not see the same positive correlation for control farmers in the endline, are reported in Table 8.

Third, in this sample we can also directly measure how much farmers learned about their veterinarians' unobserved effort through treatment. We difference farmers' expected average AI success rate with the truth. We then separate our sample according 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 7 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.

4.6 Increased government veterinarian effort for the treated

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 4 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 5 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. 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' informational rents through higher effort and lower prices. And while farmers may be able to improve AI success rates through their behavior alone, the decrease in prices that we find requires a change in veterinarian behavior.

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 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.

4.7 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.[23]

Benefit to farmers: if the treatment effect of 27 percent on AI success rates translates into just three 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, treatment farmers would earn an additional 3,225 PKR (32 USD) per year, equal to nearly half of one month's median income.[24] 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.

Cost to farmers: as we argue 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.

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

[24] This 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.

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 32 USD per farmer from an intervention that cost 8 USD per farmer. This suggests a 300 percent return.

5 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 asymmetric information. 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. 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 300 percent return to farmers on the cost of the intervention. This is driven by a 27 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.

As a testament to the scalability of our clearinghouse, we have already begun conversations within the Livestock Department about expanding the program to all of Punjab. This would require no additional fixed costs and less than proportional marginal costs. Across contexts, we are already experimenting with scaling our information clearinghouse to relieve asymmetric information between citizens and pollution regulators in Punjab. We hope to learn how crowdsourcing can work in a regulatory rather than a market environment, and for public rather than private goods.

Some of these effects of improving the flow of information depend on how connected the population is, and the price of connectivity. In another project, we are experimenting with the placing of cellular towers to understand how economic, social, and political outcomes are impacted by across-the-board decreases in the cost of transmitting information.

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.

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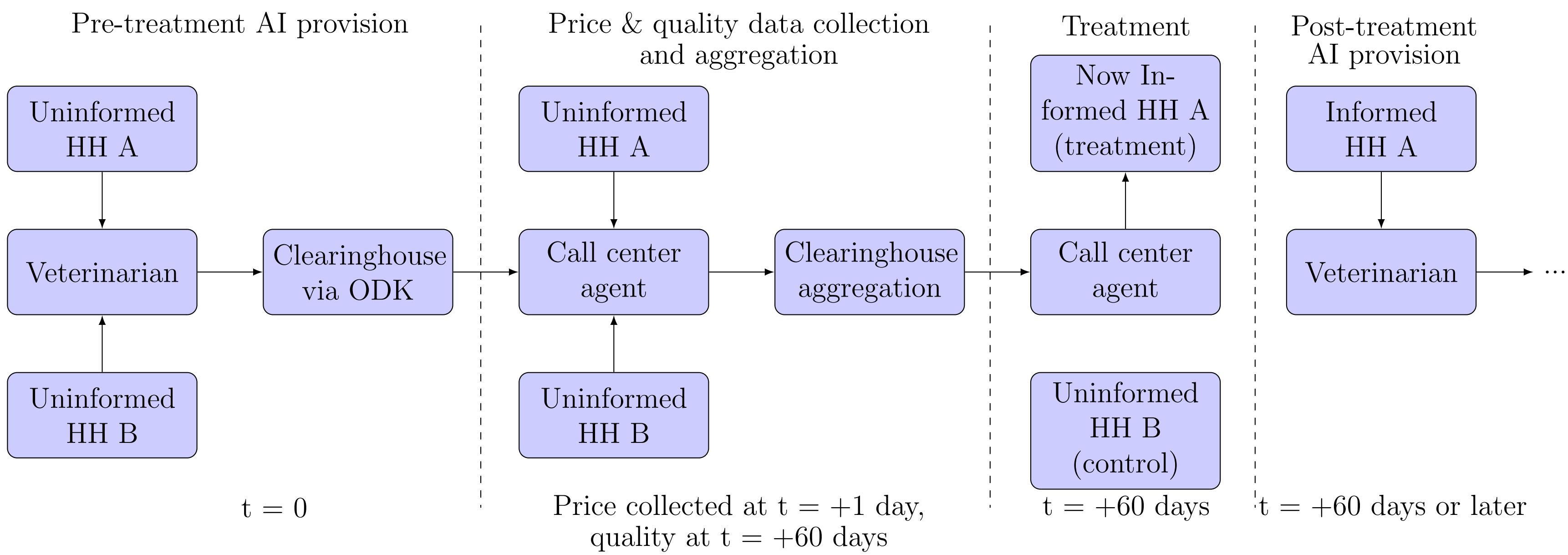
6 Tables and figures

Table 1: Treatment balance—clearinghouse data

	Treatment	Control	Difference	P-value
Satisfaction with AI service provision (1-5)	4.185	4.136	0.049	0.123
	[0.736]	[0.760]	(0.029)	
Farmer switched vets since last AI visit	0.052	0.047	0.005	0.133
	[0.222]	[0.213]	(0.0100	
AI visit charges (PKR)	196	203	-7	0.479
	[180]	[250]	(9)	
AI visit success rate (pregnancy / AI attempts)	0.686	0.687	-0.002	0.432
	[0.458]	[0.457]	(0.016)	
No of cows owned by farmer	2.544	2.447	0.097	0.312
	[3.439]	[3.053]	(0.155)	
No of buffalo owned by farmer	3.121	3.315	-0.195	0.771
	[3.777]	[6.347]	(0.366)	
Distance to closest AI center (km)	2.170	2.277	-0.107	0.825
	[2.254]	[2.259]	(0.114)	

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 6,473 pre-treatment farmer-visit-level observations from 3,094 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.

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

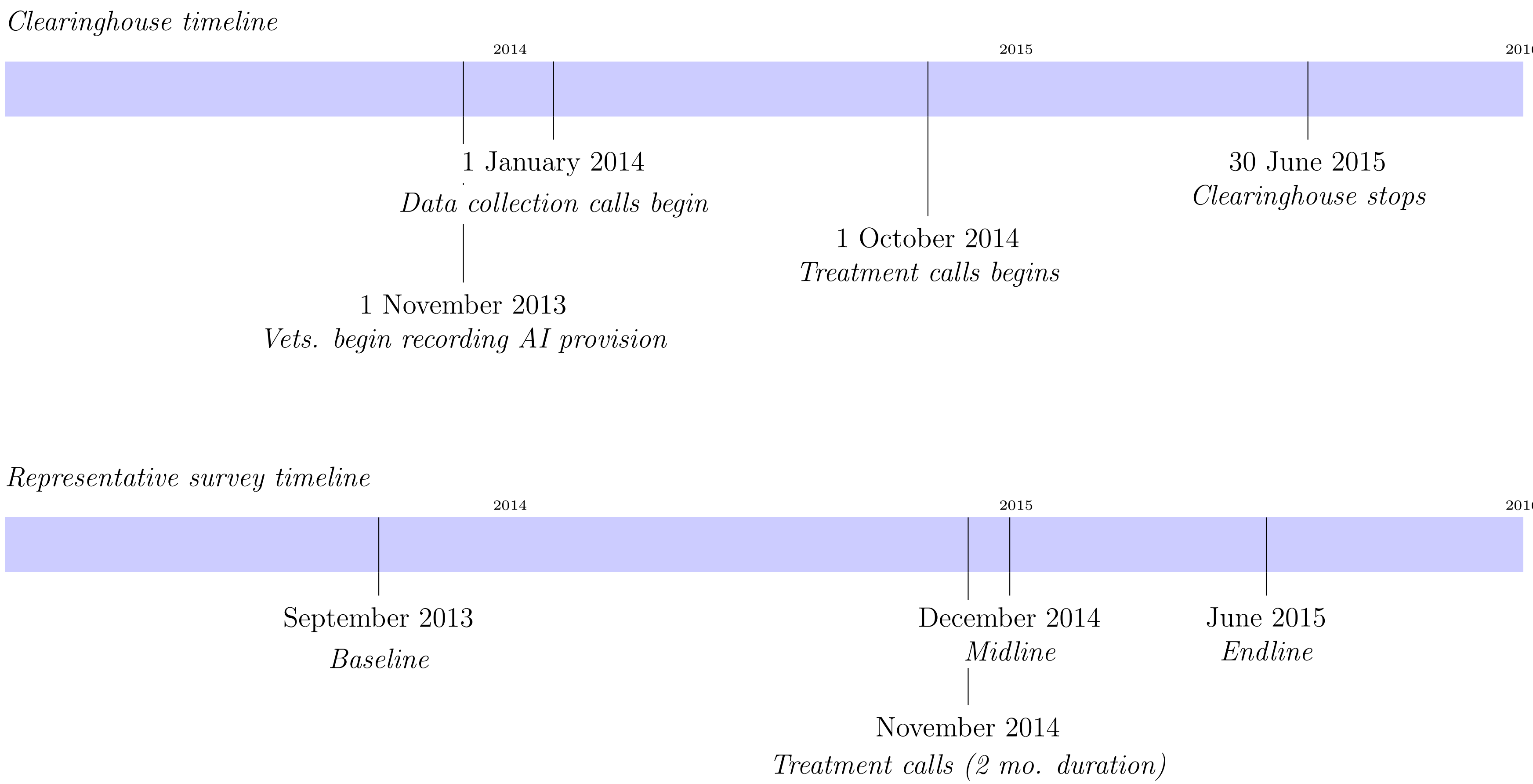


Table 2: Treatment balance—representative survey sample

	Treatment	Control	Difference	P-value
Farmer-level baseline variables—679 observations across 69 village-clusters				
Livestock is primary source of household's income (=1)	0.093	0.114	-0.021	0.252
	[0.291]	[0.322]	(0.024)	
1-10 effort household puts into selecting veterinarian	5.456	5.504	-0.048	0.842
	[2.527]	[2.513]	(0.317)	
Farmer attrited from in-person endline	0.037	0.031	0.006	0.460
	[0.188]	[0.173]	(0.014)	
Farmer-visit-level variables—1,103 pre-treatment observations from 577 farmers across 80 village-clusters				
AI service provided by gov't veterinarian (=1)	0.462	0.407	0.055	0.152
	[0.499]	[0.492]	(0.041)	
Farmer switched veterinarians since last recorded AI visit (=1)	0.192	0.233	-0.041	0.560
	[0.395]	[0.423]	(0.034)	
AI visit charges	448	434	14	0.704
	[507]	[585]	(40)	
AI visit success rate	0.687	0.734	-0.047	0.156
	[0.450]	[0.429]	(0.031)	
1-10 AI visit farmer satisfaction	7.643	8.580	-0.937	0.196
	[2.224]	[15.001]	(0.666)	
1-10 farmer estimated AI visit veterinarian success rate	6.527	6.539	-0.012	0.772
	[1.824]	[1.902]	(0.155)	

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. 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 3: Does treatment induce a veterinarian reporting bias?

	Treatment	Control	Difference	P-value
Farmer reported AI and veterinarian submitted data to call center (=1)	0.299	0.276	0.023	0.758
	[0.459]	[0.448]	(0.044)	.
Farmer reported receiving a call verifying AI service (=1)	0.287	0.240	0.047	0.566
	[0.449]	[0.422]	(0.041)	.

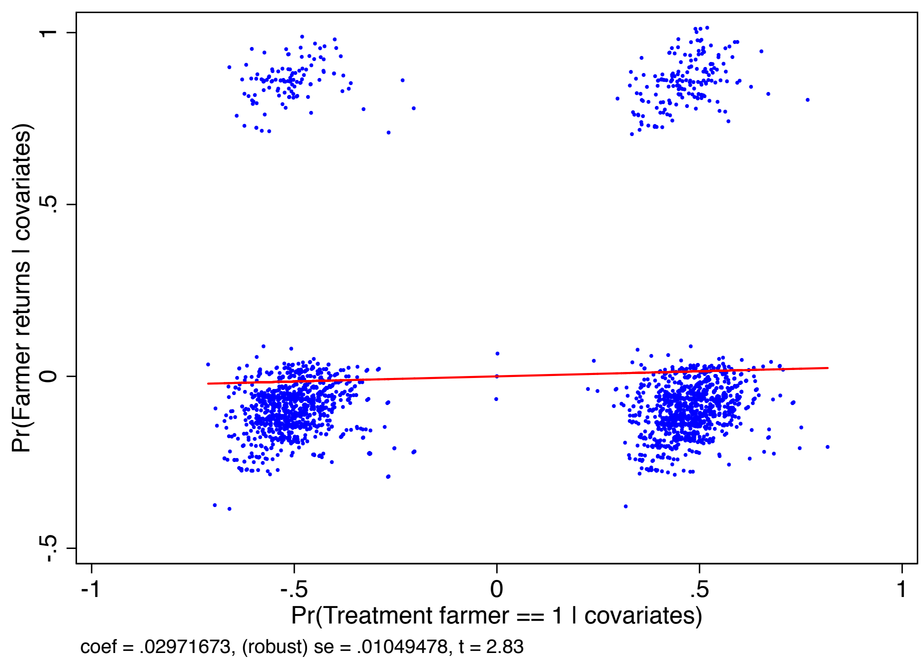
Notes: Standard deviations reported in brackets. Standard errors reported in parentheses. Means and differences are unconditional. P-values are from OLS regressions with randomization strata fixed effects and standard errors clustered at the village-cluster level. The sample consists of 730 farmer-visit-level observations from 440 unique farmers across 83 village-clusters from our endline survey, conducted in June 2015. Some regressions have fewer observations due to missing data. 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 veterinarian 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.

Table 4: Treatment effects—clearinghouse data

Outcome:	Returned	Switched veterinarians	Log price	AI success rate
	(1)	(2)	(3)	(4)
Treatment farmer (=1)	0.032***	0.007	-0.270	0.168**
	(0.011)	(0.028)	(0.170)	(0.083)
Mean of dependent variable	0.098	0.084	5.248	0.623
# Observations	3184	629	312	240
# Village-clusters	205	111	103	98
R-Squared	0.192	0.305	0.596	0.489
Sample	Pre	Post	Post	Post

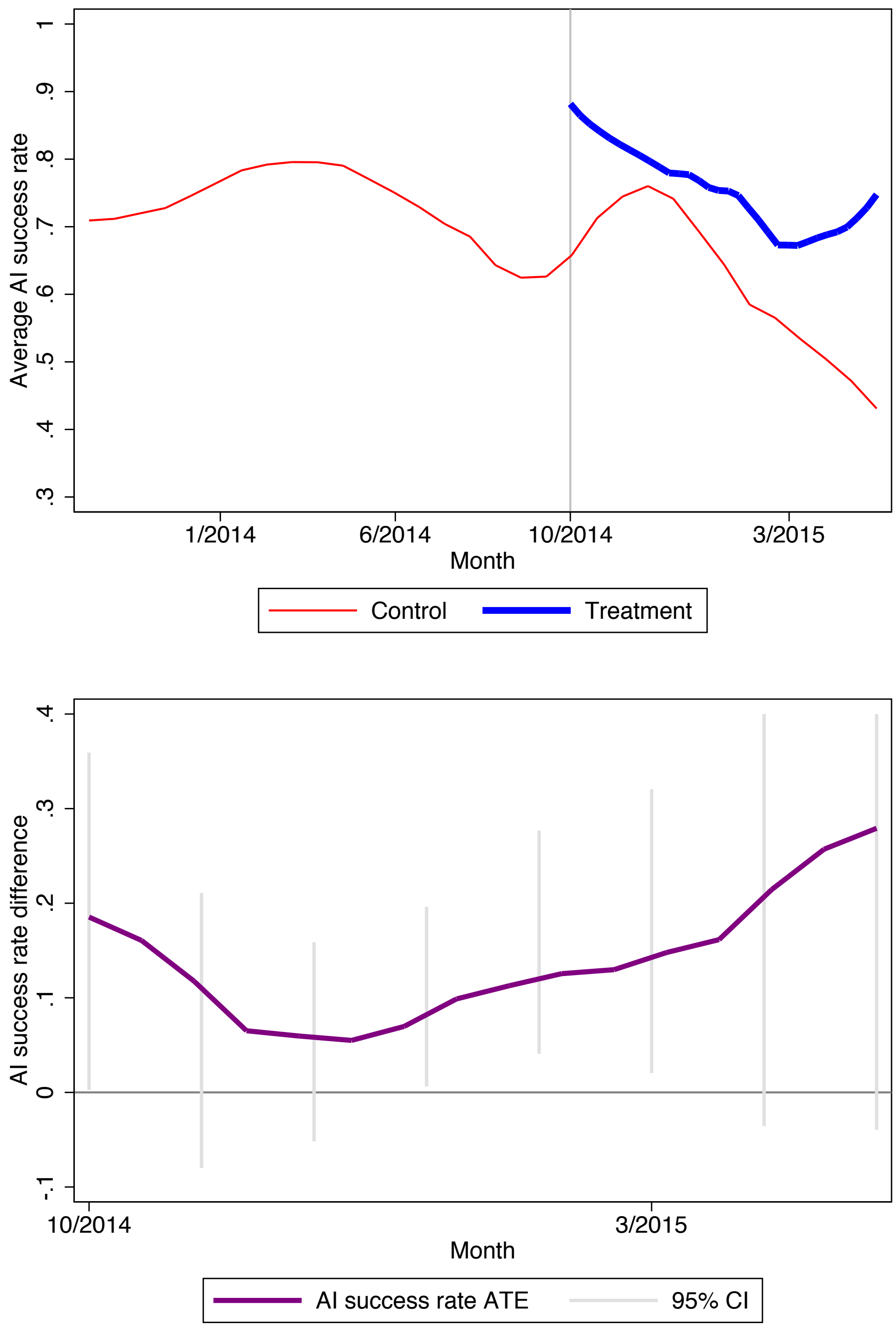
Notes :* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the village-cluster level reported in parentheses. All regressions include randomization strata fixed effects and controls for baseline mean outcomes (switched veterinarians, log price, AI success rate) as well as dummies for whether the given observation is missing each baseline mean outcome. In addition, columns (2) through (4) include exact call center script fixed effects and 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.

Figure 3: Farmer returned added-variable plot—clearinghouse data



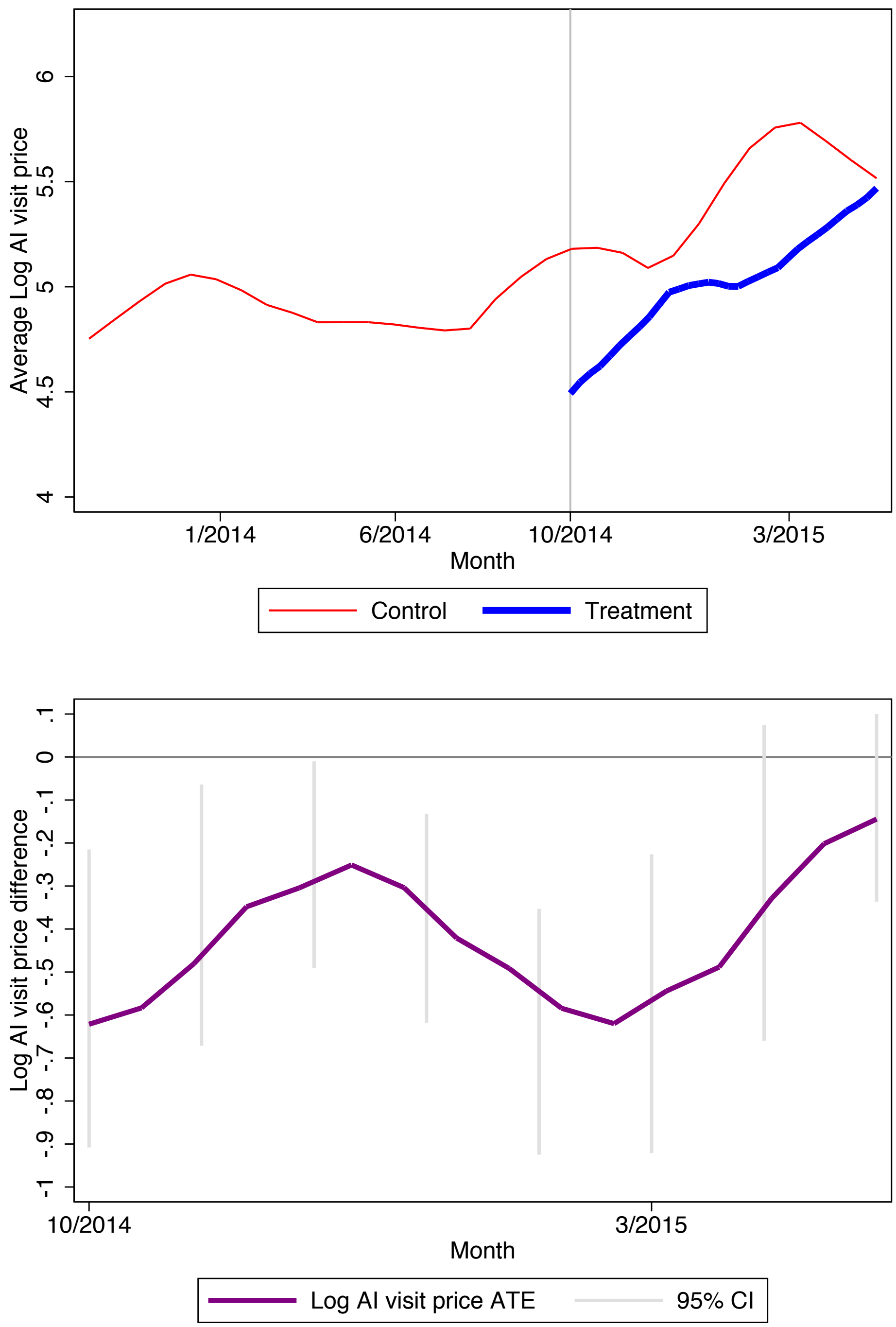
Notes: The sample is farmers that received a government AI service and were subsequently treated, regardless of whether they then returned. Beginning in October 2014 treatment farmers received information about the AI success rates of their local government veterinarians. Returned is a dummy variable 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. The covariates used to predict residual values are 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.

Figure 4: 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 bandwidth one. Confidence interval bootstrapped and truncated at 0.4.

Figure 5: 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 interval bootstrapped and truncated at 0.1.

Table 5: Treatment effects—representative survey sample

Outcome:	Log price			AI success rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment farmer (=1)	0.027	-0.146	-0.062	0.470**	0.028	0.172
	(0.405)	(0.216)	(0.164)	(0.186)	(0.187)	(0.109)
Mean of dependent variable	5.856	5.888	5.874	0.567	0.765	0.672
# Observations	69	87	156	63	79	142
# Village-clusters	27	39	53	29	35	51
R-Squared	0.633	0.655	0.540	0.498	0.281	0.271
Sample	Returned	Attrited	Both	Returned	Attrited	Both

Notes :$^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. 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 (switched veterinarians, log price, AI success rate) as well as dummies for whether the given observation is missing each baseline mean outcome. 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. Returned indicates farmers that received government AI before treatment and subsequently returned for government AI after treatment by the end of the project. Attrited indicates farmers who received government AI before treatment and instead subsequently received private AI by the end of the project. Log price and AI success rates are recalled by farmers from service provisions two to seven months ago.

Table 6: Treatment balance of returning sample—clearinghouse data

	Treatment	Control	Difference	P-value
Pre-treatment mean satisfaction with AI service provision (1-5)	4.212	4.248	-0.036	0.765
	[0.684]	[0.713]	(0.080)	
Pre-treatment mean veterinarian switching rate	0.047	0.026	0.020	0.131
	[0.218]	[0.206]	(0.019)	
Pre-treatment mean log AI visit charges	4.852	4.838	0.014	0.660
	[1.356]	[1.352]	(0.147)	
Pre-treatment mean AI success rate	0.694	0.669	0.025	0.541
	[0.445]	[0.439]	(0.051)	
Pre-treatment mean no. of cows	2.770	3.168	-0.398	0.351
	[2.785]	[2.349]	(0.384)	
Pre-treatment mean no. of buffalo	3.493	3.321	0.173	0.929
	[3.243]	[4.109]	(0.444)	
Pre-treatment mean distance to closest AI center (km)	2.413	2.007	0.406	0.728
	[2.158]	[2.190]	(0.245)	

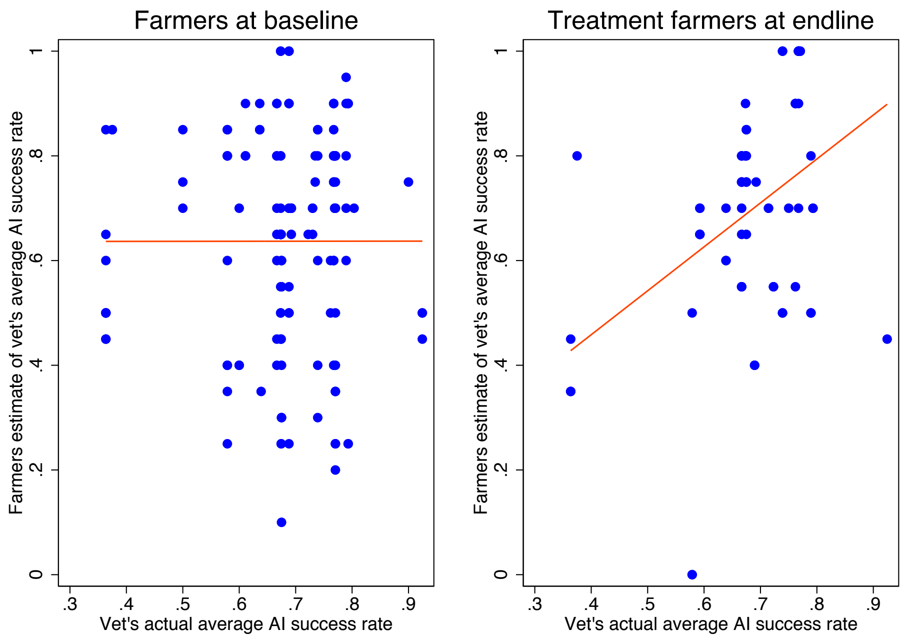
Notes: Standard deviations reported in brackets. Standard errors reported in parentheses. Means and difference are unconditional. P-values are from OLS regressions with randomization strata fixed effects and standard errors clustered at the village-cluster. The sample consists of 300 farmer-level observations across 108 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 7: 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)	0.008	-0.009	-0.169	0.010
	(0.013)	(0.035)	(0.136)	(0.115)
Mean of dependent variable	0.091	0.098	4.903	0.654
# Observations	1977	439	169	124
# Village-clusters	174	78	66	56
R-Squared	0.102	0.363	0.717	0.743
Panel B: Farmers told vet. was not in top three in area				
Treatment farmer (=1)	0.039*	0.005	-0.994	0.285*
	(0.020)	(0.079)	(1.419)	(0.161)
Mean of dependent variable	0.067	0.050	5.574	0.429
# Observations	1087	166	82	68
# Village-clusters	161	55	40	34
R-Squared	0.121	0.576	0.819	0.873
Sample	Pre	Post	Post	Post

Notes :$^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. 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 exact call center script fixed effects and 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 6: 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 8: 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	-0.130	0.839**	0.231
	(0.398)	(0.385)	(0.229)
# Observations	121	66	37
# Village-clusters	30	21	20
R-Squared	0.002	0.162	0.020
Sample	Baseline	Endline T	Endline C

Notes :* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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.

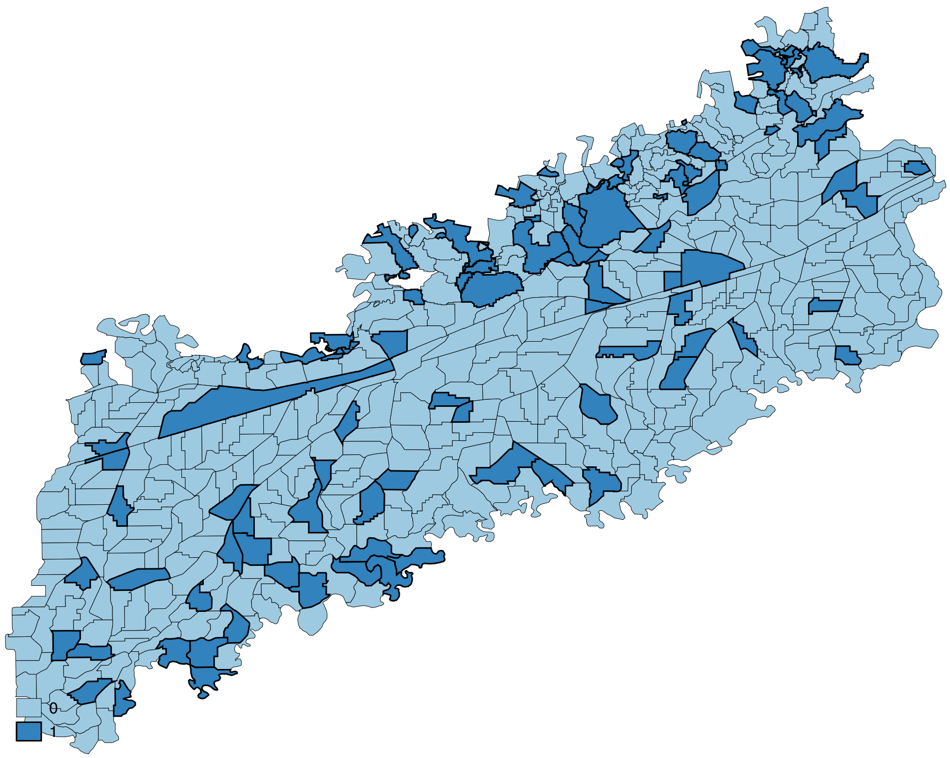
Table 9: Treatment effects by farmer expectations—representative survey sample

Outcome:	Returned	Switched veterinarians	Log price	AI success rate
	(1)	(2)	(3)	(4)
Panel A: Farmers with above median expected-actual AI success				
Treatment farmer (=1)	-0.083	0.049	0.294	0.318
	(0.135)	(0.055)	(0.493)	(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 below median expected-actual AI success				
Treatment farmer (=1)	0.113	0.369	-1.399***	0.749*
	(0.274)	(0.329)	(0.385)	(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 :*p < 0.1, **p < 0.05, ***p < 0.01. 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 veterinarian's farmers' estimate of their veterinarian's average AI success rate minus veterinarian's actual average AI success rate from clearinghouse data before October 2014. Positive values in this difference occur when farmers are told 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.

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)	0.388	0.404	-0.016	0.814
	[0.488]	[0.492]	(0.038)	
A child in the household attends public school (=1)	0.533	0.525	0.008	0.915
	[0.500]	[0.500]	(0.038)	
Household has used govt health services in past two years (=1)	0.399	0.466	-0.067	0.045
	[0.490]	[0.500]	(0.038)	
Amount of land household owns and rents for livestock	1.455	1.417	0.038	0.646
	[3.248]	[2.875]	(0.273)	
Household owns the house that they live in (=1)	0.926	0.948	-0.021	0.210
	[0.261]	[0.223]	(0.020)	
Hours of electricity per day	10.458	10.022	0.436	0.214
	[3.366]	[3.573]	(0.276)	
Household has a cooking stove/range (=1)	0.086	0.121	-0.035	0.119
	[0.280]	[0.326]	(0.024)	
Household made less than 100k PKR last year (=1)	0.320	0.301	0.019	0.349
	[0.468]	[0.460]	(0.036)	
Any member of household has hank account (=1)	0.235	0.275	-0.040	0.109
	[0.424]	[0.447]	(0.034)	
Believed it was likely that last vote was not secret (=1)	0.542	0.582	-0.040	0.396
	[0.499]	[0.494]	(0.041)	
Is likely to believe information given by gov't employee (=1)	0.776	0.815	-0.039	0.180
	[0.417]	[0.389]	(0.031)	
Average number of digits recalled	3.308	3.308	0.000	0.818
	[0.992]	[1.129]	(0.112)	
On a scale fo 0-10, how willing are you to take risks?	4.345	4.715	-0.370	0.332
	[3.008]	[6.894]	(0.503)	
Agreeableness	4.017	4.033	-0.016	0.756
	[0.743]	[0.702]	(0.057)	
Conscientiousness	4.071	4.128	-0.057	0.263
	[0.627]	[0.656]	(0.051)	
Extroversion	4.163	4.096	0.067	0.530
	[0.686]	[0.695]	(0.056)	
Neuroticism	2.363	2.375	-0.013	0.761
	[0.845]	[0.854]	(0.066)	
Openness	3.724	3.689	0.034	0.796
	[0.711]	[0.755]	(0.057)	

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 679 baseline farmer-level observations across 69 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. 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: Treatment effects—representative survey sample

Outcome:	Returned	Switched veterinarians	Log price	AI success rate
	(1)	(2)	(3)	(4)
Treatment farmer (=1)	0.063	-0.058	0.027	0.470**
	(0.062)	(0.171)	(0.407)	(0.187)
Mean of dependent variable	0.222	0.152	5.852	0.581
# Observations	251	69	70	64
# Village-clusters	72	27	28	30
R-Squared	0.235	0.457	0.633	0.503
Sample	Pre	Post	Post	Post

Notes :$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. 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.

Table A.3: Comparing farmers by pre-treatment veterinarian ranking—clearinghouse data

	Vet. in top three	Vet. not top three
Satisfaction with AI service provision (1-5)	4.170	4.142
	[0.736]	[0.769]
Farmer switched vets since last AI visit	0.051	0.071
	[0.220]	[0.257]
AI visit charges (PKR)	192	212
	[170]	[269]
AI visit success rate (pregnancy / AI attempts)	0.628	0.635
	[0.477]	[0.476]
No of cows owned by farmer	2.382	2.668
	[3.154]	[3.660]
No of buffalo owned by farmer	2.816	3.516
	[3.165]	[5.949]
Distance to closest AI center (km)	1.710	3.257
	[1.572]	[2.949]

Notes: Standard deviations reported in brackets. The sample consists of 4,788 pre-treatment farmer-visit-level observations from 2,981 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.

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