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Essays on Regulating Goods and Service Quality

by

Eric Hsu

A dissertation submitted in partial satisfaction of the
requirements for the degree of
Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Edward Miguel, Chair
Professor Frederico Finan
Associate Professor Jeremy Magruder
Assistant Professor Supreet Kaur

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Essays on Regulating Goods and Service Quality

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Eric Hsu

Abstract

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Doctor of Philosophy in Economics

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Professor Edward Miguel, Chair

Many markets for important goods and services are regulated to address market inefficiencies. This dissertation presents research on regulating goods and service quality, with a focus on two important markets in rural Kenya. The first is the market for hybrid maize seeds, an important agricultural input for the production of a staple grain. The second is electricity services, important for communication, lighting, and other productive household and commercial uses. In the rural Kenyan setting, both of these markets feature inconsistent quality, with one being dominated by many small private suppliers, and the other characterized by a single nationwide provider. The three chapters will study different aspects of measuring quality and policies that may be effective in regulating quality in these and similar settings.

The first chapter, joint with Anne Wacera Wambugu, examines whether improving consumer information can improve the quality of goods supplied by sellers. We examine this question in rural markets for hybrid maize seed in Kenya, a context in which goods quality is difficult for buyers to observe. To understand the impacts of improved consumer information, we implement a market-level intervention, randomizing rural markets in Kenya into a community-wide information campaign. Small-scale maize farmers in treated market areas were trained to identify hybrid maize seed that is quality-verified under national seed regulations. We find that observable markers on seed packets can predict seed quality, and treatment increased knowledge of these markers and affected seed purchase decisions. We show that gains in yields were experienced among subgroups of informed buyers that had larger gaps between baseline seed quality and national standards. However, we do not see effects on seed quality offered to uninformed buyers in the same communities.

The second chapter, joint with Catherine Wolfram, Edward Miguel, and Susanna Berkouwer, examines whether contracting conditionality imposed by multilateral organizations can affect the quality of electricity infrastructure and electricity services in a large donor-financed mass electrification project in Kenya. We provide causal evidence on this topic by leveraging an unusual feature of this nationwide program: the quasi-random allocation

tion of multilateral funding sources across nearby villages. The sample consists of mass-electrification sites funded by the African Development Bank or the World Bank. After construction, we collect outcome data in these communities on local infrastructure quality and on the quality of household electricity services. We find that more stringent World Bank contracting procedures led to improved pole quality, but somewhat delayed construction completion and minimal other differences in construction quality and the quality of electricity services. In comparison, a randomized audit scheme modestly improved household electricity usage.

The third chapter, joint with Anne Wacera Wambugu, turns to the issue of measuring electricity quality in a critical area of policy interest—electricity for rural healthcare services. This chapter offers a case study on the productive use of electricity and the consequences of low-quality electricity service. We find that power outages are common, with the majority of health facilities experiencing an outage in the past 30 days. Back-up power sources, while common, only partially mitigate the negative effects of episodes of poor electricity quality.

Taken together, these three chapters examine important questions related to the regulation of goods and service quality. While focused on two markets in rural Kenya, the findings have broader implications. Findings from Chapter 1 suggest that increased numbers of informed end-users may improve quality obtained from private suppliers. However, the broader market-wide impacts may be limited without a very high saturation of informed buyers. This can help inform the role of government agencies in promoting the spread of consumer information as part of their regulatory responsibilities. Findings from Chapter 2 suggest that different kinds of contracting conditionality imposed by financiers of projects in low and middle income countries may have some positive though limited effects. More extensive use of ex-post audits, though, may be an opportunity to improve the quality of the resulting infrastructure and services at relatively low cost. Findings from Chapter 3 emphasize that these policies levers matter. By focusing on the role of electricity service quality in the healthcare sector in Kenya, this chapter shows that improvements in the regulation of public service quality are likely to impact firm productivity.

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Chapter 1

Can Informed Buyers Improve Goods Quality? Experimental Evidence From Crop Seeds

1.1 Introduction

When buyers cannot perfectly observe product quality, a market breakdown can occur in which offering high-quality products may not be profitable for sellers [Akerlof \(1970\)](#). A variety of mechanisms may help counteract such a market failure by providing a credible signal of product quality to consumers ([Dranove and Jin, 2010](#)). For example, firms may use branding or warranties to show they are incentivized to provide high quality. Government agencies can set quality standards and provide certifications or licenses to assure consumers of a certain level of quality in a market.

However, mechanisms to counteract the effects of limited consumer information may function imperfectly or be non-existent in many settings. Markets in low and middle income countries (LMICs), for instance, often have features that make them particularly vulnerable to problems with imperfect information. Lower access to information technology or lower levels of education may slow the diffusion of consumer information and hinder a firm's efforts to build a reputation for providing high quality ([Aker and Blumenstock, 2015](#)). Policy solutions may be undermined by lower state capacity ([Dal Bó and Finan, 2020](#)). This could lead to less effective enforcement of quality certifications. Branding and reputation building mechanisms may also function less well if entry of deceptive counterfeits cannot be deterred through legal institutions. These factors may help explain LMIC settings where reliably obtaining high quality goods can be challenging.

In this paper, we study the effects of improving consumers' ability to detect quality in the hybrid maize (corn) seed market in rural Kenya. Several features of this setting may hinder the ability of firms to signal that their product is high quality ([Bold et al., 2017](#); [Bai, 2021](#)).

By nature, the quality of hybrid maize seeds is difficult to observe prior to purchasing and planting them. Even after planting, the performance of maize seeds depends on numerous factors, each of which can have a sizable impact on plant growth and quantities of harvested maize. As a result, maize yields tend to be noisy and give buyers a poor signal of seed quality. Typical crop planting timelines limit farmers to only observing one or two harvests per year. Therefore, infrequency of purchase may also limit consumer learning about seed quality. While regulations exist to establish minimum quality standards for seeds, enforcement is limited and there have been widespread concerns about the prevalence of counterfeit and other uncertified seeds in recent years (Okinda, 2019).

Resolving information problems and improving market-supplied quality is likely to be particularly consequential in this setting. High quality maize seeds are a productive input that is critical for maintaining food security in low-income areas of the world. Maize is a staple food in our study area in Western Kenya but also more broadly in Southern, Central, and Eastern Africa and in Central and South America. In these regions, over 20% total calories are supplied by maize (Shiferaw et al., 2011). Our study area is in the East Africa region, where food security is a widespread and chronic issue and a major focus for policy-makers. Over 30% of the population in East Africa experience severe food insecurity, lacking adequate access to enough safe and nutritious food for healthy development (FAO, 2020, 2021). This contributes to malnutrition as a major public health problem. For example, in 2019 an estimated 34.5% of under-age-five children in the region were stunted and 5.3% were moderately or severely wasted, with height-for-age or weight-for-age at least two standard deviations below the median in World Health Organization standards (FAO, 2020).

To evaluate the effects of improved consumer information, we conduct a field experiment in a sample of 386 rural market areas in Kenya. In randomly selected markets, we provided community-wide trainings to enable farmers to better detect quality-verified hybrid maize seeds. This market-level intervention allows us to identify effects of improved information on local market participants, including consumers who became informed due to treatment, uninformed consumers in the same communities, and seed sellers. We examine whether and how informed buyers are able to achieve improved agricultural outcomes. As well we examine whether uninformed buyers in treated communities may also experience benefits through improved seed quality. Using a series of surveys and secret shopper activities, we track outcomes for buyers and sellers (including knowledge, seed sales and purchases, and agricultural outcomes) for over one year after treatment. The inclusion of 104 pure control sites allow us to test whether baseline activities may have influenced outcomes. The extended follow-up period of over one year allows us to track possible convergence to a new equilibrium over the course of three planting seasons following treatment.

The findings first show that observable quality markers correlate with lab tested quality, with packets missing one or more quality marker having lower germination rates.¹ This confirms that agricultural gains should in theory be possible if farmers follow a simple strategy

¹Among packets sampled by secret shoppers, 38% of them lack one or more quality marker.

to avoid buying seeds that are not quality-verified. Yet, farmer knowledge about observable quality markers is low at baseline.

The information treatment caused buyers to have better ability to detect seed quality. Treated buyers had greater knowledge of observable quality markers and were more likely to successfully use detection techniques in subsequent planting seasons. Treated buyers frequently reported that the information that was provided affected their purchasing decisions - both which seed packets to buy and which seed seller to buy from. Consistent with these accounts, we confirm that treated buyers were more likely to leave the local market to purchase seeds. Examining agricultural outcomes, we find that treated farmers experienced about 3% higher maize yields overall, though not statistically significant. Farmers in more remote market areas, which had lower initial seed quality, experienced a statistically significant 7.7% increase in yields, with female-headed households especially benefiting. Meanwhile, we do not detect effects on quality offered to uninformed buyers in the same communities, as revealed by data from secret shoppers.

These patterns can be explained by a model in which farmers who are informed are able to conduct a search for high quality seeds—possibly leaving the local market to do so—and their increasing numbers hurt the profits of local firms. Yet, sellers’ ability to discriminate between informed and uninformed buyers can allow lower-quality products to continue to be sold to uninformed buyers. Consistent with the model’s predictions, we also find that treatment caused some seed sellers to exit the market, and we do not detect effects on prices.

Taken together, the results document new stylized facts and provide important evidence relevant for boosting yields of a staple crop. There is much work on barriers to adopting and interventions to encourage adoption of agricultural technologies that can enhance productivity. [Jack \(2013\)](#) surveys this literature, which spans studies on many types of constraints - including credit and liquidity constraints, risk aversion, lack of knowledge on costs and benefits, and heterogeneous costs, among other explanations for under-adoption - and has motivated policy responses to address market failures in the adoption of agricultural inputs ([Holden, 2019](#)). Some recent work has explored the role of low input quality as a barrier to improving agricultural productivity. Quantitative research ([Bold et al., 2017](#); [Ashour et al., 2019](#)) and news reports ([Muchiri, 2019](#); [Okinda, 2019](#)) suggest that the quality of agricultural inputs - such as seeds, fertilizer, and pesticides - is often low in rural African markets. For example, [Bold et al. \(2017\)](#) study retail quality maize seeds and fertilizer from rural Ugandan markets. They find that switching to wholesale quality of the same products causes a 40% increase in maize yields. These reports suggest that low and high-quality products often appear similar along observables (e.g. price and package characteristics), which is consistent with accounts of widespread counterfeiting ([Kenya Association of Manufacturers, 2012](#)). Our paper contributes by evaluating an intervention to improve consumer information as a way to boost usage of high quality inputs and improve agricultural productivity. The findings are directly relevant to policymakers working to promote food security. This paper also contributes important descriptive data on the quality of agricultural inputs ([Bold et al., 2017](#); [Gharib et al., 2021](#); [Kenya Association of Manufacturers, 2012](#); [Ashour et al.,](#)

2019; Michelson et al., 2021)), particularly on the relationship between observables and seed quality.

We also contribute to work related to consumer mistakes, consumer learning, and quality provision. There is a deep theoretical literature on market dynamics when agents have imperfect information (e.g. Akerlof, 1970; Wolinsky, 1983; Shapiro, 1983). However, relatively few empirical studies have tried to test theoretical predictions in a real-world setting (Bai, 2021; Jin and Leslie, 2009; Bjorkman-Nyqvist et al., 2012). We do so in a high-stakes and policy-relevant context, using an experimental evaluation closely tied to a common government policy (certification of minimum quality standards) intended to overcome a market failure arising from a difficult learning environment for consumers. This paper also relates to work on consumer misinformation and costly consumer mistakes (Bronnenberg et al., 2015; Grubb and Osborne, 2015; Handel and Schwartzstein, 2018). We document that many consumers overlook easy-to-use strategies to obtain higher quality products, behavior that is inconsistent with a full-information model with rational consumers.

Lastly, we contribute to work on regulation, monitoring, and enforcement. Several papers examine how consumer information or monitoring by consumers can complement or replace direct enforcement efforts. Different from previous work (Gonzalez-Lira and Mobarak, 2021; Annan, 2021; Naritomi, 2019), we find evidence in our setting that consumers may not be very effective at disciplining sellers, and we discuss the conditions from this settings that likely contributes to this finding.

1.2 Background

Maize (often called corn in North America) is a staple crop in many areas of the world. In Eastern and Southern Africa, maize is estimated to account for 22% of all calories consumed and is an important crop for subsistence farmers in the region (Shiferaw et al., 2011). A central challenge for policymakers concerned about food security is how to boost agricultural productivity in lower-income countries, where maize yields are a small fraction of the average in OECD countries (OECD/FAO, 2021). Increasing access to improved agricultural inputs such as high yielding seed varieties and fertilizers have been one focus among policymakers in recent decade (Evenson and Gollin, 2003).

However, recently scholarly work and local media coverage has suggested that retail maize seeds in East Africa and other agricultural inputs are variable and often sub-standard (Bold et al., 2017; Ashour et al., 2019; Muchiri, 2019; Okinda, 2019). In Kenya, the regulation of agricultural inputs falls under the responsibility of the Kenya Plant Health Inspectorate Service (KEPHIS). KEPHIS was established in 2012 by the Seed and Plant Varieties Act, and as part of its responsibilities, it tests and certifies crop seeds for sale. All seeds must be certified by KEPHIS before sale, and all sellers must register with KEPHIS as well. Importantly, seeds must test with over 90% germination rate to be certified, meaning that under ideal temperature and moisture conditions randomly sampled seeds must develop

normally into a plant. Concerns about counterfeit and other uncertified seeds led to a new initiative in 2018 to mandate e-verification for certified seeds. This requirement for all seeds sold in Kenya was layered on top of pre-existing requirements, such as a printed lot number and packaging or expiration date listed for each packet. The new e-verification allowed for an additional method to obtain and verify printed information that was visible via SMS and could not be physically tampered with. Each packet is assigned a unique secret code that is revealed upon scratching off the sticker, and the code is available for use one time only – subsequent uses would indicate that the code is valid but would alert the user that it had been used before.²

Nevertheless, concerns about seed quality remain. As KEPHIS Managing Director Esther Kimani said, tying this issue to food security: "These fake seed sellers have...been the cause of food shortages that make Kenya spend billions of shillings on imports annually." Counterfeits may not be the only possible cause of low quality seeds – other reasons may include poor storage or selling seeds after expiration or too long after testing to ensure high quality. This is consistent with academic research on maize seed performance after storage under less-than-ideal conditions ([Ghassemi-Golezani and Mamnabi, 2019](#)). Although seed testing for each seed lot that is manufactured is subject to testing requirements at KEPHIS, there are several points within the supply chain between the seed manufacturer and the small retail outlets that we study, which allows for possible lower-quality seed packets to enter the supply chain. First, the seed manufacturer relies on a network of agents and sub-agents to distribute the seeds, ending with the smallest retail shops and the farmers they sell to. Intermediaries and retail shops may store seed packets improperly, sell expired seed, or source seeds from unauthorized distributors, allowing for counterfeits or other non-quality-verified products to enter the supply chain.

Our study area is in Western Kenya in four counties - Bungoma, Busia, Kakamega and Transnzoia Counties. According to the 2014 Kenya DHS survey, 47% of households in the region experience food insecurity. In 2019, 74.5% of households in this area participated in farming activities, 92.4% of which farmed maize ([Kenya National Bureau of Statistics, 2019b](#)).

1.3 A Simple Model With Informed and Uninformed Consumers

To structure our thinking about consumers and seller behavior, we consider a simple one-period model. In this model sellers set price and quality levels in the presence of both informed and uninformed consumers who also have the option of purchasing from a seller outside the local market.

²We thank staff at KEPHIS and mPedigree for helpful conversations about recent changes in seed regulations.

Demand side

Assume there are N consumers, all of whom seek exactly 1 unit of the good. The good comes in one of two quality levels: high or low. A consumer derives utility $1 - p$ if they obtain a high quality good, and $-p$ if they obtain low quality. Let θ be the proportion of consumers who are informed. We treat θ as exogenous; one could think of the share of informed consumers as coming from a person-specific cost of acquiring information, which leads some to acquire this information and others to not acquire it.

Assume there is either 0 or 1 firm locally.³ Consumers can buy from the local firm (if there is one), or alternatively, they can buy from a firm outside the local market, paying search cost S and price P_0 . Assume that the local market is small enough that P_0 would be negligibly affected by choices of local market participants. An informed consumer, upon taking the outside option, will always get high quality. An uninformed consumer believes they can get high quality with probability $\bar{q}$ in the outside option.

Informed consumers can observe quality markers and upon visiting the local shop will know the quality of the product (either high or low) with certainty. Uninformed consumers, on the other hand, cannot observe quality. They have beliefs about the average quality in the local market ($\hat{q}$). Note that we allow $\hat{q}$ to match or not match the average quality offered by the seller.⁴

We focus on the equilibrium in which the uninformed buy locally, and the informed buy locally *only* if they receive a high quality good. Since the informed expect to get high quality from the outside option at cost $P_0 + S$, and the uninformed expect to get quality $\bar{q}$ in expectation at cost $P_0 + S$, so the following must be true in this equilibrium:

$$\begin{aligned} 1 - p &\geq 1 - P_0 - S &\implies p &\leq P_0 + S \\ \hat{q} - p &\geq \bar{q} - P_0 - S &\implies p &\leq \hat{q} - \bar{q} + P_0 + S \end{aligned}$$

We might default to thinking of the second expression as binding, in which buyers believe they get better expected quality outside the local market. Evidence from [Gharib et al. \(2021\)](#) would support this assumption, finding that farmers are willing to pay a premium for a seed packet directly from the seed company, which normally can be obtained outside the local market in town). Either way, in this equilibrium, the seller can raise prices up to $p^* = \min\{P_0 + S, \hat{q} - \bar{q} + P_0 + S\}$ without losing any of the uninformed consumers, or any

³Here we assume only up to one seller, which is clearly a simplification. However, the median market in our sample has only one maize seed seller. Evidence from some recent research has also suggested that sellers in similar rural settings in Kenya may collude and behave similarly to a local monopolist ([Bergquist and Dinerstein, 2020](#))

⁴[Bold et al. \(2017\)](#), [Ashour et al. \(2019\)](#), and [Michelson et al. \(2021\)](#) suggest that buyers of agricultural inputs do not hold accurate beliefs about product quality and that learning about quality can be very difficult. Moreover, as suggested in ([Bold et al., 2017](#)), learning by Bayesian farmers may especially be hindered when the difference in quality between low and high quality is not very large. Thus we think of this as a model of short-run buyer and seller responses when beliefs about average quality do not change.

informed consumers who get high quality. They lose informed customers who are offered low quality, who opt for the outside option. In [section A](#) we discuss the conditions needed to ensure this equilibrium, which we think is the most interesting case.

Supply side

Let x be the number of units that the firm sells. Let $q \in [0, 1]$ be the quality mix chosen by the seller. That is, proportion q of the seller's stock is high quality, while proportion $1 - q$ is low quality. For each potential customer, the seller randomizes the quality of the product according to the choice of q , and they make a take-it-or-leave-it offer.

The seller faces variable costs $(c + d\frac{q}{1-\theta+q\theta})x$, where c is the (assumed constant) per-unit cost for a unit of low-quality good, and d is the cost of upgrading a unit's quality from low quality to high quality. $\frac{q}{1-\theta+q\theta}$ is the average quality among units that are sold if the seller chooses quality mix q , but informed buyers refuse if offered low quality (which happens $q\theta$ of the time).

In addition, assume the firm faces possible punishment from regulators. For simplicity, our baseline model sets expected punishment as: $b\theta(1 - q)^2N$.⁵ One can think of this functional form as being the product of two terms – the first being the probability of detection, and the severity of punishment conditional on detection – where the probability of being detected by authorities is proportional to the number of unhappy informed customers, and the severity of punishment conditional on detection is proportional to how much cheating the firm engages in. Finally let F be the firm's fixed cost.

Thus, the firm's problem is as follows, where we write x as a function of the firm's choice of price p and quality mix q :

$$\text{Max}_{p,q} (p - c - d\frac{q}{1-\theta+q\theta}) * x(p, q) - \frac{1}{2}b\theta(1 - q)^2N - F$$

As shown in [section A](#), when an interior solution exists, optimal quality is:

$$q^* = \frac{(p - c)\theta - d + b\theta}{b\theta} = \frac{(p - c)\theta - d}{b\theta} + 1$$

Quality is increasing in enforcement strength b and decreasing in upgrade cost d . Importantly, quality is increasing in the proportion of buyers who are informed θ .

We examine the effect of more informed buyers on firm profits. From the firm's objective function, by the envelope theorem we get:

$$\frac{\partial \pi^*}{\partial \theta} = -(p - c)(1 - q^*)N - \frac{1}{2}b(1 - q^*)^2N \leq 0$$

So having more informed buyers is bad for profits and may cause sellers to exit the market.

⁵In practice, our main takeaways from the model will not be very dependent on the exact functional form.

Summary of model predictions

1. Buyers that get informed obtain higher quality goods. Being able to observe quality, they are able to search until they find high quality, leaving the local market if necessary to find another seller.
2. Having more informed buyers does not affect market prices ($\frac{\partial p}{\partial \theta} = 0$). Sellers have local market power, and prices are bounded from above by uninformed consumers' value of buying from the outside option.
3. Having more informed consumers has a negative effect on local sellers' expected profit ($\frac{\partial \pi}{\partial \theta} \leq 0$). More informed consumers reduce the local seller's market share and may also increase the expected punishment from regulators.
4. Uninformed consumers may also benefit from having more informed consumers in the local market. In the interior solution they benefit, but less than informed consumers do ($\frac{\partial q}{\partial \theta} \geq 0$). They do not benefit in an edge case. This is because uninformed consumers only benefit to the extent that more informed consumers affect the local seller's choice of quality mix.

1.4 Study Design

Sample

We randomly sampled rural markets that satisfy the following conditions: (1) it must have fewer than 100 shops, (2) it must be more than 2km from a market that has more than 100 shops, (3) it must have at baseline at least one seed seller. We consider markets that satisfy these conditions eligible to be sampled. We sample markets from four counties: Bungoma, Busia, Kakamega, and Transnzoia using a 2-stage sampling strategy—first randomly selecting sublocations (a list of which we obtained from the county commissioner offices), next tabulating all eligible markets in selected sublocations, and lastly randomly selecting one market (or two in randomly selected sublocations in Transnzoia, due to logistical reasons). This sampling strategy helps minimize clusters of markets that are very close to one another (possibly risking spillovers between treatment and control sites), and it helps the sample of markets cover both less remote and more remote areas of each county. The main study sample in treatment and control groups consists of 302 markets in 282 sublocations.

At each market, all seed sellers and eight randomly selected households within 1km from the market center were sampled to be surveyed. The sampled farmers are overwhelmingly small-holder farmers, with the median farmer planting on 1.5 acres.

In addition to markets in the treatment and control groups, 104 pure control markets were also selected using the same methodology, except that we do not observe the number of seed sellers in Feb-Mar 2020. We instead use the number of seed sellers in March 2021. For these sites we collected only data from market audit activities during the 2021 main planting

season. Due to funding limitations, we did not conduct seller surveys or household surveys in pure control markets.

Randomized experiment

Sublocations were randomized into the following treatment groups, and we describe the treatment arms in greater detail below.

<u>Treatment #1</u>	: 68 sublocations
<u>Treatment #1 & Treatment #2</u>	: 68 sublocations
<u>Control</u>	: 146 sublocations
<u>Pure Control</u>	: 104 sublocations

Figure 3.1 shows the sampled markets within the four counties, with treated markets in red.

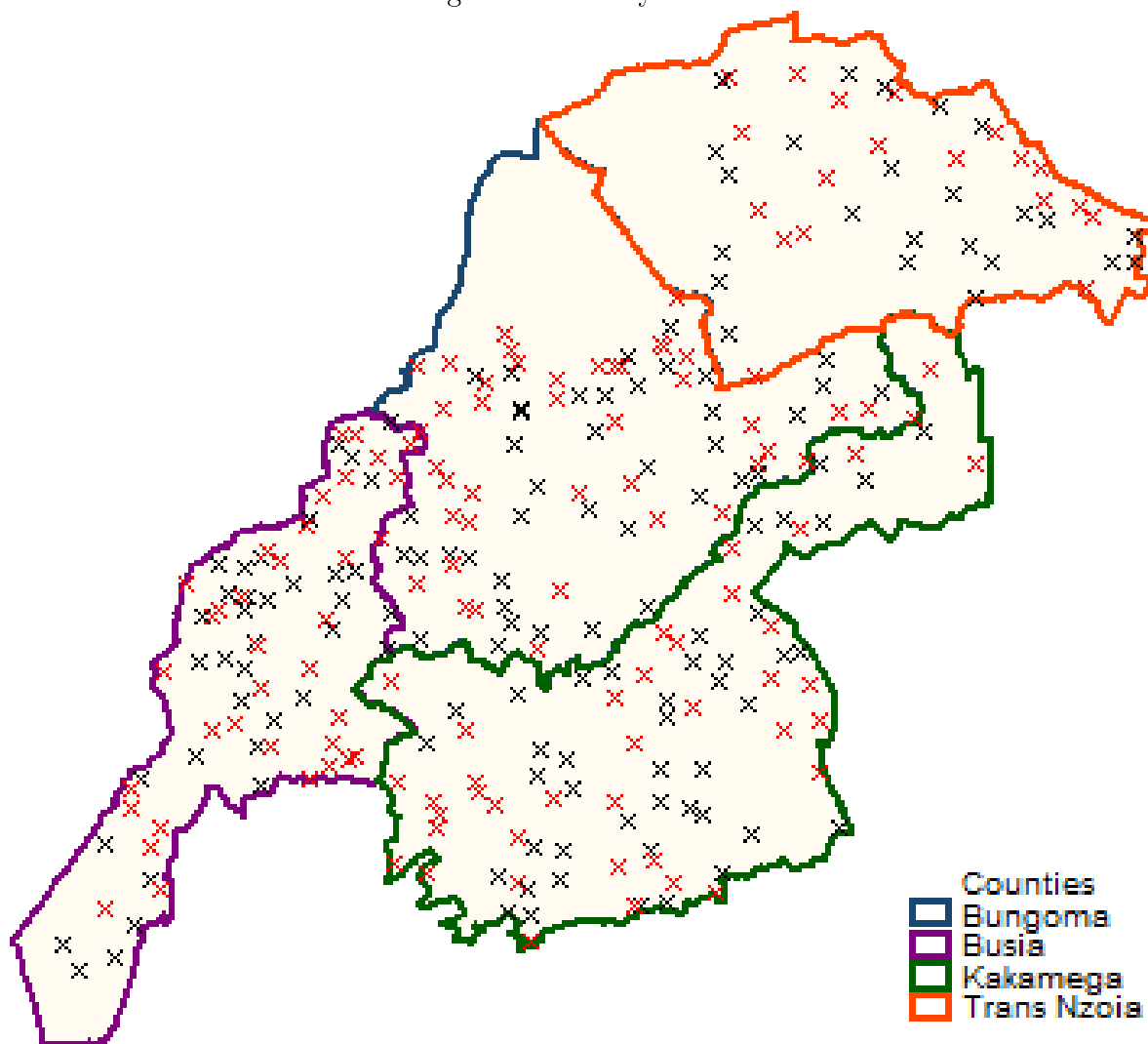
Treatment #1 provides information to help consumers distinguish between low and high-quality products. Through pilot activities and discussion with project partners, we identified a set of quality markers that are required for all certified seeds in Kenya and have variation in rural markets. These include the lot number and a sticker containing an SMS code that allows farmers to verify authenticity (see [section A](#)). In response to widespread reports of deceptive counterfeiting, starting in 2018 the Kenya Plant Health Inspectorate Service (KEPHIS) required all certified packets of seed to carry one of these scratch-off codes. Using SMS verification allows farmers to detect two types of lower-quality seeds: uncertified seeds and old seeds that were unsold from previous seasons.

Treatment #2 encourages consumers to report sub-standard seeds when they encounter them. Consumers were (1) encouraged to ask for a receipt and keep the packaging for documentation, (2) told how to report the incident anonymously to the Anti-Counterfeit Authority, a corporate representative, an agricultural officer, KEPHIS, or the local chief (who were informed by our team how to escalate reports), and (3) encouraged to discuss seed quality among friends, family, and neighbors. They also were told of documented instances where complaints led a company to give compensation or led to legal action.

Both treatments were administered in the month immediately before the main planting season in 2020, which occurs around the start of rains in March. Our team carried out the campaign by working with the local assistant chief to deliver flyers to and speak to locally influential residents at gatherings, including village elder meetings, local farmer group meetings, and barazas and chamas (local community meetings). We also spoke with individual farming households, who were recruited as survey respondents, and after being surveyed were given the information treatment. We sent a team member to deliver information at each site, taking into account gender dynamics in the local context.

All treated sites receive Treatment #1, which was designed to increase the probability of detecting non-quality-verified seeds, corresponding to an increase of θ in the model. As illustrated in the model, this can directly affect purchasing decisions (e.g. refusing a packet or switching sellers) and lead to the adoption of higher quality seeds. As well, treatment

Figure 1.1: Study Area



The study area in Western Kenya includes Bungoma, Busia, Kakamega, and Transnzoia Counties. Red X's represent treatment sites, where community-wide training to help consumers detect quality-verified seed was carried out. Black X's represent control sites. In total 302 market areas are included in the sample.

may affect sellers' decisions (e.g. selling more high-quality seeds or adjusting the price) and lead to a greater prevalence of high-quality seed in the market for even uninformed buyers. This may happen through economic channels as buyers refuse to buy when observing a poor signal (as in Wolinsky 1983). We look to predictions #1 through 4 to guide our thinking on the expected effects the information treatment. A secondary focus of our project is certain channels such as social pressure or threat of legal action that may also reduce the sellers'

payoff to cheating and additionally affect the seller’s decisions about firm entry and quality. This is explored by the addition of Treatment #2 which is layered on top of Treatment #1 for about half the treated markets and which corresponds to increasing b in the model. While this does not affect model predictions #1 and #2, it could affect the seller’s entry/exit decision by affected expected profits, and it could affect the seller’s choice of quality.

[section A](#) shows the flyers which summarize the information that was communicated at treated market areas as well as typical packets of seeds and screenshots from using the e-verification system. (Those who received flyers were encouraged to pass extras along to family, neighbors, and friends.) Focus group discussions prior to project launch as well as treatment meetings suggest that farmers perceived the provided information to be important for their livelihoods and that the information is accessible, particularly with the help of family, friends, and neighbors. We note that one of the quality markers—the KEPHIS sticker—is more technically challenging to use properly. While the application of SMS codes to quality verification may be new to many farmers, we note that the technology is often somewhat familiar from its widespread use for pre-paid mobile phone service, which may explain the ready acceptance of this aspect of the treatment.

We also varied treatment intensity such that two-thirds of treated markets received a more intensive multi-day treatment (two or three days of information dissemination); this allows us to explore differential effects from greater information saturation. We implemented this design out of concern that treatment effects may only be seen for a large enough change in θ . For example, consider the model from [section 1.3](#) in which some markets begin in an edge case with $q = 0$. In such cases, only a large-enough change in θ that causes a threshold to be crossed will induce a change in the quality provided by sellers. In practice, though, the data shows little evidence of such threshold effects from the variation in our sample in treatment intensity, and [section 3.4](#) below primarily focuses on simple treatment-control comparisons in order to maximize statistical power.

Treated market areas received on average 173.5 flyers, which we estimate as being approximately 19.7% of local seed customers (see [Table A7](#)).⁶ On average in treated markets the team trained 81.7 adults face-to-face, or about 9.1% of local seed customers. Considering village elders’ estimates for the local population, about 49% of the local (within 1km) adult population received a flyer, and 22% were treated directly face-to-face.

Data Collection and Timeline

Project activities proceeded as illustrated in [Figure 1.2](#).

In February 2020 into early March 2020 baseline surveying and treatment were carried out. After a site was initially sampled and our team made contact with village elders, baseline household and sellers surveys were carried out. Treatment meetings normally were later in

⁶We use several pieces of data to estimate this figure, including: number of sellers in a market, number of customers per seller, and percentage of local customers who shop locally for seeds.

the day during the first day of surveying, with participants convening in the mid-afternoon in a public location. Market areas that are assigned to receive additional visits for information dissemination would then have a team member return for additional activities on subsequent days.

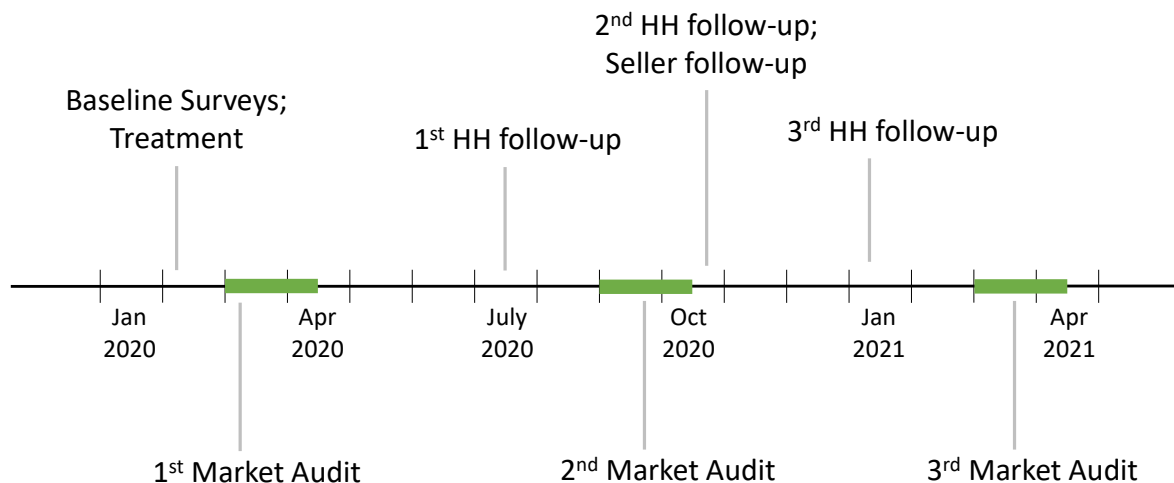
Over the following one year we conducted 3 rounds of household follow-up surveys via phone to document household seed choices and agricultural outcomes. Households self-reported harvested amounts for each maize variety they planted, which we use to compute maize yields for each household. We chose survey times strategically to be spread out around key dates in the usual crop timeline in order to minimize issues with imperfect recall of key variables (e.g. seed purchases and harvested amounts) for both the main season (with planting starting around March 2020) and the short season (with planting starting around September 2020).

To measure seed quality and possible adjustments by sellers, we conducted three rounds of market audit activities. In March 2020, August to September 2020, and March to April 2021, covert shoppers (enumerators on our research team) posed as farmers to visit markets and view up to two packets and purchase up to one packet. They used realistic scripts to purchase "seeds that are popular" or "seeds that are cheap", without naming the exact brand or variety that is desired, leaving the seller to decide which seeds to offer. In the local context, requesting seeds in this way without specifying the exact variety is perceived as normal. When outside of the market area, the secret shopper was instructed to document visible quality markers as well as data about the sellers in the market and the shopping experience. Conversations with field staff lead us to expect that secret shoppers were not recognized or treated differently from other shoppers during the relatively quick transaction.

All seed packets were repackaged into plain paper bags, labeled, and sent to KEPHIS facilities in Nakuru for purity and germination testing, to provide objective measures of two aspects of seed quality. Purity tests measure the percentage of material (by weight) in the packet that are whole seeds. Germination tests measure the percentage of whole seeds that become normally emerging plants under ideal temperature and moisture conditions. While these tests provide an objective test of seed quality, with KEPHIS staff blinded to most seed that are not visible on the seed itself, it does not capture all relevant aspects of seed quality that a farmer may care about. Notably, the lab tests we conducted does not provide information about seed performance when conditions are less-than-ideal. As well, it does not capture aspects of seed quality that affect yields beyond the germination rate. As demonstrated by Ghassemi-Golezani & Mamnabi (2019), it is expected that older or poorly-stored maize seeds will have lower yields due to deterioration of seed quality beyond the effects on germination rate alone.

The team documented the following markers during the market audit: (1) presence of valid SMS verification code, (2) presence of lot number, (3) testing date within 1 year of purchase, (4) damaged packaging (which reports have linked to tampering). A packet can lack a valid SMS verification code several ways. First, a KEPHIS sticker could be absent. Second, the code could be invalid. Third, the code could be valid but already used prior to

Figure 1.2: Timeline



The project activities included: (1) baseline surveys and information treatment conducted starting in February 2020; (2) three market audit activities conducted starting in March 2020, September 2020, and March 2021; (3) a seller follow-up survey to capture prices, quantities, and profits of sellers; (4) 3 rounds of household follow-up surveys, designed to capture key information about seed purchases and agricultural outcomes in both the main growing season and the short growing season.

purchase. Lastly, the code could be successfully applied for the first time, but the registered information may not match the information on the packet. Similarly, a packet could lack a valid expiration date either because it lacks any date whatsoever, or because it has a date but the seeds are expired.

During follow-up surveys, enumerators were not made aware of the treatment status for the sites and respondents that were being followed up on. Lab testers were not made aware

of the seed sources, and all seed samples were repackaged into plain paper bags labeled with unique alpha-numerical identifiers used internally by the research team.

One concern with the survey data may be that measurements on agricultural outcomes are self-reported and could suffer from inaccurate reporting. To guard against these concerns, we used self-reported yields to inform our sample size, anticipating possible noise from mis-measurement.⁷ Acreage, kilograms of maize harvested, and kilograms of seed purchased are also checked through repetitive questions in different parts of the survey and cross-validated with each other. For example, the total amount of land dedicated to maize should be consistent with the sum of land dedicated to each variety of maize. The surveys also phrase questions and enumerators use probing in ways that help respondents recall the answers we are seeking. For instance, we ask "how many 90kg bags of maize did you harvest during the long growing season?" as a question respondents often are able to readily recall.

Baseline Balance, Spillovers, and Other Threats To Identification

Table A8 shows baseline balance for market areas in our sample. Households in treated sublocations had household heads that were slightly less likely to have completed primary or secondary school, and they had slightly smaller plots. However, when we jointly test for differences between treatment and control on all of these outcomes, we cannot reject the null hypothesis that all characteristics are the same in both treatment and control groups ($p=0.22$). All results below showing treatment effects will control for these baseline characteristics.

In the section A, we explore the possibility that the experimental design may have been contaminated in one of two ways. First, did baseline activities influence sellers by altering their beliefs about the likelihood their products will be scrutinized? Second, did information spillover from treated sites into neighboring market areas and affect buyer and seller knowledge and behavior there? We do not find evidence consistent with these two hypotheses and so in section 3.4 below we primarily focus on comparisons between treatment and control sites.

1.5 Results

In this section we present the main results as follows. In section 1.5, we confirm that observable markers correlate with lab-tested germination rates. This suggests that farmers can indeed use simple purchasing strategies based on observables to help them acquire better quality and in theory achieve sizeable agricultural gains. In section 1.5, we describe treatment effects on buyer knowledge and usage of quality markers, and seed purchases. The findings suggest that informed farmers were able to adjust their purchasing behavior, consistent with the channels in the model. In section 1.5 we examine agricultural outcomes. We find gains in

⁷We used survey data from the Tegemeo Institute.

yield among more remote farmers, where quality markers were associated with greater gains in seed quality. This finding corresponds to model prediction #1. The higher gains obtained by more remote farmers can be interpreted through the lens of the model, where lower initial knowledge, lower enforcement, and lower initial quality (all features of more remote market areas) lead to higher treatment effects. We also show that prices paid by locally-buying farmers are not affected, corresponding to model prediction #2. In [section 1.5](#), we examine effects on sellers' decision to enter or exit the hybrid seeds market. We document a sizeable effect of treatment on seller exit, consistent with the negative effects of informed buyers on profit from model prediction #3. Lastly in [section 1.5](#), we examine effects of information on prices and quality for uninformed buyers in the market. We do not find effects on seed quality for the uninformed, contrary to model prediction #4. We discuss mechanisms within the model that would make the treatment effect on quality for the uninformed to be too small to detect. As well, we explore the consequences if sellers are able to discriminate between informed and uninformed buyers, deviating from the model assumption that sellers make a take-it-or-leave-it offer to informed buyers. This results in even smaller effects on uninformed buyers, or no effects at all in the case where sellers can perfectly discriminate (we describe the consequences in the model in [section A](#)).

Summary statistics: quality markers and their correlates

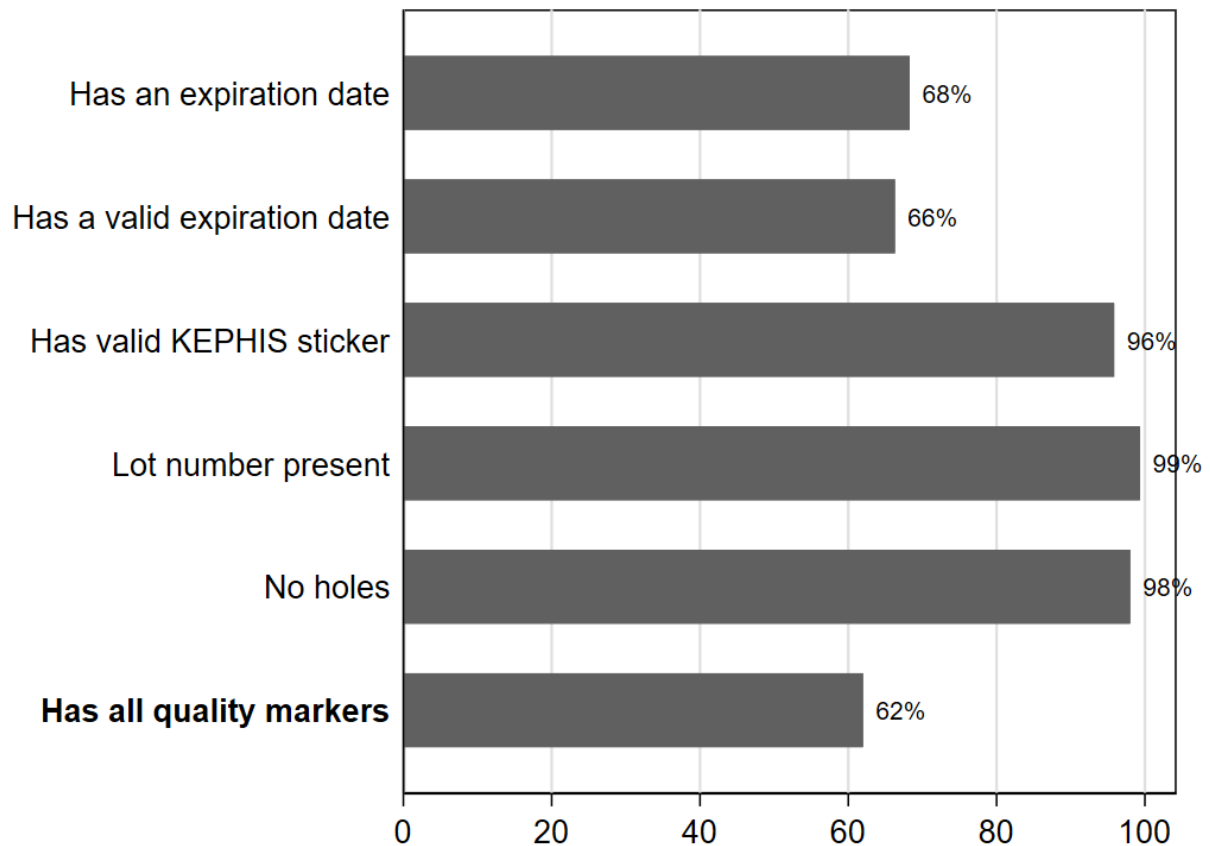
We first describe summary statistics for the data on quality markers that was collected. These descriptive statistics provide new insight into the prevalence of various observable quality markers and their correlates. Positive correlation between observable markers and objective lab-tested quality measures suggest that due to KEPHIS regulations that were implemented with some success, farmers stand to benefit if they use these markers to decide which seed packets to purchase.

[Figure 1.3](#) presents summary statistics for each quality marker. Overall, 62% of packets that our team observed had all quality markers, while 38% were confirmed to be missing one or more. Most of the missing quality markers were due to not having an expiration date, or not having a valid expiration date on the packet, though missing or invalid KEPHIS stickers and the other observable markers also contributed somewhat to missing quality markers.

We next ask if observable quality markers correlate with aspects of seed quality from lab tests. [Table 1.1](#) shows that observable quality markers are strongly correlated with germination rate but not purity. The relationship is driven primarily by two of the quality markers: (1) presence of a KEPHIS sticker, and (2) presence of a valid expiration date. Other aspects of seed quality are likely positively correlated with germination rate ([Ghassemi-Golezani and Mamnabi, 2019](#)), and so we view this as a lower bound for the yield increase that is possible if non-quality-certified seeds could be eliminated from the marketplace.

We view the observed 4.7% overall difference in lab tested germination rates as a lower bound for the changes in yield that could be achieved should farmers exert perfect discretion in choosing seed. It does not account for other characteristics of seeds that affect yield, which

Figure 1.3: Quality marker frequency



This figure shows the percentage of seed packets that feature each quality marker, calculated using data from market audit surveys which were submitted by secret shoppers on our research team. The sample includes 1495 seed packets. This includes observations from all three planting seasons in which market audit activities were conducted (long rains 2020, short rains 2020, and long rains 2021). It also includes both packets that were purchased as well as packets that were closely observed but not purchased. In the latter case, enumerators were not able to scratch the KEPHIS sticker and fully complete the e-verification steps. We categorize those packets as having a valid KEPHIS sticker so long as all other quality markers are present, and the sticker was present with all visible features of the sticker appearing to be valid. Overall, we find that 62% of packets had all quality markers present.

is expected to correlated positively with germination rate ([Ghassemi-Golezani and Mamnabi, 2019](#)) and whose effects on yield may far exceed the differences in germination rate. As well, this obviously would not account for any possible general equilibrium effects as the market adjusts. In light of findings from ([Bold et al., 2017](#)), this relatively small magnitude

for differences in germination rate (which is more easily observable to farmers than other seed characteristics), may help explain the persistence of baseline market equilibrium – if low-quality seeds were even more inferior, farmers would be able to learn more quickly and possibly unravel the pooling equilibrium that we observe where both lower and higher quality seeds are sold by the same sellers at the same price.

Table 1.1: Quality markers, seed purity, and germination rate

	Purity	Germination Rate
Has all quality markers	0.01 (0.02)	5.21*** (1.14)
Constant	99.84*** (0.01)	88.08*** (0.96)
Observations	467	467

The sample includes 968 seed packets that were purchased by secret shoppers and tested in a lab by KEPHIS staff. The sample includes packets purchased during the long growing season in 2020, the short growing season in 2020, and the long growing season in 2021. The dependent variable in column 1 is the estimated percentage of material that are whole maize seeds (in percentage points). The dependent variable in column 2 is percentage of seeds that germinated in the lab (in percentage points). [Table A9](#) and [Table A10](#) show similar results for the complete list of quality markers.

The relationship between quality markers and lab-tested germination rate differs in more remote versus less remote markets. [Table 1.2](#) shows this relationship for market areas that are farther than the median distance from the county capital, as opposed to markets that are closer than the median distance. More remote market areas have lower germination rates overall, showing the seed quality on average is farther from national standards. As well the quality markers are more informative in the sense that having all quality markers is associated with a greater gain in germination rate in more remote markets than in less remote markets.

These correlations between observable markers and lab-tested germination rates do not capture all relevant aspects of seed quality. However, we expect germination rates to correlate with yields, and the tests provide high-quality objective evidence that farmers – particularly in more remote areas – stand to gain in agricultural output if they are able to successfully use these observables to choose higher quality seeds. Lab tests are not subject to self-reporting biases and testing staff are blinded to the seed source, variety, and any characteristics of the packaging. This also reflects a sort of limited success in the KEPHIS certification process – implementation failures and other mistakes, or widespread counterfeited marks have not prevented observables from being unrelated to seed quality.

Table 1.2: Remote markets have more to gain

	Germination Rate
Has All Quality Markers * Not Remote	3.30** (1.48)
Has All Quality Markers * Remote	7.50*** (1.77)
Remote	-5.50*** (1.93)
Constant	90.47*** (1.27)
Observations	467

The sample includes 467 seed packets that were purchased by secret shoppers and tested in a lab by KEPHIS staff, and were collected from control markets. The dependent variable is the percentage of maize seeds that germinated in the lab (in percentage points). Remote markets are defined as markets with above-median distance to the county capital. "Has all QMs" equals 1 if a seed packet has all quality markers, and equals 0 otherwise.

Effects on household knowledge and seed purchases

In the following results, we will focus on overall treatment effects, as well as effects on less-remote and more-remote areas, with special emphasis on more-remote areas where baseline seed quality was worse while quality markers are associated with greater quality gains. Households in more remote areas appear similar socioeconomically (e.g. education levels, housing quality, plot size), though we note that more remote farmers tend to have higher yields and lower population density. We also expect more remote areas to be less intensely monitored by regulators.

We examine treatment effects on several measures of household knowledge, as shown in Table 1.3. Treatment substantially increased knowledge about visible quality markers, being associated with increased recall of specific elements by 35% to 120%. We note that household-level effects appear to be heterogeneous. For households with more highly educated household heads with primary school or secondary schooling completed, knowledge (by all measures) both started at a higher level as well as increased substantially more due to treatment. Generally we see that households in more remote areas start with lower knowledge and generally have slightly higher gains in knowledge, though not statistically significantly so. Knowledge levels and treatment effects among treated households are similar in more and less remote areas.

We also examine the possibility that households at control sites near treated sites may have experienced gains in knowledge due to spillovers. However, the data suggests that any

spillover effects that may have occurred are too small to be detectable. As shown in [section A](#), we do not find evidence of spillover effects within 2km, 4km, or 6km. Partly by design, due to our multi-stage sampling strategy, the sample does not have many markets that are clustered very closely together and so we do not have statistical power to detect spillovers across very short distances. At the same time, few markets are at closer distances where we might be concerned that spillovers could greatly affect estimates of treatment effects. In any case, one could think of the following treatment effects as being lower bounds, to the extent that undetected spillovers affect nearby control sites similarly but to a smaller degree than the treatment sites.

A priori, one concern that could limit the effectiveness of treatment is that many farmers could already possess the information we seek to disseminate. These results show this is not the case and confirm that treatment was able to increase knowledge of quality markers, from a relatively low baseline. While discouraging from the point of view of KEPHIS, it is encouraging for our research design that we find low awareness for the SMS-based verification system, suggesting that the intervention was well-timed to publicize it. As our contacts at KEPHIS explained, they have yet to run a major campaign to inform buyers of the verification system, while the stickers themselves say little about their purpose (see [section A](#)). As shown in [Table 1.4](#), households had little baseline knowledge about quality markers, on average naming only 0.55 markers, and with less than 7% of farmers citing the seeds' expiration date.

Given the timing of data collection, we think it is likely that the increases in knowledge represent lower bounds on the true increase in knowledge due to treatment that is relevant at the time of seed purchases. These data were collected in July 2020 at the earliest, and respondents did not know in advance when they would be called by the research team. While originally planned for immediately after the planting season ended in late April, field operations were disrupted by the onset of the COVID-19 pandemic, and so our first post-treatment measurements of knowledge come from a phone survey with the main respondent approximately five months after treatment.

After the planting season, we asked treated households whether the treatment affected their seed purchase decisions. Respondents were asked the following questions: (1) "Did the information we provided about quality markers help?", (2) "Did it influence your decision of where to buy seeds? Please explain", (3) "Did it influence your decision of which seeds to buy? Please explain". [Figure 1.4](#) summarizes their responses, in which 44% of respondents said that the information affected what seeds they purchased or where they purchased them.

We examine in [Table 1.4](#) whether treatment affected whether and where households bought hybrid maize seed. Columns 1 and 2 show that households shifted their source for seeds, with 5.8% fewer households (though this is 17.7% of households that are expected to purchase locally) sourcing seeds from the local market, and a similar amount (19.2% of the control mean) obtaining seeds elsewhere. Consistent with these findings, evidence from [Gharib et al. \(2021\)](#) suggests that farmers have higher willingness to pay for seed packets

Table 1.3: Household knowledge and usage of markers

Panel A: Household Knowledge			
	(1)	(2)	(3)
	no. quality marks known	e-verif known	expiration known
treated	0.207*** (0.035)	0.104*** (0.011)	0.025*** (0.008)
Observations	4645	4645	4645
Control Mean	0.56	0.13	0.07
Treatment Effect (%)	37.11	79.95	37.13

Panel B: Usage of markers		
	(1)	(2)
	Able to verify	Used e-verif successfully
treated	0.070*** (0.011)	0.075*** (0.009)
Observations	4645	4645
Control Mean	0.14	0.07
Treatment Effect (%)	51.49	106.68

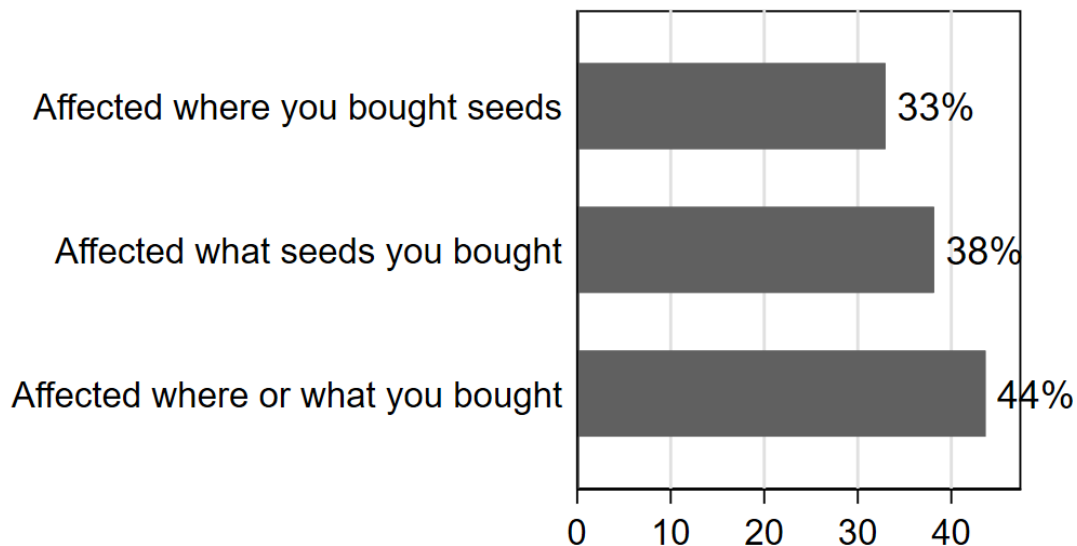
The table shows the effects of information treatment on household knowledge and household usage of markers. The sample includes observations from both the main planting season in 2020 and the short season in 2020, with knowledge measured approximately in late July 2020 and in November 2020. Standard errors are clustered by sublocation. In Panel A, the dependent variables are (1) the number of quality markers named by the respondent, (2) whether e-verification was named as a way to verify seed quality (=1 if yes), (3) whether expiration date was named (=1 if yes), and (4) whether e-verification was reported to be understood at least somewhat well (=1 if yes). In Panel B, the dependent variables are (1) whether the respondent was able to verify that seeds from that season were of high quality, and (2) whether e-verification was used successfully.

that have certain quality markers present.⁸ These results are consistent with the mechanisms in the model, in which informed buyers that cannot obtain high quality seeds locally find it worthwhile to continue their search by buying outside the local market.

Overall, however, households were not more likely to buy hybrid seeds. It is possible that even if farmers updated expectations about the quality of seeds that they can expect to get (when using the quality markers), the shift is not near the threshold of pushing expected economic returns to be positive. Another consideration is that additional market imperfections barriers (such as credit constraints or incomplete insurance markets) may be important in limiting movement on the adoption margin (Karlan et al., 2014).

⁸Gharib et al. (2021) estimates the effect of training (on two of our seven quality markers) on willingness-to-pay for maize seed packets.

Figure 1.4: Do treated households think the information helped?



The sample includes 742 respondents in treated market areas. These respondents were asked the following questions: (1) "Did the information we provided about quality markers help?", (2) "Did it influence your decision of where to buy seeds? Please explain", (3) "Did it influence your decision of which seeds to buy? Please explain". 44% of respondents indicated that the information that was provided affected either where or what seeds were purchased.

Effects on household agricultural outcomes

We next examine how the information campaign affected household agricultural outcomes. Germination rate, harvest amount, and yield (kilograms harvested per acre) are the main outcomes we will examine in this section. While inherently noisy and affected by other farmer inputs, these agricultural outcomes also capture elements of seed quality that are not captured in lab tests.

Table 1.5 shows that treatment caused a noisy increase in yields among households, with the more substantial gains among households in smaller and more remote markets. Effects on self-reported and lab-measured germination rates are weakly positive at best and are

Table 1.4: Household Seed Choice

Panel A: Full sample			
	(1)	(2)	(3)
	Bought hybrid at local market	Bought hybrid elsewhere	Bought hybrid
treated	-0.058*** (0.019)	0.053*** (0.018)	-0.001 (0.013)
Observations	3717	3771	3771
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	0.33	0.28	0.61
Treatment Effect (%)	-17.74	19.27	-0.12
Panel B: Less remote			
	(1)	(2)	(3)
	Bought hybrid at local market	Bought hybrid elsewhere	Bought hybrid
treated	-0.059** (0.029)	0.043* (0.026)	-0.015 (0.019)
Observations	1852	1876	1876
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	0.35	0.29	0.64
Treatment Effect (%)	-16.85	14.94	-2.33
Panel C: More remote			
	(1)	(2)	(3)
	Bought hybrid at local market	Bought hybrid elsewhere	Bought hybrid
treated	-0.051** (0.023)	0.054** (0.023)	0.013 (0.017)
Observations	1865	1895	1895
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	0.29	0.26	0.56
Treatment Effect (%)	-17.47	20.96	2.33

This table shows treatment effects on household seed purchase decisions. Results in Panel A include households in both the main season in 2020 and the short season in 2020. All standard errors are clustered by sublocation. The dependent variable in column 1 equals 1 if the respondent bought hybrid seeds at the local market in that planting season (and 0 otherwise). The dependent variable in column 2 equals 1 if the respondent bought hybrid seeds from a location other than the local market in that planting season (and 0 otherwise). The dependent variable in column 3 equals 1 if the respondent bought hybrid seeds from any source in that planting season. In panels B and C, we split the sample by the median distance of a market to the county capital.

insufficient to explain measured increases in yields. There are a few possibilities to explain this despite positive effects on yields. Expired seeds do not germinate as well, while non-hybrid seed may germinate as well as hybrid seed but will have less yield. We cannot rule out the presence of both of these types of sub-standard seed. Research shows that other aspects of poor quality seed can affect yields in ways other than those mediated by germination rates (Ghassemi-Golezani and Mamnabi, 2019). Of course, it is also possible that self-reported germination rates are too noisy to detect the relatively modest differences that we expect between high and low quality, being a fact from months before harvest and being possibly a less salient fact for farmers to remember.

To check the plausibility of these yield gains, let's assume that 38% of packets do not have a quality marker (average in control group for secret shoppers), and remote markets see a 7.7% increase in yield. Assuming the households follow the strategy from the model in section 1.3, then this implies that informed buyers that got a different quality level than in the counterfactual got a 20.3% gain in yield, while the remaining buyers would have gotten high quality even in the counterfactual and had a gain of 0%.

For reference figures to help us judge the plausibility of these gains we can look toward related research. Fabregas et al. (2019) do a meta-analysis of 7 digital agricultural extension interventions and find an average of 4% increase in output. Bold et al. (2017) find a 13-18% increase in yield when switching from retail to wholesale quality seeds; they find a 28-38% increase in yield when switching from local to wholesale quality seeds.⁹ Ghassemi-Golezani and Mamnabi (2019) used artificial aging to lower seed quality seeds that had 2% or 7% lower germination. Compared to these comparison groups, the higher quality seeds had 23.5% or 64.5% higher yield. Thus, our results compare favorably to other agricultural informational interventions, and the magnitudes are comparable with what might be expected (given evidence from poor quality seeds in Uganda, or evidence on the performance of old seeds from controlled trials).

We do not see any effects on the prices that households paid (Table A27), nor do we see evidence of greater price dispersion among treated households (Table A28). This suggests that the estimated agricultural gains led to welfare gains for the households.

Gains in remote areas are driven by female-headed households (Table A15). This suggests that resolving information frictions may help close the gender gap in agricultural productivity in LMICs (Diirro et al 2018; Wambua et al 2018). This finding also mirrors a result from Annan et al (2021) in which female customers in Ghana benefit more than male customers from an information treatment that helps them avoid being defrauded by mobile money agents.

For completeness we show results splitting the sample by remoteness as well as the other three pre-specified subsets of farmers to examine heterogeneous effects. As discussed, we see larger gains (and significantly so) for more remote farmers (Table A18), and we see somewhat larger gains (though not statistically significantly so) for female-headed households

⁹Even larger increases were seen when fertilizer quality was also increased.

(Table A19) and lower education households (Table A20). Table A21 Together with the evidence we have seen from more remote farmers as compared to less remote farmers, this is suggestive of a pattern in which larger gains are experienced by sub-groups with lower seed quality, since female-headed households and lower-education households self-report lower germination rates and have less knowledge about quality markers (see Table A16). These sub-groups also tend to be less likely to use hybrid seeds.

One caveat to our results is that we cannot rule out changes in other complementary inputs (e.g. labor input, quality of fertilizer and pesticide, etc). We are able however to check whether the information treatment affected usage of fertilizer, and we do not see effects (see Table A22)

Effects on seller entry/exit

We next examine the effect of information treatment on seller entry and exit. The dependent variable in this analysis is the number of sellers present at the market.¹⁰ As shown in Table 1.6, the number of sellers per market decreased by about 0.27 seller in the SGS2020 and LGS2021 seasons due to treatment. This is about 17% percent decrease in the number of sellers. Panels B and C show that these effects are driven especially by less-remote markets with decrease of 0.42 sellers (24% decrease).

The pattern of sellers exiting due to treatment is consistent with model prediction #3, where informed buyers cause sellers to expect increased costs, fewer customers, and greater risk of punishment, all driving down expected profits. The pattern of sellers exiting due to treatment is also confirmed in our separate dataset of recruited sellers, from whom we collected baseline and endline data. We speculate that the pattern of greater treatment effects on seller exit in less-remote markets may be due to their having better alternative business opportunities. We observe that sellers in less remote markets tend to offer a greater variety of agricultural inputs, being both more likely to sell other types of vegetable seed as well as being more likely to sell other agricultural inputs.

Other possible stories do not appear to find support in the data. For example, based on our household surveys of local residents (see Table 1.4), sellers in less remote markets do not appear to have lost more market share due to treatment when compared to less remote areas. As well, we don't see evidence consistent with lower profit margin per unit in less remote areas (no differences in effects on prices, and no differences in measured profit-per-unit among

¹⁰Due to logistical difficulties during the main planting season in 2020, we did not collect the total number of sellers present at markets if there were more than 2 seed sellers, and we did not visit all markets. Due to the limit time in March 2020 available before field operations were forced to stop, the field team visited a subset of about 75% of markets in the sample, with this subset driven by logistical considerations and time constraints rather than treatment status or other market characteristics. For the 2020 main planting season, we know for this subset of markets how many sellers were present, or if there are more than 2 sellers, then how many are present among up to two sellers that were randomly selected during baseline activities.

stayers.) While firms with lower profits were somewhat more likely to exit (Table A24), we do not see differences in baseline profit between more and less remote sellers.

Effects on seed quality and prices for uninformed buyers

The data show no detectable effects on visible quality markers or on purity and germination rates of sampled seeds obtained by covert shoppers working for the research team.

As Table 1.7 shows, overall and in more remote markets, we see a 0-2% increase in packets with all quality markers present. However, these estimates are not statistically significantly different from zero and are far too small to credibly be a driving reason for the increases in yields of the magnitudes that we see. We do not detect effects on purity or germination rate from lab tests.

We interpret this finding to mean that uninformed buyers in treated markets did not obtain higher quality. One reason effects on the uninformed would be even smaller would be if sellers can discriminate between informed and uninformed buyers. In the extreme case in which sellers can perfectly discriminate between informed and uninformed buyers, the uninformed never benefit at all. We formalize this case in section A.

We also do not see evidence that treatment affected prices for uninformed buyers as revealed by the secret shopper data (Table A29).

1.6 Discussion

Widespread and effective information campaigns can be difficult and costly to implement. While we pursue an in-person community approach to disseminating information, this design was informed at least partly by research needs – limiting spillovers, simplicity, highly targeted exposure to information, bundling treatment with data collection activities to save on costs. Using plausible assumptions to estimate the cost of only providing the information campaign (with no data collection costs), we estimate that 22,000 USD was spent to train 12255 residents via face-to-face conversations. This is equivalent to 1.77 USD per person. Valuing harvested maize at 30 USD per 90 kilograms, the benefit to more remote farmers is about 34:87 USD.

Of course, we take such cost-effectiveness figures with a grain of salt. Point estimates come with confidence intervals, and extrapolating will depend on being able to effectively target, given the heterogeneous effects in our study sample. One may think of the intervention as lowering the cost of acquiring information. We would expect that other related information interventions may have similar effects and may scale well. For example, reforming the messaging in existing e-verification system to improve accessibility to the information, updating standards for product labeling including standardizing formatting for expiration dates, and a mass-media campaign via newspaper or radio may cost-effectively disseminate information to consumers.

1.7 Conclusion

In this paper, we study empirically the effects of reducing information frictions in the hybrid maize seed market in rural Kenya. We evaluate a randomized market-level information campaign to examine quantify effects for both informed and uninformed buyers in treated market areas. We monitored prices and seed quality that sellers offer for over one year to allow for sellers to respond to the increase in the number of informed buyers.

We show first that farmers stand to gain from receiving information about observable quality markers. The information campaign affected farmers' purchasing decisions and led to gains in maize yields, particularly in more remote areas where baseline seed quality is lowest and the observable quality markers are associated with the greatest gain in seed quality. Second, while improved information caused sellers to exit the market, we do not observe effects on prices or quality among the stayers. This is consistent with a model in which sellers are able to discriminate between informed and uninformed buyers. Patterns in the data for other outcomes also tell a story consistent with a simple model in which treatment induces households to adjust purchasing behavior, and dampen expected seller profit, causing sellers to exit.

Our findings suggest that that policies that help improve consumer information may be beneficial to supporting the productivity of small-holder farmers. Reforming existing requirements for information displayed on seed packets for consumers to promote greater understanding about seed certification may be helpful. Yet, the absence of benefits in this study for uninformed consumers points to remaining challenges to overcome the difficult learning and reputation-building environment and their impacts on the market equilibrium.

Table 1.5: Household agricultural outcomes (All seasons)

Panel A: Full sample			
	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	-0.22 (0.72)	32.86 (39.96)	24.55 (30.21)
Observations	2274	2328	2111
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	85.01	850.07	890.77
Treatment Effect (%)	-0.26	3.87	2.76

Panel B: Less remote markets			
	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	-0.95 (1.08)	-40.84 (56.17)	-41.36 (46.33)
Observations	1182	1241	1110
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	85.10	745.45	866.14
Treatment Effect (%)	-1.11	-5.48	-4.78

Panel C: More remote markets			
	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	-0.13 (0.96)	104.61* (61.21)	77.79* (42.52)
Observations	1092	1087	1001
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	84.88	1013.02	928.37
Treatment Effect (%)	-0.15	10.33	8.38

This table shows treatment effects on household agricultural outcomes. Results in Panel A include households in both the main season in 2020 and the short season in 2020. All standard errors are clustered by sublocation. The dependent variable in column 1 is the self-reported germination rate of seeds planted in that season (in percentage points). The dependent variable in column 2 is the number of kilograms harvested in that planting season. The dependent variable in column 3 is the number of kilograms per acre achieved in that planting season. In panels B and C, we split the sample by the median distance of a market to the county capital.

Table 1.6: Seller Entry/Exit

Panel A: Full sample	
	Number of sellers at market
treated	-0.273** (0.133)
Observations	495
County FE	Yes
Control Mean	1.58
Treatment Effect (%)	-17.26
Panel B: Less Remote	
	Number of sellers at market
treated	-0.417** (0.186)
Observations	256
County FE	Yes
Control Mean	1.71
Treatment Effect (%)	-24.38
Panel C: More Remote	
	Number of sellers at market
treated	-0.114 (0.193)
Observations	239
County FE	Yes
Control Mean	1.39
Treatment Effect (%)	-8.18

This table shows treatment effects on seller entry and exit in the local market. The sample in Panel A includes markets in the main season in 2020 and the short season in 2020. All standard errors are clustered by sublocation. The dependent variable equals the number of sellers present at that market, as reported by secret shoppers. In panels B and C, we split the sample by the median distance of a market to the county capital market.

Table 1.7: Quality Markers (all seasons)

Panel A: Full sample			
	(1)	(2)	(3)
	Has all quality markers	purity	germ_rate
treated	0.012 (0.028)	-0.277 (0.274)	-0.539 (0.753)
Observations	1212	878	878
County FE	Yes	Yes	Yes
Control Mean	0.57	99.85	91.84
Treatment Effect (%)	2.16	-0.28	-0.59

Panel B: Less Remote			
	(1)	(2)	(3)
	Has all quality markers	purity	germ_rate
treated	0.005 (0.041)	-0.007 (0.017)	-1.325 (0.853)
Observations	654	462	462
County FE	Yes	Yes	Yes
Control Mean	0.59	99.85	92.98
Treatment Effect (%)	0.82	-0.01	-1.42

Panel C: More Remote			
	(1)	(2)	(3)
	Has all quality markers	purity	germ_rate
treated	0.038 (0.039)	-0.548 (0.553)	1.033 (1.405)
Observations	558	416	416
County FE	Yes	Yes	Yes
Control Mean	0.53	99.84	90.03
Treatment Effect (%)	7.16	-0.55	1.15

This table shows treatment effects on seed quality offered to secret shoppers posing as uninformed buyers. The sample in Panel A includes markets in the main season in 2020, the short season in 2020, and the main season in 2021. All standard errors are clustered by sublocation. The dependent variable in column 1 equals 1 if the seed packet has all quality markers present. The dependent variable in column 2 is the estimated percentage of material that are whole maize seeds (in percentage points). The dependent variable in column 3 is percentage of seeds that germinated in the lab (in percentage points). the number of sellers present at that market, as reported by secret shoppers. In panels B and C, we split the sample by the median distance of a market to the county capital. In panels B and C, we split the sample by the median distance between a market and the county capital.

1.8 Transition from Chapter 1 to Chapter 2

This first chapter has focused on the hybrid maize seed market in rural Kenya. In this setting and many other rural settings in low and middle income countries, agricultural inputs in general are largely distributed by small private retail shops, with some government regulations. The decentralized nature of the market and presence of many suppliers makes it interesting to—as we have in Chapter 1—study how consumer knowledge may affect their choices in the market as well as the choices of the many suppliers.

In contrast, electricity services are often characterized by a highly concentrated market. In Kenya, electricity from the national grid is distributed by the state-controlled utility, Kenya Lighting and Power Company (KPLC). Unlike in Chapter 1, end users have few choices. Chapter 2 of this dissertation will turn to the electricity sector. The centralized provision of services with strong government influence provides an interesting contrasting setting in which the focus will be on regulation and oversight of the main service provider.

Chapter 2

What Does Donor Conditionality Do? Causal Evidence From Kenyan Electrification

2.1 Introduction

“You are not allowed to think, but you are told: do what we want you to do. If I make a suggestion, which I know is good for my people, and they say no, you must follow this... The donors, it is a pity that they tell us: do this, you must accept it.”

— *Daniel Arap Moi, then President of Kenya, 1992*

“Traditionally, the World Bank has viewed conditionality as a blueprint package of policy conditions attached to tranche release of single loans. We propose a broader view of conditionality as an evolving process in support of a policy compact based on mutual commitment.”

— *World Bank Operations Evaluation Department, 2000*

Between 2009-2019, multilateral agencies provided on average USD 32.9 billion per year in loans to low- and middle-income countries (LMICs), of which on average USD 8.4 billion went to LMICs in Sub-Saharan Africa.¹ Agencies often seek to improve accountability by imposing stringent conditions on the use of this financing. The question of the effectiveness of conditionality has been widely debated by governments, multilateral agencies, and academics

¹Net loans are calculated as total loan disbursements minus principal and interest payments. This calculation includes some country-year observations for which net financing is negative. Multilateral agencies include the World Bank, regional development banks, and other multilateral and intergovernmental agencies.

ever since its first widespread usage in the 1980s. Proponents argue it can lead to structural transformation ([Archibong et al. \(2021\)](#)) and reduce corruption ([WB 2016](#)). Others question its effectiveness, criticizing for example the lack of recipient countries' independent decision making and the widespread scope for political interference.² In FY2020/21, the Government of Kenya spent 5% of gross national revenue on principal and interest payments on multilateral and bilateral debt obligations.

Despite its importance, this debate has suffered from a dearth of causal evidence ([Easterly \(2019\)](#)). The infrequent and endogenous allocation of multilateral organizations' tranches of financing to specific countries or projects leaves little room for randomization or other exogenous sources of causal identification. In addition, much existing research focuses on policy conditionality, whereas the form of conditionality has evolved substantially since the 1980s. Today, most multilateral organizations use exclusively procedural conditionality.

This paper exploits natural policy variation to generate some of the first causally identified evidence on the benefits and costs of procedural donor conditionality and its mechanisms. We do so in the context of the USD 600 million nationwide Last Mile Connectivity Project (LMCP), one of Kenya's largest public infrastructure construction projects. It was launched in 2015 by the Government of Kenya and Kenya Power (Kenya's electric utility) with the aim of connecting all Kenyan households to electricity by 2020. The government and Kenya Power first jointly selected around 8,520 villages to be LMCP sites. The identification strategy leverages a useful feature of the program: LMCP sites were assigned to be funded either through the World Bank (WB) or the African Development Bank (AfDB) without obvious regard to factors that would impact the project outcomes this paper studies. Yet, key LMCP features—household eligibility and pricing, network specifications, and tariff structures—were identical across villages, as was Kenya Power's oversight and eventual ownership and operation of electricity networks in all LMCP villages.

Here we have a case where two funders (WB and AfDB) with notably different degrees of conditionality are funding sites within the same government program, often with different funders supporting literally neighboring villages. The key difference between WB- and AfDB-funded sites is that each multilateral organization has its own set of procedural conditions aimed at improving construction processes. WB conditions applied to the 3,200 sites funded by the WB and AfDB conditions applied to the 5,620 sites funded by the AfDB.³ Construction was outsourced to private contractors selected through international competitive bidding in accordance with each funder's contracting processes. Importantly, WB conditions are significantly more stringent: while the AfDB awarded 11 contracts in a single bidding round, the WB awarded 25 contracts across three staggered bidding rounds. In addition, upon procurement the WB inspected every single pole prior to construction. After construction, sites were inspected according to audit regulations before being handed off to

²See for example [Andersen et al. \(2022\)](#); [Moyo \(2009\)](#); [Mosley \(1986\)](#); [Easterly \(2003\)](#); [Öhler et al. \(2012\)](#); [Temple \(2010\)](#); [Kilby \(2000\)](#).

³AfDB funding was spread across two tranches, Phase I and a Phase II: this paper focuses on Phase I as it was launched at the same time as the WB sites.

Kenya Power for perpetual management. To disentangle the role of these two components of conditionality—contracting and audits—we implement a randomized auditing intervention (with the support of partners at the WB, the AfDB, and Kenya Power) designed to mimic existing audit structures.

A key contribution of the paper is the collection of high-frequency construction and power quality outcomes, building on a small but growing literature emphasizing the importance of detailed infrastructure measurement (see [Olken \(2007\)](#) for an early example of this approach). We manually track construction progress for 380 villages across five counties in rural Kenya, and collect three types of on-the-ground outcomes at all sites where construction had been completed by the end of our surveying activities in 2021. First, we measure construction quality for key infrastructure such as transformers, poles, and wires. Second, we deploy state-of-the-art sensors to measure minute-by-minute respondent-level power outages and voltage quality. Third, we conduct socioeconomic surveys to understand connection experiences and energy usage.

To identify the causal impacts of conditionality, we use the ad-hoc assignment of sites to funders, which we argue was quasi-random. While this allocation was not experimentally randomized, funders’ sites are spatially interspersed: 95% of WB sites in our sample are located within 10 km (6 mi) of an AfDB site, and there are often different funders in literally neighboring villages. Among Kenya’s 290 constituencies, 210 contain at least one AfDB and one WB site.⁴ The econometric analyses therefore include highly localized constituency fixed effects to account for any cross-constituency geographic or socioeconomic differences. We conduct a battery of robustness tests of baseline balance using geographic, satellite, census, and Kenya Power administrative data and quantify the extent of any baseline imbalances across a range of covariates. While we cannot rule out some selection bias, the correlation with our key outcome variables is minimal, and these covariates do not contribute meaningfully to the results.

The analysis generates several key findings. First, the WB’s relatively detailed ex ante contracting conditions caused significant extensive margin delays. Construction at the median WB-funded site started 10 months after the median AfDB-funded site. Five years after the project was announced, 14% fewer WB sites than AfDB sites had seen any construction, and among completed villages, there were 16% fewer customer connections at WB sites. Second, the WB’s intense pole inspections improved some attributes of on-the-ground pole and construction quality, however we can rule out meaningful improvements in reliability and voltage quality. The estimates of WB conditionality on other intensive margin outcomes such as household installation quality, cost, and energy usage are positive but modest in size and not statistically significant. Finally, the randomized audit treatment generated moderate improvements in household connectivity and energy usage. International contracting is a repeated game context where poor audit outcomes can harm future eligibility in poten-

⁴A constituency is a relatively small geographic unit: the average population is approx. 185,000. All but one constituencies in our five study counties contain at least one AfDB site and at least one WB site.

tially costly ways. The combination of these results suggests that more streamlined ex ante contracting procedures could minimize delays, while enhanced ex post audits can improve construction quality at relatively low cost and with little delay.

These results have noteworthy limitations. Kenya has relatively high institutional capacity compared to many of its African neighbors: conditionality may be more important elsewhere. Conditionality may also generate positive benefits in ways that we were unable to measure, for example by strengthening institutional capacity in the Kenyan public sector. It is also possible that differences in construction quality will not be measurable until well into the future: we can only rule out differences up to five years after construction. We elaborate on these points in the conclusion.

This paper contributes to a longstanding debate about the effectiveness of conditions imposed by multilateral organizations when providing financing to low- and middle-income country governments, dating back to debates about the ‘Washington Consensus’ in the late 1980s ([Williamson \(2009\)](#); [Rodrik \(2006\)](#)). [Mosley \(1986\)](#), for example, argued that the WB “made it clear that penalties would attach to any failure by the Kenyan Government to comply with the conditions” and that conditionality has “genuine cost for the recipient.” [Easterly \(2002\)](#) argues that misaligned incentives have caused much WB conditionality to be ineffective in increasing economic growth. Importantly, African scholars have had a range of different perspectives in this debate. In her provocatively titled book *Dead Aid*, [Moyo \(2009\)](#) argues how aid conditionality can distort markets and be abused for political gain. [Archibong et al. \(2021\)](#) acknowledge that market-oriented reforms based on Washington Consensus may have lead to short-term frictions, but argue that by strengthening institutions conditionality may have boosted long-term economic growth.

The nature of donor conditionality has changed significantly since the 1980s, in part due to changes in international financial institution leadership. James Wolfensohn, as President of the WB from 2005-2015, lowered policy conditionality related to structural adjustment, focusing instead on poverty reduction and streamlining WB and IMF procedures. His successor, Paul Wolfowitz, emphasized the anti-corruption goals of conditionality, which continue to drive much of the procedural conditionality that remains widespread across most funding sources today. Between 2012–2017 the WB awarded approximately USD 13.6 billion annually in contracts through bank-financed projects, and the stringent regulations we study in this paper continue to apply comprehensively across all WB projects. Research on procedural conditionality in particular has found this can cause politically-motivated delays and incur costs that exceed the benefits ([Kersting and Kilby \(2016\)](#); [Kilby \(2013\)](#)).

Concerns around political interference, which have also been raised for decades, remain relevant: [Andersen et al. \(2022\)](#) find that significant portions—roughly 5 to 10% on average—of WB financing are transferred to offshore financial havens (and probably to some politicians’ private bank accounts) in the months after a tranche transfer. In documenting substantial delays to the WB contracting process we build on [Marx \(2018\)](#); [Kilby \(2013\)](#); [Kersting and Kilby \(2016\)](#), who find that these delays can be political in nature. The rise of Chinese lending to LMICs has also shifted the debate, given the Chinese government’s frequent claims of non-

interference in local policymaking and politics (Isaksson and Kotsadam (2018); Dreher et al. (2021); Ping et al. (2022)). However, the lack of transparency in Chinese aid conditions to African countries leaves it unclear how much this is rhetoric or reflects reality, and whether China seeks to influence bilateral relations through other multilateral financing (Kaya et al. (2021)).

In studying the role of independent monitoring in conditionality, we build on Kilby (2000) who finds that modest WB monitoring can be effective but that the marginal costs currently outweigh the marginal cost. This also contributes to a literature on independent monitoring, which has been shown to increase state performance (Olken (2007); Ferraz and Finan (2008); Finan et al. (2017); Duflo et al. (2018)). This is especially the case when actors are engaged in a multi-period game that allows organizations to credibly threaten to punish under-performance by withholding future opportunities.

Studying accountability in the context of rural electrification per se is important because the government implementation of mass electrification programs in poor countries is widespread and ongoing. Poor construction quality can harm reliability and voltage quality and undermine these programs' socioeconomic objectives. Blimpo and Cosgrove-Davies (2019) find that in some countries in Sub-Saharan Africa, most connected households "reported receiving electricity less than 50 percent of the time in 2014," and that this may undermine the economic growth that household connections were designed to generate. In rural Kenya, Lee et al. (2020c), find that transformer outages in a rural region of Kenya near our study counties frequently last for more than four months, and may therefore contribute to the low uptake and impacts of household electricity. This may explain why Lee et al. (2020c) and Kassem et al. (2022) find limited impacts in rural Kenya. In India, Burlig and Preonas (2021) find that improved reliability increases the impacts of rural electrification in larger villages. To the extent that low quality infrastructure exacerbates poor power quality and slows economic growth, identifying opportunities to improve construction quality may lead to meaningful improvements in economic outcomes.

2.2 Kenya's Last Mile Connectivity Project

On May 27th, 2015, Kenya's President Uhuru Kenyatta announced the launch of the LMCP with a goal of connecting "*one million new customers to electricity each year*" (Kenya Presidency (2015)). The program would primarily target households living near existing transformers, who could be connected to the existing local electricity network at relatively low cost. In a press conference two weeks after President Kenyatta's announcement, Kenya Power's then- Managing Director Ben Chumo added that the program was designed to facilitate "*the government's objective of providing 70% households with electricity by 2017 and universal access by 2020*" (Kenya Power (2015a)). This target date was later extended to 2022. The program has generally been effective: nationwide household access to electricity was 25% in 2009 but had risen to 70% by 2019 (KNBS 2009, KNBS 2019a).

The LMCP’s USD 600 million cost was financed through loans and guarantees from the AfDB and the World Bank (WB), together with loans from the European Investment Bank (EIB) and the Agence Française de Développement (AFD), a grant from the European Union, and funding from the Government of Kenya (GoK). This paper focuses on transformer sites funded by the WB and by Phase I of the AfDB, which we refer to jointly as Phase I of the LMCP.

While the LMCP was financed through various channels, it was a single nationwide project implemented by Kenya Power under a single set of implementation specifications. As of 2019, there were around 60,000 transformers across Kenya, which convert high- and medium voltage power (33kV or 11kV respectively) down to low voltage levels (usually 0.415kV) that can be connected to households via local conductors. In many rural areas, these transformers were originally constructed between 2005 and 2013 as part of a nationwide push by Kenya’s Rural Electrification Authority (REA) to connect all public facilities—such as markets, schools, health centers, and water points—to electricity (REA 2008a, [Berkouwer and Dean \(2021\)](#)). The LMCP’s objective was to extend the low voltage network in order to connect all households located within 600 meters of the existing transformers selected for the program, a process referred to as ‘maximization.’⁵

Kenya Power and the GoK jointly selected which transformers would be included in the LMCP, targeting an equitable distribution of LMCP sites across Kenya’s 47 counties. The process of determining exactly how many and which transformers in each constituency would be maximized involved extensive back-and-forth written communications between Kenya Power and each constituency’s Member of Parliament (MP) that factored in cost and human development considerations. This process produced a nationwide list of LMCP transformers.

Kenya Power also developed a clear set of LMCP household eligibility and process criteria that were implemented homogeneously across all financing sources. Eligible households benefit from a reduced electricity connection price, from USD 350 down to USD 150, and from the ability to pay in monthly instalments rather than upfront. The program was also touted as reducing the red tape frequently associated with new electricity connections: the long and laborious process of applying for electricity, which can take months and often requires significant paperwork, would be replaced by a system where Kenya Power contractors proactively visit households to initiate connections, with minimal effort for households. The number of unconnected households to be connected per LMCP site was usually between 20 and 100.

AfDB Phase I would maximize 5,320 existing transformers using a USD 135 million AfDB loan ([Kenya Power \(2017a\)](#)). A WB loan of USD 150 million would cover the cost of

⁵Households could choose not to get connected. In practice, this was rare, although some (mostly elderly) residents did make this decision. While statistics of this phenomenon are not available nationwide, [Lee et al. \(2020c\)](#) find that four percent of participants randomly selected to receive a free electricity connection chose not to receive one.

the maximization of an additional 3,200 transformers.⁶ LMCP transformers were assigned to be funded by either the WB or the AfDB in an ad hoc quasi-random manner. Section 2.3 discusses this in more detail.

Corruption and contracting

There is widespread concern that political interference and corruption within Kenya Power could jeopardize the quality, cost-efficiency, timeliness, and equity of the construction process (The Star (2018); Kenya Power (2018a, 2020); ESI Africa (2020); Wolfram et al. (2021)). In 2019, for example, bidding collusion led to “the supply of substandard wooden poles for [USD 8 million]” (The Nation (2021)). In one egregious case of corruption, Kenya Power’s CEO Ken Tarus and his immediate predecessor Ben Chumo were arrested in July 2018 (Reuters (2018)). They were eventually charged—alongside several other senior Kenya Power officials—with various charges relating to corrupt procurement practices that resulted in the loss of more than USD 2 million worth of public funds, with Tarus charged with an additional USD 1.5 million worth of fraudulent payments also related to fraudulent procurement practices.

These types of events are not unique to Kenya, and not unique to the electricity sector. The WB defines corruption as “*the abuse of public funds and/or office for private or political gain*” (WB 2015). The World Bank’s extensive regulations—detailing the procurement, financial management, and disbursement of funds—apply well beyond Kenya and across a range of industries:

“Borrowers using the Regulations spend billions each year procuring works, services, or goods from third-party suppliers, contractors and consultants. Procurements under these Regulations happen in over 170 countries across the globe [and] range from highly complex infrastructure, cutting edge consultancy, major pieces of plant/equipment, and high tech information technology.”

World Bank Procurement Regulations for Borrowers (2020)

Over the past 20 years the WB has increased its efforts to combat corruption at all levels. One lever is the ‘debarment’ by the WB or AfDB of private contractor with egregious performance. This has happened several times to LMCP contractors (Kenya Power (2018a); Spotlight East Africa (2020)), which we discuss in more detail in Section 2.2. Being debarred in this manner generally applies globally: under-performance in one country can lead to disqualification from contracts in other countries. Independent monitoring by agencies that frequently provide large contracts across multiple countries can therefore be a meaningful economic threat for contractors.

⁶The WB provided additional funding to install 1,000 new transformers. For comparability those projects are excluded from this paper’s analyses.

International financing conditions

While multilateral development banks have made much progress in harmonizing procurement policies for donor-financed projects, differences in procurement guidelines and in interpretation persist ([The World Bank Group, 2014](#)). Compared to other development banks, the World Bank's policies have been described as more prescriptive, with some concerns that inflexibility of such policies may make them time-consuming without necessarily limiting fraud and improving outcomes ([The African Development Bank, 2014a](#); [The World Bank Group, 2014](#)). Some of these patterns in World Bank and AfDB policies can be seen reflected in the implementation of the LMCP.

For Phase I of the LMCP, Kenya Power announced 40 contracts that domestic and international private sector contractors could bid on. Each contract specified a portion of the construction process for all sites assigned to a specific funder located in a specific geographic cluster of Kenya's 47 counties, each consisting of several hundred transformers that were to be maximized.

Both funders also awarded one contract to an external consultant to oversee project implementation. While the exact tasks differ by funder, broadly speaking both consultants were responsible for overseeing progress, conducting construction audits, and ultimately inspecting sites prior to awarding Joint Measurement Certificates (JMCs) which certify that a contractor completed construction at a site and that the site can be handed over to Kenya Power for activation. Beyond this basic setup, the two international agencies employed different contracting methodologies, with the WB's conditions significantly more stringent than the AfDB's.

The AfDB took a more hands-off approach, referred to as a 'turn-key' system, consisting of 12 contracts in total. 10 of these contracts corresponded to 10 geographical clusters of counties. Whichever contractor won a particular contract would be responsible for the entire construction process of all transformers located in that contract's counties. This process included identifying unconnected and eligible households at each transformer site, developing engineering designs for an efficient extension of the low-voltage network to reach those households, procuring the materials required to complete those designs, and implementing construction. Each contractor therefore had full ownership over the entire construction process in a specific set of sites, allowing them to potentially leverage any associated synergies. These contracts excluded only the final metering phase: this was contracted out in a single nationwide 11th contract to better facilitate the integration with Kenya Power's existing technologies. Finally, the 12th contract was the external consultant contract.

The WB was significantly more closely involved in the bidding process, the selection of contractors, the quality and costs of construction materials, and the auditing of contractors' performance during the process. Rather than providing turn-key contracts, the WB segregated contracts across construction phases. A single contract was first issued for a contractor to complete designs of all sites nationwide, detailing the proposed local low-voltage networks and also specifying the materials required to complete the designed construction. Once the

design contract had been awarded and completed, a series of procurement contracts were issued to procure the relevant materials. The procurement of materials was separated into 20 separate contracts: 12 contracts for the supply of wooden poles, 3 for concrete poles, 2 for conductors, 3 for cables, and one contract to supply all meters nationwide. As per the WB guidelines, each contract went through the WB’s standard bidding and contracting process. In addition, the WB issued 6 contracts for the construction of the proposed designs using the procured materials, with each contract containing a geographically clustered set of sites. Finally, similar to the AfDB, they issued one final contract for the external consultant.

The staggered nature of the WB requirements causes significant delays. For example, the request for proposals for procurement contracts cannot be published until the engineering designs are finalized, as this determines the procurement requirements. When factoring in bid writing, bid elicitation, bid review, and actual contracting, the contracting process for any single stage can take months.

Initial funding approvals from the AfDB and WB were finalized at a similar time – the AfDB in November 2014, and the WB in March 2015. Project appraisal reports released in October 2014 for the AfDB and March 2015 for the WB indicate that contracts for both sets of activities were planned to be signed by early 2016 ([The African Development Bank, 2014b](#); [The World Bank Group, 2015](#)). In December 2015, Kenya Power and the AfDB signed all 12 contracts needed to launch construction ([Kenya Power \(2015b\)](#)). By mid-2016, procurement for construction activities funded by the WB had begun, with the first contracts signed in February 2017. In November 2017, the WB signed the final six contracts required to commence construction, a substantial delay relative to the project timeline at the time of approval ([Kenya Power \(2017a\)](#)). [Figure 2.1](#) compares the construction timeline for each multilateral. Incidentally, right around the same time—in November 2017—the AfDB also signed 15 additional turn-key contracts to begin maximization of an additional 5,200 sites as part of its *Phase II*.

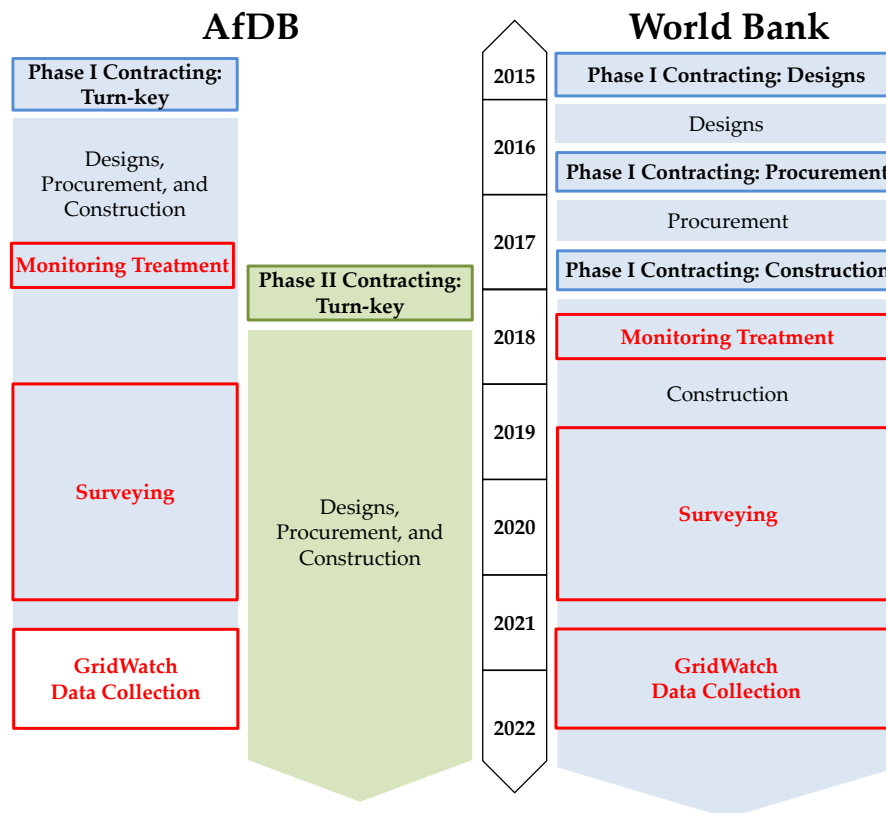
In addition to general project coordination, a key objective for the consultant hired in the AfDB’s 12th contract was to track construction progress and inspect a small subset of projects post-construction. Additional monitoring by an independent group that is not contracted by Kenya Power may therefore help reduce collusion in this context.

Role of private contractors

Many contractors are large construction firms with a track record of completed projects in many countries. Contractors that won the AfDB- and WB-funded LMCP contracts were a mix of Kenyan firms and international firms, with some joint ventures comprised of two or more firms.

The winners of the 12 AfDB contracts had been selected from among a combined 110 bidders. Six of the 11 materials and works contracts winners were Kenyan—Neo Electric Company, Etrade Company, Polyphase Systems, Rwathia Distributors and Kiewa Group consortium, Metsec Cables, and Empower Installations—while five were foreign: AEE Power

Figure 2.1: Timeline of contracting and construction by multilateral



Timeline of the experimental components and the contracting process for the WB and the AfDB. The AfDB used turn-key contracting while the WB separated its contracting into individual stages. By the time the WB had completed its Phase I contracting process and was ready to begin construction in early 2018, the AfDB Phase I was approaching completion, and AfDB was launching its Phase II. AfDB sites that had been completed prior to the implementation of the monitoring treatment in late 2017 were excluded from randomization. Surveys were conducted strictly after construction completion.

(Spain), Angelique International (India), Shenzhen Inhemeter Company (China) and Gammon Limited (India) (Capital Business 2015). There is no blanket provision preventing firms from submitting—or being awarded—bids with both international funders simultaneously. Indeed, many of the AfDB contractors named above have in the past bid on—and in many cases been awarded—WB contracts.

That said, international infrastructure contracting is a repeated game, and poor performance can cause contractors to face constraints. As an example, in October 2018 the WB Sanctions Board imposed “a sanction of debarment” on the Indian company Angelique International for “fraudulent practices as defined in Paragraph 1.16(a)(ii) of the January 2011 Procurement Guidelines.” (WB 2017; WB 2011). The WB appears to have stricter

standards, leading to a higher frequency of sanctions against private contractors. We do not see this as a concern around selection bias from an identification perspective, but rather we view this as a mechanism through which conditionality may operate. International procurement and construction bidding can be thought of as a repeated game among a small set of actors, and contract performance can have serious ramifications on long-term performance. We exploit this threat in our randomized audits treatment, discussed in Section 2.3.

Similar to the AfDB, the set of contractors awarded WB contracts also included a mix of Kenyan and International firms. Kenyan firms were primarily awarded bids for the supply of wooden and concrete poles. Contracts for three more technical materials went to firms in India and China: the supply of conductors was awarded to TBEA Deyang Cable Co. Ltd. (China) and Dynamic Cables Pvt. Ltd. (India) and the supply of meters was awarded to Shenzhen Clou Electronics Co. (China).

Less is known about the numerous subcontractors hired by these contractors. For example, since AfDB contractors were responsible for the design, procurement, and construction of sites, it was standard for these contractors to subcontract these components out to smaller firms. This process receives relatively little oversight from donors ([The African Development Bank, 2014a](#)). As an example, in the Indian company Gammon Limited procured more than 4,400 of its concrete poles through World Poles Limited, a Kenyan company whose website claims their “electricity concrete poles strictly adhere to [Kenya Power] specifications” and that “each concrete pole is passed through rigorous testing to ensure excellent rigidity, long life and accuracy in terms of designs as well as dimensions.” Qualitatively, the companies from which AfDB contractors procured materials appear similar to the contractors who were awarded procurement contracts by the WB.

Household construction responsibilities

The tender documentation for both the WB and the AfDB contained detailed specifications for the materials and installation procedures of poles, wiring, conductors, fuses, and—ultimately—meters. But a correctly installed electricity connection with a functioning meter is of little benefit to a household without power sockets or light switches. The final household connection is thus crucial. During LMCP, households were responsible for installing—or hiring a local handyman to install—internal wiring, defined as anything between the meter and the appliances a household consumes. The field surveys indicate that households who were connected prior to the LMCP spent on average USD 125 on internal wiring.

For most households, the internal wiring was thus a significant financial and logistical barrier, on top of Kenya Power’s connection fees. To address this issue, Kenya Power decided to provide low-income households who could not afford internal wiring with an electrical panel—a ‘readyboard’—that would allow them to be connected without wiring being completed beforehand. In May 2015, President Kenyatta described this policy as follows: *“The Ministry of Energy has also come up with designs that will enable households that do not have internal wiring in their houses to use electricity by providing a ‘ready board’... [it]*

has switches, sockets and bulb holders and those who do not have wiring in their houses will be able to use electricity soon as they are connected” (Kenya Presidency (2015)).

Household awareness of costs

Beneficiaries under the LMCP are connected via ‘pre-paid’ Kenya Power meters, meaning they must buy electricity credits in advance of using electricity. Once they consume their prepaid electricity, they are disconnected, and only re-connected only after they buy more credits.

To recover the USD 150 connection fee, Kenya Power automatically enrolled households into a payment plan consisting of 36 monthly installments of around USD 4 per month. This charge was to be automatically added to households’ accounts on a monthly basis. Kenya Power’s initial implementation was such that any electricity payments the household made were directed towards paying off this debt prior to being directed towards electricity credits. However, this generated confusion among households: as an example, if a household runs out of electricity credit in January, and then does not consume any electricity in February or March, they would have to pay at least USD 16.01—4 months worth of connection fees—to be able to consume any electricity in April. The contribution was thus capped at 50% of any topup amount (Kassem et al. (2022)).

This barrier was not only a significant financial hurdle, but one that was unanticipated and poorly understood. According to Kenya Power, households should have been informed of the payment structure as part of the consent process, which was the very first step in the construction process, but it is unclear whether this was implemented in practice. To verify whether this process was correctly implemented, and to test whether donor conditionality and monitoring can improve adherence to these guidelines, the household survey (described in Section 2.4) measures respondent understanding of the aggregate costs of an electricity connection under the LMCP. 58% of households do not recall ever having been told that they would have to pay Kenya Power for the connection; 44% thought they would not have to pay.

LMCP was intended to connect all unconnected households to electricity, however, in practice connection was not universal. At the average site at least 7% of compounds were not connected to the grid, and at the 90th percentile site 25% of households were not connected.⁷ The most common reason (given by 31% of respondents) is that they were not present or available during the days on which construction or sign-up were administered. Second, even though the LMCP program specifications indicate there were to be no upfront connection fees, 23% of respondents still report having been unable to pay, generally for one of two reasons. They may not have been able to afford the internal wiring required by Kenya Power to be connected: 16% of unconnected households report this to be the reason. This

⁷Enumerators only counted unconnected compounds that were within connection distance of the existing electricity network, so this may be an underestimate. Section 2.4 provides more details on surveying methodology.

contradicts the claim by Kenya Power that households who were unable to afford these upfront costs were to be given a free readyboard. Households also report numerous instances of bribery: 6% of households explicitly having been asked for money by the contractor, with amounts generally ranging from USD 5 to USD 50. This statistic is similar among households connected through LMCP, of whom 8% report having been asked for a bribe at least once during the process. Tragically, a small number of households report having paid an individual claiming to be a contractor, only to never hear from them again. Importantly, only 2.8% of respondents report not wanting a connection, for example because they're simply not interested in having electricity or because they think electricity is unsafe.

2.3 Research Design

To estimate the causal impact of conditionality on construction outcomes we exploit the quasi-random assignment of construction sites to two different international donors that implemented different contracting and oversight structures. We then implement randomized audits to examine how ex-post audits affect project outcomes. This section describes both sources of variation in more detail.

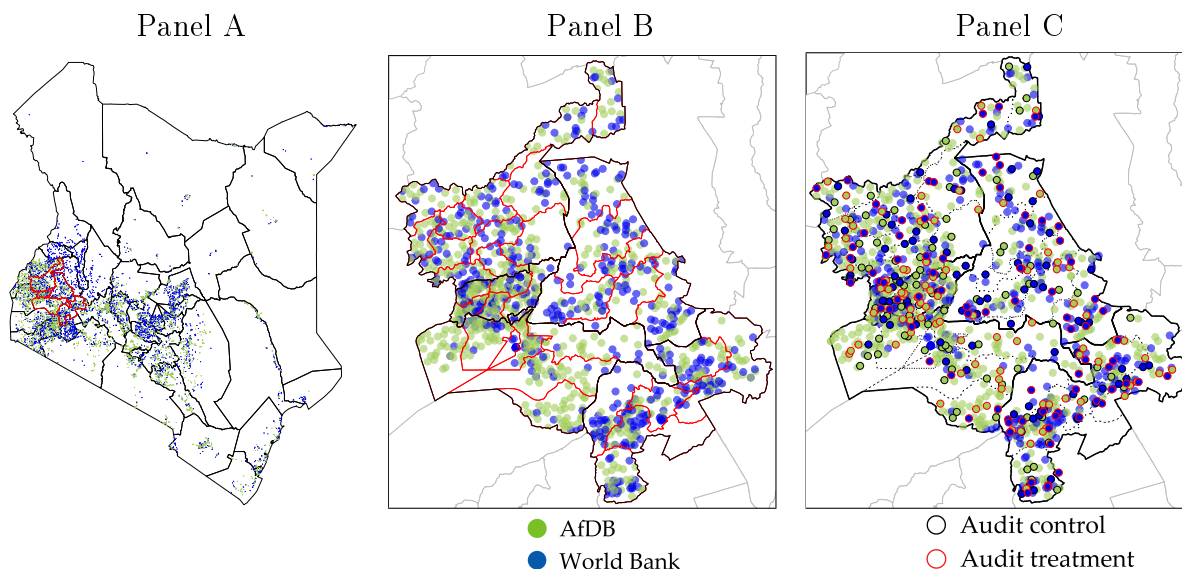
Quasi-random assignment of sites to international financing

The transformers selected to be maximized under the LMCP were assigned to be financed by either the WB or the AfDB. From June 2016 through December 2019 we had extensive private meetings with key Kenya Power personnel responsible for the LMCP. This included meetings with the General Manager for Connectivity (who was responsible for all of Kenya Power's activities connecting new households to power) and the two Project Managers who oversaw the nationwide implementation of the LMCP portions funded by the AfDB and WB, respectively, including all contracting, procurement, and construction activities. We read dozens of letters of correspondence between Kenya Power and individual Members of Parliament discussing and deciding which transformers would be included in each phase of the LMCP. The consistent pattern that we observed was that assignment was ad hoc and did not follow any particular allocation rule. Given that the mandates were identical—to connect all households within 600 meters—Kenya Power and the GoK did not appear to see any strategic benefit in having a transformer be funded by one or the other.

Panel A of [Figure 2.2](#) maps the nationwide distribution of sites, with each site colored according to the organization that financed it and oversaw its implementation. In line with explanations provided by the electric utility, there do not appear to be systematic differences in how funders were assigned to sites.

Our causal identification leverages this quasi-random allocation of LMCP Phase I transformer sites to be funded and implemented by either the WB or the AfDB. Out of 8,520 nationwide LMCP Phase I sites, 1,139 are located in the five study counties: Kakamega,

Figure 2.2: Sites by funding source and audit treatment status



Panel A displays locations of the nationwide LMCP Phase I sites colored by funding source. Highlighted in red are the five counties where we conduct engineering and socioeconomic surveys (Kakamega, Kericho, Kisumu, Nandi, and Vihiga). Panel B focuses on this area and emphasizes constituency boundaries within the 5 counties, highlighting that the assignment of sites to funders does not appear to be geographically clustered in the study area. Panel C shows audit treatment and control sites circled in red and black respectively (see Section 2.3 for detail).

Kericho, Kisumu, Nandi, and Vihiga. Panel B of Figure 2.2 maps this subset of sites. The five study counties are made up of 36 constituencies, of which 35 have at least one WB site and at least one AfDB site, thus we restrict site selection to these constituencies and include constituency-level fixed effects in the primary outcomes regressions. 95% of WB sites among this sample are located within 10km of an AfDB site (this holds vice versa as well). Panel C presents the interaction of funding source and randomized audit treatment, discussed in Section 2.3.

Of course, it is possible that an assignment mechanism was in use that we were not aware of. Speculatively, Kenya Power employees may have been aware that AfDB construction would proceed more quickly than WB construction and therefore assigned sites that were considered a priority to receive AfDB funding—while we have no evidence of this, possible reasons could include political expediency, economic growth expectations, or personal favor. Importantly for identification, most mechanisms for biased selection would not undermine the primary research findings that WB conditions caused significant delays but did not affect power quality, construction quality, or household electricity usage.

To assess the potential scope for selection, we conduct several balance checks. Table 2.1 conducts balance checks at the ward level for the number of WB-funded LMCP sites by

Table 2.1: Number of LMCP sites per ward, by funder and 2009 census socioeconomic characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Primary	Secondary	Consumption	Youth HQ	Wall HQ	Roof	Solar	Elec
Number of LMCP sites	-0.04 (0.07)	0.15 (0.09)	0.08 (16.19)	-0.10* (0.05)	-0.22 (0.15)	0.42* (0.16)	0.04* (0.02)	-0.05 (0.16)
... of which WB	0.22 (0.14)	-0.09 (0.18)	-24.44 (33.96)	-0.05 (0.10)	0.75* (0.31)	0.26 (0.34)	0.08* (0.04)	-0.24 (0.33)
Observations	780	780	780	764	780	780	780	780
Mean	57.6	20.7	3086.7	49.4	18.0	75.7	2.1	11.4

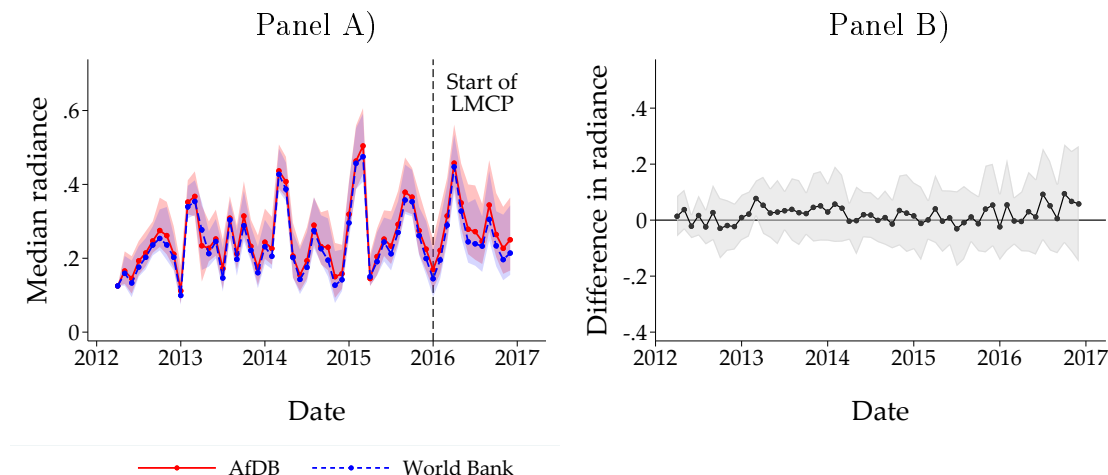
This table tests for correlations between the number of sites in a ward allocated to each funder and baseline characteristics among wards with at least 1 LMCP site. Columns (1)-(7) estimates percentage of, in order: households with a high quality wall; households with a high quality roof; households with electricity; households with solar; individuals who completed primary school education; individuals who completed secondary school education; and, individuals who are 14 years or younger. Column (8) estimates consumption per capita in KSH. All regressions include constituency FE. Data source: 2006 Household Budget Survey and 2009 Census data. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

baseline socioeconomic characteristics (measured during the 2006 Household Budget Survey and 2009 Census data) conditional on the number of LMCP sites in each ward. The number of WB-funded sites in a ward is not correlated with the fraction of households with a high quality roof or electricity, the fraction of individuals with primary or secondary education, the fraction of residents who are 14 years or younger, and average consumption. There is a slight difference in the fraction of households with a high quality wall and the fraction of households with a solar panel, but since these are significant only at the 10 percent level, we attribute this to statistical noise.

Figure 2.3 compares trends in monthly Visible Infrared Imaging Radiometer Suite (VIIRS) nighttime radiance, or ‘nightlights’ (Elvidge et al. (2017)). WB and AfDB sites have indistinguishable nighttime brightness levels and trends prior to the LMCP construction period. Columns (1) and (2) of Table A1 present a pooled balance test of pre-LMCP radiance levels confirming these were statistically indistinguishable.

Despite these similarities, there are modest differences between WB and AfDB sites. Most transformers had been connected as part of a push by REA between 2005 and 2013 to connect all public facilities nationwide to electricity. There appear to be some differences in the likelihood of transformers located near specific types of facilities to be assigned to one funder or the other. For example, 30 out of the 132 AfDB transformers we surveyed (23%) were located near a secondary school, whereas only 10 out of the 118 WB sites were (8%). We do not have a clear explanation for this: we attribute these differences to statistical noise and all regressions therefore control for these baseline characteristics. Table A2 quantifies these differences using field surveys and administrative data. The difference is largest for secondary schools. WB transformers are slightly more likely to have a market center nearby,

Figure 2.3: Site-level nighttime radiance by funding source



Panel A presents median monthly nighttime radiance from VIIRS between 2012-2017 per site-month, with bands of the 25th to 75th percentile. Panel B confirms that radiance is statistically indistinguishable across World Bank and African Development Bank-funded sites (estimates include constituency FE). [Table A1](#) confirms baseline balance using a pooled regression of these data.

or no public facility at all.

In addition, there are small differences in site geography across funding sources. Columns (3) and (4) of [Table A1](#) indicate that WB-funded sites have a 13% higher average land gradient, indicating they were more likely to be located in hillier villages.

Extensive robustness checks confirm that delays and construction quality are uncorrelated with land gradient and facility type, and that the results are constant across the entire support of land gradient and facility type. To minimize any remaining source of biases, all regressions control for land gradient and facility type. Section [2.5](#) discusses these robustness checks in more detail.

While contractors were allowed to bid on WB and AfDB contracts simultaneously, we do not observe any instances of a firm winning at least one WB and one AfDB contract. It is possible that donor conditions generate selection: firms with certain characteristics may be more likely to bid on WB or AfDB contracts, and this may cause differences in outcomes. Speculatively, firms with more streamlined operations may choose to submit for works funded by donors with more stringent requirements. We do not view this as a threat to identification: rather, we consider selection along this dimension a mechanism through which conditionality may operate.

Randomized Monitoring

Out of the 1,139 sites in the region, we select 380 sites for the randomized monitoring experiment.⁸ We assign 190 to the treatment group and 190 to the control group. Assignment is stratified by site constituency and which multilateral the site is funded by. We discuss site selection in more detail in Section 2.4.

The randomized monitoring is implemented jointly with the WB and the AfDB as follows. During in-person meetings set up for this purpose, members of the research team notify contractors that an independent, international team of engineers will audit a specific list of selected sites once construction is complete. During the meeting they provide formal, written notification that is signed by senior management at Kenya Power, the WB, and the AfDB (Figure B1). This notification also includes the specific set of sites within their contract region that were selected to be audited. In communications with WB officials (in both Washington D.C. and Nairobi), the WB indicated they would take contractor-level evidence of leakage (on both WB and AfDB funded projects) into account in future contracting. This setup can therefore be thought of as a repeated game environment. Contractors depend on their repeated relationship with international organizations such as the WB and the AfDB for future projects in many sectors—many also work in sectors outside electricity. This provides an incentive for contractors to implement high-quality infrastructure projects, or at least to be perceived as doing so, in order to win future contracts. To emphasize this incentive the notification states this explicitly. Section 2.2 discusses the role of *debarment* by international organizations in more detail.

Unbeknownst to the contractor, the list of sites that they are told will be audited is in fact a randomly selected subset of the full set of sites that are surveyed by our research team. Given the random selection, any difference in construction outcomes between the sites about which contractors are notified and the control sites can be attributed to contractors' response to the monitoring.

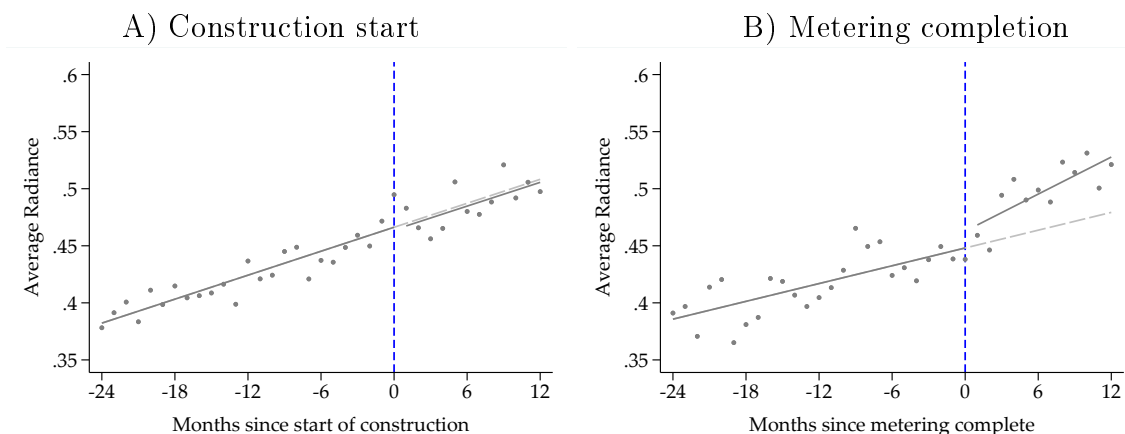
2.4 Data

We use rich nationwide administrative data on construction progress at thousands of construction sites provided by the electric utility to conduct sample selection. Of the 1,099 LMCP Phase I sites located in the five study counties, we randomly select 380 to be included in the randomization sample, stratifying on constituency and funder. We randomly assign 190 to the monitoring treatment group and 190 to the monitoring control group, stratifying by funder and by constituency.

Field managers conducted frequent short assessments with village representatives—over the phone or in person—at all 380 sites to determine the status of construction progress at

⁸This matches the sample size specified in the Pre-Analysis Plan submitted to the AEA RCT Registry, [available here](#).

Figure 2.4: Event study: nightlights after construction progress



Data on construction progress for 190 AfDB sites and 190 WB sites located in the five study counties collected through phone surveys with local village representatives. Nighttime radiance data from VIIRS. [Figure B2](#) confirms that nightlights also do not respond to the ‘pole installation’ and ‘stringing’ stages.

each site over time. This yields a panel dataset of construction progress at the site level. To verify the accuracy of these reports, [Figure 2.4](#) plots event study graphs of nighttime radiance in the months before and after key construction stages. As expected, the start of construction does not affect nighttime radiance. On the other hand, in the 12 months after the completion of household metering, nighttime radiance increases sharply. This lends credence to the construction reports.

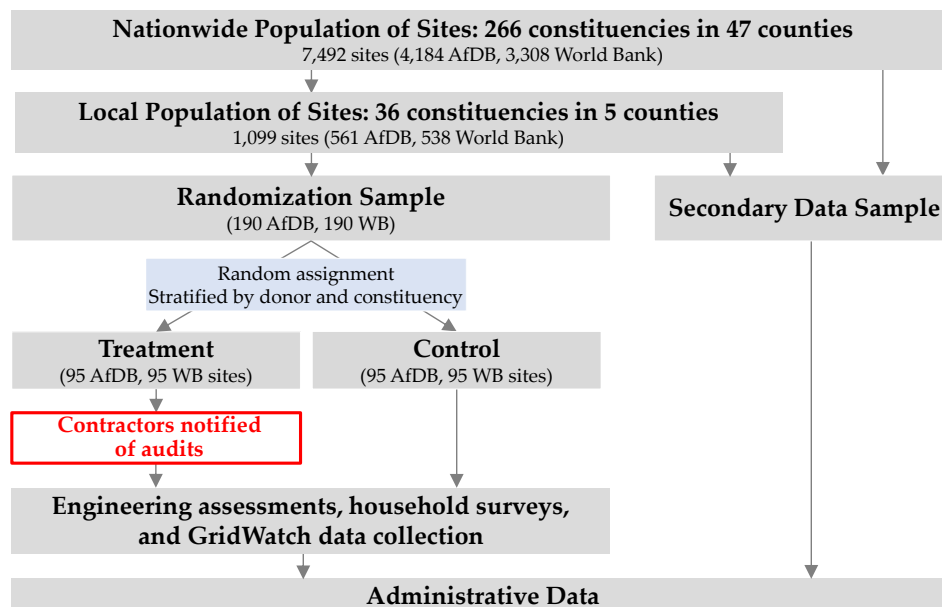
We conduct on-the-ground engineering assessments and socioeconomic surveys at all 250 sites where construction had made significant progress by the end of field activities in May 2021. Construction had still not been completed—and usually not even begun—in the remaining sites, limiting surveying activities there to short assessments of any initial planning activities (for example, whether a census of unconnected households had been taken and whether any materials had been delivered). To minimize differences caused by the construction delays, assessments and surveys are conducted between six and twelve months after construction is reported to have begun at a site.⁹ Roughly half of the surveying sites are WB sites and half are AfDB sites. [Figure 2.5](#) provides an overview of these elements in the context of the randomization and data collection.

Engineering assessments

In the technical engineering assessments, field officers take detailed measurements on the quality of the materials and construction of the electricity network in each sample transformer

⁹Due to logistical constraints, in some cases surveys were conducted several months earlier or later.

Figure 2.5: Project design



Sample selection and randomization. Contractor notification was implemented in 2017-2018 and assessments and surveys were done 2018-2021. Engineering assessments and household surveys were completed at the 250 sites where construction had been completed by the end of surveying activities in 2021. Additional tracking of construction progress at the remaining sites continued through 2022.

community. The specific measures included in the survey were developed in coordination with REA engineers. The engineering assessment consists of two parts. In the initial infrastructure census, field officers systematically record the locations of all poles in the low-voltage network, as well as their connectivity, up to 700 meters from the existing transformer. The 700 meter radius exceeds the government guideline of 600 meters, and therefore allows us to test whether construction was completed beyond the eligible region. They also document the number of poles in the low-voltage network that are further than 700 meters from the transformer and are within sight.

The field officers also record the number of drop-down cables (the final connection between a customer meter and the electricity network) connected to each pole, whether the drop-down cable connected a residential compound or a firm, as well as any unconnected compounds located near the pole. This provides a measure of the number of connected and unconnected firms and households across the entire site.

In cases where the local network was too large to complete in a single day, field officers selected a random subset of phases to assess, and then recorded all poles in every selected sub-phase. At these sites, scaling the surveyed connections proportionally to the fraction of the grid that was surveyed yields an estimate of the total number of household connections

at that site.

In the second part of the engineering assessment, the field officers record more detailed measurements for one in every five poles and lines in the network.¹⁰ The detailed measurements include quality measurements of the pole itself, such as angle relative to the ground, whether it is wood or concrete, whether it is firmly placed in the ground, whether it has a pole cap, and whether it has any visible cracks. Additional pole measurements include whether it has the appropriate grounding wires, stay wires, and struts; and whether the stay wires and struts are correctly installed and have correctly installed insulators (if applicable).¹¹ Measurements taken of the conductors (wiring) that connect the poles include whether it has appropriate ground clearance and clearance from other objects (such as flora or structures), and whether any electric lines cross. Measurements of the drop-down cables from the pole to the customer include the distance between the pole and the customer's structure, and whether the cable ends at a meter or whether it appears to be an illegal connection. Finally, measurements of the central transformer at each site include whether the poles on which the transformer is mounted are leaning excessively, the number of missing or bypassed fuses, and whether the transformer has any other obvious defects.

Table C1 presents summary statistics on transformers, poles, and households surveyed at 250 transformer sites. At around one quarter of transformers at least one fuse was missing or had been bypassed. We surveyed on average 87 poles per site, of which about a quarter had a large crack. 95 percent of surveyed households were connected in 2016 later, and the median year in which households were connected was 2019.

Household and firm survey data

After the infrastructure census had been completed, a random subset of the (connected and unconnected) residential compounds and firms that had been recorded at the end of drop cables were selected for more detailed socioeconomic surveys. The goal of the household and firm surveys was to understand both their experience with the construction process, and potentially, how heterogeneous construction quality impacts community experiences with electricity. For example, anecdotally, households are occasionally asked to contribute manual labor to construction for example by digging their own holes for distribution poles, even though this is strictly against Kenya Power policy. Higher quality construction and installation could also potentially reduce local power outages and increase power reliability, which could have tangible benefits for household well-being and firm productivity and profits. The survey also asks how many meters are at the compound, and how many they requested.

¹⁰In smaller sites, field officers would conduct detailed measurements of every third or fourth pole instead. The survey had been pre-programmed to automatically perform this calculation and provide instructions to the field officers at the end of the censusing activity.

¹¹For a random 10 percent subset of poles (or one in every 6th pole in case the network has more than 120 poles) we also collect data on additional pole attributes, such as pole height, circumference at various points, and characteristics of each strut or stay that provides support for that pole.

Anecdotal evidence suggests that multiple meters are often installed within a single home compound, suggesting that Kenya Power metering data could overstate the total number of households that are connected nationwide.

Power quality: outages and voltage

To measure household-level reliability and voltage quality we deploy the GridWatch technology ([Klugman et al. \(2021\)](#)) with a subset of the households and firms that had participated in the socioeconomic surveying activity. GridWatch measures minute-by-minute power state and voltage at the household level, and can be easily installed by plugging a PowerWatch device (shown in [Figure B3](#)) into a power outlet. The device transmits data to the cloud in near real-time over the cellular network, and stores data locally to transmit later in the case of network failure. The central GridWatch server aggregates data to detect patterns in power outages and reduce noisy signals.

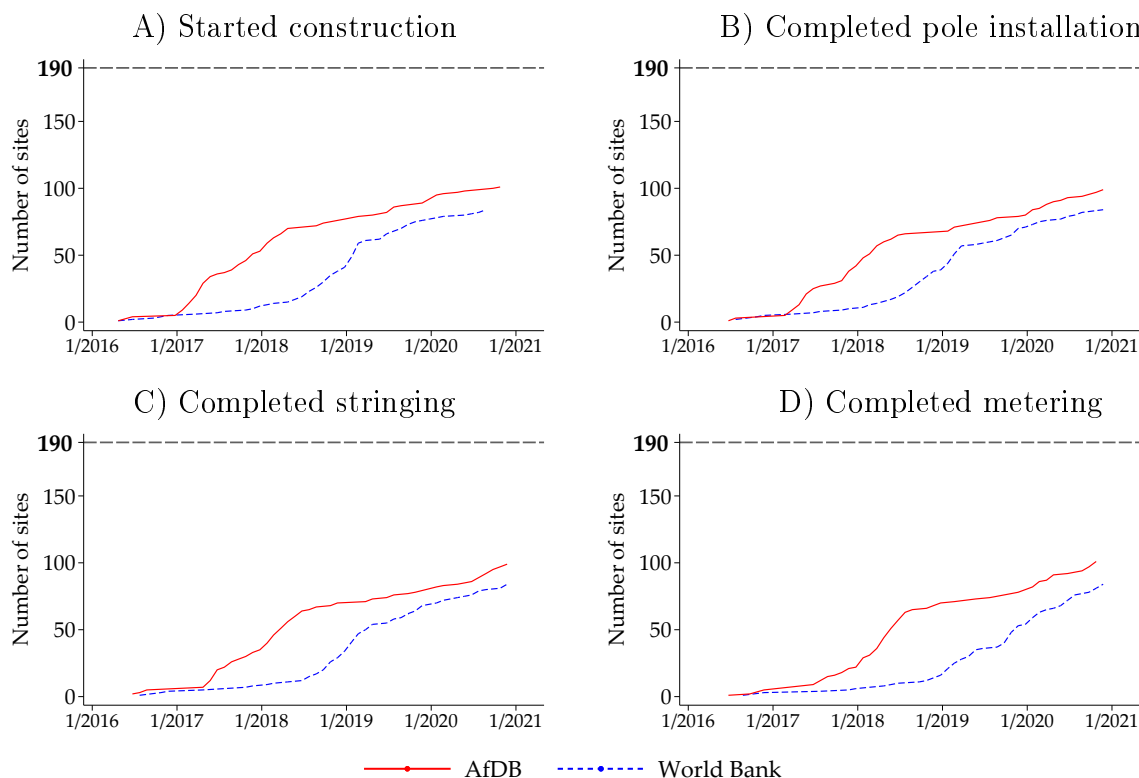
Our primary outcome variables of interest measure outages and voltage quality aggregated at the daily level. Outages are measured as hours of outage per site per day. Voltage quality is measured as the fraction of time voltage is within 10% of Kenya’s nominal voltage of 230V ([IEEE 2019](#)). We therefore code daily voltage data as the fraction of time outside 207–253V. Since the staggered two-month deployment rounds are overlapping, all regressions include day-of-sample fixed effects (in addition to constituency fixed effects) to control for power quality confounds such as weather and demand.

We collect power outage and voltage quality across 150 sites for two months each, staggered between June 2021 and June 2022 by deploying four PowerWatch devices per site at a time. For more information about the GridWatch deployment methodology see [Klugman et al. \(2019\)](#).

2.5 Results

Section [2.5](#) presents extensive margin results: how many sites saw construction, and among these sites, how much construction was completed? The next two sections examine intensive margin results, defined as construction quality conditional on having a physical connection. Section [2.5](#) presents measurements of power outages and voltage quality collected using the GridWatch technology, and Section [2.5](#) presents results from the on-the-ground household and engineering assessments. Section [2.5](#) seeks to disentangle how two key features of conditionality in this context—contracting and auditing—contribute to these results. Section [2.5](#) quantifies correlations between funder, delays, and three potential sources of site selection bias—land gradient, facility type, and time since construction—and concludes that, while some correlations exist, these cannot explain our results. Finally, Section [2.5](#) compares the cost effectiveness of donor conditionality and independent monitoring.

Figure 2.6: Construction progress by funding source



Data for 190 AfDB sites and 190 WB sites located in the five study counties collected through phone surveys with village representatives.

Extensive margin results

Figure 2.6 demonstrates that by December 2020 construction had been completed at significantly fewer WB sites than AfDB sites. Panel A demonstrates that this lag is caused by the initial lag in starting construction, which resulted from the extensive ex ante contracting procedures. The lag between when the median AfDB site and the median WB site started construction was 10 months. This is in line with the timeline shown in Figure 2.1.

Once construction started, WB construction appears to have progressed more rapidly across all three construction phases: pole installation, stringing, and metering completion (Panels B, C, and D of Figure 2.6 respectively). By the end of stringing completion, the lag between WB and AfDB was reduced to six months. Still, the increased speed of construction was not sufficient to fully close the gap. By the end of tracking surveys in late 2020, fully four years after the start of construction, household metering had been completed at 53% of AfDB sites but at only 45% of WB sites. This timeline is in line with Kenya Power’s own nationwide metrics, which reported that “a total 109,880 customers had been connected

under phase one translating to 49% of the target” as of mid-2018 ([Kenya Power \(2018b\)](#)).

Among sites where construction had been completed, there was less construction at WB sites. The average number of customer connections at completed sites is 95 at AfDB sites and 83 at WB sites ($p < 0.10$). The average number of poles is 129 at AfDB sites and 112 at WB sites ($p < 0.05$). [Table A4](#) presents these results in more detail for all sites (not just completed sites). Regressions include constituency fixed effects and scale the field measurements to adjust for the fact that in more densely populated sites we only surveyed a subset of the grid.¹² Columns (5) and (11) show that the difference is caused in part by less construction outside the 600 meter boundary at WB sites, suggesting stricter adherence to the official program requirements.

These increases in delays and lower absolute construction at LMCP sites speak to the costs of WB conditionality. These are important to enumerate but inconclusive on their own from a policy perspective: the higher costs might be worth it if they lead to significant benefits. To examine this rigorously, the following sections examine power quality, construction quality, and household experiences.

Power quality

We collapse the minute-by-minute data collected by the GridWatch devices to 150,000 hourly household-level power state and voltage observations. The primary analyses aggregate these data to 7,500 site-day observations.¹³ Panels A and B of [Figure 2.7](#) present the hours of outage per day and fraction of time experiencing poor voltage quality, respectively, for WB and AfDB sites. Panels C and D estimates a separate coefficient for each day of the sample, as per the following equation:

$$y_{id} = \beta_0 + \sum_{n=1}^{365} \beta_n \gamma_{n=d} \text{WB}_i + \gamma_d + \gamma_c + \epsilon_{id},$$

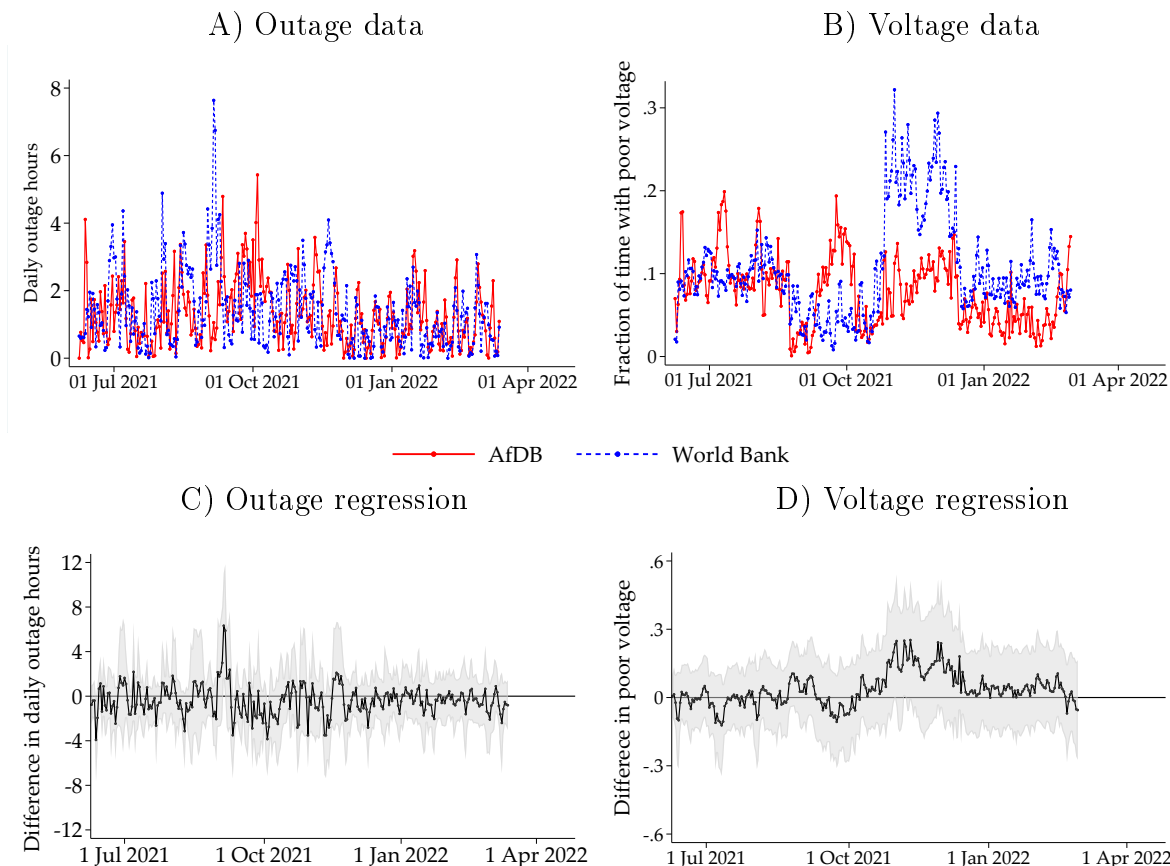
Where y_{id} is either hours of outage or fraction of time with poor voltage quality at site i on day d , WB_i is an indicator variable for whether site i is funded by the WB (as opposed to the AfDB), and γ_d and γ_c are day-of-sample and constituency fixed effects, respectively. Standard errors are clustered by site.

The estimates show no significant difference in power outages or in voltage quality between AfDB and WB sites. WB conditionality does not appear to have caused statistically

¹²At sites that appeared too large to survey, we first recorded the number of distinct branches in the LV that started at the transformers, and then randomly pre-selected the number of branches that the field team is able to complete in the time that was allocated for the site. To obtain site-wide estimates, we scale the on-the-ground measurements according to the fraction of the grid that was surveyed.

¹³7,500 $\neq \frac{150,000}{24}$ because on some days fewer than 24 hours of data were recorded. We include days where at least 12 hours of data is recorded.

Figure 2.7: Reliability and voltage quality by funding source



In panels A and C, outages are defined as hours of outage per day, averaged across sites. In panels B and D, poor voltage is defined as the fraction of time voltage is outside $230 \pm 10\%$ per day, averaged across sites. In the voltage graphs, periods with power outages are missing voltage measurements. The results look similar when coding such periods as having $V=0$, which is technically correct.

or economically meaningful reductions in outages or improvements in voltage quality. [Table A5](#) confirms that there are no significant differences in either outcome even when data are pooled.

Construction procedures and materials might affect power quality through several engineering channels. First, voltage quality tends to worsen with distance from the central transformer, both due to the distance traveled along the LV electricity wire as well as the consumption of electricity by meters connected more closely to the transformer ([Jacome et al. \(2019\)](#)). Second, voltage quality could worsen with the passage of time, as infrastructure ages. Higher quality construction might make infrastructure more resilient and slow any associated decay. We examine the correlation between 10th percentile of voltage quality and distance to the transformer along the LV network, as well as the correlation between voltage

quality and time since construction, for WB sites and AfDB sites separately (Figure B4).¹⁴

The time since construction varies across our sample since stringing was completed between June 2017 and January 2021, while GridWatch devices recorded data between June 2021 and June 2022. We surveyed the WB electricity networks when they were relatively newer. Associated biases, if any, would favor WB sites. We discuss this in Section 2.5. There appears no strong correlation along either dimension, and no difference between funders. At both AfDB and WB sites, the grid appears resilient to aging, at least for the first five years after the completion of stringing.

Table A7 shows that distance along the LV network—expressed in terms of number of connections between the device and the transformer—worsens voltage quality. Donor conditionality does not appear to affect the resilience of voltage quality to the addition of customer connections along the LV network.

Engineering assessment and survey results

While WB conditionality does not impact the quality of household electricity connections relative to AfDB procedures, it may still have improved construction quality outcomes in ways that did not translate to measurable medium-run changes in power quality. Table 2.2 estimates the impact of donor conditionality and independent monitoring—and their interaction—on the engineering measurements and socioeconomic outcome surveys. The primary outcomes are indices of the outcomes described in Section 2.4. All indices are standardized to have a mean of zero and a standard deviation of one. We estimate outcomes 1–3 at the site level using the following estimating equation:

$$y_i = \beta_0 + \beta_1 \text{WB}_i + \beta_2 \text{Treat}_i + \beta_3 \text{Treat}_i \cdot \text{WB}_i + \gamma_c + \epsilon_i$$

Where y_i is the outcome variable for site i , WB_i and Treat_i are indicator variables for whether site i is WB-funded and a Treatment site, respectively. Table 2.2 reports estimates for the variables of interest, namely β_1 , β_2 , and β_3 . γ_c is a constituency fixed effects. Standard errors are clustered by site.

We estimate outcomes 4–11 at the respondent level using the following estimating equation, where y_{ji} is the outcome variable for respondent j (either a firm or a household), located at site i :

$$y_{ij} = \beta_0 + \beta_1 \text{WB}_i + \beta_2 \text{Treat}_i + \beta_3 \text{Treat}_i \cdot \text{WB}_i + \gamma_c + \epsilon_{ij}$$

Table 2.2 shows the results. Two key results stand out. First, construction quality is slightly higher at WB sites. An analysis of the sub-components (Table A8) shows that this is partly a result of improved pole and pole installation quality at WB sites, possibly a result of the WB's intense pole inspections prior to construction. Second, the WB ex ante contracting

¹⁴The results look similar when using mean voltage. Using the 10th percentage of voltage quality is in line with engineering expectations around how resilience might affect voltage quality.

Table 2.2: Primary engineering and socioeconomic outcomes

	WB Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 1: Construction quality index	0.52** (0.25)	-0.27 (0.20)	0.21 (0.36)	208
Outcome 2: Network size and configuration index	0.05 (0.19)	-0.02 (0.18)	0.23 (0.26)	227
Outcome 3: Construction timing index	-1.01*** (0.19)	-0.34* (0.20)	0.36 (0.28)	208
Outcome 4: Household installation quality index	0.03 (0.12)	0.21 (0.13)	-0.19 (0.17)	929
Outcome 5: Household cost, experience, bribery index	0.12 (0.12)	0.09 (0.10)	-0.03 (0.16)	929
Outcome 6: Reliability and safety index	-0.14 (0.13)	-0.04 (0.11)	0.08 (0.19)	929
Outcome 7: Knowledge index	0.09 (0.09)	0.02 (0.10)	-0.01 (0.14)	929
Outcome 8: Electricity Usage index	0.17 (0.13)	0.29** (0.12)	-0.17 (0.17)	929
Outcome 9: Household socioeconomic outcomes index	0.15 (0.11)	0.18 (0.12)	-0.16 (0.17)	929
Outcome 10: Firm Performance Index	0.25 (0.19)	0.09 (0.18)	-0.18 (0.26)	349
Outcome 11: Political and Social Beliefs index	0.03 (0.07)	0.04 (0.09)	-0.02 (0.12)	929

Outcome variables are indices constructed from groups of variables standardized to have mean 0 and standard deviation 1. Each column presents results when the treatment variable is either: (1) WB funding source, or (2) the randomized audit treatment. In rows 1–3, observations are transformer sites; standard errors are shown in parentheses. For rows 4 through 8, observations are occupants of connected compounds. All regressions control for site land gradient and public facility type. Standard errors are clustered by transformer site and shown in parentheses. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$. The sub-components for each index are presented in [Table A8](#), [Table A9](#), [Table A10](#), [Table A11](#), [Table A12](#), [Table A13](#), [Table A14](#), [Table A15](#), [Table A16](#), [Table A17](#), and [Table A18](#).

requirements caused a delay of 0.9 standard deviations, in line with the results presented in [Figure 2.1](#) and [Figure 2.6](#) (delays by construction stage are presented in [Table A10](#)).

While the difference the aggregate network size and configuration index is not significant (Outcome 2; detail provided in [Table A9](#)), Columns (5) and (6) of [Table A4](#) suggest the lower extensive margin construction at WB sites is driven in part by lower construction in the area between 600-700 meters from the transformer. At AfDB sites, we see more frequent construction outside of the 600m boundary. This is in line with [Kassem et al. \(2022\)](#), who find that almost 30% of LMCP households are located beyond 600m of the transformer.

The fact that this occurs less at WB sites indicates more stringent adherence to official LMCP rules, which could be viewed as a positive attribute, however it does result in fewer connections per site. The remainder of the estimates are generally positive but modest in size and not statistically significant.

For Outcome 4 (household installation quality; [Table A11](#)) we replicate the index omitting the question asking the respondent whether they have a readyboard, since it is not obvious whether the presence of a readyboard is a positive or negative component. Its presence simultaneously indicates Kenya Power provisions and a lack of household preparedness (see [Section 2.2](#) for more detail). Outcomes 5, 6, and 7 show little difference in household experiences. As an example, 41% of AfDB respondents and 43% of WB respondents reported having been told that they would have to pay Kenya Power for their electricity connection prior to construction. More detail is provided in [Table A12](#), [Table A13](#), and [Table A14](#).

Disentangling ex ante contracting and ex post auditing

[Table 2.2](#) indicates that the randomized audit treatment increased household electricity usage (Outcome 8) and household socioeconomic outcomes (Outcome 9). An investigation of the index sub-components (shown in [Table A15](#) and [Table A16](#)) shows that these results are driven primarily by increased use of lighting, appliances, and purchases of electricity tokens at treatment sites. In addition, the audit treatment increased the number of poles constructed at AfDB sites but not at WB sites (Columns (2) and (4) of [Table A4](#)). This is consistent with the fact that AfDB contractors were able to change site designs and thus increase construction at treatment sites, whereas WB construction contractors were constrained by the site designs that had already been finalized by a separate contractor.

These benefits came at the cost of a small delay caused by the audit treatment, however this delay is significantly smaller than the delay caused by WB contracting, is statistically significant only at the 10% level. Furthermore, in the aggregate these delays generally do not persist beyond the initial stages ([Figure B5](#)).

We find no difference in firm performance (Outcome 10) or political and social beliefs (Outcome 11) along any dimension. Finally, we find no significant interactions between the WB conditions and the randomized audit treatment.

Robustness

[Section 2.3](#) discussed the mechanism through which villages were arbitrarily assigned which multilateral development bank to be financed through. While assignment is largely uncorrelated with observable characteristics, three notable differences stand out. This section explores the extent to which such selection may drive the results above.

First, WB-funded sites have a 13% higher average land gradient. It is plausible that hilliness slows construction and that this difference explains the WB delays. [Table 2.3](#) therefore examines the correlation between land gradient, funder assignment, and construction

Table 2.3: Impact of gradient and facility type on months to stringing completion

	(1)	(2)	(3)	(4)	(5)
World Bank (=1)	8.2*** (2.3)	9.5*** (2.6)	8.9*** (2.8)	9.6*** (2.7)	9.1*** (3.0)
Land gradient			0.8 (0.9)		1.1 (0.9)
Health center				1.0 (7.5)	5.3 (8.3)
Secondary school				-1.8 (3.7)	-0.9 (3.9)
Primary school				5.4* (2.8)	7.0** (3.1)
Market center				-1.2 (3.4)	-0.7 (3.7)
Religious building				-3.5 (3.6)	-3.7 (3.8)
Other				3.1 (7.3)	9.4 (9.0)
Observations	183	183	168	178	163
Constituency FE	No	Yes	Yes	Yes	Yes

Stringing is completed at WB sites on average 8.1 months later than at AfDB sites. Controlling for land gradient and facility type does not affect these estimates meaningfully, and land gradient and facility type appear largely uncorrelated with time to stringing completion. [Table A19](#) displays the same for months to metering completion. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

delays as measured by time to stringing completion.¹⁵ The results are reassuring. Land gradient is uncorrelated with construction delays, both unconditionally and conditional on funder. The primary delays caused by WB conditionality estimated in Columns (1) and (2) persist in a stable manner when controlling for land gradient in Columns (3) and (5). The lag between WB and AfDB is approximately constant across the entire land gradient support ([Figure B6](#)). The difference in land gradient is therefore unlikely to explain the results.

Second, WB sites are significantly less likely to be located near a secondary school, and more likely to be located near a market center or no public facility at all ([Table A2](#)). Columns (4) and (5) of [Table 2.3](#) confirm that the gap in timing between WB and AfDB sites persists when controlling for facility type. There is no correlation between facility type and delay, with the exception of religious buildings: stringing appears to have been completed approximately five months sooner on average at religious buildings. 40 transformers (11% of sites in our sample) are near religious buildings—14 at WB sites and 26 at AfDB sites—

¹⁵This is our preferred outcome because it captures significant construction process, but has a higher sample size than the subset of sites where metering was completed.

and most are near at least one other public facility as well. The small sample size and balance across funder suggest this is unlikely to be driving any of the observed differences. Furthermore, the gap in timing between WB and AfDB sites is not significantly different across facility types (Table A6). Evaluated together, these analyses make it unlikely that baseline differences in land gradient or facility type contribute meaningfully to the results.

Third, the GridWatch devices recorded data between June 2021 and June 2022, even though stringing at most AfDB sites was completed between 2017 and 2019 and stringing at most WB sites was completed between 2018 and 2020. Thus, the GridWatch data measured WB sites when they were on average newer than the AfDB sites surveyed at the same time. If the aging of the grid negatively affects reliability and voltage quality, then this bias would favor WB in the results. Figure B4 confirms that voltage quality is constant over time, and that the lack of difference in voltage quality between the WB and the AfDB persists even among sites where the time since stringing completion was approximately equal.

Cost effectiveness

AfDB’s turn-key contracting methodology prevents a site-by-site analysis: contract amounts aggregate design, procurement, and construction costs across hundreds of sites, usually spanning several counties, such that even Kenya Power lacks the detailed information about contractor activities that would enable them to dis-aggregate costs. However, we can compare aggregate financial cost across contracts.

We conduct a back of the envelope calculation of the costs of household electrification. The WB and AfDB Phase I aggregate nationwide project costs both equaled USD 150 million (of which approximately 90% was financed through multilateral lending and 10% through GoK contributions). According to Kenya Power’s annual report (2017b), the WB and AfDB Phase I projects sought to connect an additional 314,200 and 296,000 customers, respectively.¹⁶

On aggregate, the costs of the two programs appear to be similar, at approximately USD 500 per connection. This is similar to the USD 739 average total cost per connection that Lee et al. (2020c) estimate under a 100% electrification scenario in a similar area in rural Kenya. The difference could be attributed to implementation efficiencies derived from the nationwide coordination of design, procurement, and construction activities.

Still, these results come with important caveats. Perhaps most importantly, we do not observe leakage. It is possible that WB contracting requirements meaningfully reduce leakage of funds that were recently observed, for example, by Andersen et al. (2022). We cannot observe or rule out differences in funds leakage, but to the extent that this would have reduced the availability of funds for intended construction, this does not appear to have affected construction outcomes.

¹⁶These two numbers are not strictly comparable, as the AfDB targeted the maximization of 5,320 transformers while the WB targeted the maximization of 3,200 transformers in addition to the installation of 1,000 new transformers.

The research team budgeted \$125,000 for data collection at the 380 sites or about \$329 per site. It is worth noting that the level of detail of these data collection efforts far exceeded the intensity of monitoring communicated to contractors. While letters delivered as part of the monitoring treatment only notified contractors of four outcomes that would be monitored, the research team collected data to construct 94 pre-specified outcomes. These data collection expenditures represent approximately one percent of AfDB's total budgeted construction costs per site.

2.6 Conclusion

This research evaluates the impact of differences in donor conditionality set by multilateral organizations when financing government infrastructure investments outsourced to private sector contractors. We study this topic in the context of the Last Mile Connectivity Project, which is one of Kenya's largest public infrastructure construction projects. The roughly USD 600 million cost of the program is financed in large part by international donors, including in particular the World Bank and the AfDB. We exploit quasi-random variation in the assignment of specific communities designated for inclusion in the LMCP to be funded by either the World Bank or the AfDB. Contractors who win bids issued by the World Bank are required to comply with the World Bank's relatively more stringent conditions. To investigate the difference between two key components of donor conditionality—contracting requirements and audits—we implement a randomized audit treatment where contractors are informed of a subset of sites that are selected for monitoring. Our measurements include high-frequency household-level outage and voltage data, on-the-ground engineering measurements of the local electricity network, and household and firm socioeconomic surveys inquiring about the connection experience, knowledge, and electricity usage.

We find that the World Bank's requirements cause significant delays in implementation, with households at WB sites receiving their electricity connections on average 6–10 months later than households in sites that are funded by the AfDB. We can rule out that the conditions causing these delays generated statistically or economically meaningful improvements in power outages or voltage quality. That said, construction quality is modestly higher at WB site, driven in part by higher quality pole installation potentially as a result of the WB's intense pole inspections prior to construction. The estimates on several other key outcomes—such as household installation quality, cost, and electricity usage—are positive but generally modest in size and not statistically significant.

The randomized audit treatment generates a 0.3 standard deviation improvement in household electricity usage and a 0.2 standard deviation improvement in socioeconomic outcomes, while causing significantly fewer delays than the WB approach. This suggests some benefits can be captured from this much less burdensome component of donor conditionality.

These analyses generate several tangible policy recommendations. First, more streamlined upfront contracting could prevent significant delays without necessarily compromising

construction quality. Second, more rigorous auditing—either ex post or ex ante—could help boost construction quality. This is likely especially the case when target contractors participate in a repeated game where poor construction performance could threaten a contractor’s future contracting opportunities.

Several important limitations are worth noting. First, WB conditionality could generate substantial benefits that are unobservable to our research team, such as improved institutional capacity or accounting practices in Kenya public sector organizations. If this were the case, conditionality may still be an important policy tool. Second, the latest we inspect a site is five years after construction. While we see no correlation between construction quality and time since construction, it is possible that construction quality worsens over time, and that the stringent WB contracting procedures will improve grid resilience against such depreciation over a longer time horizon. Finally, Kenya is a relatively high-capacity state in the region, and its internal regulatory system may be sufficiently rigorous so as not to benefit meaningfully from additional WB requirements. It is possible that our results would not hold in a lower-capacity state. Additional research is needed to understand these dimensions and potential impacts of donor conditionality over time and in other settings.

2.7 Transition from Chapter 2 to Chapter 3

This chapter has focused on the quality of electricity infrastructure and services in a mass electrification project, and policies that can improve outcomes for rural households. We have focused on quality related to initial connections for households to the national grid. Many applications of electricity, however, take place in businesses with electricity powering productivity-enhancing equipment. During the COVID-19 pandemic especially, energy for continued health services has been particularly important.

Chapter 3 of this dissertation turns to examining specifically the usage of electricity and the impacts of service interruptions in the healthcare sector in rural Kenya. The geographical coverage will be broader, covering 10 counties in Kenya. Differently from households in Chapter 2, a significant portion of rural health facilities were connected to the grid prior to the Last Mile Connectivity Project’s mass electrification efforts. As a result, quality of service may hinge as much on the reporting and maintenance process as on the quality of initial construction.

Chapter 3

Monitoring Public Service Quality: Electricity Reliability at Kenyan Healthcare Facilities

3.1 Introduction

Electricity is a public service important to the functioning of firms of all sizes in many industries. During the COVID-19 pandemic the continued delivery of health services has depended critically on reliable access to electricity. Health facilities use electricity for numerous functions such as lighting, data management, refrigeration, sterilisation, and provision of running water. While grid connections in sub-Saharan Africa have grown rapidly in recent years, power reliability has varied and in many places remains a critical issue that hurts productivity ([Cole et al., 2018](#)). In the healthcare sector, a 2013 systematic review finds that in eight countries with data in sub-Saharan Africa, only 34 percent of hospitals had reliable electricity in the week prior to the survey ([Adair-Rohani et al., 2013](#)).

In this study, we document the quality of electricity services in Kenya and the impacts of service interruptions on healthcare providers. We focus on rural facilities, which tend to have less reliable power and disproportionately service particularly vulnerable low-income communities. From early 2021 to early 2022, the research team conducted three rounds of surveys tracking electricity reliability in a representative sample of rural health facilities spread across ten counties in Kenya. We find that poor service quality is costly to health facilities in the form of increased monetary costs and diminished quality of services. Power outages are common, with the majority of facilities experiencing an outage in the past 30 days. Voltage fluctuations caused equipment damage at about a third of facilities within the past six months. Back-up power sources, while common, only partially mitigate the negative effects of episodes of poor electricity quality.

This research relates to work examining the role electricity access plays in sustaining

economic activity. A number of studies have examined how electricity access affects household and firm outcomes (see [Lee et al. 2020a](#) for a review), with evidence of impacts especially for women ([Dinkelman, 2011](#)) and richer households ([Lee et al., 2020b](#)). Other work finds that among users, valuation and usage of electricity depends critically on the quality of the electricity supply ([Burgess et al., 2020](#)). This project contributes by examining electricity quality in rural areas, and the consequences of poor quality for health services providers.

Reliable electricity is important for firm output. In Kenya, firm losses caused by outages are estimated to be valued at 5% of annual sales ([The World Bank, 2018](#)). As well, unreliable electricity can cause under-adoption of productive appliances by reducing their economic returns. Some recent work has explored the extent to which poor quality electricity and frequent outages limit access to electricity even for households and businesses that are connected to the grid. We contribute new data and document consequences in the healthcare sector in rural areas, where these issues with service equality are expected to be particularly acute.

3.2 Background

Healthcare facilities in Kenya are registered under one of six levels. Under the Kenya Essential Packages for Health (KEPH) framework, the highest service levels are associated with national and county referral hospitals, while at the lowest levels are community facilities equipped to test and treat for common health issues.

This study focuses on facilities classified under KEPH levels 2, 3, and 4. Level 2 and level 3 facilities are typically local level dispensaries and medical clinics that handle basic outpatient services and may refer patients to higher level facilities for more advanced procedures. Level 4 facilities typically are the lowest level referral hospital in the system, with up to one per sub-county. Just over half of facilities that are classified under levels 2, 3, and 4 are government-run. The remaining facilities are privately owned. They are largely private practices, although faith-based organizations also comprise about one-fifth of the private organizations.

Electricity is critical for a number of services for these facilities. For instance diagnostic testing often requires refrigerated reagents or electric equipment such as for imaging. Incubators for neonatal care require power, as does cold storage for many medicines and vaccines. Many types of productivity enhancing equipment such as lights, computers, and sterilizers also require electricity.

Many of Kenya's health centers were electrified within the last 20 years as part of a nationwide effort to prioritize the connection of public facilities (e.g. markets and school) to the national grid. Only 16% of health facilities were grid-connected by 2003. However, 37% were connected by 2008, 76% by 2016, and 78% by 2018 ([REA, 2008b, 2016; REREC, 2018](#)).

3.3 Data

Sample selection

The sample includes 812 rural health services providers in the Western, Coast, Eastern, and Rift Valley regions of Kenya, with median health facility connected to electricity in 2012. [Figure 3.1](#) illustrates the ten counties from which electrified health facilities were sampled. This paper’s focus is on local clinics and dispensaries (categorized as KEPH level 2), health centers (KEPH level 3), and sub-county hospitals (KEPH level 4). Using the Ministry of Health’s list of all public and private healthcare facilities in Kenya, we randomly sampled 812 facilities that were connected to the electricity grid. This required phone calls conducted in January 2021 to determine whether registered facilities have electricity. Only KEPH level 2, 3, and 4 facilities that were connected to grid electricity were eligible to be chosen to be surveyed.

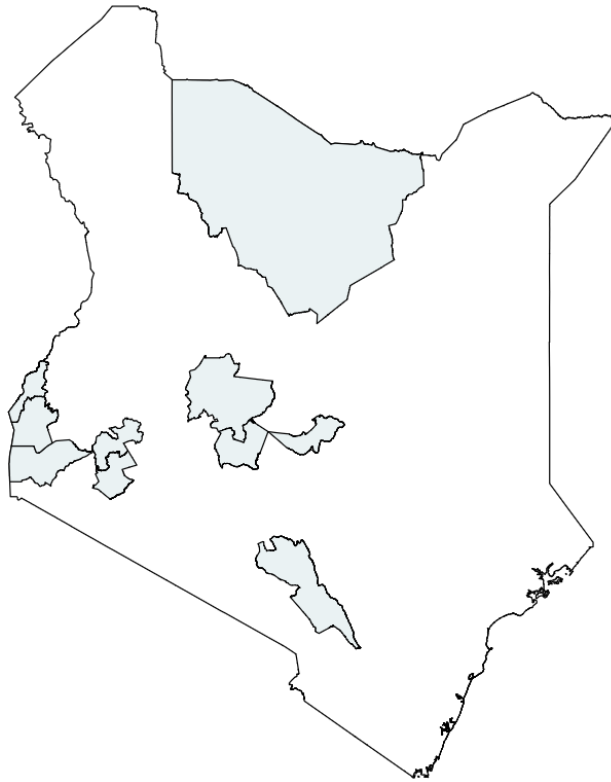
[Table C1](#) shows summary statistics on our sample. The number of initially contacted health facilities is less than 812 partly due to non-response and partly because some facilities in the master list were no longer operational. 77% of facilities were successfully contacted at least once, with most facilities being surveyed in two or all three rounds.

Data collection

Technicians, managers, and other staff at the selected health facilities were recruited to report on power outages, equipment usage and failure, alternative energy sources, and energy costs via phone interviews. We conducted three rounds of surveys to monitor electricity reliability over time between January 2021 and March 2022. This field plan allowed the team to obtain high-quality data without in-person interactions.

The first round of phone surveys began in January 2021; the second round began in April 2021; the third round began in August 2021. Between the second and third rounds of data collection, all data collected to date were shared with Kenya Power staff to facilitate better targeting of limited resources and improve power reliability at health facilities within our sample. Project activities ran concurrently with a training program designed to improve county medical engineers’ understanding of power systems to improve energy safety and reliability during the COVID-19 threat. On average about two engineers from each county participated in the training.

Figure 3.1: Study Area



The study area includes Bomet, Busia, Homa Bay, Kericho, Laikipia, Makueni, Marsabit, Nyandarua, Samburu, and Tana River Counties. In total 812 health facilities are included in the sample.

3.4 Findings

In this section we present the main findings. First we show statistics on the frequency and severity of outages. Next we discuss the impacts of outages and voltage fluctuations on costs and quality of service, in the presence of backup energy sources that health facilities often employ. Lastly we show statistics on the frequency of outage reports to KPLC and discuss reasons for the high numbers of unreported outages.

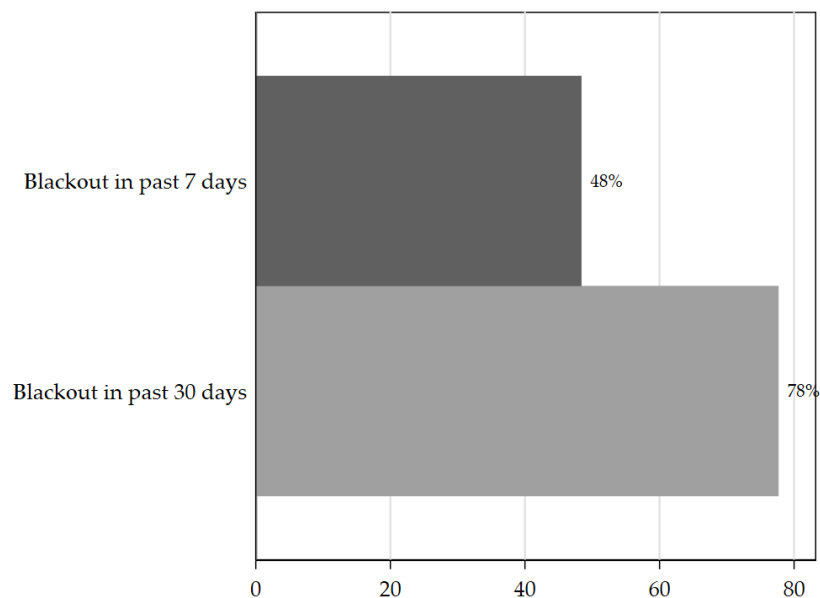
Frequency of power outages

Figure 3.2 shows the percentage of health facilities that experienced any blackout in the past 7 and 30 days. Blackouts are common, the figure shows, with the majority of facilities (78%) experiencing a blackout in the past 30 days. As shown in Figure 3.3, a typical facility experiences about 10 hours of blackout per week. A significant subset of facilities experienced

extended blackouts. 18% percent of facilities experienced 40 hours or more of blackout in a week; 25% of facilities experienced a blackout lasting 1 day or longer within the past 3 months. These patterns of outages at health facilities are similar to patterns documented in [Wolfram et al. \(2022\)](#) among newly electrified rural Kenyan households, suggesting that health facilities and these mass electrification sites in general may face similar infrastructure challenges. [Figure 3.4](#) shows also that these outages are heterogeneously distributed spatially; e.g. Homa Bay and Makueni Counties disproportionately have more severe issues with power outages. As seen in [Figure C1](#), these patterns may also be seen through the lens of the rainy season. Likely due to poor clearance from vegetation and other issues with the infrastructure, outages are more common during the rainy season as uninsulated lines short and imperfect grounding leads to local outages.

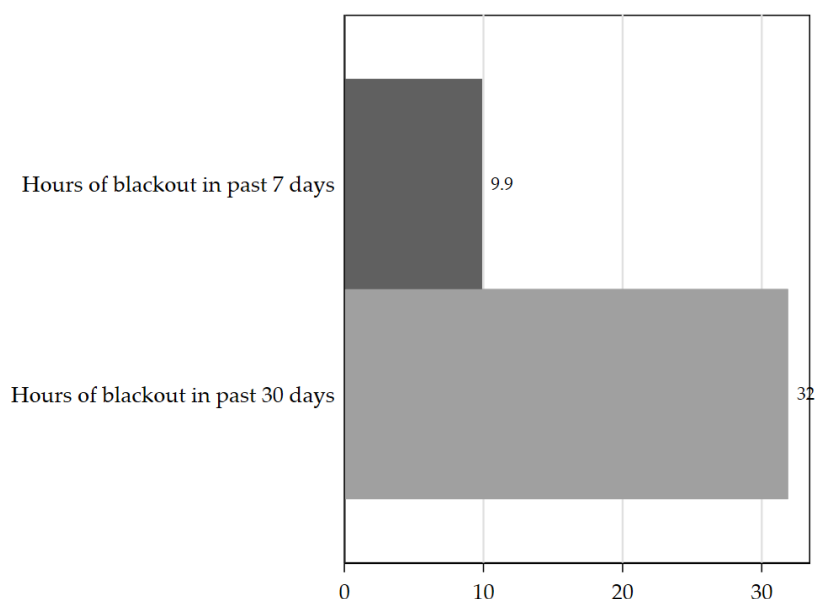
Data from rounds 1 and 2 were shared with regional Kenya Power staff. Internal analysis suggested that overloading and poor maintenance were among leading causes behind the documented outages. As evidenced by blackouts reported in round 3 of the survey, power quality may have improved after the research team reported data to KPLC from rounds 1 and 2 (see [Figure 3.5](#)). We cannot, however, rule out other possible reasons for improved power quality seen in the latest round of data collection.

Figure 3.2: Incidence of blackouts in the past 7 and 30 days



This figure shows the percentage of health facilities in the sample that experienced any blackout in the past 7 days and the past 30 days. N=1271.

Figure 3.3: Hours of blackouts in the past 7 and 30 days



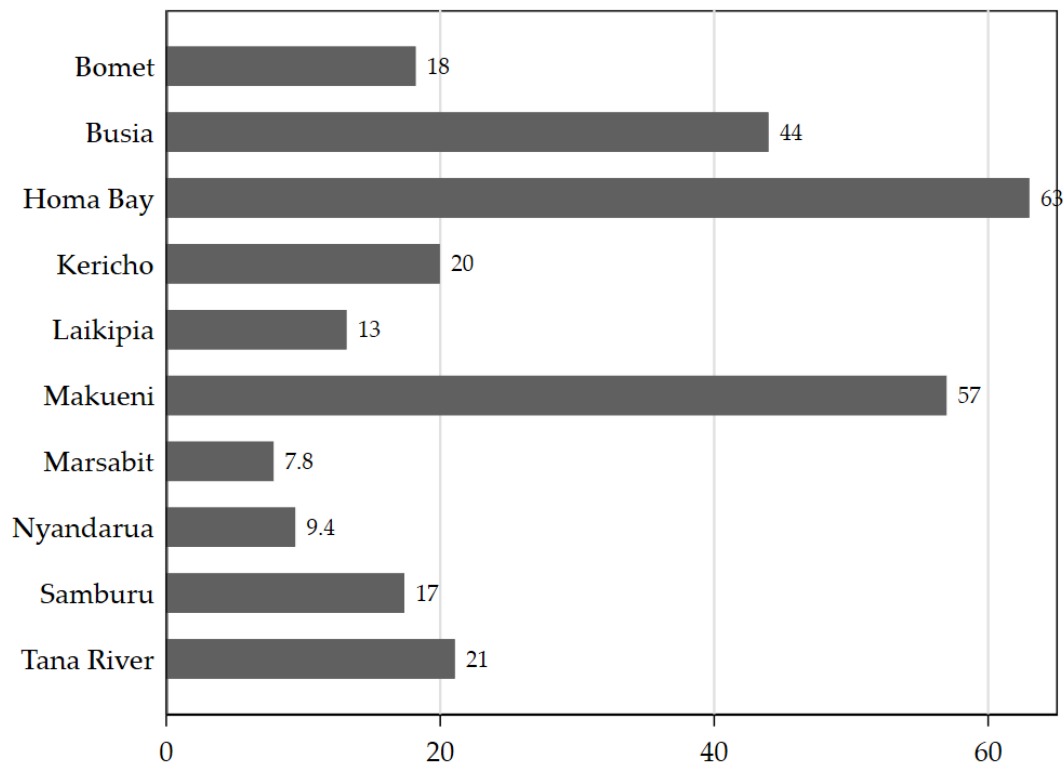
This figure shows the average number of hours of blackouts experienced by health facilities in the sample in the past 7 days and the past 30 days. N=1221.

Implications for costs and service quality

Grid electricity powers a variety of appliances and equipment that is necessary for delivering health services. Loss of power, or excessive voltage fluctuations can leave a health facility without use of essential equipment, or it can damage equipment. In response to grid electricity that is often unreliable, many facilities have backup power systems. As shown in [Figure 3.6](#), 62% of facilities have a generator as a backup power source. 42% have a solar system. In fact, only 1.3% of facilities have no backup power source at all. Slightly less than half of the solar backup systems were financed by private donors.

Despite the ubiquity of backup power sources, facilities report that power outages affect the provision of health services and costs incurred by the facility. 40% indicated that the facility's total energy costs are affected by outages, likely due to the costs of maintaining and running backup power sources. 67% of facilities reported that health services are affected by outages. This can partly be explained by the types of equipment that typically only operates when grid electricity is available. According to our data, a typical facility has on average 3 to 4 types of equipment that do not run when grid power is offline and only backup power is available. Delays in switching to backup power may also disrupt operations, and about 12% of facilities indicated that their backup power source has itself previously

Figure 3.4: Hours of blackouts in the past 30 days, by county



This figure shows the average number of hours of blackouts experienced by health facilities in the sample in the past 30 days. N=1221.

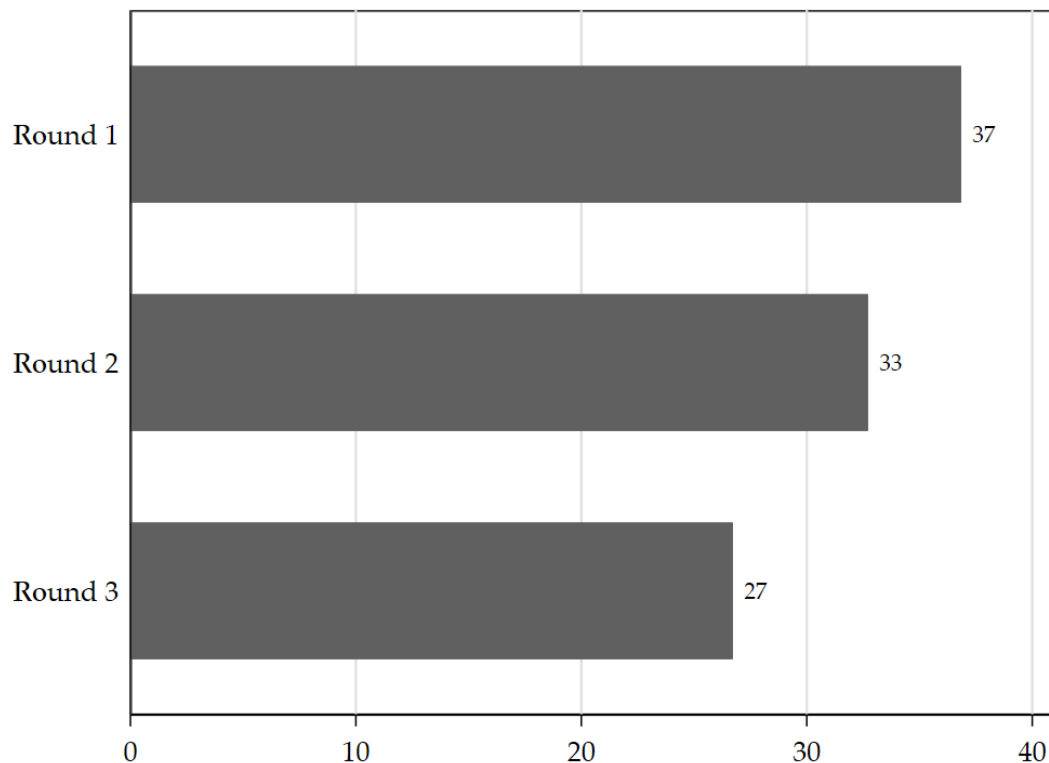
failed. Besides outages, voltage fluctuation can be another source of poor quality electricity. 34% of facilities reported that the facility had equipment that was damaged due to voltage fluctuations within the past 6 months. ¹

Reporting outages to Service Provider

Despite these issues with electricity service quality, we find that not all incidents are reported to KPLC. 36% of facilities with recent outages state that they reported the outages to KPLC. Among those that reported the issue, 91% said that KPLC responded in some way to the report, and 84% said that KPLC managed to fix the issue. We find that facilities experiencing

¹On the extensive margin, we don't see a correlation between the frequency of outages and the number of grid-powered pieces of equipment.

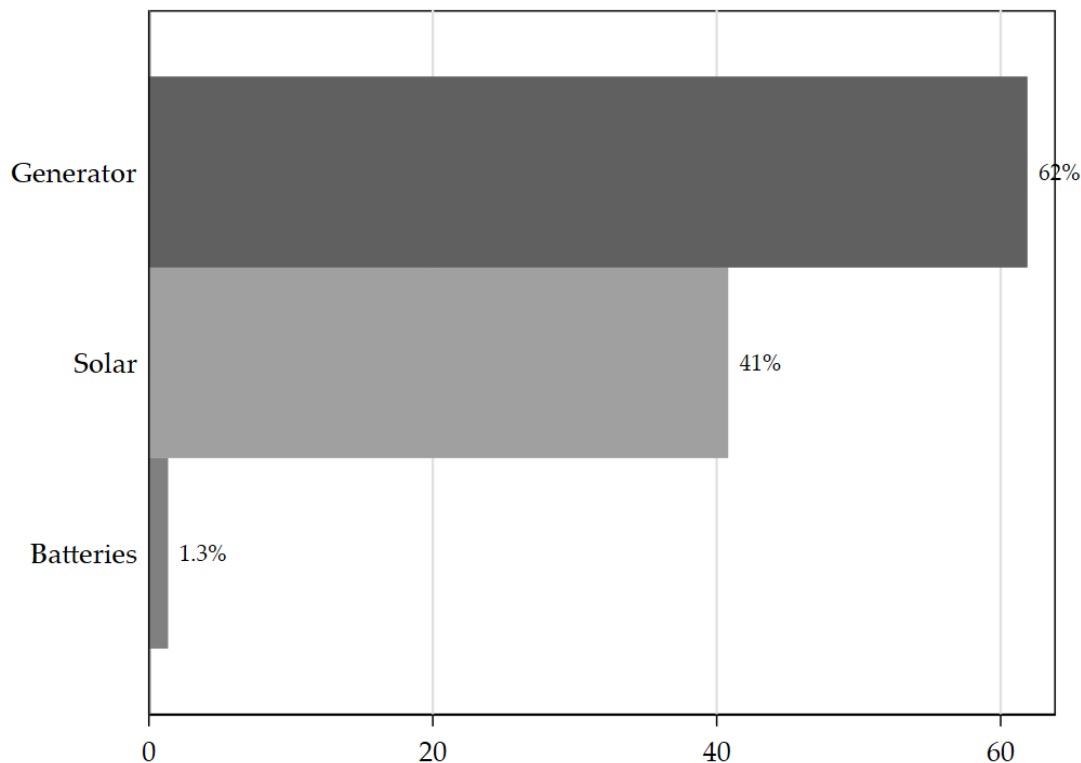
Figure 3.5: Hours of blackouts in the past 30 days, by survey round



This figure shows the average number of hours of blackouts experienced by health facilities in the sample in the past 30 days. The sample is split by survey round. N=1221.

greater problems with blackouts are more likely to report. Among those experiencing a blackout in the past 30 days, facilities with more than 13 hours of outages were more than twice as likely to report the issue to KPLC. Yet, the data show that service interruptions are frequently recurring. 57% of facilities experienced a blackout within 30 days of being surveyed in multiple survey rounds. Reporting an outage, meanwhile, is not associated with fewer future blackouts. These patterns are suggestive that reporting may not have large effects on power quality, particularly for recurring but transient outages, which could limit how frequently facility staff are incentivized to make an effort to report information outages.

Figure 3.6: Backup Power Sources



3.5 Conclusion

This paper studies the quality of electricity at rural health facilities in Kenya. This study is limited by its focus on grid-connected health facilities; many facilities are off-grid without access to the national grid at all and may have different priorities in energy needs. Among grid-connected facilities though, which are the majority of health facilities in Kenya, we document that the quality electricity service – with low quality marked by outages or voltage fluctuations – has effects on the health services provided by facilities as well as their energy costs. Despite the widespread use of backup energy sources by rural facilities, they only partially mitigate the negative effects of power outages. Outages appear to occur regularly for most facilities, especially during rains. The findings suggest that beyond increasing the number electricity connections, increasing the productive use of electricity in rural areas will likely require investments to improve the quality of service that is provided. Short outages are common but often left unreported to the utility company, suggesting that improved

reporting and maintenance processes may help address recurring problems. Future work should explore ways to bolster the quality of electricity service in rural areas, in particular for facilities providing community health services.

Dissertation Conclusion

This dissertation provides new evidence on regulating goods and service quality, with a focus on markets in low-income settings. Chapter 1 provides evidence on how improved consumer information affects quality provision in agricultural input markets in rural Kenya. Chapter 2 examines how contracting conditions affect the quality of electricity infrastructure and the resulting quality of electricity services. Chapter 3 documents consequences of poor electricity service among rural healthcare facilities in Kenya, in spite of mechanisms for end-users to report poor service quality and have infrastructure problems be addressed. Together, these chapters use two markets in rural Kenya as case studies to: (1) examine policies that can improve the regulation of both privately and publicly provided goods and services, and (2) document potential welfare gains that improved regulations could bring about.

These three chapters contribute to the body of knowledge addressing important questions, with applications to settings beyond the context of rural Kenya. Under what market conditions will informed consumers be able to affect on the quality of goods offered by firms? What role can reporting by end-users play in enhancing the efficacy of regulations on goods and service quality? How and in what circumstances should ex-post monitoring be implemented to improve the outcomes of public projects and services? Beyond the partial answers provided in this dissertation there is much room for future work to continue to fill gaps in knowledge on these questions and connect theory and evidence to help inform policymaking.

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Appendix A

Appendix for Chapter 1

A.1 Information campaign details

Figure A1: Treatment #1 Flyer

How to check maize seed quality

Quality markers to look for:

- 1) Clearly displayed company logo together with the seed variety
- 2) Clearly displayed weight of the seed packet. For example: 2kg, 10kg.
- 3) Packaging with no holes. The seed packet should not have broken or taped seals that could allow seeds to be removed or tampered with.
- 4) A printed lot number—this code allows seed packets to be easily traceable
- 5) A recent packaging or testing date. These dates will tell you if the seeds are old. Older seeds may germinate poorly.
- 6) Seeds should not be split or broken, and the coloring on the seeds should not come off easily
- 7) A KEPHIS sticker:



- Check that the SMS code is valid
- Check that the variety, lot number, and testing date match the packaging

Valid	Not valid
OK Monsanto DK8031 Species: Zea mays Variety:DK8031 Lot No: 18-23463HP Class: C1G Testing Date: Jan/2019 More: ghub.ai/zH9d	No 219823200694 IS NOT A VALID CODE. Check and send correct code. The seed may not be genuine. Call 0709891000 or ke@mpedigree.net. More: ghub.ai/X2XD No: <u>261710114026</u> was a Valid Code BUT was used on <u>2019-03-14 14:23:56</u> by <u>7151****8</u>. MPedigree service. hub.goldkeys.net More: ghub.ai/zzfc

Figure A2: Treatment #2 Flyer

**If you are concerned about the quality of your seeds,
you may contact the following:**

- Kenya Plant Health Inspectorate Service (KEPHIS)
Phone Number: 020-3597209
- Anti-Counterfeit Authority (ACA)
Phone Number: 020 2280111
- Your assistant chief

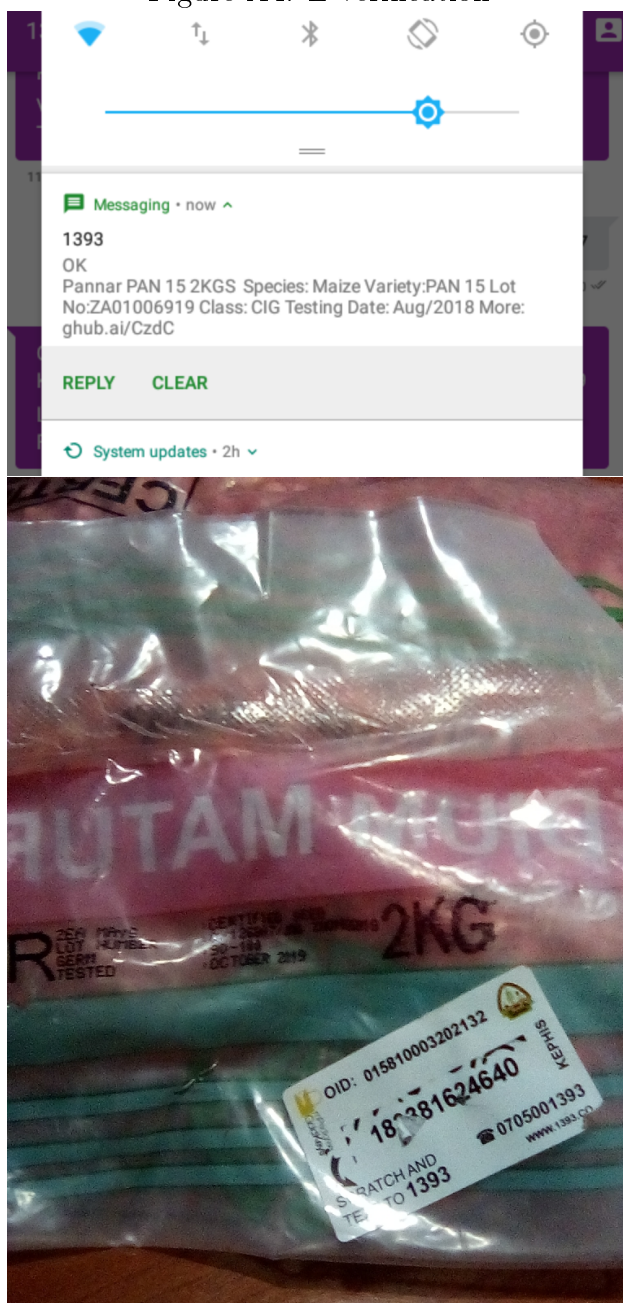
Other Information:

- Not all seeds are of the same quality. It is important to discuss with friends, family, and neighbors the quality of seeds that you purchase. This can help them avoid seeds that may be lower quality.
- When buying and planting maize seeds, it can be helpful to ask the seller for a receipt and keep the empty packet after planting. In case there is any issue with the quality of the seeds, you can refer to these items to confirm the type of seed, as well as the time and place where you purchased the seeds.

Figure A3: Maize variety with and without quality marks



Figure A4: E-verification



A.2 Model details

In [section 1.3](#), the firm's problem is as follows, where we write x as a function of the firm's choice of price p and quality mix q :

$$Max_{p,q}(p - c - d\frac{q}{1 - \theta + q\theta}) * x(p, q) - \frac{1}{2}b\theta(1 - q)^2N - F$$

Accounting for how consumers respond to the firm's choice of p and q , the firm's problem becomes:

$$Max_q(p - c - d\frac{q}{1 - \theta + q\theta})(1 - \theta + \theta q)N - \frac{1}{2}b\theta(1 - q)^2N - F$$

This simplifies to:

$$Max_q(p - c)(1 - \theta + \theta q)N - dqN - \frac{1}{2}b\theta(1 - q)^2N - F$$

Taking derivatives with respect to q , we get:

$$\begin{aligned}\frac{\partial}{\partial q}(\cdot) &= (p - c)\theta N - dN + b\theta(1 - q)N \\ \frac{\partial^2}{\partial q^2}(\cdot) &= -b\theta N \leq 0\end{aligned}$$

So the second derivative of the firm's objective function with respect to q is everywhere weakly negative. Thus, we can define conditions under which the firm chooses $q = 1$ or $q = 0$:

$$\begin{aligned}q^* &= 1 \text{ if } \left. \frac{\partial}{\partial q}(\cdot) \right|_{q=1} = (p - c)\theta N - dN \geq 0 \\ q^* &= 0 \text{ if } \left. \frac{\partial}{\partial q}(\cdot) \right|_{q=0} = (p - c)\theta N - dN + b\theta N \leq 0\end{aligned}$$

An interior solution exists when $(p - c)\theta - d \in (-b\theta, 0)$. In this case, we can solve for the optimal quality:

$$q^* = \frac{(p - c)\theta - d + b\theta}{b\theta} = \frac{(p - c)\theta - d}{b\theta} + 1$$

This relationship is increasing in θ , but this relationship weakens when enforcement is higher.

$$\begin{aligned}\frac{\partial q^*}{\partial \theta} &= \frac{d}{b\theta^2} \\ \frac{\partial^2 q^*}{\partial \theta \partial b} &= -\frac{d}{b^2\theta^2}\end{aligned}$$

A.3 Model with perfect discrimination

We present in this sub-section a variant of the base case model. What if sellers can discriminate between informed and uninformed consumers? For example, sellers may be able to sell lower quality, unless informed consumers reveal themselves by asking about a missing quality marker, in which case the seller is able to offer a different quality mix.

Below, we consider perfect discrimination, where the seller can sell different quality mixes q_1 to informed buyers and q_2 to uninformed buyers. The firm's costs now are:

- $(c + d)\theta N q_1$ for the informed
- $(c + dq_2)(1 - \theta)N$ for the uninformed
- $\frac{1}{2}b\theta(1 - q_1)^2N$ for expected punishment

The firm's problem is:

$$\text{Max}_{p, q_1, q_2} (p - c - d)\theta q_1 N + (p - c - dq_2)(1 - \theta)N - \frac{1}{2}b\theta(1 - q_1)^2N - F$$

Since only the informed will leave the local market or report, the firm's optimal choice for q_2 is zero. Taking the derivative with respect to q_1 , we get:

$$\begin{aligned} \frac{\partial}{\partial q_1}(\cdot) &= (p - c - d)\theta N + b\theta(1 - q_1)N \\ &\geq 0 \end{aligned}$$

Optimal quality for the informed is $q_1^* = 1$. And so the quality that informed consumers are offered doesn't depend on θ .

Also as with the model without seller discrimination, more informed customers is bad for profits:

$$\begin{aligned} \frac{\partial \pi^*}{\partial \theta} &= (p - c - d)N - (p - c)N \\ &= -dN \\ &\leq 0 \end{aligned}$$

Summary of model predictions:

1. Buyers that get informed obtain higher quality
2. Having more informed buyers does not affect market prices ($\frac{\partial p}{\partial \theta} = 0$)
3. Having more informed consumers has a negative effect on seller profit ($\frac{\partial \pi}{\partial \theta} \leq 0$)
4. Uninformed consumers never benefit. They always receive low quality. ($\frac{\partial q_2}{\partial \theta} = 0$)

A.4 Conditions for equilibrium

In [section 1.3](#) we consider the equilibrium in which sellers set price low enough to entice uninformed buyers and informed buyers who are offered high quality to buy locally. Below, we discuss the alternative cases where price is set higher or lower.

Alternative case #1: the firm could choose a lower price to keep even informed consumers who get low quality. Those consumers get $1 - P_0 - S$ in the outside option and $-p$ at the local firm. Therefore, this informed consumer who gets low quality will stay if: $-p \geq 1 - P_0 - S$, or $p < P_0 + S - 1$.

The optimal quality in this case will be $q=0$. The firm's profits are:

$$(p - c)N - \frac{1}{2}b\theta - F$$

Price cannot drop below c or else the firm is sure to exit the market. Thus we can rule out this scenario if buyer's valuation of a high quality product is large enough. We also note that we see a good number of packets with all quality markers, suggesting that we generally do not observe an equilibrium with $q = 0$.

Alternative case #2: the firm chooses a higher price $P_0 + S$ to keep only the informed buyers who get high quality (uninformed buyers feel they are better off getting the higher quality in the outside option). Note that as $\hat{q} - \bar{q}$ goes to zero, this becomes a sub-optimal choice for any values for the parameters, since the firm loses a large segment of customers (the uninformed customers) for a vanishingly small gain in price per unit, and must stock exclusively high quality for all of the informed customers to keep them buying locally and prevent the outside option from being the more attractive option. We note also that the share of uninformed is generally large in our sample. Among control group households, and using either knowledge of expiration date or knowledge of the KEPHIS sticker to mean that the consumers is "informed", we estimate that 84% are uninformed.

A.5 Spillover Effects

In this subsection, we examine whether nearby treatment sites had effects on the major outcomes. To do so, in the main estimating equation to recover treatment effects, we control for the number of study sites within 2, 4, or 6 kilometers, and we also include the number of treated sites within that distance.

In [Table A1](#) we examine possible spillovers on household knowledge.

Table A1: Households knowledge (spillovers)

Panel A: Spillovers within 2km

	(1)	(2)	(3)
	no. quality marks known	e-verif known	expiration known
Number of treatment sites within 2km	-0.035 (0.057)	0.002 (0.018)	0.003 (0.013)
Number of control sites within 2km	0.024 (0.061)	0.027 (0.019)	-0.004 (0.014)
Observations	2305	2305	2305
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes

Panel B: Spillovers within 4km

	(1)	(2)	(3)
	no. quality marks known	e-verif known	expiration known
Number of treatment sites within 4km	-0.019 (0.031)	0.006 (0.010)	-0.004 (0.007)
Number of control sites within 4km	0.018 (0.025)	0.011 (0.008)	-0.001 (0.006)
Observations	2305	2305	2305
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes

Panel C: Spillovers within 6km

	(1)	(2)	(3)
	no. quality marks known	e-verif known	expiration known
Number of treatment sites within 6km	0.014 (0.020)	0.008 (0.006)	0.004 (0.005)
Number of control sites within 6km	0.033** (0.017)	0.007 (0.005)	0.005 (0.004)
Observations	2305	2305	2305
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes

We examine in [Table A2](#) whether treatment may have had other spillover effects. First, we examine effects on firm entry and exit.

As well we examine in [Table A3](#) possible spillover effects on quality markers and lab tested quality.

Effects of Baseline Surveys

We compare control sites (which received baseline surveys but no information campaign) with pure control sites (which received no baseline activities of any kind and were visited for the first time in March to April 2021).

For data from the market audit survey, we have data for the LGS2021 planting season for both control markets and pure control markets. [Table A4](#) shows no statistically significant differences in the numbers of sellers or in the prevalence of quality markers in control markets as compared to pure control markets.

Table A2: Seller Entry/Exit Spillovers (spillovers)

Panel A: Spillovers within 2km

	(1)
	Number of sellers at market
Number of treatment sites within 2km	-0.184 (0.156)
Number of control sites within 2km	0.053 (0.206)
Observations	371
County FE	Yes
HH Baseline Controls	

Panel B: Spillovers within 4km

	(1)
	Number of sellers at market
Number of treatment sites within 4km	-0.101 (0.126)
Number of control sites within 4km	-0.072 (0.077)
Observations	371
County FE	Yes
HH Baseline Controls	

Panel C: Spillovers within 6km

	(1)
	Number of sellers at market
Number of treatment sites within 6km	-0.057 (0.067)
Number of control sites within 6km	-0.038 (0.061)
Observations	371
County FE	Yes
HH Baseline Controls	

Table A3: Quality Markers and Lab Tests Spillovers (spillovers)

Panel A: Spillovers within 1km

	(1)	(2)	(3)	(4)	(5)
	purity	germ_rate	Has all quality markers	purity	germ_rate
Number of treatment sites within 2km	0.014 (0.019)	-1.372 (1.301)	-0.036 (0.050)	0.014 (0.019)	-1.372 (1.301)
Number of control sites within 2km	-0.008 (0.017)	0.019 (1.171)	-0.016 (0.044)	-0.008 (0.017)	0.019 (1.171)
Observations	484	484	662	484	484
County FE	Yes	Yes	Yes	Yes	Yes
HH Baseline Controls					

Panel B: Spillovers within 4km

	(1)	(2)	(3)	(4)	(5)
	purity	germ_rate	Has all quality markers	purity	germ_rate
Number of treatment sites within 4km	-0.009 (0.009)	-0.060 (0.645)	-0.017 (0.024)	-0.009 (0.009)	-0.060 (0.645)
Number of control sites within 4km	0.003 (0.008)	0.080 (0.557)	0.005 (0.021)	0.003 (0.008)	0.080 (0.557)
Observations	484	484	662	484	484
County FE	Yes	Yes	Yes	Yes	Yes
HH Baseline Controls					

Panel C: Spillovers within 6km

	(1)	(2)	(3)	(4)	(5)
	purity	germ_rate	Has all quality markers	purity	germ_rate
Number of treatment sites within 6km	-0.000 (0.006)	0.584 (0.399)	-0.021 (0.015)	-0.000 (0.006)	0.584 (0.399)
Number of control sites within 6km	-0.001 (0.005)	0.472 (0.360)	0.006 (0.014)	-0.001 (0.005)	0.472 (0.360)
Observations	484	484	662	484	484
County FE	Yes	Yes	Yes	Yes	Yes
HH Baseline Controls					

Table A4: Control vs Pure Control: LGS2021 Firm Entry/Exit

Panel A: Full Sample	
	(1)
	Number of sellers at market
Baseline Surveys Administered	0.098 (0.157)
Observations	195
County FE	Yes
Control Mean	1.771

Table A5: Control vs Pure Control: LGS2021 Quality Markers

	(1)	(2)	(3)	(4)	(5)	(6)
	Has all quality markers	Has valid KEPHIS sticker	Lot number present	Has expiration date	Has valid expiration date	No holes
Baseline Surveys Administered	-0.014 (0.058)	-0.025 (0.021)	-0.008 (0.008)	-0.020 (0.057)	-0.030 (0.057)	0.019 (0.019)
Observations	258	254	265	266	266	266
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.616	0.966	0.994	0.662	0.637	0.987

	(1)	(2)
	purity	germ_rate
Baseline Surveys Administered	0.054 (0.039)	3.154* (1.818)
Observations	165	165
County FE	Yes	Yes
Control Mean	99.86	91.90

Table A6: Control vs Pure Control: LGS2021 Prices

	(1)
	Price paid for 2kg hybrid
gotbaseline	-2.635 (7.227)
Observations	266
County FE	Yes
Control Mean	483.2

A.6 Extra Tables

Table A7: Treatment Delivery

	Flyers distributed	Residents trained directly
Avg number per site	173.5	81.7
% of local customers	19.7%	9.1%

Table A8: Baseline Balance

Variable	Control Mean	Treatment Mean	Difference
HH Head is Male	0.700	0.710	0.0100 '(0.02)
HH Head Age	49.97	50.64	0.670 '(0.64)
Completed Primary School	0.620	0.580	-0.04* '(0.02)
Completed Secondary School	0.270	0.230	-0.04** '(0.02)
Home quality index	1.440	1.440	0 '(0.05)
Acres Planted (2019 main season)	1.220	1.180	-0.0400 '(0.07)
Germination rate (2019 main season)	82.03	82.29	0.260 '(0.94)
Hybrid maize yield	930.5	910.0	-20.44 '(69.73)

Sample includes 2431 households surveyed at baseline. P-value for joint test for differences between treatment groups is 0.229. For all tests for differences between the treatment group and the control group, we control for county dummies (site selection was stratified by county) and calculate standard errors clustered at the sub-location level, which is the unit of randomization)

Table A9: Quality markers and seed purity

	Purity	Purity	Purity	Purity	Purity	Purity
Has all quality markers	0.01 (0.02)					
Has valid KEPHIS sticker		-0.04 (0.03)				
Lot number present			-0.03 (0.06)			
Has expiration date				0.04** (0.02)		
Has valid expiration date					0.03* (0.02)	
No holes						-0.05 (0.06)
Constant	99.84*** (0.01)	99.88*** (0.03)	99.87*** (0.06)	99.81*** (0.02)	99.82*** (0.02)	99.89*** (0.06)
Observations	467	464	467	468	468	468

Table A10: Quality markers and germination rate

	Germination Rate	Germination Rate	Germination Rate	Germination Rate	Germination Rate	Germination Rate
Has all quality markers	5.21*** (1.14)					
Has valid KEPHIS sticker		0.92 (2.11)				
Lot number present			-2.58 (4.37)			
Has expiration date				3.24** (1.45)		
Has valid expiration date					6.33*** (1.20)	
No holes						-4.31 (4.36)
Constant	88.08*** (0.96)	90.91*** (2.03)	94.29*** (4.34)	89.03*** (1.33)	86.96*** (1.05)	96.00*** (4.33)
Observations	467	464	467	468	468	468

Table A11: Households knowledge (by gender of household head)

Panel A: Male-headed

	(1)	(2)	(3)
	no. quality marks known	e-verif known	expiration known
treated	0.241*** (0.044)	0.119*** (0.014)	0.029*** (0.010)
Observations	3141	3141	3141
Control Mean	0.58	0.14	0.07
Treatment Effect (%)	41.33	85.96	41.57

Panel B: Female-headed

	(1)	(2)	(3)
	no. quality marks known	e-verif known	expiration known
treated	0.132** (0.056)	0.070*** (0.018)	0.016 (0.013)
Observations	1502	1502	1502
Control Mean	0.51	0.11	0.06
Treatment Effect (%)	25.90	62.88	26.23

Table A12: Households knowledge (by education)

Panel A: At Least Primary School Completed

	(1)	(2)	(3)
	no. quality marks known	e-verif known	expiration known
treated	0.290*** (0.050)	0.116*** (0.016)	0.033*** (0.011)
Observations	2720	2720	2720
Control Mean	0.64	0.16	0.08
Treatment Effect (%)	45.35	72.02	39.74

Panel B: Primary School Not Completed

	(1)	(2)	(3)
	no. quality marks known	e-verif known	expiration known
treated	0.122*** (0.045)	0.097*** (0.015)	0.020* (0.010)
Observations	1923	1923	1923
Control Mean	0.43	0.08	0.05
Treatment Effect (%)	28.23	118.87	43.26

This table shows treatment effects on household knowledge about quality markers. The sample is split according to whether the household head has completed primary school.

Table A13: Household Seed Choice (by season)

Panel A: Long Growing Season 2020

	(1)	(2)	(3)
	Bought hybrid at local market	Bought hybrid elsewhere	Bought hybrid
treated	-0.064** (0.026)	0.052* (0.027)	-0.001 (0.012)
Observations	1833	1882	1882
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	0.49	0.45	0.93
Treatment Effect (%)	-13.13	11.58	-0.15

Panel B: Short Growing Season 2020

	(1)	(2)	(3)
	Bought hybrid at local market	Bought hybrid elsewhere	Bought hybrid
treated	-0.053** (0.021)	0.055*** (0.017)	0.000 (0.023)
Observations	1884	1889	1889
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	0.17	0.10	0.29
Treatment Effect (%)	-30.60	52.41	0.01

Table A14: Household Seed Choice (by education)

Panel A: At Least Primary School Completed

	(1)	(2)	(3)
	Bought hybrid at local market	Bought hybrid elsewhere	Bought hybrid
treated	-0.040* (0.022)	0.052*** (0.020)	0.016 (0.015)
Observations	2247	2275	2275
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	0.31	0.29	0.60
Treatment Effect (%)	-13.15	17.95	2.70

Panel B: Primary School Not Completed

	(1)	(2)	(3)
	Bought hybrid at local market	Bought hybrid elsewhere	Bought hybrid
treated	-0.085*** (0.028)	0.054** (0.027)	-0.027 (0.022)
Observations	1470	1496	1496
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	0.36	0.25	0.62
Treatment Effect (%)	-23.61	21.60	-4.42

This table shows treatment effects on household seed choice. The sample is split according to whether the household head has completed primary school.

Table A15: Household agricultural outcomes (Remote Markets; By Gender)

Panel A: Male-headed Households

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	-1.51 (1.20)	90.30 (72.06)	18.48 (52.28)
Observations	757	748	696
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	84.92	1058.80	987.12
Treatment Effect (%)	-1.77	8.53	1.87

Panel B: Female-headed Households

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	3.30 (2.02)	126.88 (117.79)	202.59** (83.50)
Observations	335	339	305
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	84.77	910.46	792.15
Treatment Effect (%)	3.89	13.94	25.57

Table A16: Seed quality vs gender and education

(1)	(1)	(1)
Germination rate	Germination rate	Germination rate
HH head is male	Completed primary	Above median plot size
1.45* (0.82)	2.40*** (0.78)	-0.47 (0.76)
Constant	Constant	Constant
81.09*** (0.67)	80.65*** (0.60)	82.31*** (0.54)
Observations	Observations	Observations
2317	2317	2317

(1)	(1)	(1)
Can identify low quality	Can identify low quality	Can identify low quality
HH head is male	Completed primary	Above median plot size
0.03* (0.02)	0.05*** (0.01)	0.01 (0.01)
Constant	Constant	Constant
0.12*** (0.01)	0.11*** (0.01)	0.13*** (0.01)
Observations	Observations	Observations
2415	2415	2415

Table A17: Household agricultural outcomes (By Season)

Panel A: Long Growing Season 2020

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	0.02 (0.76)	21.06 (50.03)	18.57 (32.68)
Observations	1748	1709	1631
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	84.91	1058.79	933.97
Treatment Effect (%)	0.03	1.99	1.99

Panel B: Short Growing Season 2020

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	-0.99 (1.35)	21.79 (24.11)	41.99 (72.56)
Observations	526	619	480
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	85.34	284.34	747.31
Treatment Effect (%)	-1.16	7.66	5.62

Table A18: Household agricultural outcomes (By Remoteness)

Panel A: Less Remote

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	-0.95 (1.08)	-40.84 (56.17)	-41.36 (46.33)
Observations	1182	1241	1110
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	85.10	745.45	866.14
Treatment Effect (%)	-1.11	-5.48	-4.78

Panel B: More Remote

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	-0.13 (0.96)	104.61* (61.21)	77.79* (42.52)
Observations	1092	1087	1001
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	84.88	1013.02	928.37
Treatment Effect (%)	-0.15	10.33	8.38

Table A19: Household agricultural outcomes (By Gender)

Panel A: Male-headed Households

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	-0.92 (0.85)	36.15 (46.70)	-3.05 (36.12)
Observations	1591	1617	1473
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	85.09	894.98	918.20
Treatment Effect (%)	-1.08	4.04	-0.33

Panel B: Female-headed Households

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	1.31 (1.34)	41.30 (68.18)	88.61 (57.52)
Observations	683	711	638
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	84.83	747.69	826.87
Treatment Effect (%)	1.54	5.52	10.72

Table A20: Household agricultural outcomes (By Education)

Panel A: At Least Primary School Completed

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	0.22 (0.79)	53.46 (58.50)	13.86 (39.46)
Observations	1380	1395	1276
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	84.66	1010.47	969.79
Treatment Effect (%)	0.27	5.29	1.43

Panel B: Primary School Not Completed

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	-1.04 (1.26)	-0.09 (51.81)	36.02 (43.15)
Observations	894	933	835
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	85.57	592.48	761.76
Treatment Effect (%)	-1.21	-0.02	4.73

Table A21: Household agricultural outcomes (By Plot Size)

Panel A: Above median plot size

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	0.36 (0.98)	62.43 (68.86)	52.33 (40.57)
Observations	1166	1179	1067
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	84.64	1085.80	866.99
Treatment Effect (%)	0.42	5.75	6.04

Panel B: Below median plot size

	(1)	(2)	(3)
	Germination rate	Kgs harvested	Yield
treated	-0.80 (0.94)	-10.44 (38.55)	-7.61 (43.09)
Observations	1108	1149	1044
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	85.41	603.07	916.01
Treatment Effect (%)	-0.94	-1.73	-0.83

Table A22: Household complementary inputs (LGS2020/SGS2020)

Panel A: Full sample	
	(1)
	Used Fertilizer
treated	-0.01
	(0.01)
Observations	2433
County FE	Yes
HH Baseline Controls	Yes
Control Mean	0.94
Treatment Effect (%)	-0.94
Panel B: Less Remote	
	(1)
	Used Fertilizer
treated	-0.02
	(0.02)
Observations	1285
County FE	Yes
HH Baseline Controls	Yes
Control Mean	0.95
Treatment Effect (%)	-2.19
Panel C: More Remote	
	(1)
	Used Fertilizer
treated	0.00
	(0.02)
Observations	1148
County FE	Yes
HH Baseline Controls	Yes
Control Mean	0.93
Treatment Effect (%)	0.47

We split the sample by median distance from the county capital

Table A23: Seller Entry/Exit (LGS2020/SGS2020/LGS2021)

Panel A: Full Sample	
	Number of sellers at market
treated	-0.179* (0.103)
Observations	719
County FE	Yes
Control Mean	1.43
Treatment Effect (%)	-12.52
Panel B: Less Remote	
	Number of sellers at market
treated	-0.291* (0.149)
Observations	365
County FE	Yes
Control Mean	1.56
Treatment Effect (%)	-18.68
Panel C: More Remote	
	Number of sellers at market
treated	-0.050 (0.145)
Observations	354
County FE	Yes
Control Mean	1.25
Treatment Effect (%)	-4.00

All standard errors are clustered by sublocation

Table A24: Seller Entry/Exit (Surveyed Sellers Only; By Baseline Profit)

Panel A: Above median baseline profit		
	(1)	(2)
	lgs_sold_seeds	sgs_sold_seeds
treated	0.003	-0.038
	(0.034)	(0.047)
Observations	236	236
County FE	Yes	Yes
Control Mean	0.92	0.18
Treatment Effect (%)	0.31	-21.51

Panel B: Below median baseline profit		
	(1)	(2)
	lgs_sold_seeds	sgs_sold_seeds
treated	-0.041	-0.099*
	(0.038)	(0.056)
Observations	178	178
County FE	Yes	Yes
Control Mean	0.95	0.27
Treatment Effect (%)	-4.37	-37.24

Table A25: Household reporting

Panel A: Full Sample				
	(1)	(2)	(3)	(4)
	report_yesno_lgssgs	n_report_recip_lgssgs	report_to_auth_lgssgs	report_to_nei_lgssgs
treated	0.012** (0.006)	0.033** (0.014)	0.004 (0.003)	0.012*** (0.004)
Observations	4493	4493	4493	4493
Control Mean	0.04	0.07	0.01	0.01
Treatment Effect (%)	34.59	50.59	37.79	83.72

Panel B: Less Remote				
	(1)	(2)	(3)	(4)
	report_yesno_lgssgs	n_report_recip_lgssgs	report_to_auth_lgssgs	report_to_nei_lgssgs
treated	0.011 (0.008)	0.019 (0.019)	0.013** (0.005)	0.003 (0.006)
Observations	2248	2248	2248	2248
Control Mean	0.03	0.07	0.01	0.02
Treatment Effect (%)	31.01	27.10	124.58	17.91

Panel C: More Remote				
	(1)	(2)	(3)	(4)
	report_yesno_lgssgs	n_report_recip_lgssgs	report_to_auth_lgssgs	report_to_nei_lgssgs
treated	0.013 (0.009)	0.046** (0.020)	-0.003 (0.004)	0.021*** (0.006)
Observations	2245	2245	2245	2245
Control Mean	0.04	0.06	0.01	0.01
Treatment Effect (%)	36.29	73.17	-28.57	182.76

Table A26: Seller Entry/Exit (By Type; SGS2020/LGS2021)

Panel A: Full Sample					
	(1)	(2)	(3)	(4)	(5)
	agrovet_or_stockist	duka	offer_otherseeds	offer_otherinputs	offer_hhgoods
treated	-0.047 (0.052)	0.100** (0.050)	-0.034 (0.056)	-0.009 (0.043)	-0.024 (0.054)
Observations	447	447	447	447	447
County FE	Yes	Yes	Yes	Yes	Yes
Control Mean	0.60	0.29	0.55	0.78	0.38
Treatment Effect (%)	-7.83	33.88	-6.07	-1.08	-6.23

Panel B: Less Remote					
	(1)	(2)	(3)	(4)	(5)
	agrovet_or_stockist	duka	offer_otherseeds	offer_otherinputs	offer_hhgoods
treated	-0.102 (0.075)	0.145* (0.075)	-0.073 (0.078)	-0.016 (0.059)	0.013 (0.075)
Observations	243	243	243	243	243
County FE	Yes	Yes	Yes	Yes	Yes
Control Mean	0.65	0.25	0.58	0.82	0.34
Treatment Effect (%)	-15.81	57.16	-12.50	-1.96	3.74

Panel C: More Remote					
	(1)	(2)	(3)	(4)	(5)
	agrovet_or_stockist	duka	offer_otherseeds	offer_otherinputs	offer_hhgoods
treated	0.044 (0.073)	0.032 (0.070)	0.018 (0.086)	0.023 (0.073)	-0.094 (0.082)
Observations	204	204	204	204	204
County FE	Yes	Yes	Yes	Yes	Yes
Control Mean	0.53	0.37	0.51	0.73	0.45
Treatment Effect (%)	8.40	8.70	3.52	3.17	-20.86

Table A27: Prices for hybrid maize seed (household self-reported)

Panel A: Full sample

	(1)	(2)	(3)
	Avg price per kg	Avg price per kg from local market	Avg price per kg from elsewhere
treated	1.782 (1.917)	0.265 (2.128)	2.816 (2.454)
Observations	2110	1105	974
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	220.22	224.77	219.80
Treatment Effect (%)	0.81	0.12	1.28

Panel B: Less remote

	(1)	(2)	(3)
	Avg price per kg	Avg price per kg from local market	Avg price per kg from elsewhere
treated	2.666 (2.840)	3.569 (3.167)	1.390 (3.793)
Observations	1102	601	498
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	223.89	228.20	222.59
Treatment Effect (%)	1.19	1.56	0.62

Panel C: More remote

	(1)	(2)	(3)
	Avg price per kg	Avg price per kg from local market	Avg price per kg from elsewhere
treated	0.273 (2.358)	-3.319 (2.921)	2.605 (3.414)
Observations	1008	504	476
County FE	Yes	Yes	Yes
HH Baseline Controls	Yes	Yes	Yes
Control Mean	214.73	219.39	215.68
Treatment Effect (%)	0.13	-1.51	1.21

Table A28: Price dispersion for hybrid maize seed (household self-reported)

Panel A: Full sample		
	(1)	(2)
	sd_price_hybridS_h	rng_price_hybridS_h
treated	0.299 (2.075)	-1.900 (4.498)
Observations	198	222
County FE	Yes	Yes
HH Baseline Controls	Yes	Yes
Control Mean	15.15	31.55
Treatment Effect (%)	1.98	-6.02
Panel B: Less remote		
	(1)	(2)
	sd_price_hybridS_h	rng_price_hybridS_h
treated	2.369 (2.753)	9.814 (6.554)
Observations	93	105
County FE	Yes	Yes
HH Baseline Controls	Yes	Yes
Control Mean	15.07	31.04
Treatment Effect (%)	15.72	31.61
Panel C: More remote		
	(1)	(2)
	sd_price_hybridS_h	rng_price_hybridS_h
treated	-0.109 (2.940)	-9.101 (6.224)
Observations	105	117
County FE	Yes	Yes
HH Baseline Controls	Yes	Yes
Control Mean	15.23	32.15
Treatment Effect (%)	-0.72	-28.30

Table A29: Prices for hybrid maize seed (from market audit; All Seasons)

Panel A: Full sample	
	(1)
	Price paid for 2kg hybrid
treated	0.091 (5.102)
Observations	693
County FE	Yes
HH Baseline Controls	Yes
Control Mean	480.30
Treatment Effect (%)	0.02
Panel B: Less Remote	
	(1)
	Price paid for 2kg hybrid
treated	2.115 (6.867)
Observations	390
County FE	Yes
HH Baseline Controls	Yes
Control Mean	479.15
Treatment Effect (%)	0.44
Panel C: More Remote	
	(1)
	Price paid for 2kg hybrid
treated	-2.009 (7.118)
Observations	303
County FE	Yes
HH Baseline Controls	Yes
Control Mean	482.34
Treatment Effect (%)	-0.42

All standard errors are clustered by sublocation. We examine effects on market areas separately in the LGS2020, SGS2020 and LGS2021 planting seasons. We examine effects on above and below median baseline population; below and above median distance to the nearest major market. Keeping in mind that lower-population areas had larger shifts in the market equilibrium, and that sellers may have taken at least one season to shift their entry/exit decisions, we also examine the subset of markets with below median population during LGS2020 and during SGS2020 and LGS2021

Appendix B

Appendix for Chapter 2

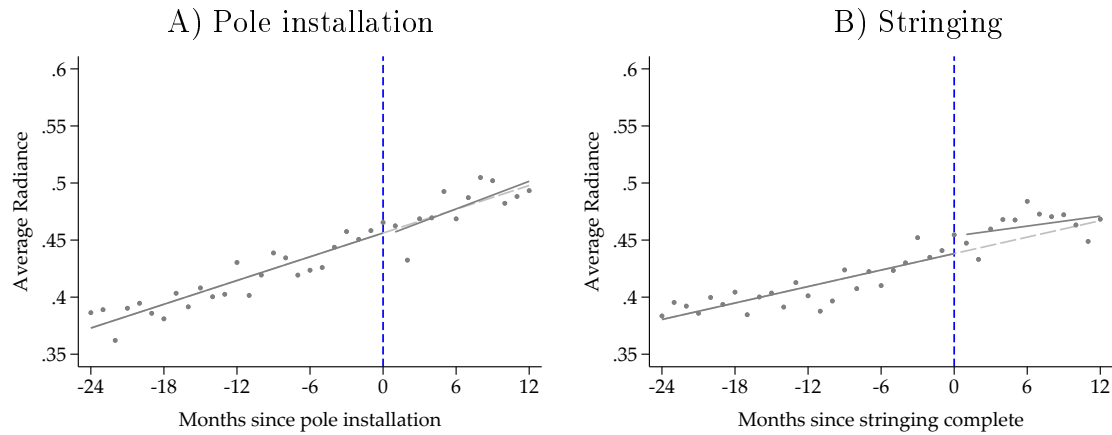
B.1 Appendix Figures

Figure B1: Monitoring Intervention

 THE WORLD BANK IBRD • IDA	 Kenya Power	
Contractor XYZ ADDRESS P.O. Box YYY-ZZZ Nairobi, Kenya June 2017		
TO: CONTRACTOR NAME RE: ENHANCED MONITORING PROGRAM ("EMP") FOR LMCP MAXIMIZATION SITES		
Dear Sir/Madame:		
Kenya Power aims to provide the highest quality of electricity to all Kenyans. To achieve this goal, an international team of engineers will closely audit the quality of construction at a number of Last Mile Connectivity Project ("LMCP") maximization sites. These independent audits will be performed as part of the Enhanced Monitoring Program ("EMP"), and will target both African Development Bank and World Bank project sites. The results of the EMP audits will be shared with project supervisors, financiers, and international agencies, all of which may impose consequences on future contracting opportunities, as they see fit.		
Upon project completion, EMP technicians will extensively measure the quality of various aspects of construction, including: - Distance between poles - Line sag - Quality of connection between transformer and LV wiring - Blackouts and electricity reliability post-connection		
We wish to inform you of the sites that have been awarded to you that have been selected for the EMP. Please find attached a list of these sites.		
Sincerely yours,		
 L Senior Energy Specialist The World Bank	 S Principal Power Engineer The African Development Bank	 J Electrification Project Manager Kenya Power & Lighting Company

This figure displays the monitoring intervention sent to contractors. Each contractor's name, contact information, and site details were entered individually. The names and positions of the relevant representatives from Kenya Power, the World Bank, and the AfDB were entered, and the letter was signed by these parties. The letters were then hand-delivered to management at the relevant contractors by members of our research team to ensure receipt, together with the list of treatment sites referenced in the letter.

Figure B2: Event study: nightlights after construction progress (pole installation and stringing)



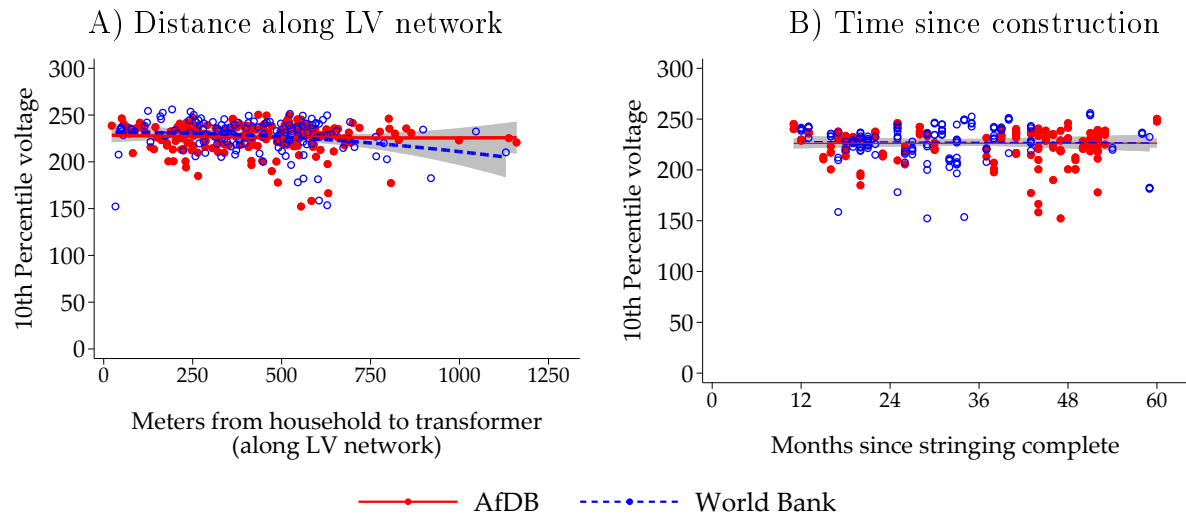
Data on construction progress for 190 AfDB sites and 190 WB sites located in the five study counties collected through phone surveys with local village representatives. Nighttime radiance data from VIIRS.

Figure B3: A PowerWatch device



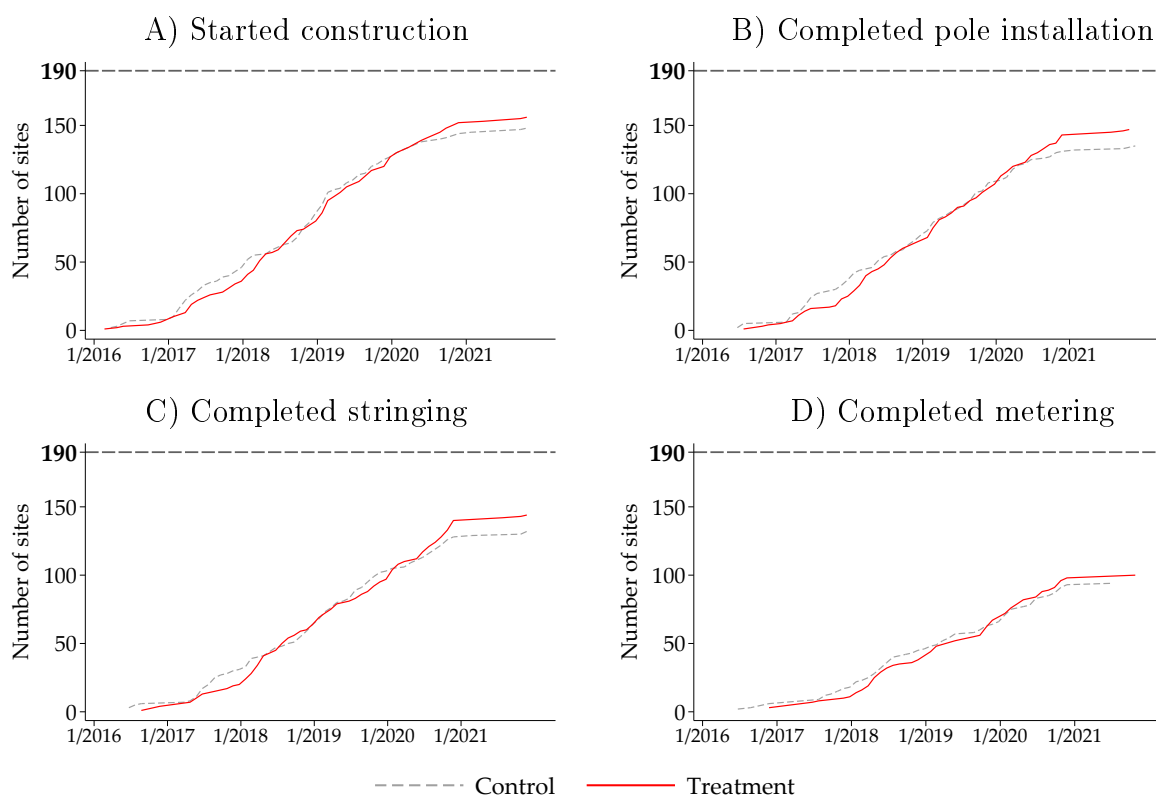
A PowerWatch device, part of nLine's GridWatch technologies used to measure household-level power outages and voltage.

Figure B4: Voltage quality resilience to distance and infrastructure aging



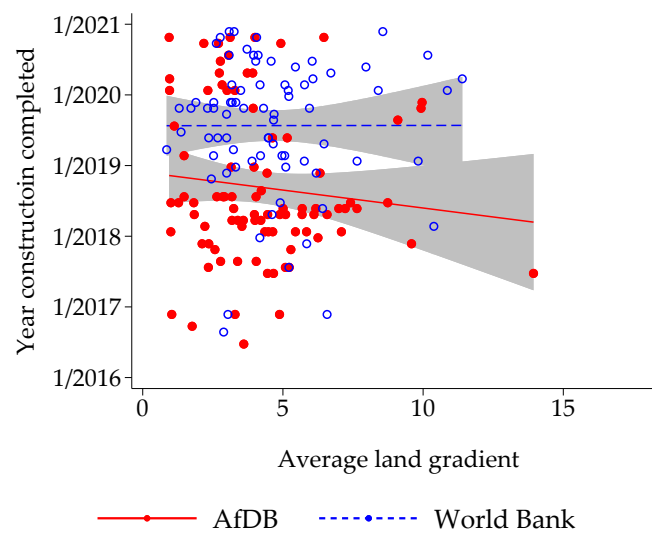
Panel A explores how a household's distance to the central transformer (as measured along the LV network) affects voltage quality. Panel B explores how the passage of time since the initial completion of construction affects voltage quality. Neither appear to strongly affect results. WB and AfDB exhibit similar trends.

Figure B5: Construction progress by audit treatment status



Data for 190 control sites and 190 treatment sites located in the five study counties collected through phone surveys with village representatives.

Figure B6: Construction delays and land gradient



Average land gradient is calculated for each site over the 600 meter radius around its transformer. Land gradient is uncorrelated with construction delays, both unconditionally and conditional on funder. The lag between WB and AfDB is approximately constant across the entire land gradient support. Data source: Shuttle Radar Topography Mission (SRTM) Global Digital Elevation Model. Gradient is measured in degrees from 0 (perfectly flat) to 90 degrees (perfectly vertical) ([Dinkelman \(2011\)](#)).

B.2 Appendix Tables

Table A1: Geographic balance of World Bank and African Development Bank sites

	VIIRS Radiance		Land Gradient	
	(1)	(2)	(3)	(4)
World Bank (=1)	-0.006 (0.065)	-0.027 (0.065)	0.989*** (0.309)	0.573** (0.239)
Observations	51446	51446	347	347
Month FE	No	Yes	No	No
Constituency FE	No	Yes	No	Yes
Control Mean	.41	.41	4.36	4.36

Columns (1) and (2) estimate monthly average site-level nighttime radiance measured using VIIRS averaged across the 600 meter radius. Standard errors are clustered by site. Columns (3) and (4) estimate average site-level land gradient recorded using 90-meter Shuttle Radar Topography Mission (SRTM) Global Digital Elevation Model, a measure of site mountainousness. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A2: Transformer facility type

<i>Panel A) Sample field data</i>			
	N	AfDB Mean (SD)	WB (SE)
Health center	250	0.05 (0.22)	-0.00 (0.03)
School	250	0.50 (0.50)	-0.13* (0.07)
Market center	250	0.17 (0.38)	0.09* (0.05)
Religious building	250	0.20 (0.40)	-0.10* (0.05)
Other	250	0.08 (0.28)	-0.03 (0.04)
None	250	0.27 (0.44)	0.12* (0.06)
<i>Panel B) Sample administrative data</i>			
	N	AfDB Mean (SD)	WB (SE)
Health center	378	0.06 (0.24)	-0.03 (0.02)
School	378	0.09 (0.29)	0.18*** (0.04)
Market center	378	0.13 (0.33)	0.03 (0.04)
Religious building	378	0.05 (0.22)	-0.03 (0.02)
Other	378	0.09 (0.29)	0.03 (0.03)
None	378	0.08 (0.27)	0.29*** (0.04)
<i>Panel C) Nationwide administrative data</i>			
	N	AfDB Mean (SD)	WB (SE)
Health center	378	0.06 (0.24)	-0.03 (0.02)
School	378	0.09 (0.29)	0.18*** (0.04)
Market center	378	0.13 (0.33)	0.03 (0.04)
Religious building	378	0.05 (0.22)	-0.03 (0.02)
Other	378	0.09 (0.29)	0.03 (0.03)
None	378	0.08 (0.27)	0.29*** (0.04)

Most transformers were constructed between 2005-2015 through a nationwide program by Kenya's Rural Electrification Authority to connect public facilities to electricity. We test whether transformers connected to certain types of facilities were more or less likely to be assigned to WB or AfDB funding. Total shares can exceed 1 because some transformers are located near multiple public facilities. We test this separately using field data collected during our surveys, administrative data for our entire sample, and nationwide administrative data. All regressions include constituency FE.

Table A3: Summary statistics

	Mean	SD	25 th	50 th	75 th	N
Transformer missing fuse	0.23	0.42	0	0	0	250
Number of transformer lines	3.13	0.99	3	3	4	250
Number of poles	87.24	36.68	61	82	109	206
Number of leaning poles (<85deg)	1.94	2.74	0	1	3	206
Number of cracked poles	23.31	18.19	11	19	32	206
Number of stays	55.94	25.11	38	53	71	206
Households surveyed	3.83	1.60	3	4	5	250
Connected households surveyed	3.20	1.63	2	3	4	250
Year households connected	2018.85	1.16	2018	2019	2020	184

Summary statistics for surveying sites.

Table A4: Poles and customer connections per site

<i>Panel A) Poles</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
World Bank (=1)	-16.0*	-1.9	-13.1	-0.1	-2.9***	-1.9
	(8.2)	(11.7)	(7.9)	(11.3)	(0.9)	(1.3)
Treatment (=1)	10.2	23.2**	10.1	22.1**	0.2	1.1
	(8.1)	(11.2)	(7.8)	(10.7)	(0.9)	(1.2)
World Bank X Treatment		-27.2*		-25.2		-2.0
		(16.2)		(15.6)		(1.8)
Observations	228	228	228	228	228	228
600 M Boundary	All	All	Inside	Inside	Outside	Outside
Control Mean	84.70	84.70	81.28	81.28	3.42	3.42

<i>Panel B) Customer connections</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
World Bank (=1)	-14.5*	-11.5	-12.7	-10.3	-1.8**	-1.2
	(8.3)	(11.9)	(8.2)	(11.7)	(0.7)	(1.0)
Treatment (=1)	5.9	8.7	5.9	8.0	0.0	0.7
	(8.2)	(11.3)	(8.0)	(11.1)	(0.7)	(1.0)
World Bank X Treatment		-5.8		-4.5		-1.3
		(16.4)		(16.1)		(1.4)
Observations	228	228	228	228	228	228
600 M Boundary	All	All	Inside	Inside	Outside	Outside
Control Mean	61.42	61.42	58.77	58.77	2.65	2.65

Panel A studies the number of poles and Panel B studies the number of customer connections. Counts account for the fact that the grid was often too large to be fully covered by field officers, and instead only a randomly selected subset was surveyed: at 93% of sites we surveyed at least 50% of the network. The mean and median portion surveyed were both two-thirds. All regressions include constituency FE. Standard errors shown in parentheses. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A5: Donor and audit impacts on outages and voltage

	Outage			Voltage		
	(1)	(2)	(3)	(4)	(5)	(6)
World Bank (=1)	0.280*		0.076	0.018		-0.004
	(0.159)		(0.184)	(0.018)		(0.027)
Treatment (=1)		-0.056	-0.269		-0.014	-0.037
		(0.140)	(0.212)		(0.024)	(0.030)
World Bank (=1) × Treatment (=1)			0.426			0.046
			(0.351)			(0.046)
Observations	7203	7203	7203	6829	6829	6829
Control Mean	1.24	1.18	1.18	.1	.09	.1

Columns (1)-(3) display hours of outage per day. Columns (4)-(6) display the fraction of time electricity is supplied with no or bad voltage (defined as being outside $\pm 10\%$ of nominal voltage, 230V. Power quality is measured using GridWatch technology. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A6: Heterogeneity in WB delay by facility type

	Time to stringing completion (months)			
	(1)	(2)	(3)	(4)
World Bank (=1)	18.5	9.4	8.8*	-6.0
	(13.5)	(6.6)	(5.0)	(5.3)
Observations	9	69	53	17
Control Mean	41.5	53.16	50.52	43.1
Sample	Health centers	Schools	Market centers	Religious buildings

While there are small differences between funder type in the facility type associated with each transformer (Table A2) this does not drive heterogeneity in the impact of WB conditionality on construction delays when compared with AfDB sites.

Table A7: Resilience of voltage to distance from transformer

	(1)	(2)	(3)
Distance Along Wire	-0.000 (0.003)	-0.000 (0.003)	-0.000 (0.003)
Customer Connections	-0.490*** (0.160)	-0.490*** (0.163)	-0.615*** (0.230)
World Bank		0.043 (1.305)	-0.788 (2.741)
World Bank=1 $\times$ Distance Along Wire			-0.002 (0.008)
World Bank=1 $\times$ Customer Connections			0.261 (0.347)
Constant	237.937*** (1.345)	237.918*** (1.459)	238.452*** (1.507)
Observations	377314	377314	377314
Control Mean	235.69	235.69	235.69

Standard errors are clustered by respondent and shown in parentheses. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A8: Construction quality

	AfDB Mean	World Bank Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 1: Construction quality index	0.00 [1.00]	0.52** (0.25)	-0.27 (0.20)	0.21 (0.36)	208
* Transformer does not have bypassed fuse (=1)	0.40 [0.49]	-0.15* (0.08)	-0.07 (0.08)	0.03 (0.12)	250
Pole does not have a crack ≥ 1 cm (=1) (of accessible poles)	0.72 [0.45]	0.06 (0.03)	0.00 (0.03)	-0.01 (0.05)	17776
Pole leaning at ≥ 85 degrees (=1)	0.97 [0.16]	0.01 (0.00)	-0.00 (0.00)	0.01 (0.01)	17972
Line has ≥ 0.5 m horiz clearance (=1) (of poles w/ conductor)	0.93 [0.25]	-0.03*** (0.01)	-0.02 (0.01)	0.03* (0.02)	16657
Pole has cap (=1) (of non-concrete poles)	0.28 [0.45]	0.34*** (0.05)	0.04 (0.04)	-0.04 (0.07)	15193
Stay/strut properly installed (=1) (of poles with a stay or strut)	0.92 [0.28]	-0.00 (0.02)	-0.01 (0.02)	-0.01 (0.03)	2627
Stay/strut required, installed (=1) (of poles requiring stay/strut)	0.76 [0.43]	0.21*** (0.05)	0.04 (0.05)	-0.03 (0.06)	8068
Insulator properly installed (=1) (of poles w stays insulated)	0.99 [0.08]	-0.04** (0.01)	-0.02*** (0.01)	0.02 (0.02)	2523
Insulator required and installed (=1) (of poles with a stay)	0.98 [0.12]	0.00 (0.01)	-0.00 (0.01)	-0.02 (0.01)	2551
Pole has grounding wire (=1)	0.34 [0.47]	0.03* (0.02)	-0.03** (0.01)	0.03 (0.02)	17972

The construction quality index (shown here in row 1) is a standardized average of sub-components shown in the remaining rows. Transformer bypassed fuse is measured once at each site. All other outcomes are measured for all poles measured in the engineering assessment survey (described in Section 2.4). Standard errors are clustered by site. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A9: Network size and configuration

	AfDB Mean	World Bank Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 2: Network size and configuration index	-0.09 [1.19]	0.06 (0.23)	-0.04 (0.22)	0.34 (0.33)	226
Deviation in Pole Count (relative to design)	68.76 [62.49]	4.51 (12.76)	3.40 (12.82)	2.78 (17.91)	192
Deviation in Drop Cables (relative to design)	39.03 [26.11]	3.52 (7.10)	-0.84 (5.74)	-0.27 (9.90)	174
Fraction of compounds at site, within 100m of LV line, electrified	0.89 [0.12]	-0.02 (0.03)	-0.01 (0.03)	0.05 (0.04)	227
Fraction of poles ≤ 600 m from transformer	0.94 [0.08]	0.02 (0.02)	0.00 (0.01)	0.01 (0.02)	227

The network size and configuration index (shown here in row 1) is a standardized average of sub-components shown in the remaining rows. All outcomes are measured at the site level. Compound data is collected in the household and firm survey data (described in Section 2.4). Pole data is collected in the engineering assessment survey (described in Section 2.4). * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A10: Construction timing

	AfDB Mean	World Bank Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 3: Construction timing index	-0.00 [1.00]	-1.01*** (0.19)	-0.34* (0.20)	0.36 (0.28)	208
LMCP construction start date (months since Jan 2015)	37.22 [11.38]	10.82*** (1.91)	4.39** (1.92)	-2.71 (2.75)	250
Pole erection completion date (months since Jan 2015)	45.11 [15.06]	9.24*** (2.68)	2.93 (2.69)	-1.28 (3.81)	248
Stringing completion date (months since Jan 2015)	46.82 [15.37]	8.74*** (2.76)	2.14 (2.66)	-0.95 (3.80)	247
Metering completion date (months since Jan 2015)	47.56 [14.33]	13.49*** (2.82)	2.66 (2.75)	-3.95 (3.84)	206
months between construction start and pole erection complete	7.75 [10.20]	-1.35 (2.16)	-1.30 (1.87)	1.27 (2.73)	248
months between pole erection complete and stringing complete	1.89 [4.41]	-0.82 (0.75)	-0.57 (0.67)	0.17 (0.90)	245
months between stringing complete and metering complete	0.88 [8.05]	5.50*** (1.63)	0.07 (1.46)	-1.61 (1.97)	204

The construction timing index (shown here in row 1) is a standardized average of sub-components shown in the remaining rows. All outcomes are measured at the site level and collected via surveys with village representatives (described in section 2.4). * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A11: Household installation quality

	AfDB Mean	World Bank Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 4: Household installation quality index	-0.01 [1.01]	0.03 (0.12)	0.21 (0.13)	-0.19 (0.17)	929
Outcome 4 (omitting readyboard question)	-0.02 [1.01]	0.13 (0.12)	0.21 (0.13)	-0.23 (0.17)	929
Electricity has flowed to this household (=1)	0.81 [0.39]	0.04 (0.06)	0.08 (0.06)	-0.03 (0.07)	941
Household has ≥ 1 meter (=1)	0.85 [0.35]	0.09** (0.04)	0.07* (0.04)	-0.06 (0.06)	941
Household has meter that has worked (=1)	0.77 [0.42]	0.05 (0.06)	0.10* (0.06)	-0.03 (0.08)	940
Household has a readyboard (=1)	0.26 [0.44]	-0.15*** (0.04)	0.01 (0.05)	0.06 (0.07)	941
(-) Number of unrequested meters (of hhs w/ meter)	0.51 [0.50]	-0.03 (0.07)	0.09 (0.06)	-0.00 (0.09)	710
(-) Weeks from paperwork to receiving meter (of hhs w/ meter)	13.62 [25.15]	4.76* (2.83)	-1.74 (2.44)	3.24 (3.41)	881
(-) Weeks from meter to receiving electricity (of hhs with elec)	2.43 [4.13]	-0.14 (0.44)	-0.73 (0.49)	1.63** (0.74)	758

The household installation quality index (shown here in rows 1 and 2) is a standardized average of sub-components shown in the remaining rows. Row 2 omits the readyboard question as it is the presence or absence of a readyboard is not strictly an indication of quality. All outcomes are measured at the household level and collected in the household and firm survey data (described in Section 2.4). For outcomes marked with a (-), a higher value indicates a lower quality. For all other outcomes, a higher value indicates higher quality. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A12: Household cost, experience, and bribery

	AfDB Mean	World Bank Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 5: Household cost, experience, bribery index	0.03 [0.99]	0.12 (0.12)	0.09 (0.10)	-0.03 (0.16)	929
Days given to fulfill paperwork reqs (of LMCP hh)	41.95 [79.67]	15.91 (13.46)	-1.11 (11.31)	5.30 (18.22)	827
Did not require own wiring before connection (=1)	0.77 [0.42]	-0.03 (0.05)	0.01 (0.05)	-0.06 (0.07)	853
(-) KSH spent on wiring (of hh that did wiring) (w)	7785.42 [6786.77]	-1094.02 (728.48)	-791.64 (732.23)	1425.01 (1024.53)	706
(-) Up-front connection payment (Ksh) (w)	6651.15 [8889.12]	-796.05 (858.83)	-569.46 (911.14)	1161.68 (1233.75)	922
Connected by KPLC/REA (=1)	0.98 [0.13]	0.01 (0.02)	0.01 (0.01)	0.01 (0.02)	834
Was not asked for bribe (=1)	0.91 [0.29]	0.02 (0.03)	-0.01 (0.03)	-0.01 (0.04)	941
Didn't do unpaid manual labor for connection (=1)	0.96 [0.19]	-0.02 (0.02)	-0.00 (0.02)	0.05* (0.03)	926
(-) Amount paid so far in installments (Ksh) (w)	2700.19 [4536.35]	-173.80 (533.20)	-145.72 (513.96)	-199.13 (730.18)	876
Satisfaction with electricity installation (1-5 scale)	4.21 [1.08]	-0.03 (0.13)	0.08 (0.13)	-0.03 (0.18)	941
(-) Hours in past month with very low voltage	1.59 [6.64]	2.51 (1.82)	-1.91 (1.65)	3.23 (2.41)	599
(-) Repair costs for devices damaged b/c electricity (Ksh)	31.49 [207.08]	-10.70 (29.67)	-65.88** (32.50)	28.09 (39.40)	601

The household cost, experience, and bribery index (shown here in row 1) is a standardized average of sub-components shown in the remaining rows. All outcomes are measured at the household or firm level and collected in the household and firm survey data (described in Section 2.4). For outcomes marked with a (-), a higher value indicates a lower quality. For all other outcomes, a higher value indicates higher quality. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A13: Household and firm reliability and safety

	AfDB Mean	World Bank Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 6: Reliability and safety index	0.01 [1.00]	-0.14 (0.13)	-0.04 (0.11)	0.08 (0.19)	929
Had power in past 7 days (=1) (of electrified hh)	0.88 [0.32]	0.06* (0.04)	0.11*** (0.03)	-0.13** (0.05)	784
No regular blackouts (=1) (of electrified hh)	0.59 [0.49]	-0.11* (0.06)	-0.05 (0.06)	0.08 (0.08)	784
No blackout in past 7 days (=1) (of hh w/ power last 7 days)	0.40 [0.49]	-0.01 (0.07)	0.05 (0.07)	-0.03 (0.10)	700
(-) Hours power not working in past 7 days (of hh w/ power last 7 days)	7.15 [15.08]	2.05 (1.87)	1.17 (1.85)	-3.79 (2.48)	697
No blackouts ≥ 30 days in past year (=1) (of electrified hh)	0.95 [0.23]	-0.06 (0.04)	-0.02 (0.03)	0.02 (0.05)	784
No injury fr/ electricity in past year (=1) (of electrified hh)	0.99 [0.10]	0.00 (0.01)	-0.01 (0.01)	-0.01 (0.02)	784
No damage fr/ electricity in past year (=1) (of electrified hh)	0.99 [0.09]	-0.01 (0.01)	-0.02** (0.01)	0.02* (0.01)	784

The household reliability and safety index (shown here in row 1) is a standardized average of sub-components shown in the remaining rows. All outcomes are measured at the household or firm level and collected in the household and firm survey data (described in Section 2.4). For outcomes marked with a (-), a higher value indicates a lower quality. For all other outcomes, a higher value indicates higher quality. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A14: Knowledge

	AfDB Mean	World Bank Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 7: Knowledge index	0.03 [1.00]	0.09 (0.09)	0.02 (0.10)	-0.01 (0.14)	929
Told correct total cost of connection (=1) (of hh w/ drop cable)	0.29 [0.46]	0.03 (0.06)	0.01 (0.06)	0.03 (0.09)	927
Correctly told to pay monthly (=1) (of hh told of connxn cost)	0.05 [0.22]	-0.05*** (0.02)	0.00 (0.02)	0.02 (0.02)	927
Knows how much still owed for connection (=1)	0.43 [0.50]	0.15*** (0.06)	0.01 (0.06)	-0.06 (0.09)	941
Knows 20th token costs same as 1st (=1) (of hh who have topped up)	0.75 [0.43]	0.04 (0.06)	0.02 (0.06)	-0.04 (0.09)	704

The knowledge index (shown here in row 1) is a standardized average of sub-components shown in the remaining rows. All outcomes are measured at the household or firm level and collected in the household and firm survey data (described in Section 2.4). * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A15: Electricity Usage

	AfDB Mean	World Bank Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 8: Electricity Usage index	-0.01 [1.00]	0.17 (0.13)	0.29** (0.12)	-0.17 (0.17)	929
Electricity is main source of lighting (=1)	0.73 [0.44]	0.05 (0.06)	0.13** (0.06)	-0.10 (0.08)	941
Electricity is main source of cooking (=1)	0.00 [0.00]	0.00 (.)	0.00 (.)	0.00 (.)	941
Household has topped up (=1) (of hh w/ prepaid meter)	0.86 [0.35]	0.01 (0.05)	0.10** (0.05)	-0.01 (0.07)	833
Electricity spending past month (Ksh) (of hh w/ meter) (w)	183.32 [241.41]	-18.56 (25.19)	3.10 (26.04)	0.64 (34.23)	891
Hours of lighting used at night in past week	2.55 [2.68]	0.20 (0.26)	0.42 (0.27)	-0.27 (0.37)	939
Hours of lighting used in morning in past week	0.68 [1.39]	0.38*** (0.13)	0.40*** (0.13)	-0.31 (0.19)	941
Number of appliances that use the grid	1.91 [1.51]	0.31* (0.17)	0.33** (0.16)	-0.26 (0.23)	935
Number of households in this compound connected	1.13 [0.68]	0.01 (0.04)	0.03 (0.06)	-0.03 (0.07)	941

The electricity usage index (shown here in row 1) is a standardized average of sub-components shown in the remaining rows. All outcomes are measured at the household or firm level and collected in the household and firm survey data (described in Section 2.4). * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A16: Household Socioeconomic Outcomes

	AfDB Mean	World Bank Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 9: Household socioeconomic outcomes index	-0.02 [0.99]	0.15 (0.11)	0.18 (0.12)	-0.16 (0.17)	929
Connection allowed pursuing employment, business (1-5) (of connected hh)	2.54 [1.19]	0.25 (0.15)	0.14 (0.16)	0.18 (0.22)	784
Connection affected earnings (1-5) (of connected hh)	3.25 [0.78]	0.14 (0.09)	0.01 (0.10)	0.08 (0.14)	784
Connection permitted changing hours worked (1-5) (of connected hh)	3.65 [0.86]	0.04 (0.12)	0.04 (0.11)	0.01 (0.17)	784
Connection affected amount of food consumed (1-5) (of connected hh)	3.10 [0.45]	0.13** (0.06)	0.07 (0.06)	-0.04 (0.09)	784
Connection affected health (1-5) (of connected hh)	3.59 [0.86]	-0.07 (0.11)	-0.04 (0.11)	0.13 (0.15)	784
Connection affected children's education (1-5) (of connected hh w/ children)	4.32 [0.85]	0.33*** (0.09)	0.21** (0.10)	-0.22* (0.13)	688
Connection affected knowledge about news (1-5) (of connected hh)	4.16 [0.97]	0.14 (0.10)	0.11 (0.10)	-0.11 (0.14)	784
Connection affected security (1-5) (of connected hh)	3.59 [1.36]	-0.46*** (0.18)	-0.10 (0.17)	0.22 (0.26)	784
Connection permitted changing kerosene spending (1-5) (of connected hh)	1.50 [0.98]	-0.04 (0.10)	0.04 (0.10)	-0.01 (0.14)	784
Connection changed phone charging freq. (1-5) (of connected hh)	3.12 [1.49]	0.55*** (0.18)	0.35** (0.17)	-0.48* (0.26)	784
(-) Kerosene spending, last week (Ksh) (w)	30.14 [62.40]	-14.28** (5.83)	-8.13 (5.64)	23.15** (9.02)	937
Owns home (=1)	0.99 [0.10]	0.00 (0.01)	0.00 (0.01)	-0.01 (0.02)	941
Number of rooms in primary residence	3.54 [1.66]	-0.21 (0.14)	0.08 (0.14)	-0.13 (0.19)	941
High-quality floors (=1)	0.38 [0.49]	0.03 (0.05)	-0.02 (0.05)	-0.09 (0.07)	941
High-quality roof (=1)	1.00 [0.06]	-0.01* (0.01)	0.01 (0.00)	-0.00 (0.01)	941
High-quality walls (=1)	0.22 [0.41]	-0.00 (0.04)	0.05 (0.04)	-0.05 (0.06)	941
Buildings in compound (of compounds with hh)	2.94 [1.57]	-0.16 (0.15)	-0.18 (0.21)	0.18 (0.25)	744
Electrified buildings in compound (of compounds with hh)	1.64 [1.31]	-0.06 (0.10)	0.14 (0.18)	-0.12 (0.19)	744

The household socioeconomic outcomes index (shown here in row 1) is a standardized average of sub-components shown in the remaining rows. All outcomes are measured at the household or firm level and collected in the household and firm survey data (described in Section 2.4). For outcomes marked with a (-), a higher value indicates a lower quality. For all other outcomes, a higher value indicates higher quality. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A17: Firm Performance

	AfDB Mean	World Bank Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 10: Firm Performance Index	-0.00 [1.03]	0.25 (0.19)	0.09 (0.18)	-0.18 (0.26)	349
Firm uses electricity (=1)	0.64 [0.48]	0.18** (0.08)	0.10 (0.08)	-0.08 (0.11)	329
Firm planning to buy electrical equipment in next year (=1)	0.43 [0.50]	0.10 (0.09)	0.03 (0.08)	-0.11 (0.12)	329
Firm uses elec beyond lighting and cell charge (=1) (of those that use elec)	0.36 [0.48]	-0.06 (0.08)	-0.18** (0.07)	0.16 (0.11)	334
Number of appliances owned by Firm	1.23 [1.13]	0.22 (0.23)	-0.00 (0.20)	-0.10 (0.30)	334
Firm household has high quality roof (=1)	0.89 [0.31]	0.08 (0.06)	0.05 (0.07)	-0.10 (0.08)	296
Firm household has high quality walls (=1)	0.49 [0.50]	-0.05 (0.12)	0.12 (0.11)	-0.09 (0.15)	296

The firm performance index (shown here in row 1) is a standardized average of sub-components shown in the remaining rows. All outcomes are measured at the firm level and collected in the household and firm survey data (described in Section 2.4). * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A18: Household Political and Social Beliefs

	AfDB Mean	World Bank Effect Estimate	Audit Treatment Estimate	Interaction Estimate	N
Outcome 11: Political and Social Beliefs index	0.01 [1.00]	0.03 (0.07)	0.04 (0.09)	-0.02 (0.12)	929
HH electrification in top 2 most-important govt policies (=1)	0.21 [0.41]	-0.01 (0.04)	-0.02 (0.04)	0.01 (0.05)	941
Thinks govt doing good job providing electricity (=1)	0.98 [0.14]	0.01 (0.01)	-0.00 (0.01)	-0.00 (0.02)	941
Voted in August 2017 election (=1)	1.15 [4.43]	0.09 (0.19)	0.49 (0.36)	-0.14 (0.41)	941

The household political and social beliefs index (shown here in row 1) is a standardized average of sub-components shown in the remaining rows. All outcomes are measured at the household or firm level and collected in the household and firm survey data (described in Section 2.4). * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Table A19: Impact of gradient and facility type on months to metering completion

	(1)	(2)	(3)	(4)	(5)
World Bank (=1)	10.0*** (1.8)	11.0*** (2.0)	10.0*** (2.2)	12.5*** (2.1)	11.8*** (2.3)
Land gradient			1.0 (0.7)		1.2* (0.7)
Health center				4.9 (5.7)	9.1 (6.2)
Secondary school				0.4 (2.8)	1.7 (2.9)
Primary school				4.4** (2.1)	5.1** (2.3)
Market center				-4.2 (2.6)	-4.1 (2.8)
Religious building				2.5 (2.7)	2.3 (2.9)
Other				4.5 (5.6)	10.7 (6.7)
Observations	185	185	170	179	164
Constituency FE	No	Yes	Yes	Yes	Yes

Stringing is completed at WB sites on average 8.1 months later than at AfDB sites. Controlling for land gradient and facility type does not affect these estimates meaningfully, and land gradient and facility type appear largely uncorrelated with time to stringing completion. [Table 2.3](#) displays the same for months to stringing completion. * ≤ 0.10 , ** $\leq .05$, *** $\leq .01$.

Appendix C

Appendix for Chapter 3

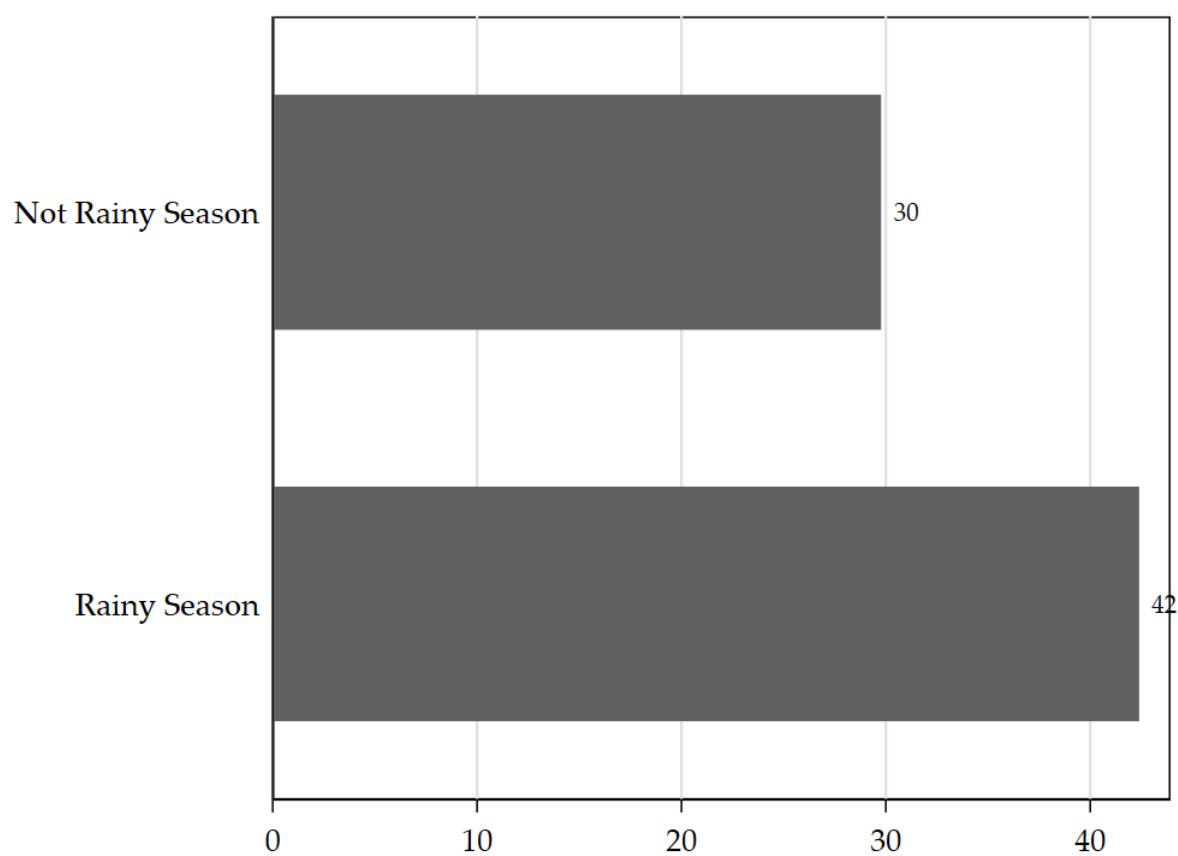
C.1 Additional Tables and Figures

Table C1: Summary statistics

	N	Mean	SD	Min	25 th	50 th	75 th	Max
No. of surveys completed	627	2.05	0.75	1.00	1.00	2.00	3.00	3.00
No. blackouts (in past 30 days)	514	5.05	9.74	0.00	1.00	2.00	5.00	120.00
No. days with blackouts (in past 30 days)	469	4.65	6.29	0.00	0.00	2.00	6.00	30.00
No. grid-powered equipment	581	7.89	5.49	1.00	4.00	7.00	11.00	27.00
Has refrigerator (=1 if yes)	627	0.85	0.36	0.00	1.00	1.00	1.00	1.00

This table presents summary statistics for the sample of health facilities. Facilities were sampled from Bomet, Busia, Homa Bay, Kericho, Laikipia, Makueni, Marsabit, Nyandarua, Samburu, and Tana River Counties.

Figure C1: Hours of blackouts in the past 30 days, by rainy season



This figure shows the average number of hours of blackouts experienced by health facilities in the sample in the past 30 days. The sample is split according to whether the past 30 days intersected the rainy season for that region. N=1221.