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ESSAYS IN DEVELOPMENT ECONOMICS, CASE OF MALAWI

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

DOCTOR OF PHILOSOPHY

in

ECONOMICS

by

Valentina Brailovskaya

June 2018

The Dissertation of Valentina Brailovskaya
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Abstract

Essays in Development Economics, Case of Malawi

by

Valentina Brailovskaya

My dissertation focuses on two important aspects that affect micro-entrepreneurs in developing countries: electricity outages and saving constraints. In my first chapter, I study the impacts of infrastructure deficiencies on small business owners in the context of the electricity supply in Malawi - a country where power is very unreliable and only 25% of small businesses are even connected to the grid. Access to reliable electricity is a requirement for development. Yet, most research focuses only on the effects of the first stage - establishing connection to the grid. However, the quality provision is quite poor and existing customers experience frequent outages. Using high frequency data collected on 391 small businesses across the city of Blantyre, I document the following: (1) power outages are very frequent, occur on 67% of days and last on average for 6.6 hours; (2) outages do not affect an average business due to the fact that the majority are not even connected; (3) however, the effects are large for the sub-sample of businesses highly reliant on power (those with electrical appliances), and as a result of blackouts, those businesses lose 25% of daily profits compared to non-outage days; (4) the profit losses are permanent - there is no evidence of profit increases in the following days; (5) the income shock results in reduced total weekly household-level expenditures, and affected households spend 4.8% less for every outage experienced during the week. My results suggest that outages have limited average impacts on small business owners, but businesses relying on electricity to generate income are heavily affected.

My second chapter is a pilot study for the first chapter, which informed the design and development of the main study. In this chapter, I study effects of electricity interruptions among market vendors across 5 markets in Lilongwe, Malawi. I overcome several empirical challenges associated with estimation of the effects. First, due to data limitations and heavy under-reporting of outage instances, I aggregate definition of an outage to a market level and use various constructions of outages definitions to verify the results. Second, I use a bootstrap method estimate clustered standard errors with a few clusters. To estimate the effects, I use the detailed logbooks kept by small businesses owners to study the short-term effects of blackouts on business outcomes. Since outages are so frequent, only 31% of market vendors select into electricity dependent operations. These businesses are affected by power outages and lose 7-16% of revenues on days of blackouts. Due to likely presence of measurement error in the independent variable, I conclude that the presented effects are lower bounds of the true impacts.

The third chapter, co-authored with Jonathan Robinson (Associate Professor of economics, University of California at Santa Cruz) and Shilpa Aggarwal (Assistant Professor of Economics and Public Policy at Indian School of Business), focuses on the effects of provision of savings accounts to micro-entrepreneurs in a developing country. Labeling savings accounts for goals might increase savings for those goals but limit liquidity for other uses. We conduct a field experiment with small business owners in urban Malawi, in which we randomly offer people one or multiple labeled savings accounts - mobile money or simple lockboxes. Since access to a secure place to save is the same for those with one as with multiple accounts, any effect of the incremental account should be due to labeling. We show

that (1) the savings products were popular, especially the box (95% of respondents used the box at least twice within 4 months compared to 53% in the mobile money group); (2) takeup of multiple lockboxes was high (66% of those offered multiple boxes actually used it at least once) compared to 6% in mobile money groups; (3) total deposits across all saving sources were higher for the group with multiple boxes compared to the single box holders; that (4) the savings products had a number of effects on downstream outcomes, including increased labor supply in farming activities, increased spending on certain categories such as holiday spending and school supplies for children; but that (5) there is no evidence that the labeled accounts limited liquidity of money for non-labeled purposes, and we do not detect that mental accounting deterred households from using savings account to deal with health shocks, even though about half of respondents reported that they were reluctant to withdraw from the account. Lastly, we conclude that mobile money technologies are not able to induce the same saving behaviors as lockboxes likely due to higher transactional costs.

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Chapter 1

Unreliable Electricity: Effects of Power Outages on Small Businesses in Urban Malawi

1.1 Introduction

There is broad agreement among development practitioners that electrification is instrumental for long-term economic growth. In Sub-Saharan Africa 70% of people live without power (International Energy Agency 2013). Increasing energy access in developing countries has been a major priority for development organizations in recent years, with the World Bank allocating $7.49 billion to energy projects around the world[1]. However, potential benefits from electrification might be limited if the power supply is unstable, and

[1]The data is obtained from the World Bank Annual Report 2016. Spending by region in billions: $1.5 - Sub-Saharan Africa, $2.1 East Asia and Pacific, $0.46 in Europe and Central Asia, $0.24 in Latin America and the Caribbean, $1.09 Middle East and North Africa, $2.1 South Asia.

electricity in Sub-Saharan Africa is notoriously unreliable: 78.9% of businesses experience outages, totaling to an average of 49 hours a month[2]. Such frequent power outages may significantly limit the welfare gains from electrification[3], and researchers have started to recognize the importance of reliability[4]. In fact, a World Bank survey survey with of firms with more than 5 employees revealed that businesses listed blackouts as the second biggest obstacle to growth (after access to finance), and reported that blackouts reduced annual sales by 8%[3].

Given unreliable power supply, it is important to document the implications of chronic outages on the viability of the private sector, and, particularly among small micro-enterprises since they contribute at least 33% of national income and 45% of total employment in developing countries (World Bank 2015). While there is increasing interest in understanding power reliability, there is little academic research quantifying the effects. There is a documented 2.8% GDP growth reduction due to power outages ([12][5]) relying on cross-country comparison. Studies on large firms show modest decreases in revenues ([11]), heterogeneous impacts of blackouts on productivity depending on firm type and the ability to adjust production methods ([9]), and increases in operation costs ([70]). However, less is known about how micro-enterprises are affected. To date, the most related research on this topic is by [79] who find an 11% drop in revenues due to power outages among tailors in Ghana. My study makes three contributions to the literature. First, it paints the landscape of electricity unreliability in a developing country using a precise measure of outages ob-

[2]The survey is conducted with firms larger than 5 employees. Statistics obtained from Enterprise Surveys http://www.enterprisesurveys.org, The World Bank

[3]Examples of studies that show positive impacts: [55], [75], [100], [88], [90], [89], [92], [21]

[4]See [97], [115], [47]

[5]The study uses variation in lightning density as an instrument for outages

tained via high frequency survey data on micro-entrepreneurs. Second, it examines effects of power interruptions on small firm's productivity across different occupations. Third, it adds to the growing literature of the impacts of unreliable infrastructure on welfare ([14]) and is the first paper to examine impacts of income shocks induced by electricity interruptions on expenditures.

The study takes place in Malawi, where electricity provision is particularly poor. Due to a high reliance on hydro-power and declining water levels that feed generation plants, generation capacity has fallen to 60% of the full potential. Under-generation causes frequent load shedding (or rolling blackouts) across the country. In fact, the admnistriative data on power outages from the Malawi Electricity Company suggests that an average feeder in the city of Blantyre - the second largest city in the country, with population over 1 million - was shut off on 17 days in September 2017, with each blackout lasting over 10 hours[6]. Additionally, aging equipment is fragile[7], frequently malfunctions and causes numerous faults, contributing to more hours in the dark. Given the unreliability of power supply, only 25%[8] of small businesses are connected to the grid and the majority operate businesses that do not require electricity.

The sample is comprised of 400 small firms across the city of Blantyre, 142 of which are connected to the grid. I document average effects of blackouts on small businesses using the full sample, but zoom in on 74 which are connected to the grid have electric appliances to examine mechanisms behind the results. These businesses are referred to as "high-dependent" throughout the paper. The sample was randomly selected from a census of

[6]The dataset is at the feeder level and is not yet matched to geographical variables.
[7][125] report that the average age of equipment is 30 years
[8]Statistic obtained from the project census data

small businesses in randomly selected areas in urban Blantyre[9] and vary in occupations. To measure outcomes, I designed and administered high-frequency phone surveys to the business owners over the course of two months. Respondents were called twice a week and were asked about labor supply, input composition, revenues, profits, secondary income-earning opportunities, blackouts and household-level outcomes. Variation in interruptions within a firm across time, as well as variation in exposure across the city within a day, allows for a quasi-experimental estimation of short-term effects of power outages on business outcomes.

My findings reveal the following. First, blackouts are widespread, and the average business owner reports an experiencing an outage on 67% days, with 1.06 outages per day, lasting for an average of 6.6 hours during the business hours. While power outages are usually expected (67% of outage instances are anticipated), the exact timing is not always known and 60% of blackout hours are unforeseen. The known outages result from the rolling blackouts (load shedding) implemented by the power company, the schedule of which is posted in the newspapers and the company's website once a week. The unknown outages are either faults in the system (which are not anticipated by either the power company or business owners) or are rolling blackouts unknown by an individual business owner due inattention to the posts or company's non-compliance with its own scheduled. Despite the fact that the respondents report anticipating blackouts on most days, their predictions are only accurate 20% of the time. This indicates that (1) the public announcements about upcoming blackouts is not salient; and/or (2) there is a high number of faults in the system;

[9] A number of exclusion criteria were applied to select the sample. A detailed discussion is in the Data Section.

(3) the company does not follow its own schedule. Thus, I conclude that while some of the blackouts are correctly anticipated, an average blackout is not known. Further, anticipation of an outage tomorrow has no impacts on business outcomes today, suggesting that no prior adjustments are made in preparations for future productivity shocks. Lastly, I verify that the main results are not driven by anticipated blackouts by estimating the main effects on a sub-sample of unknown outages.

Second, I examine impacts of power outages on the full sample to measure average effects and a sub-sample of businesses with an electricity connection. I further split the sub-sample with an electricity connection into two sub-groups: (1) "highly-dependent" businesses with electrical appliances and (2) businesses which do not own appliances that are essential for income generation. I document that there are null effects on an average business since the majority of businesses are even not connected, but also the null effects persist for businesses connected to the grid but which are not "highly-dependent". However, there are large and significant effects on "highly-dependent" business. For this sub-sample of businesses, outages reduce labor supply by 9.5% points on the extensive margin, totaling to an average 1.79 work hours lost per day. The estimates are higher than documented in the existing literature on effects of power interruptions on labor supply in [79] who look at average effects on all tailors (both with and without electricity connection). I find no substantial evidence of substitution into other jobs (secondary businesses or casual labor) to make up productive hours lost.

Third, on days with outages, the "highly-dependent" businesses lose 2.7 customers (23% compared to non-outage days) and $1.32 in profits (25% compared to non-outage

days). The losses are proportional to outage duration, but are not linear, and the drops in profits are only statistically significant from zero for outage durations larger than 5 hours. Therefore, businesses are able to recover the losses within a day if the blackouts are short. Using specifications with lagged outages, I do not find evidence of temporal shifts in income, suggesting that none of the lost income is made up. Since the majority of the transactions are small and "immediate" i.e. the shops don't rely on large orders that can be postponed, it is likely that the weekly losses are never made up.

Are the sales permanently lost to the economy if they are not shifted temporally within a business? This scenario is unlikely since there are bigger enterprises with an alternative power supply that offer comparable goods and services[10]. I find suggestive evidence that businesses with alternative power supply (generators, solar lamps and other) do not lose on days of power outages, indicating that self generation is a successful strategy in mitigating losses[11]. However, the power generation is an expensive investment and the majority of business owners do not generate enough profit to justify the investment. Thus, the distributional effects of power outages are not equal and may be more severe for small firms which have already "graduated" into more sophisticated business models but are not large enough to self-generate.

Fourth, I extend the analysis to household expenditures to examine consumption responses to income shocks generated by the power outages. For "highly-dependent" businesses, there is a drop of total expenditures, and for every blackout the total weekly ex-

[10] Anecdotes suggest that larger businesses with alternative energy sources charge higher prices to cover costs of self-generation, but I do not have empirical support for this

[11] There is not enough power to draw definitive conclusions since only 15 businesses in the sample have an alternative power supply at baseline, and there are only 6 businesses with among the sub-group with appliances

penditures decrease by $1.4 (4% of average weekly expenditures). In total, this represents an approximately 20% drop in weekly expenditures, compared to weekly averages. The decreases are driven by household non-food expenditures (including electricity bills), non-essential entertainment expenses and other unexpected expenditures. Further, there is an increase in consumption of street food. These categories represent a smaller fraction of total expenditures and I conclude that while blackouts do affect consumption choices, the welfare impacts are harder to measure since the drops in expenditures comes from non-essential expenditure categories.

An important caveat to note is that the study provides short term impacts of power interruptions on firms that have already selected into electricity occupation. Thus, the provided estimates are lower bound of the true effects on the economy as a whole if there were permanent improvements to quality. Specifically, short term impacts do not incorporate gains from improved operations of the current businesses (efficiency gains from purchases of new equipment), profitability of new entrants (businesses currently not relying on power) and entirely new business models that would emerge given permanent changes.

While these results are specific to Malawi, which has particularly poor service provision, the results can be relevant for other countries that experience frequent blackouts. The quality of power supply in Sub-Saharan countries is likely to remain a challenge in the near future. The demand for connections is expanding quickly due to population growth and urbanization, and is predicted to grow by 84% by 2035 ([131]). Further, generation capacity is highly dependent on availability of natural resources; 42% of the countries in Africa rely on hydro-generation[12], with 21% of the countries being fully dependent on it.

[12]Source: World Bank development Indicators. Data on electricity generation is missing for 46% of the

The recent instability in rain schedules and changes in weather patterns has led to droughts and lower water levels to feed generation plants ([24], [64]), cutting the generation capacity to 50% across the region. This has been the largest cause of frequent load-shedding in the recent decades in countries like Malawi, Zambia, Kenya, Tanzania, Zimbabwe and Mozambique ([32]). While there may be untapped potential in hydro-power generation across the continent[13], continued uncertainty of water levels in rivers and reservoirs threatens the capacity to provide reliable power ([105]). Thus, unless there is greater diversification of energy generation (investments in solar generation, for example) and expansion of current capacity, the quality will persist as a major challenge in the region.

1.2 Background

Malawi is a small, landlocked country in South-East Africa, bordering Zambia, Tanzania and Mozambique. Its area is 118 kilometers squared (45,560 miles squared) approximately one third of the size of California. The population is about 18 million people, with 85% of the population living in rural areas. Blantyre is the second largest city in the country, with an estimated population of about 1 million. Despite being the second largest city, the population density is low relative to other major African cities[14].

Over 95% of electricity in Malawi is generated through 7 hydro-electric stations, located along the Shire River. The complete process from generation to distribution is

Sub-Saharan countries. Out of the countries for which the data is not missing, 42% of the countries rely on hydro power for more than half of electricity generation

[13]Technically feasible power generation is 1,750,000 GWh/year, compared to current 76,000 GWh/year. Source: 2001 World Atlas and Industry Guide International Journal of Hydropower and Dams

[14]For example, population density in Nairobi is 4,850 people per squared kilometer, compared to 2,576 in Blantyre

owned by a single state company - Electricity Supply Commission of Malawi (ESCOM)[15]. In recent years, erratic rain schedules have caused a major reduction in the water levels in lakes and rivers across the country, the lowest in the past 15 years, causing only 60% generation of total capacity (ESCOM 2017). The situation is expected to worsen, and with unpredictable rain schedules, the water level continues to decline. To avoid overload on the electrical grid and prevent system-wide blackouts, ESCOM frequently implements load sheddings – temporary interruptions of electricity supply to reduce the load on the generation plants. In addition to load shedding, there is a need for continuous maintenance of decaying equipment and infrastructure. Frequent repairs cause either large shut-downs of power affecting large areas of the city (in which all connections on the same feeder are affected) or smaller neighborhood-level blackouts. Load Shedding and faults lead to an almost daily interruption in some areas across Malawi.

1.3 Data

The data for this project was collected as a part of a parent study to examine the effects of saving accounts on small businesses in Blantyre, Malawi ([3]).

1.3.1 Census

The sample of businesses for the study was drawn as follows. Blantyre city is divided into 26 wards. Each ward is split into enumeration areas (EA), defined by the Malawi Census 2008. Wards differ in size and contain between 2 and 48 EAs (15 on

[15]The total electricity generation is fairly low 2,069 GWH (Gigawatt Hour), compared to neighboring countries with similar populations Zambia (11,338 GWH) and Zimbabwe (7,297 GWH). Source: BP Statistical Review of World Energy 2017, International Energy Statistics 2016

average). To increase the geographical coverage of the sample, 3 enumeration areas were randomly selected from each ward, resulting in 77 EAs[16]. We sought to interview all small businesses in each EA during the census; however, this was not feasible in some EAs with a high density of businesses (typically because larger markets were located in those areas). In these EAs, we randomly selected a fraction of businesses for an interview.

The summary of business counts during the census is presented in Table A.1, Panel A. In total, 75 out of 77 EAs were visited and all businesses counted. The remaining two EAs did not contain any businesses[17]. Overall, 9,848 businesses were counted across these EAs. Out all businesses, 82% were classified as small businesses (were not supermarkets or wholesale stores). In the end, 3,457 businesses were randomly selected to be interviewed, out of which 90.8% were willing to be interviewed[18]. All businesses were up to 3 times before the record was finalized. Out of all businesses eligible for the census interview, 92.4% agreed to participate in the census survey.

The census served as a screening tool to determine eligibility of each business for study participation, and six criteria were used[19]. First, mobile money agents were excluded since one of the treatment arms in the parent study involved opening mobile money accounts. Second, only small businesses were enrolled in the project[20]. Third, to accurately estimate labor supply responses to electricity outages, business owners were excluded if they were not involved in the daily operations or plan to close in the next 6 months. Fourth, we initially

[16] One ward had only 2 EAs

[17] 1 was an industrial area and another contained farms fields

[18] The remaining 9.2% refused to engage in conversations with the enumerators before the initial contact; 2% of those businesses were not interviewed because either the owner was under 18 years old or the business was visited 3 times and the shopkeeper available was under 18 years old.

[19] The majority of the criteria was relevant for the parent study on savings

[20] We defined small businesses as ones with no more than two permanent employees (excluding the owner)

planned on asking the respondents to keep logbooks, and therefore required that business owners be literate. Lastly, polygamous business owners were excluded because of difficulty recording household-level information for multiple households. While each criteria did not exclude a large fraction of businesses individually, the combination of eligibility criteria disqualified 33.3% of business owners. Out of all eligible businesses, 99.2% consented to participate further in the study. Thus, a pool of 1,937 eligible businesses was available for the random sample selection.

To ensure adequate power for the electricity study, business connected to the grid were over-sampled: 35% and 65% of the study sample were drawn from the businesses connected and not-connected to the grid respectively, whereas the population contained only 25.7% of businesses connected to power. The sampling was stratified by financial access (defined as either having a bank or a mobile money account) and distance to the closest mobile money agent - relevant factors for the parent study. The full study sample is composed of 801 businesses.

1.4 Baseline

Prior to conducting a baseline survey, we verified the exclusion criteria from the census, and also checked whether respondents had vision problems[21]. This set of criteria excluded 16.6% (8.1% for the vision and 7.8% for the other). Further, 10% refused to take part in the study, and 400 business owners were enrolled in the study, with 142 connected

[21] In the original design of the study, the respondents were supposed to fill out physical logbooks, which would require them to see the text clearly. However, post baseline, it was decided that a less burdensome way to collect data of comparable quality would by conducting phone surveys.

to the grid in total and 74 with electric appliances (referred to as "highly dependent").[22] (Table A.1, Panel B). The location of sampled businesses are presented in Figure 1.1[23]. The composition of the sample broken by business category (Food Sellers, Non-Food Sellers and Services), by more granular business is summarized in Table A.2. The classification of businesses into the "highly-dependent" sub-group is derived from baseline asset holdings and includes businesses with productive equipment i.e. those with salon/barbershop tools, entertainment (such as TVs and radios), computer/office equipment and any other professional tools (electric sewing machines, welding equipment or any other).[24]

A detailed baseline survey containing questions on demographics, business outcomes, savings, cash flow and electricity usage was administered to the full sample. All project participants received a simple phone to minimize attrition during the phone surveys.[25] Further, in order to verify the self-reported data with the administrative data supplied by the ESCOM[26], shops connected to the grid were asked to point to the transformer which their shop was connected to. The number, the picture and the GPS location of each transformer were recorded[27]. Overall, the transformer information was obtained for 128 respondents[28]. The mean number of businesses per transformer is 3.5, the median is 3,

[22]The full sample contained 801 respondents but only 400 participated in high frequency data collection relevant for the electricity study

[23]The GPS location contains random noise to protect privacy of the project participants

[24]This group does not include businesses which report having only a refrigerator at baseline. While refrigerators may be important for generating income, short blackouts should not prevent businesses from being able to make sales, since it takes several hours before the inventory in the fridge is defrosted and losses are incurred.

[25]The other reason was to ensure that mobile money account holders in the parent study could use their accounts.

[26]The administrative data on outages is provided at the feeder level (which is a network of transformers) and mapping of transformers to feeders is currently not available. ESCOM is in the process of censusing all transformers in the city and will provide the mapping once it is complete. Administrative-level data will be incorporated into the analysis

[27]Transformer number was missing on 51% of the visited transformers, and GPS and pictures aided in identification of respondents who are connected to the same transformer

[28]Transformer information is not available for 14 businesses: 10 respondents did not agree to provide

and 37 businesses do not share transformers with any other businesses in the sample.

1.4.1 Phone surveys

The primary source of data is the High-Frequency Phone Surveys conducted twice a week for a two month period starting September 2017. Prior to the start of the data collection, each respondent was randomized into days of the week on which the phone calls are made. Out of 400 respondents, only 3 respondents asked to be called on different days and the rest complied with the original day assignment. Time preference was recorded to improve the compliance with the phone calls, and the respondents received phone calls on the preferred time of the day (morning, afternoon or evening). The respondents were compensated with phone credit equaling to $2 USD/per week for participation in the surveys. In addition to the base compensation, at each phone call they participated in a lottery and could win extra $3 with 1/4 chance[29]. The compensation and the lottery wins were sent directly to the respondents' phones immediately after each phone call was completed.

There were two types of phone interviews: short and long. Each survey consisted of questions about past 3-4 days, depending on the day of the week. The illustration of the call schedule is displayed in Figure A.1. To minimize recall bias and improve the accuracy of responses, each question was asked about a particular day of the week i.e. "What was your business revenue on Monday" where "Monday" was automatically calculated by electronic surveying tool. The short interview only consisted of questions on business outcomes (revenues, profits[30], input composition, labor supply, etc.). Businesses connected to electricity

transformer information or did not know where it was located, 4 respondents were initially coded as not connected to the grid, and information on the transformer was not collected at baseline

[29]The primary goal of the lottery was to study the effects of income shocks in the parent study

[30]The question asks the respondents to report their income after expenses and wages to employees, fol-

were asked about the number of blackouts in their shops, while businesses not connected to the grid were asked about blackouts in the area where they work. Follow-up questions on outage durations were asked if the response on was greater than zero. Information on duration was collected via three questions: blackout hours before 7 am, between 7am and 6pm, and after 6pm. The exact timing of an outage was not asked to avoid interview fatigue and minimize under-reporting[31]. In addition, the respondents were asked the number of blackout hours they expected having on a particular day (to differentiate between known and unknown outages) and how many hours of blackouts they expected tomorrow. The latter was asked for two reasons: (1) to independently validate future expectations versus realized outages and obtain a measure of quality of predictions, and (2) to determine whether expectation of an outage tomorrow affects today's behavior. The longer interview consisted of the short interview questions and an additional section on savings, loans, expenditures and economic shocks experienced in the past week.

If the respondent was not reached on the scheduled day after three attempts, up to three make-up calls were made the following day. Since it's difficult to recall information with longer recall period, information for only up to 4 days back was asked at every phone call and if a respondent missed a scheduled call, then one day of information was lost as a result. Attrition at the Phone Surveying stage is reported in Table A.1, Panel C. Out of 400 businesses in the sample, 391 were reached for at least 1 phone call (70 out of 74 in the "highly-dependent" sub-sample). For an average and median respondent, the daily

lowing the formulation in [104]

[31] An alternative formulation would be to ask the start and end times of each outage, however, recalling start and end time of multiple outages in a day may be subject to greater measurement error. Furthermore, the respondents were asked the same questions every week and had an opportunity to learn the sequence of the questions. It's likely that if the respondents knew that reporting of less blackouts could lead to shorter interview, they might have under-reported the instances

panel consists of 49.4 and 52 days respectively (47.2 and 52 for the "highly-dependent" sub-sample).

Field officers were carefully monitored to ensure high survey completion rates. First, phone records made by each field were automatically uploaded to a spreadsheet, which the project's research assistant checked daily. Second, the research assistant verified that the field officer made three attempts (one hour apart) throughout the day to reach the respondent. Third, the submitted surveys were cross-checked with the phone records. Enumerators that followed the protocol were rewarded with weekly bonuses. This protocol incentivized surveyors to reach respondents and made it virtually impossible to shirk and fabricate surveys.

Phone Surveys Attrition

Missing days in the constructed daily panel for each business owner would bias the estimates if the missing days were not missing at random, but rather were correlated with blackouts. One possible concern is that a respondent was not able to charge the phone prior to the phone call and skipped the scheduled appointment because of an outage. In this setting, I can directly verify if calls were skipped because of the power outages. If the field team did not reach the respondent on the day of the scheduled call, the makeup call would follow the next day (illustration of the process is displayed in Figure A.2). Since the make up calls were identical to the regular surveys, information on blackouts for prior days was collected. Thus, by matching blackout records to missed scheduled phone calls, it is possible to verify whether the probability of missing surveys is higher on blackout days. The results are reported in Table A.1, Panel D. The probability of skipping a call for non-

outage reasons 8% for those who are connected to the grid. Having an outage on the day of the schedule call has no impact on the probability of reaching a respondent. Further, if a call was missed on the scheduled date, then actual missing date in the data is 3-4 days prior, and it is unlikely that those outcomes are correlated with an outage on the day of the scheduled call.

Attrition during the Phone Survey (the number of surveys completed on each day) is presented in the Figure A.3. Overall, the dropouts from the data were minimized and the completion rates at the start are very similar as at the end of the surveying period, and the vast majority of the respondents has completed the full round. To examine whether baseline characteristics are predictive of the completion rates, I examine the the correlation between demographics/baseline business indicators and the number of surveys an individual completed. The results for the "highly dependent" category are presented in Table A.3. The dependent variable is number surveys completed conditional on being reached at least once. Age and monthly profits (expressed in USD) are correlated with the number of surveys completed, suggesting that age increases the completion rates, while higher profitability decreases the rates. However, both coefficients are small and unlikely to bias the results. Further, the p-value from an F-test on joint significance is large and does not indicate that characteristics are jointly predictive of the attrition rates. Lastly, the estimation strategy relies on within-person variation in which the baseline characteristics are less relevant to the validity of the estimates.

1.5 Summary Statistics

Summary statistics on all 400 business owners in the sample are presented in Table 1.1 (column 1), all connected business owners (column 2), and the "highly dependent" group with appliances (column 3). The three business types are presented separately to characterize the sample and sub-samples, and to observe differences in means as the sample is reduced from all businesses to those highly dependent on power[32]. Demographics, business characteristics, financial access and financial security, electricity usage and outages are presented in Panels A through E. The full sample is split evenly between males and females, however, as "less sophisticated" business types are excluded (movement from column 1 to column 3), the sample becomes dominated by males, and 73% of the "highly" dependent category are male. The same monotonic decrease can be seen in age (age decreases with movement from column 1 to column 3), and education which is one year higher for "highly dependent" businesses. This suggests that (1) selection into electricity dependent occupations is highly correlated with demographic characteristics, and (2) "highly dependent" businesses are in not representative of an average small business owner.

All firms are small (selected to have less than 2 employees for the participation in the study), with an average of 0.15 employees for all businesses and 0.37 for the "highly-dependent" sub-group. Average monthly profits are $84.6 for the full sample, $100.7 for the sample connected to the grid and $85.7 for the "highly-dependent" category. Interestingly, selection into the "highly dependent" business type does not result in average higher profits compared to an overall mean for those who are connected to the grid. This may be partially

[32] A more detailed discussion of factors responsible for selection into electricity-dependent categories is presented in Section 1.9

driven by poor electricity supply which significantly impacts their ability to generate income. Values of inventory holdings is low for the "highly dependent" businesses since the majority are service providers (salons, printing shops, welders as indicated in Table A.2), however, the value of business tools and equipment is the largest for those businesses, with an average of $207.03.

Bank account ownership is low considering the fact that the businesses are located in the second largest city in the country. The probability of holding a bank account is 32% for the full sample, however, is the largest for the "highly dependent" category where 44% of business owners have one. Total value of assets and cash savings are $617.06 for the full sample, and $834.1 for the "highly-dependent" sample, with the highest value for the sub-group connected to the grid without appliances (as suggested in column 2).

By definition, the sample with appliances (the "highly-dependent" sub-sample) has the highest ownership of electric tools, and 50% own salon/barbershop equipment, 24.3% provide entertainment (TVs, radios, stereo), 25.7% have other professional tools and 17% have computers/other office equipment. The ownership of alternative power supply (generators/car batteries/solar panel) is extremely low and only 8% of the "highly-dependent" sub-sample has an access to an alternative energy source during blackouts. Conditional on having another source of energy, 66.7% have a generator or solar panels, while the rest report using kerosine lamps/candles for lighting or charcoal for cooking during blackouts. Cost is the main deterrent of acquiring an alternative power supply, and 85% report that it's too expensive to invest in it.

Since the number of businesses connected to the grid is 35% in the full sample, only

41% report being affected by outages, with the majority experiencing losses in revenues, however, there are businesses who report gaining customers on blackout days (for example, charcoal sellers who report higher demand for their products). However, 94.6% of the highly-dependent sample is affected by outages. Across the three samples, about 40-46% of blackouts are known to businesses in advance. Those represent rolling blackouts (or load shedding) scheduled by the power company and posted in the newspapers/website a week in advance. Despite, the fact the that schedule is posted in the newspapers, only 4% of the full sample pay attention to those announcements (12.9% for the highly-dependent users). Radio is also an important source of information and 31.4% of the full sample reporting knowing about blackouts from the radio announcements. However, the main source of knowledge is other people, and 60% report knowing about blackouts through "word of mouth". The latter is likely subject to error and reduce probability of correctly anticipating blackouts.

Outages

In Table 1.2, I present descriptive statistics of outages from the panel data constructed from the High-Frequency Phone Surveys for the full sample (columns 1 and 2), and for the "highly-dependent" sample (Columns 3 and 4). All averages for Panels A and B are calculated within an individual. An average business experiences power outages on 67% of the days, while the "high-dependent" sample reports experiencing blackouts on 77% of the days (Panel A). It's likely that the differences are not driven by location of businesses (since the presence of all types of businesses is similar across geographical areas as indicated in Figure 1.1) but rather reflects the fact that those with high reliance on power are more attentive to blackouts and recall instances with greater precision. The rest of the

values are very similar across the two groups and the rest of the discussion will focus on the "highly-dependent" sub-sample.

Conditional on having an outage, the average duration between 7am-6pm is 6.63 hours and 9.72 hours throughout the day (Panel B). The respondents report that 36% of outages are unknown i.e. there was an outage but the respondent anticipated no outages. Those are likely to be outages due to faults or inattention to load shedding announcements. Despite the majority of outages being foreseen, 62% of hours are not anticipated suggesting that there is high uncertainty of the duration conditional on having an outage. Outages that the respondent reports knowing about are longer (10.23 hours compared to 9.16 for unexpected outages). This either suggests that shorter outages are faults that are fixed within a day and longer outage are load shedding, or the respondent is more aware of longer load shedding instances (presumably known in advance) because they represent more extreme events.

There are two sources of variation in outages that permit the estimation of the effects: (1) across time and within the individual to allow for business fixed effects; and (2) across individuals within a day. Both sources of variation are sufficiently present as suggested in Panel D. Intracluster correlation (ICC), a measurement of similarity of outcomes within the cluster compared to across clusters, suggests of high within-EA (0.20), within-ward (0.18), and within-transformer (0.3) correlation of binary outage reports. It's possible that some blackouts is localized at the business level and do not affect anyone else on the same transformer (for example, a defect in a wire systems connecting a shop and its transformer), however, 97% reported blackouts affected other shops and suggest that

the majority of blackouts affect more than just a single business and are likely to occur at the transformer level. The ICC of 1 within-transformer should not be expected, since several neighboring transformers are likely affected at the same time (which would decrease within transformer correlation compared to across transformers), but higher value of within-transformer ICC relative to other geographical units suggest of validity of the reports. The relatively high ICC for ward suggests that blackouts affect large areas.[33] A demonstrative example of outage reports across the city for September 29th is displayed in Figure 1.2. There is variability of outage exposure within a business across time: the average standard deviation of duration (including zeros on non-outage days) is 5.14 hours. There is also sufficient presence of "alternating states of the world" i.e. the percentage of days on which the blackout day was followed by a non-blackout day is 27% which indicates that the blackout schedule is not characterized by a single long-lasting outages which followed a long non-outage period.

1.6 Estimation Strategy

The estimation relies on a fixed effect model, and the effects of outages are found by comparing outcomes within an individual business, controlling for date specific effects (weather shocks and any other date-specific factors that affect the city as a whole) and recall day fixed effects[34]. The relationship between electricity outages and the outcomes of

[33] It's important to note that the geographical units such as ward and enumeration areas are only approximations of an outage radius, and the correct geographical boundaries of blackouts are defined by a network of transformers (called feeders - the level at which the ESCOM shuts off the power). Mapping of feeders to the geography of the city is not yet completed by the power company.

[34] Since the data relies on recalled values of outcomes and the treatment variables, omitting recall day may positively bias the estimates if both outages and outcome variables are forgotten with time

interest is causal only if, conditional on control variables, the timing of the electricity outage is not correlated with any other characteristics. In the short run, the assumption is plausible because the businesses are very small and do not have any control over decisions made by the power company. A detailed discussion on threats to internal validity is provided in Section 1.8.

1.6.1 Base Specifications

Base estimation relies on a fixed model:

$$Y_{it} = \beta_0 + \beta_1 * Outage_{it} + \beta_2 * LotteryWin + \sigma_i + \delta_t + \lambda_d + \epsilon_{it}$$

where i is an individual business, t is date. Y_{it} is the outcome variable, $Outage_{it}$ expressed in binary terms (a value of 1 if there was a reported outage on that day and 0 otherwise) or in hours (before 7am, between 7am to 6pm, and after 6pm), and $LotteryWin$ is the amount of a random payout received on the day of the phone call. The coefficient of interest in this regression is β_1 which is the effect of an outage on business outcomes. All of the specifications include individual business fixed effects (σ_i), date fixed effects to control for date shocks that affect the whole city (δ_t), and recall day fixed effects to remove variation that is induced by the differential recall period (λ_d). Errors are clustered at the transformer level, since the blackouts are likely to affect all businesses on the same transformer at once.

1.6.2 Temporal Effects

To understand whether the incurred losses are made up on the days following outages, I include lagged binary variables in the following form:

$$Y_{it} = \beta_0 + \beta_1 * Outage_{it} + \beta_2 * LotteryWin + \beta_3 * Outage_{it-1} + \beta_4 * Outage_{it-2} +$$
$$+\beta_5 * MissingDay1_{it-1} + \beta_6 * MissingDay2_{it-2}$$
$$+\sigma_i + \delta_t + \lambda_d + \epsilon_{it}$$

$Outage_{it-1}$ and $Outage_{it-2}$ are indicators of whether there were outages yesterday and the day before yesterday respectively; $MissingDay1_{it}$ and $MissingDay2_{it}$ removes variation induced by missing data because of skipped days[35]. β_3 and β_4 are effects of outages in prior days on today's outcomes. Positive β_3 and β_4 would suggest that having outages in the past has positive effects on outcomes today.

1.6.3 Analysis of Weekly-Level Outcomes

Further, I investigate whether the documented daily effects persist at the weekly level on (1) business outcomes and (2) household weekly consumption, (3) giving and receiving loans, (4) giving and receiving transfers, and (5) savings (Specification 1.6.3). In the face of economic shocks, individuals may be more likely to cut expenditures or to rely on other sources of income to smooth consumption. Because the information on weekly outcomes was asked once a week, I construct a matching outage variable $NumberOutages_{iw}$ that is the sum of outages experienced in the past 7 days. The same aggregation procedure

[35]To avoid dropping observations due to missing lags, I replace the lagged variable with zero and create a control variable that takes a value of 1 if the original observation is missing

is followed to construct business outcomes at the weekly level from the daily reports. This week week does not correspond to the calendar week because each respondent is interviewed on a different day[36]. While attrition is fairly low as indicated in A.1, missing days in the middle of the data collection may impact manually aggregated measures constructed from the daily values. Specifically, day-level outages aggregated to the weekly level are underestimated by construction if the respondent was not reached on the scheduled day in the past week. This matters less for the daily estimation and the missing day is not included in the estimation[37], it does, however, impact the estimation at the weekly level because weeks with full 7 days of information (and 6 outage and 1 non-outage day) are pooled together with weeks during which 1 day was skipped and 6 outages were recorded. To address this issue I include number of non-missing days within a week ($NonMissingDays_w$) as a control variable. Weekly lottery win ($LotteryAmount_{iw}$) is included as a control to absorb variation in outcomes induced by the random payouts. Interview date fixed effects ($\gamma_{intdate}$) are included to control for time. Errors are clustered at the transformer level.

$$Y_{iw} = \beta_0 + \beta_1 * NumberOutages_{iw} + \beta_3 * NonMissingDays_{iw} + \beta_4 * LotteryAmount_{iw} + \gamma_{it}^{intdate} + \sigma_i + \epsilon_{iw}$$

[36]Prior to the start of the data collection, each respondent was randomly assigned the calling schedule

[37]A randomly skipped day should not bias the estimates if the day is excluded. This is plausible because even if an interview was skipped because a respondent's cell phone ran out of charge, the skipped day is not the day of the interview, but a day 3-4 days before.

1.7 Results

1.7.1 Business Outcomes

Daily Effects

In Table 1.3, I present the results of power outages on a full set of business outcomes: labor supply on the extensive margin (columns 1 and 2), on the intensive margin (column 3) , volumes of customers (column 4), revenue and income from main business (columns 5 and 6), earnings from secondary business or any casual labor (column 7), and total daily income (8) which is the sum of columns (6) and (7). Monetary outcomes are expressed in USD and are winsorized at 5% level to address outliers. The dependent variable is a binary indicator of an outage. I present four separate sets of results: effects of blackouts on all businesses (Panel A), effects on businesses that are connected to the grid (Panel B), effects on businesses connected to the grid but who do not have appliances - non "highly-dependent" sub-sample (Panel C), and effects on businesses connected to the grid with appliances - "highly-dependent" sub-sample (Panel D). All four estimates may be of relevance to the policy makers seeking to understand the impacts of blackouts on the different sectors of the economy.

First and foremost, an average business in the sample is not affected by power outages (Panel A) and this is largely driven by the fact that the majority of businesses in the sample and in the population do not rely on power. Limiting the sample to those connected to the grid (Panel B) uncovers more pronounced effects: these businesses experience losses to hours worked (a decrease of 1 hour compared to non-blackout days),

customers (a drop of 9% compared to non-blackout days) and results in losses in income of 12% (relative to non-blackout days). This point estimates are comparable to those found in [79] and [40]. Further, I break down the sample with a connection into non "highly-dependent" and "highly-dependent" businesses. The classification is described in detail in Section 1.4. While this method limits the sample size to only a fraction of the original sample, it allows for a more precise understanding of the effects and the mechanisms driving the results. Examination of the results in Panels C and D reveals that the losses documented on all users (Panel B) are solely driven by a sub-sample of "highly-dependent" businesses with appliances. These businesses (mostly services as indicated in Table A.2) are likely to generate very little income during blackouts since the majority of services they provide are reliant on electricity supply. The rest of the discussion focuses on the sub-sample of "highly-dependent" businesses, since the they are the ones truly impacted by the blackouts.

First and foremost, business owners 9.5% points less likely to work at all on days of outages. The results are likely driven by morning outages that are detected at the beginning of the workday. Unconditional on working, total hours worked are reduced by 1.79 hours (compared to 8.66 hours of work on non-outage day). The probability of taking a secondary earning activity is higher on blackout days, but the instances are so low (5% of businesses even engage in other non-permanent income earning activities on non-blackout days), that the lack of significance of this result may be attributable to low prevalence.

Second, there are significant losses to business productivity. Due to blackouts, the number of customers decreases by 2.7 (23.7% relative to non-outage days), revenues decrease by \$1.4 (12%) and profits are reduced by \$0.57 (25.1%). As a result total daily

income is $1.024 (21.4%) lower. These estimates are driven by both adjustments of labor supply on the extensive margin by those who chose not to work as a result of outages, and on the intensive margin[38]. It is interesting to note that while the revenues drop by 12%, the losses in profits are twice as large. This may be indicative of the fact that these businesses recover some of the revenue losses by selling less profitable goods/services (with lower profit margins).

In Table 1.5, I present the results of blackout duration expressed in hours before the work day (before 7am), roughly during the work day and before the sunset (between 7am and 6pm), and after work (after 6pm). The reduction in labor supply on the extensive margin (column 1) is visible only for blackouts that affect morning hours. The majority of the losses are incurred during the day likely because customers don't shop at night. Duration of an outage has a significant impact on all business indicators, and the point estimate of an additional hour is comparable to the effects in Panel A for an average outage duration.

Further, I investigate whether losses experienced by business owners are linear and in outage duration. I obtain separate point estimates for blackouts of different durations during the day using no-blackout days used as counter-factual. The estimation strategy is identical to the base estimation except it is run on different sub-samples. The effect sizes from the regressions are plotted against outage durations for labor supply and profits in Figures A.4 and A.5, respectively. For outages shorter than 4 hours, labor supply and profits remain unaffected, indicating that businesses can make up losses within a day given a shorter blackout. For outages longer than 5 hours, losses become larger and are statistically

[38] As shown in Table A.4 where the results presented are conditional on working on that day

significant from zero, increasing linearly with duration. This finding suggests increasing blackout frequency and decreasing duration may create more favorable conditions for business owners. It is remarkable to note that even with outages close to 11 hours, businesses do not lose 100% of profits. This suggests that despite being connected to grid and having appliances that rely on power, business provide still provide services or sell goods in spite of having no power.

Temporal Shifts

Next, I am interested in understanding the extent to which the documented losses in income and labor supply are permanent (in the short run). If the losses incurred on outage days are immediately made up on the next day, then the results obtained using the base specification are misleading and don't represent losses but instead shifts in sales across short periods of time. I answer this question via two specifications: (1) I include lagged dependent variables as controls for the past 2 days i.e. outages yesterday and the day before yesterday; (2) I look at the effect of having a non-outage (good day) today and an outage (bad day) yesterday; (3) and I aggregate outcomes to a weekly level to examine whether effects of an additional blackout in a week are comparable to daily losses. The first specification informs whether any sales are transferred across time unconditional on outage today, the second specification sheds light on whether revenue is made up given a good day, and the third shows whether the effects of one additional blackout persist at the weekly level.

The effects on outcomes of interest with lagged terms are presented in Table 1.6. There is no evidence that any losses are transferred within a business across days (next day

or the day after) or in the following week as presented in the appendix Table A.5 (with lags up to 7 days). Examination of the effects of having alternating outage days is presented in the appendix Table A.6. β_1 is the effect of having a good day (non - outage day) and the coefficient is reversed in sign compared to the main specification by construction. β_2 is the effect of having an outage yesterday, and β_3 is the additional effect of having a good day today and bad day yesterday. A positive β_3 coefficient would indicate that given a bad day yesterday (with a blackout) and a good day today (without the blackout), profits are made up given the opportunity to operate at full capacity. However, β_3 is not statistically significant from zero, indicating no evidence that any sales are made up from a bad day that is followed by a good day.

Lastly, I confirm that the losses are permanent using the weekly aggregates, following Specification 1.6.3. The results are presented in Table A.7 and should be interpreted as effects of having an additional blackout in the week on weekly outcomes. The statistical power is limited with this specification since there are at most 8 observations/individual. First, the reduction of labor supply does not persist at the weekly level, suggesting that the number of working hours lost is partially made up throughout the week. Second, there are significant losses to revenues and profits, and with every additional blackout day, revenues and profits drop by \$3.5 and \$0.99. The coefficients are comparable to losses documented at the daily level, and suggests that no income is made up within a week.[39]

[39]Similar effects are present when outcomes are aggregated to two weeks and the results are displayed in Table A.8

Coping Mechanisms

What can businesses do to ameliorate the income fluctuations induced by the frequent blackouts? Income diversification may be a powerful tool to mitigate the effects of the shocks. One potential solution is to seek casual labor opportunities on days of outages. However, availability of informal labor opportunities are differential across the city and average effects across all businesses regardless of the location may be less informative. In areas with higher business concentration casual labor work may be more common than in less commercial areas. Thus, I explore whether takeup of casual labor or other earning activity is higher in areas with high concentration of businesses areas compared to low business density areas[40]. The results are presented in Table 1.7 (columns 1 through 4). Coefficient on outage is the effect for businesses located in areas with lower number of shops in the area, $Outage * HighDensity$ is the additional effect of being in the area with many other shops and sum of the two coefficients is the effect of outages for firms in the high density areas. P-value from an F-test of the sum of the two coefficients is reported at the bottom of the table. This regression does not provide causal evidence of effect of being in the high density, since selection into high-density areas are likely to be correlated with other unobservable characteristics. Instead this analysis shows the difference in the takeup of secondary activities for businesses that have better opportunities to find those jobs. Indeed, for firms in the high-density areas, the probability of making money outside of the primary business is 53% higher (2.9% points increase compared to 5.4% on non-outage days). This shows that given larger supply of casual jobs, business owners seek

[40]The classification of an enumeration area as high or low was done at census.

to compensate the lost income with other earning opportunities. However, the gains from secondary income are not large enough to cover the losses from the main business and the total income losses are negative for both groups. The same pattern is not detected for those owners who reported having a secondary occupation at baseline (columns 5-8, Panel B). This result may be driven by the fact that secondary occupations are fixed incomes (either from salaried occupations or rentals) or seasonal income from agricultural activities, where labor demand is less flexible.

Is investment into alternative power a successful loss-mitigation strategy? In Table Panel C (columns 7-8), I explore the effects of outages on businesses with an alternative power source. Due to a low number of businesses with alternative power supply at baseline (6 in total), the estimates are noisy and it's difficult to make definitive conclusions. However the direction of the effect suggests that there the losses to those businesses are mitigated (the sum of the two coefficients is positive 0.159) and there may be gains on days of outages. Since this sample was purposefully selected to have no more than 2 employees and is restricted to very small enterprises, it is possible that the gains are transferred to establishments outside of the sample, with more capital and financing options to invest in self-generation. In fact, businesses owners in the sample report that 59% of lost sales are transferred to other shops, presumably offering comparable goods/services during outage hours. This is in line with anecdotal evidence and larger shops with generators operate during blackouts and have more customers.

1.7.2 Expenditures

Next, I extend the analysis to non-business outcomes to investigate whether the decreases in income have effects on welfare indicators, such as household-level expenditures. I examine the outcomes at the weekly and measure the impacts on longer-term expenditure patterns. Results are presented in Table 1.8. The dependent variables are expenditure categories expressed in USD and winsorized at 5% level to address outliers. Expenditures consist of the following: raw food (meats, produce, staples), restaurant and prepared foods, household (any household bills or expenses), medical (both preventative and treatment, for self and household members), traditional (expenses on funerals, births, weddings and other ceremonies), discretionary (clothes, beauty), entertainment (cigarettes/alcohol and other sin goods), unexpected expenditures (any other unplanned expenditures), and other (educational expenses, transport and other). In column 10, I report the total weekly expenditures. The table also contains information on borrowing (columns 11 and 12), net transfers (column 13), deposits and withdrawals (columns 14 and 15). The results are discussed in dollars and in percentage terms by comparing the effect sizes to the mean of the dependent variable[41].

First and foremost, there is a drop in total expenditures and the magnitude is equivalent to the daily losses in income. For every additional blackout experienced during the week, business owners reduce total expenditures by $1.42 (4.7% of weekly expenditures), leading to a total drop of 21% for an average week. This results suggests that some of the blackouts are unanticipated, business owners do not correctly predict the severity of the

[41]The "pure" control mean is not available since the are no weeks with zero blackouts during the data collection

shock and are unable to fully smooth consumption. Examination of the composition of the expenditures offers greater insights into the mechanisms behind the detected drops.

Second, there are no decreases in food consumption (Groceries Category in column 1). However, there is a significant increase in consumption of prepared food. Anecdotal evidence suggests that purchases of ready to eat food increase during power outages due to difficulty of preparing food in the dark or non-functioning electric appliances at home[42]. Consumption of ready-to-eat food is not necessarily an sign of financial prosperity in this context since quality of ingredients used to prepare street food may be inferior to home cooked meals. Prepared meats may be close to expiration dates, cooking oil is reused and there is anecdotal evidence of food poisonings caused by consumption of street food. Thus, consumption of such meals may be welfare reducing on its own due to lower food quality and higher likelihood of illnesses.

Third, there is a statistically significant drop in household expenditures in response to an additional weekly outage, which is equally driven by electricity payments as well as any other household expenditures. The drop in bill payments is likely mechanical and transitory. It is possible that business owners conduct business close to where they live[43]. Consumption of electricity is reduced with the number of outages business experiences which decreases household weekly expenditures on electricity. The other driver of the results is household items and it's possible that business owners choose cheaper alternatives in face of income shocks and buy goods of lower quality.

Fourth, I detect statistically significant decreases in entertainment expenditures.

[42] Only applicable for 39% of households that use electric stoves.
[43] 30% run businesses from home and 70% have households connected to power

These include spending on alcohol, cigarettes and any other sin goods. The decrease is large compared to average expenditures (36%) for every blackout, but in absolute terms the expense is a very small fraction of total expenditures. The reduction in this expenditure category may result from a drop in disposable income and cash on hand, but could also be driven by the fact market areas constitute social settings where friends gather. Skipping work due to blackouts results in lower presence in the shop and less frequent opportunities to engage in social activities and "have a beer". Lastly, there is a decrease in unexpected expenditures but the coefficient is small and economically not significant.

Overall, there is a non-linear response of household-level expenditures to income shocks generated by blackouts and there are complex mechanisms driving individual component of the total expenditures. The large fraction of the decrease comes from household items (possibly purchasing cheaper household products or foregoing some of the less essential items) and bill payments (due to decreases in electricity consumption from blackouts). Decreases in sin good expenditures point to potentially positive impacts of blackouts on long term health. Drops in other unexpected expenditures may be suggestive of households reduced resilience to other income shock or could be driven by drops in other non-essential purchases, which may or may not be welfare reducing. Finally, there is an increase in consumption of low-quality street food likely driven by inability to cook at home. Thus, some of the expenditure categories are responsive to blackouts, however, the essential purchases such as foods remain unaffected, and I conclude that while households are affected by income shocks induced by blackouts, the overall welfare impacts are harder to measure.

1.8 Causal Inference

The underlying assumption of this approach is that conditional on control variables, the assignment of outages is as good as random, unconfounded by other factors. The disruptions are exogenous to individual business owners in a sense they do not have control over when power is out. These businesses are small and electricity company does not incorporate their outcomes in deciding the timing of blackouts. However, since the schedule of load shedding is posted in the newspapers every week and ESCOM follows its schedule[44], it's possible that business owners change behavior in anticipation of an outage. Thus, it is essential to investigate (1) whether outages are correctly predicted by business owners and (2) whether there is change in behavior in anticipation of blackouts in the future.

The design of the surveying tool allows for assessing the accuracy of predictions. At each phone call, a respondent was asked to report the hours of outages they anticipate experiencing tomorrow. At the next phone call (3-4 days later), a respondent was asked the standard questions about the number and duration of outages that the business experienced in the past few days. Thus, for a selected number of days, there are a measure of future expectations of blackouts that can be matched with realized outage. The majority of the time, respondents expected non-zero hours of outages the next day, but this expectation is likely driven by the fact that outages are extremely common. In fact, in follow up surveys, the respondents report that 90% of their knowledge of blackouts comes from the past experience and not public announcements. The test of accuracy of the predictions is reported in Table 1.10. The dependent variable is a binary indicator of an blackout, and

[44]Correlations between scheduled and actual times are high, with correlation coefficients of 0.82, 0.45, and 0.46 for start, end and duration respectively. Source: ESCOM administrative data

the independent variable is a prediction of a blackout made in the past. The probability of a blackout if a respondent does not predict one is 66%, and it increases to 74% if there is a prediction of the blackout. Thus, on average, a business owner can accurately predict less than 20% of blackouts. This result is consistent with intuition since a fraction of blackouts is from the scheduled load shedding that the company implements systematically. Further, I empirically verify expectation of a blackout tomorrow has null effects on behavior today by matching their own future predictions of outages tomorrow to today's outcomes. The results are presented in Table 1.11. Despite anticipating a blackout, business owners do not change labor supply or are able to generate higher income. Thus, expectation of an upcoming shock does not have a real implications on current business outcomes.

Second, it is possible that vendor's activities overload on the system and cause a blackout. In this case, omitting customer volumes would bias the coefficient because it's correlated with both outage instance and shop-level outcomes. However, if this was the case then the coefficient would be upward biased since customer volumes are positively correlated with business outcomes and outages instances, which would make the presented coefficient a lower bound of effect.

Third, the areas across the city are not equivalent in the blackout exposure and a few selected areas rarely experience load shedding (such as hospitals or industrial complexes). It is, thus, possible that more successful businesses relocate to places that more conducive to business. However, in the short run the location of the businesses is fixed. This concern would more valid if the businesses were followed in the long run and strategically moved around the city to conduct businesses in areas less prone to blackouts.

1.8.1 Robustness Checks

Next, I examine the extent to which the estimate on profits changes across various specifications, the results are presented in Table A.9. In column 1, I provide estimates for the unanticipated outages only which constitute 33% of total outages[45]. The coefficient on unanticipated outages is comparable to the main coefficient. Thus, the reported effects are not driven by only anticipated outages.

In columns 2 through 4, I define an outage at the transformer level[46], and assign an outage to all businesses connected to single transformer if more than 50% of businesses report an outage (column 2), if more than 65% report an outage (column 3) and if all but 1 or 2 people report an outage[47] (column 4). The latter is designed to capture outages that the vast majority of businesses agrees on. The coefficients for less strict definitions are smaller than the results from self reports, but the last definition is very similar to the results in the base specification. This suggests that a more objective measure of outages produces results that are comparable to self-reports.

In columns 5, I use neighbors' as an instrument for self-report to address potential measurement errors. All neighbors with an electricity connection within 1 kilometer are considered and are calculated based on GPS locations of the shops. Instrumenting with neighbors reports can be a powerful tool to overcome issues with mis-reporting, however, this setting may not be ideal for two reasons. First, there may be measurement error in

[45]The unanticipated outages are derived from an answer to question of how many hours of outages a respondent expected on that day. If the answer was zero, the blackout was classified as an unanticipated.

[46]Self reports are used for the 14 businesses that have missing transformer information as described in Section 1.3

[47]1 person for transformers with less than 5 businesses, and 2 people for transformers which have more than 5 businesses

neighbors reports. Half of the neighbors are not "heavily-dependent", are not impacted by blackouts as shown in Table 1.3 and may not be able to recall outages precisely. In fact, they are 6% points less likely to report an outage 3 days ago compared to yesterday's reports, suggesting that the accuracy of their reports is compromised. Second, neighbors within one kilometer may be on different transformers and get affected on different times. These two factors reducing the strength of the first stage. In fact, as reported in column 5, the first stage is weak and the IV estimate is noisy.

Recall Bias

Bi-weekly phone interviews provide an opportunity to measure the extent of the recall bias associated with difficulty recalling events in the past, and identify variables that are most affected. In Table A.10, I present measurement of recall bias for the variables of interest. Dependent variables are work hours (column 1), number of customers (column 2), revenues (column 3), profits (column 4) and three measures of outages: binary indicator of whether there was an outage, number of outages, and the duration of outages (columns 5 through 7). The independent variables are recall day fixed effects and should be interpreted relative to values provided for "yesterday" (recall day 1). Recalling outages may be difficult, and furthest events in the past are recalled with lower frequency compared to the yesterdays' reports, but the differences are not statistically significant in recalling an instance of outages (column 5), but are lower for the number of outages reported suggesting that overall the there is no systematic recall bias for binary reports, but more detailed information such as the number or duration is harder to recall. I verify the to which extent the recall bias impacts the estimates by only including reports from yesterday (Table A.11). The coefficients on

profits are larger but only by \$0.2 suggesting that the recall bias associated with recalling events in the recent past is not substantial.

Validity of Outage Reports

In order to check the validity of the reports I perform a simple exercise in which I calculate the agreement rate between the neighbors and benchmark it against a placebo agreement rate. First, I generate non-exclusive neighbor pairs using the GPS coordinates of the respondents[48]. Second, I compare the reports within the neighbor pair and calculate agreement rate between two reports within a date defined as 1 if the two reports agree (are either both 0 or both 1) and 0 if the reports disagree and are non-missing. If one of the reports is missing, the observations are not included in the calculation. Next, I find the percentage of times neighbors agree on reports and generate an average agreement rate for each neighbor pair. High agreement rate between the neighbors' reports suggest the reports are accurate. Placebo agreement rate is generated using the same procedure, however, the true reports are replaced with randomly generated reports with probability of 0.71 reporting an outage and 0.29 reporting a non-outage, which correspondents to probability of reporting an outage in the data. Only non-empty reports in the original data are replaced with the randomly generated report. The results exercise for both real and placebo agreement rate are presented in Figure A.7. If the agreement rates in the real data were a result of a random chance, the distribution of the real agreement rate would resemble the grey line. Rightward skewness of the distribution of the real agreement relative to placebo rates suggests that the reports are not correlated by mere chance. Comparison of the two distributions via

[48]Neighbors are shops located within 1 km of each other. Out of 142 users, there were 134 neighborhoods

Komogorov-Smirnov test yields a p-value of zero, suggesting that the two distributions are statistically distinct.

1.9 Selection into Electricity-Dependent Occupations

Despite the average electrification level of 42% in urban Malawi, only 25% of the businesses are connected to the grid. Further, in spite of numerous outages that create volatility in income, the average profitability of businesses connected to the grid is still higher by 25% higher compared to those not connected. Why don't more businesses connect to the grid? I offer two explanations for this results. First, they are capital constrained and unable to invest in business upgrades. In addition, uncertainty of power supply makes it even more challenging to undertake risky investments. Second, there are unobservable factors such an entrepreneurial potential that drive the selection and are also correlated with business growth. While I am unable to determine which factors cause selection into electricity dependent occupations since all the measurements were taken post factum, correlates between owners' characteristics and business electrification may shed light on possible drivers of the decision.

First, examination of business type composition by connection in Figure A.6 from the project's census data on 1,937 businesses reveals that connection to the grid is highly correlated with occupation choice. Except for a few business types (convenience stores, tailors, and bars), the majority of occupations are either electricity-dependent or not. Electricity dependent businesses also require more specialized training (welding, tailoring, repairing and selling electronics), computer skills (printing and music services) or investments into

more costly inventory with potentially higher returns (electronics, hardware). In contrast, businesses that do not depend on power require little specialization (produce and charcoal sellers, and convenience stores). Thus, the decision to run an electricity-connected business is highly correlated with occupation choice.

Second, in Table A.12 Panel A, I present baseline characteristics of the three groups: businesses not connected to the grid (column 1), differences/pvalues between those and connected businesses which do not do not have appliances (columns 3 and 4), differences/pvalues between non-connected and connected the "highly-dependent" category with appliances (columns 4 and 5). The comparison between the three groups allows to observe baseline correlates with decisions to select more "sophisticated" business models.

Businesses connected to the grid (with and without appliances) are more educated and predominantly males. Examination of differences in business outcomes reveals those businesses are larger nearly by all measures: revenues, profits, number of employees, the value of inventory and equipment. All of the differences are large and statistically significant, except the experimentally obtained measures of patience, loss and risk aversion[49]. Financial access between the two groups is also evident: businesses connected to the grid are 57% - 73% more likely to have a bank account. The sheer difference in characteristics between those connected and not are parallel to those found in the literature between businesses that grow and those that do not ([106], [128], [95]).

However, an interesting pattern emerges in examination of the differences be-

[49]Impatience is measured as the percentage of time respondent chooses an earlier payment between today and 2 weeks later (or 2 weeks vs 4 weeks later) at various interest rates. Loss aversion index is the percentage of times a respondent chooses not to play a game in which the given amount amount would be lost. Risk preference index is the percentage of times an individual chooses the less risky investment between the two options. All game responses were incentivized by a random question chosen for a payout.

tween businesses with a connection (without appliance) and those with appliances (which are highly dependent on electricity supply). Despite significantly higher investments into equipment, the profitability of those businesses is compromised by the frequent blackouts, and they make $31.14/month less compared to those who do not rely on power as heavily (column 2). Thus, in the state of the world with frequent power outages business expansion has diminishing and even negative returns, and past a certain point it becomes less profitable to graduate into more "sophisticated" businesses. This result explains the prevalence of businesses that do not rely heavily on power. Since the only way to maintain profitability for a "highly-dependent" business is to invest into self-generation, business owners that are credit constrained will abstain from adopting these business models. Furthermore, it may discourage human capital investments since professions with specialized skills (which also require electrical tools) offer worse average returns. Overall, the lack of incentives to pursue more "sophisticated" careers may have long term consequences on development and growth of the economy. Higher educated employees with better earning potential suffer from poor infrastructure and are hindered from investments into equipment and creation of innovative businesses.

1.10 Discussion and Conclusion

In this paper I estimate short term effects of electricity outages on small businesses urban Malawi. First, I document that an average businesses experience interruptions on 67% of the days, each lasting 6.6 hours. Such frequent power outages partially explain why only 25% of small business owners are connected to the grid. Decision to run an electricity-

dependent business is highly correlated with baseline characteristics of the business owner (education, gender), financial access and initial wealth levels. Those businesses are also likely to require more specialized skills. However, heavy reliance on power is not profitable due to frequent power outages and earnings of those with the highest values of equipment are similar to businesses not connected to the grid.

Second, I establish that the effects on an average business are zero, however, that is driven by the fact that only 40% of the sample are connected to the grid. Even among those businesses, the effects are null for those without electric appliances. However, I document a 25% decrease in daily income among a sub-group which are "highly-dependent" on power and rely on electricity supply to generate income. Businesses incur losses only from outages longer than 5 hours, suggesting that they are able to make up for losses within a day during shorter blackouts. The documented losses are not transferred within a business across time (as confirmed with specifications with lagged variables and estimating effects over aggregated weekly outcomes) and are likely permanent to the individual business owner. Self-generation is a powerful strategy for mitigating losses to businesses, however, are only owned by 11% of businesses connected to the grid. Losses to sales are unlikely all permanently lost to the economy as a whole and are plausibly transfers to larger enterprises who rely on self-generation.

Third, I document a decrease in expenditures for businesses "highly-dependent" on power, and the decrease is equivalent to the reduction in daily income, suggesting that households are not able to smooth consumption over the income shocks, but the responses are complex. In close examination of dis-aggregate expenditure categories, I find statistically

significant effects are found for expenditure categories that constitute a smaller fraction of total expenditures, and find no evidence of changes in expenditures on essential food purchases. The large fraction of the decrease comes from household items and bill payments. The latter is likely driven by decreases in electricity consumption from blackouts. Decreases in sin good expenditures point to potentially positive impacts of blackouts on long term health. Finally, there is an increase in consumption of low-quality street food likely driven by inability to cook at home, which may be a welfare reducing outcome since this food is likely of lower quality. Thus, some of the expenditure categories are responsive to blackouts, however, the essential purchases such as foods remain unaffected, and I conclude that while households are affected by income shocks induced by blackouts, the overall welfare impacts are harder to measure.

Power supply is likely to remain challenging in the near future and the permanent solution is to increase generation capacity, improve infrastructure and diversify sources of generation. However, those solutions are costly and require large scale investments that may not be attainable to developing countries in the short run. Under the status-quo i.e. with continued power outages, there are several steps that can be taken to alleviate the burden of blackouts on small businesses. First, businesses in market areas can engage in cost sharing schemes and acquire a generator. Anecdotal evidence suggests that businesses already do that but the practice is not common. A back of the envelope calculation suggests that generator cost can be made up within 1 month[50]. Further, cost sharing will only work for market areas since shops have to be in close physical proximity. Such schemes, even

[50] Assumptions on the costs are obtained by the project field team. A large generator (costing about $270) can power about 10 small shops. Each shop would need to contribute $27 up front (23% of the monthly profit). Given the average losses are $1.12 USD, the costs of the generator can be made up in 24 blackout days. Fuel costs per shop per day are $0.69 ($6.94 in total).

if economically justified, could be challenging to implement due to moral hazard of using shared equipment, difficulty of determining true usage costs of each contributor, and credit and savings constraints that inhibit the ability of small firms to invest in equipment in the first place. Instead, the generator could be owned by a single business in the market areas and provide generation services to neighboring shops. Under this scheme and assuming the same costs, the business can rent out electricity at cost of $1 per shop per day (lower than the average losses but higher than fuel costs), generating $10 in revenues and $6.94 in profits. This fixed cost of the investment is recovered after operating for approximately 38 blackout days.

The second solution is to increase salience of scheduled outages and improve ESCOM's ability to communicate directly with customers. Baseline data suggests that the main source of scheduled blackouts is other people or past experiences. However, that could be subject to errors and costly to acquire. Further, the newspapers where ESCOM posts announcements cost about $0.83 (17% of daily profits), which may not be affordable for small scale businesses. Given the prevalence of cell phone technology (92% of respondents had a cell phone at baseline) ESCOM can adopt targeted text messages which provide information about upcoming blackouts. In the follow up survey, 89% of all business owners would find this service useful, with willingness to pay 5.3 times larger than a cost of a text message. Informational text messages are shown to have positive impacts on voter turnout ([7], [103]), adherence to medical treatments ([78]), after-school program attendance ([107]), and may also improve organization's public image. Access to information about upcoming blackouts may help businesses plan for the productivity shocks by strategically timing pur-

chases of perishable inventory, seeking alternative income opportunities or more precisely adjusting working hours to match the blackout schedule. Lastly, shorter outages (less than 5 hours during the day) are less welfare reducing for small firms, thus, depending on feasibility electricity companies may consider switching to shorter but more frequent blocks of load shedding.

1.11 Tables and Figures

Figure 1.1: Distribution of Businesses in the Study: Connected without appliances(yellow), Collected with appliances (orange), Non-Connected (blue)

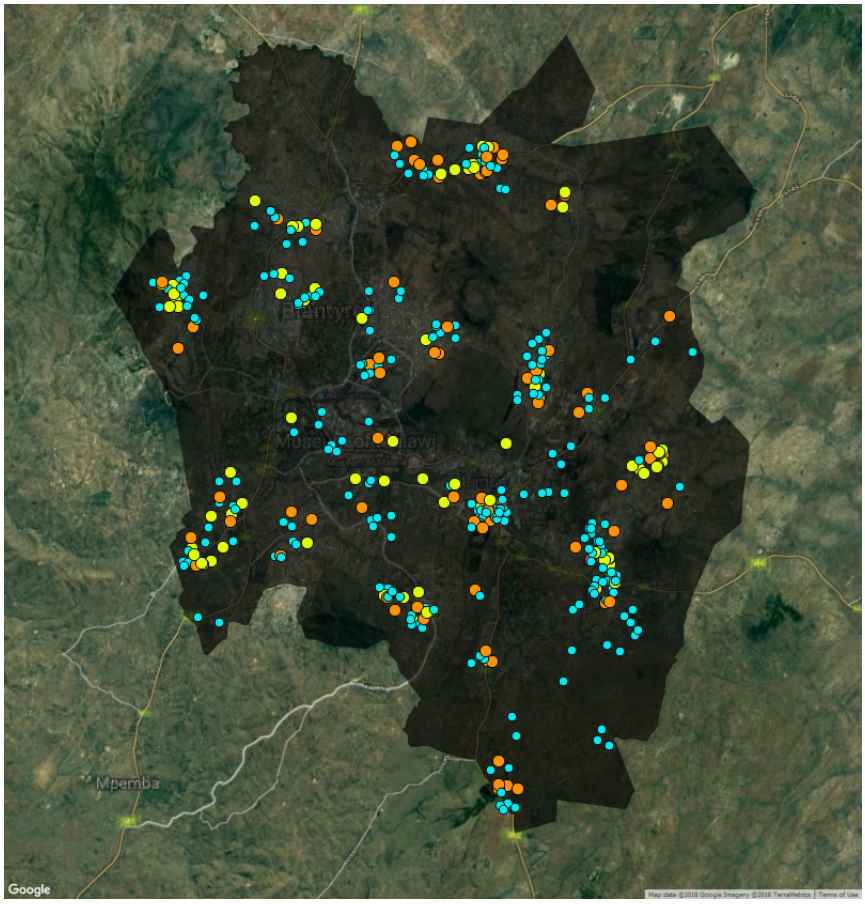


Figure 1.2: Outages (stars) and Non-Outages (green squares) on September 29th 2017

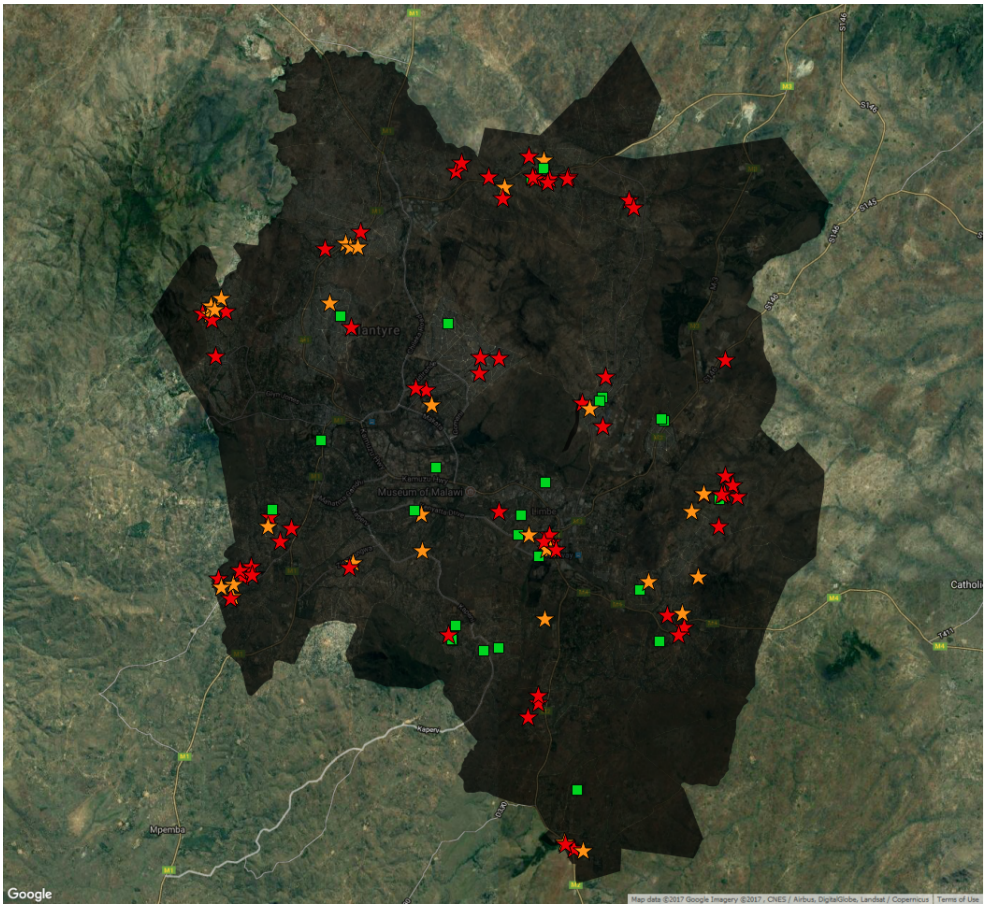


Table 1.1: Sample Characteristics of Businesses Connected to the Grid

	(1) All Businesses Mean/SD	(2) All Connected Mean/SD	(3) "Highly Depedent" Mean/SD
	N=400	**N=142**	**N=74**
Panel A: Demographics			
Male	0.535	0.662	0.730
Married	0.765	0.732	0.635
Age	34.127	33.852	31.770
Education	9.065	9.754	10.135
House Connected to Electricity	0.605	0.754	0.676
Panel B: Business Outcomes			
Number of Employees	0.153	0.303	0.378
	[0.024]	[0.056]	[0.088]
Monthly Revenue	352.487	387.717	217.078
	[20.568]	[41.094]	[28.954]
Monthly Profit	84.641	100.719	85.792
	[3.723]	[7.314]	[7.255]
Value of Inventory	104.880	162.803	63.542
	[10.179]	[21.817]	[16.712]
Value of Tools and Equipment	91.609	179.693	207.036
	[7.423]	[15.836]	[22.499]
Panel C: Financial Access & Security			
Has a Bank Account	0.320	0.430	0.446
	[0.023]	[0.042]	[0.058]
Total Value of Assets	617.064	936.431	838.175
	[38.056]	[81.171]	[111.557]
Total Value of Cash Savings	103.660	133.498	125.034
	[6.850]	[13.503]	[18.157]
Dual Income HH	0.417	0.352	0.311
	[0.025]	[0.040]	[0.054]
Has a Secondary Earning Activity	0.223	0.282	0.257
	[0.021]	[0.038]	[0.051]
Second Earning: Average Monthly Income	12.418	21.893	14.934
	[2.187]	[5.165]	[6.189]
Panel D: Electricity Usage			
Appliances			
Salon, Barbershop Tools	0.093	0.268	0.500
Entertainment	0.088	0.254	0.243
Fridge	0.056	0.159	0.000
Tools for Business	0.048	0.138	0.257
Computer, Other Office Equipment	0.033	0.094	0.176
Has Alternative Power Supply	0.037	0.106	0.081
Generator/Battery	0.033	0.600	0.500
Solar Panels	0.007	0.133	0.167
Panel E: Outages			
Business Affected by Outages	0.412	0.831	0.946
Percentage of Outages Known About	0.466	0.417	0.400
Conditional on Knowing, source of information:			
Newspaper	0.040	0.089	0.129
Radio	0.314	0.375	0.290
Word of Mouth	0.606	0.464	0.548
Always on the Same Day	0.051	0.054	0.097

Note: Baseline Data. All monetary values are in USD, winsorized at 5% to address outliers

Table 1.2: Outage Characteristics from Self-Reports

	(1)	(2)	(3)	(4)
	All Businesses N=391		**"Highly Dependent" N=70**	
	Mean (SD)	**N**	**Mean (SD)**	**N**
Panel A: Number of Outages				
Percent of Days with Outages	0.67	391	0.77	70
Number of Outages Per Day	1.07	391	1.05	70
	(0.14)		(0.10)	
Panel B: Conditional on Having an Outage				
Percent of Outages Affected Other Shops	–	–	0.98	70
Duration (out of 24 Hours)	9.54	389	9.72	70
	(2.36)		(1.91)	
Duration (7am-6pm)	6.54	389	6.63	70
	(1.57)		(1.24)	
Percentage of Outages That the	0.33	388	0.36	70
Respondent Reports Not Knowing About in Advance	(0.24)		(0.25)	
Duration of Outage that	8.88	362	9.16	65
Respondent Reports Not Knowing About	(2.51)		(2.52)	
Duration of Outages that	10.38	381	10.23	69
Respondent Reports Knowing About	(2.39)		(2.21)	
Percentage of Hours of that	0.60	388	0.62	70
the Respondent Reports Not Knowing About	(0.18)		(0.17)	
Panel C: Spatial and Temporal Variation				
Within Day: ICC of Outage Reports				
Ward	0.18	18598	0.18	3232
EA	0.21	18598	0.20	3232
Transformer	–	–	0.30	3232
SD of Outage Duration Within a Business Across Time	5.17	391	5.12	70
Percentage of Alternating Outage/Non-Outage Day	0.30	391	0.27	70

Note: Averages are calculated within individual.

Table 1.3: Effects of Outages on Business Outcomes For All Businesses, Full Sample

	Labor Supply			Income				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Extensive Margin							
	Worked Today	Worked Second Occupation	Total Work Hours	Primary Busines Customers	Revenue	Profit	Secondary Occupation Profit	Total Income
Panel A: All Businesses								
Outage	0.002	0.001	-0.174	-0.444	-0.255	-0.107	0.011	-0.091
	(0.009)	(0.005)	(0.109)	(0.31)	(0.27)	(0.07)	(0.03)	(0.08)
Observations	18,598	18,598	18,598	18,384	18,586	18,451	18,591	18,595
# of Businesses	391	391	391	391	391	391	391	391
Non-Outage Mean	0.82	0.05	8.23	17.32	14.49	3.499	0.284	3.687
Non-Outage SD	0.38	0.22	4.79	18.6	19.16	3.859	1.8	4.012
Panel B: All Connected								
Outage	-0.044***	0.003	-0.970***	-1.785***	-0.75	-0.589***	0.027	-0.538***
	(0.016)	(0.012)	(0.253)	(0.54)	(0.46)	(0.15)	(0.07)	(0.16)
Observations	6,428	6,428	6,428	6,326	6,424	6,388	6,424	6,427
# of Businesses	138	138	138	138	138	138	138	138
Non-Outage Mean	0.85	0.07	9.10	18.63	17.34	4.779	0.439	5.033
Non-Outage SD	0.36	0.25	5.04	19.54	22.01	4.428	2.396	4.562

Note: Estimates are presented for all businesses connected to the grid in Panel A, for businesses connected to the grid but without electric appliances in Panel B. Independent variable is a binary indicator of whether there was an outage on a particular day. All regressions include individual, date, recall day fixed effects and lottery wins as control variables. Monetary outcomes are expressed in USD. Monetary outcomes and customers and are winsorized at 5% level. Standard errors are clustered at the transformer level for businesses connected to the grid and at the enumeration area level for businesses not connected to the grid.

Table 1.4: Effects of Outages on Business Outcomes For All Businesses, Full Sample

	Labor Supply			Income				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Extensive Margin							
	Worked Today	Worked Second Occupation	Total Work Hours	Primary Busines Customers	Revenue	Profit	Secondary Occupation Profit	Total Income
Panel C: Connected but without Appliances, non "Highly-Dependent" group								
Outage	0.009	-0.011	-0.088	-0.751	-0.088	-0.038	0.012	-0.06
	(0.021)	(0.020)	(0.240)	(0.82)	(0.84)	(0.20)	(0.12)	(0.21)
Observations	3,196	3,196	3,196	3,101	3,195	3,164	3,195	3,195
# of Businesses	68	68	68	68	68	68	68	68
Non-Outage Mean	0.83	0.08	9.57	26.59	23.03	4.998	0.556	5.298
Non-Outage SD	0.38	0.28	5.50	21.55	24.43	4.766	2.774	4.912
Panel D: Sub-group with Appliances, "Highly-Dependent" group								
Outage	-0.095***	0.018	-1.799***	-2.702***	-1.463**	-1.132***	0.05	-1.024***
	(0.026)	(0.011)	(0.408)	(0.69)	(0.57)	(0.21)	(0.07)	(0.22)
Observations	3,232	3,232	3,232	3,225	3,229	3,224	3,229	3,232
# of Businesses	70	70	70	70	70	70	70	70
Non-Outage Mean	0.87	0.05	8.66	11.36	11.95	4.575	0.328	4.783
Non-Outage SD	0.34	0.23	4.52	14	17.85	4.078	1.967	4.192

Note: Estimates are presented for businesses connected to the grid without appliances in Panel C and with electric appliances in Panel D. Independent variable is a binary indicator of whether there was an outage on a particular day. All regressions include individual, date, recall day fixed effects and lottery wins as control variables. Monetary outcomes are expressed in USD. Monetary outcomes and customers and are winsorized at 5% level. Standard errors are clustered at the transformer level for businesses connected to the grid and at the enumeration area level for businesses not connected to the grid.

Table 1.5: Effects of Outage Duration on Business Outcomes, "Highly-Dependent" Sub-sample

	Labor Supply			Income				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Extensive Margin							
	Worked Today	Worked Second Occupation	Total Work Hours	Primary Busines Customers	Primary Busines Revenue	Primary Busines Profit	Secondary Occupation Profit	Total Income
Duration in hours								
Before 7am	-0.019**	0.006	-0.111	-0.404***	-0.373***	-0.126***	0.075	-0.048
	(0.009)	(0.003)	(0.098)	(0.145)	(0.115)	(0.036)	(0.050)	(0.050)
7am-6pm	-0.017***	0.002	-0.264***	-0.462***	-0.230***	-0.150***	0.005	-0.139***
	(0.00)	(0.00)	(0.05)	(0.10)	(0.07)	(0.02)	(0.01)	(0.03)
6pm-12am	-0.004	0.004*	-0.127**	0.004	0.043	-0.028	-0.002	-0.024
	(0.00)	(0.00)	(0.05)	(0.09)	(0.08)	(0.03)	(0.02)	(0.03)
Observations	3227	3227	3227	3220	3224	3219	3224	3227
R-squared	0.12	0.03	0.13	0.09	0.04	0.07	0.03	0.06
# of Businesses	70	70	70	70	70	70	70	70
Non-Outage Mean	0.87	0.05	8.66	11.36	11.95	4.58	0.33	4.78
Non-Outage SD	0.34	0.23	4.52	14.00	17.85	4.08	1.97	4.19

Note: Independent variable is duration expressed in hours before start of the work day, during the work day and after the work day . All regressions include individual, date, recall day fixed effects and lottery wins as control variables. Monetary outcomes are expressed in USD. Monetary outcomes and customers and are winsorized at 5% level. Standard errors are clustered at the transformer.

Table 1.6: Effects of Outages on Business with Lagged Terms, "Highly-Dependent" Sub-sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Worked Today	Work Hours	Num Customers	Main Revenues	Main Profits	2nd Income	Total Income
Outage (B1)	-0.094***	-1.804***	-2.694***	-1.516**	-1.149***	0.05	-1.039***
	(0.03)	(0.41)	(0.67)	(0.56)	(0.21)	(0.07)	(0.22)
Outage Yesterday (B2)	0.023	0.168	0.71	0.417	0.169	-0.038	0.17
	(0.02)	(0.26)	(0.49)	(0.47)	(0.18)	(0.09)	(0.19)
Outage 2 Days Ago (B3)	-0.01	-0.416**	-0.553	-0.604	0.036	-0.008	0.03
	(0.02)	(0.19)	(0.48)	(0.38)	(0.16)	(0.06)	(0.17)
Observations	3,232	3,232	3,225	3,229	3,224	3,229	3,232
R-squared	0.079	0.087	0.048	0.035	0.051	0.023	0.045
# of Businesses	70	70	70	70	70	70	70
Non-Outage Day Mean	0.866	8.663	11.36	11.95	4.575	0.328	4.783
Non-Outage Day SD	0.341	4.515	14	17.85	4.078	1.967	4.192
P-Val from F-test: B2+B3=0	0.59	0.386	0.799	0.765	0.402	0.553	0.393

Note: Independent variable is a binary indicator of whether there was an outage on a particular day, yesterday, and the day before yesterday. All regressions include individual, date, recall day fixed effects, lottery wins as control variables and controls for missing days. Monetary outcomes are expressed in USD. Monetary outcomes and customers and are winsorized at 5% level. Standard errors are clustered at the transformer level.

Table 1.7: Heterogeneous effects on businesses in Market Areas, with Secondary Income Sources and with Alternative Power Supply

	Low vs High Business Density Areas				Secondary Income Source at Baseline				Alternative Power Supply			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Workd		Total	Total	Workd		Total	Total	Workd		Total	Total
	Main	Secondary	Hours	Income	Main	Secondary	Hours	Income	Main	Secondary	Hours	Income
Panel A: Low vs High Business Density Areas												
Outage	-0.153***	0.001	-2.811***	-1.440***								
	(0.05)	(0.02)	(0.79)	(0.36)								
Outage*High Density	0.076	0.03	1.786**	0.735								
	(0.06)	(0.02)	(0.86)	(0.46)								
Pabel B: Secondary Income Source at Baseline												
Outage					-0.118***	0.01	-2.048***	-1.182***				
					(0.03)	(0.01)	(0.44)	(0.25)				
Outage*2nd Income					0.03	0.02	0.80	0.50				
					(0.06)	(0.03)	(0.74)	(0.61)				
Panel C: Alternative Energy Source												
Outage									-0.115***	0.02	-1.924***	-1.160***
									(0.03)	(0.01)	(0.45)	(0.23)
Outage*Alt Energy									0.047	-0.017	1.219**	1.319**
									(0.04)	(0.02)	(0.56)	(0.61)
Observations	3,232	3,232	3,232	3,232	3232	3232	3232	3232	3232	3232	3232	3232
# of Businesses	70	70	70	70	70	70	70	70	70	70	70	70
Non-Outage Mean	0.854	0.0549	8.663	4.783	0.85	0.05	8.66	4.78	0.85	0.05	8.66	4.78
Non-Outage SD	0.353	0.228	4.515	4.192	0.35	0.23	4.52	4.19	0.35	0.23	4.52	4.19
Pval (B1+B2)	0.01	0.02	0.00	0.01	0.07	0.28	0.08	0.21	0.02	0.86	0.03	0.79

Note: Independent variables are a binary indicator of whether there was an outage on a particular day and an interaction of an outage with a baseline characteristic. All regressions include individual, date, recall day fixed effects and lottery wins as control variables. Monetary outcomes are expressed in USD winsorized at 5% level. Standard errors are clustered at the transformer level.

Table 1.8: Effects of Outages on Household Weekly-Level Outcomes, "Highly-Dependent" Sub-sample (Part 1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Expenditures									
	Food									
	Groceries	Ready To Eat	HH	Med.	Trad.	Dsic.	Entertain.	Unexp.	Other	Total
# of Outages	-0.013	0.174**	-0.862***	-0.033	-0.043	0.053	-0.141**	-0.239*	-0.19	-1.412**
	(0.19)	(0.07)	(0.29)	(0.06)	(0.07)	(0.13)	(0.05)	(0.12)	(0.43)	(0.68)
Observations	472	473	473	473	473	473	473	473	473	473
R-squared	0.097	0.153	0.294	0.181	0.116	0.146	0.145	0.192	0.147	0.194
# of Businesses	70	70	70	70	70	70	70	70	70	70
Dep Var Mean	10.37	2.289	5.212	0.66	1.481	3.386	0.39	0.23	6.656	29.71
Dep Var SD	6.072	2.512	7.864	1.781	2.359	3.419	1.352	1.687	10.06	20.7
# Outages/week Mean	4.531	4.531	4.531	4.531	4.531	4.531	4.531	4.531	4.531	4.531
# Outage/week SD	2.006	2.006	2.006	2.006	2.006	2.006	2.006	2.006	2.006	2.006

Note: Independent variables are: number of outages per week (aggregated from individual reports), aggregated lottery payment wins for the week, and a number of non-missing days in the week. Dependent variables are weekly expenditures expressed in USD and winsorized at 5% level. Individual and call date fixed effects are included. Errors are clustered at the transformer level.

Table 1.9: Effects of Outages on Household Weekly-Level Outcomes, "Highly-Dependent" Sub-sample (Part 2)

	(11)	**(12)**	**(13)**	**(14)**	**(15)**
	Loans		Transf.	Savings	
	Taken	Given	Net	Deposits	Withdrawals
# of Outages	0.375	0.303	0.072	-0.136	0.12
	(0.40)	(0.28)	(0.45)	(0.37)	(0.37)
Observations	473	473	473	473	473
R-squared	0.091	0.165	0.123	0.139	0.144
# of Businesses	70	70	70	70	70
Dep Var Mean	1.69	2.11	-0.42	14.55	4.01
Dep Var SD	8.43	6.53	9.85	17.90	12.14
# Outages/week Mean	4.531	4.531	4.531	4.531	4.531
# Outage/week SD	2.006	2.006	2.006	2.006	2.006

Note: Independent variables are: number of outages per week (aggregated from individual reports), aggregated lottery payment wins for the week, and a number of non-missing days in the week. Dependent variables are weekly expenditures expressed in USD and winsorized at 5% level. Individual and call date fixed effects are included. Errors are clustered at the transformer level.

Table 1.10: Relationship Between Predicted and Realized Number of Outage Hours: Real vs. Simulated

	(1) Realized Outage (Binary)
Prediction of an Outage (binary)	0.123**
	(0.05)
Observations	476
R-squared	0.015
Number of Business	66
Average Y if X=0	0.622

Table 1.11: Effects of Anticipating an Outage Tomorrow on Business Outcomes Today, "Highly-Dependent" Sub-sample

	(1) Worked Y/N	**(2)** Hours Worked	**(4)** Main Business Revenue	**(5)** Profit	**(6)** Total Income
Prediction of an Outage Tomorrow	-0.018	-0.598	0.154	0.086	-0.228
	(0.06)	(0.50)	(0.75)	(0.34)	(0.25)
Observations	442	442	442	442	442
R-squared	0.06	0.08	0.05	0.05	0.10
# of Businesses	64	64	64	64	64
Non-Outage Mean	0.82	8.11	17.04	3.96	0.14
Non-Outage SD	0.39	5.02	23.46	4.23	1.56

Note: Monetary outcomes are expressed in USD and are winsorized at 5% level. Individual and date fixed effects are included. Lottery wins included as a control. Errors are clustered at the transformer level.

Chapter 2

Power Outage Responses among Market Vendors in Urban Malawi

2.1 Introduction

There is general agreement among development practitioners that electrification is instrumental for long term economic growth ([98]). However, the current access to electricity in Sub-Saharan Africa is poor and 70% of the population live without power (International Energy Agency 2013). Thus, achieving universal electrification of the developing countries has been a major priority for the development organizations in the recent years, with World bank allocating 17% of its 2016 budget towards energy projects (The World Bank Annual Report 2016). However, access to the grid is only the first step to reaping the benefits of electrification because being connected does not guarantee reliable power supply. Specifically, in Sub-Saharan Africa, existing customers experience about 9 outages a month, each last-

ing 5.6 hours on average ([11]). Such frequent power outages significantly limit the claimed welfare gains from electrification, and only recently development organizations have started to recognize the importance of quality of power versus focusing solely on new connections ([97], [115]).

The reasons for power outages are diverse and complex, and vary based on the location but there are common factors that developing countries share. First, current electricity supply is limited to due decaying infrastructure and lack of funding. Second, the demand for connections is expanding quickly with population growth. In fact, forecasts predict that by 2035 consumption of energy in developing countries will grow by 84% ([131]), compared to 14% in developed countries. If adequate infrastructure improvements are not made to match the growing demand, the existing systems will experience greater overload. Lastly, generation of electricity heavily relies natural resources and some are vulnerable to weather fluctuations. 42% of the countries in Africa rely on hydro-generation[1], with 21% of the countries being fully depended on it. The recent instability in rain schedules has led to longer dry seasons and low water levels that feed the generation plants ([24]), cutting the generation capacity to 50% across the region. This has been the largest cause of frequent load-shedding in the past decades in countries like Malawi, Zambia, Kenya, Tanzania, Zimbabwe and Mozambique ([32]). While there is claimed untapped potential in hydro-power generation across the continent[2], the future water levels in rivers and reservoirs is unclear due to irregularities in rainfall which threatens the capacity to provide reliable

[1]Source: World Bank development Indicators. Data on electricity generation is missing for 46% of the Sub-Saharan countries. Out of the countries for which the data is not missing, 42% of the countries rely on hydro power for more than half of electricity generation

[2]Technically feasible power generation is 1,750,000 GWh/year, compared to current 76,000 GWh/year. Source: 2001 World Atlas and Industry Guide International Journal of Hydropower and Dams

power ([105]). Thus, given the status-quo, the electricity provision is unlikely to improve in the near future.

Given the current and persistent challenge of power supply in developing countries, it is important to understand the economic implications of electricity interruptions in the short run - to measure the immediate impacts, and the long run - to gauge the effects on growth of the economy. Poor power supply limits foreign capital flows reducing private sector development ([17]), hinders business expansion by creating a barrier to investment into more productive technologies ([1], [110]), causes losses to GDP and decreases economic growth ([12]). In fact, electricity interruptions are listed as the third biggest obstacle for business development(World Bank Enterprise Survey 2016). To ameliorate the impacts of power outages, manufacturing plants devise various adaptation strategies, such as financing alternative power course, shifting operations from less to more productive hours, and changing input composition ([70], [9], [11], [118]). In contrast, little is known about the impacts of interruptions on smaller firms. This is an especially important question, given the extent to which small business contribute to emerging economies: they bring at least 33% to the national income and provide 45% of total employment(World Bank 2015). On the one hand, the burden may be higher for smaller firms due to a proportionally higher cost of acquiring or paying for alternative energy. On the other hand, business owners may be well adapted to the fragile power supply, and are able to shift income inter-temporally and work more or harder on more productive days or hours. In fact, only a single study presents quasi-experimental results of power interruptions on small business owners - [79].

This project adds to the growing literature on the measurement the economic

impacts of unreliable power supply on the daily operations of small business owners in a developing country. Using high frequency data, I estimate the effects of power interruptions on business outcomes such as labor supply, revenues and profits. In contrast to [79] who only look at the specific business type - tailors, I study the impacts of power interruptions on all types of small businesses that do and do not rely on electricity. This gives an opportunity to examine the behavior of businesses that directly rely on power and compare them to non-electricity reliant businesses. High frequency data on daily business outcomes allows for an estimation of impacts of electricity outages within an individual business across time, comparing outcomes on days with no power interruptions to days with power interruptions. The aims of this paper are the following

(1) Document the reliance of on power supply among small business owners in a developing country.

(2) Provide descriptive statistics of power supply effect among small business owners.

(3) Estimate the impacts of the power supply interruptions daily activities. What are the effects of outages on business outcomes? Are businesses well adapted to the interruptions in electricity?

My results show that outages are very frequent and affect over half of the days during the data collection. Second, I learn that among market vendors, only 31% actually use electricity for conducting business and the rest are not electricity dependent. Third, I examine differential impacts on two types of businesses: electricity users (businesses that

directly rely on electricity), and non-users (business that are not electricity dependent), and find that users lose 7.3%-15.8% of daily revenue due to electricity outages, however, this does not affect self-reported daily profits. Effects on non-users range from 0%-3.5% losses in revenue, but I refrain from conclusions because those effects do not hold across robustness checks. Lastly, I do not find evidence of shifts in labor supply in response to outage. I hypothesize, that given the fact that outages are unpredictable in duration, business owners tend to wait out the outage in their shops rather than adjust labor supply.

The paper is structured as follows. In section 2, I discuss relevant strands of literature; section 3 gives background on electricity generation and provision of power in Malawi. In section 4, I describe data and the study. In section 5, I focus on estimating strategy and inference. Section 6 discusses results and presents robustness checks. I conclude with a discussion of results in Section 7 and 8.

2.2 Background

The importance of infrastructure, electricity in particular, and its impact on development has largely been recognized and studied. In particular, a number of studies examine the effects of providing new connections to previously under-served areas on various outcomes from household productivity and earning opportunities to health and children' education. Most of the studies conclude that electrification has positive impacts on welfare, however, exhibit fluctuations across contexts. There are documented positive impacts of rural electrification on employment through expansion of employment opportunities for household members ([50], [132], [75], [55], [119], [47]). The main mechanisms behind the

results are a) increased work hours due to extended hours with light b) employment shift from agricultural to non-agricultural sector, and c) freed up time from household work. Positive effects are found on rural economic development in [92], [115], [100], but this result is challenged by [41]. However, substitutes to electrification (such a provision of solar micro-grids) do not bring about changes in socioeconomic developments [8]. Positive effects of electrification on household income are found in [100], [88], [89], [109], [115] but are not supported in [26]. Improvements in education outcomes for children are documented in [90], [5], [127], [115], but are questioned in [27], [26] and [119]. Better health outcomes due to electrification are found in [112] and [21], with the main mechanism being improvements in air due to substitution away from kerosene lights into electrical bulbs. Electricity provision had positive impacts on productivity: manufacturing sector [113]) and in agriculture ([94]), but are not found in [93].

Recent literature has started to recognize the importance of power quality and documents losses of benefits from electrification due to frequent blackouts. [47] find that a newly established connection increases non-farm household income only by about 9% in 10 years, while the higher quality of electricity contributes to increased in the income by 29%. [115] find negative and significant impacts on virtually all development indicators with increased hours of blackouts. The magnitude is particularly striking, and every hour increase in outages is associated with 0.3 percent reduction in annual income. [40] and [68] report a permanent increase in birth-rates and lower birth weights for the babies born during black-out periods; with latter documenting worsening of long-run economic well-being for the mothers who had an unplanned pregnancy during the power outages.

Significant attention has been dedicated to study the impacts of power quality on manufacturing plants and large firms, showing that businesses incorporate expectations of reliability of electricity in their short and long term decisions. In the long run, firms report that power considerations have influenced their choice of location (59.8%), input combination (53%), and even the type of business (53%) they entered into. Alternative private power supply is expensive, and makes up 15% of equipment costs for large firms but are 25% of all costs for smaller firms. ([118], [1]). In the short run, the effects of power outages on firms depends on initial power reliance, and some businesses are able re-optimize the means production to mitigate the effects of power outages by using a different composition of inputs. In particular, [9] finds that effect on production is dependent on initial reliance on power. Firms that are able to shift operations to non-outage hours, make up for the losses incurred during outage times. Firms that are not flexible in the input composition, incur greater costs due to inefficient allocation of inputs. Firms that are not able to produce without electricity incur losses of 8.9% in response to a 10% increase outage. [11] further investigate the burdens of the interrupted power supply on manufacturers, and estimate the costs related to private provision through generators to be 0.18% of revenues for every 1% point increase in power interruptions, with smaller manufacturing plants being impacted more. Costs of production increase due to electricity interruptions by 9-22% due to shift of factors of production and switching from "make" to "buy" in obtaining intermediate goods ([70]).

The most relevant papers to this study are [39] and [79]. [39] study the effects of a month-long power outage in Zanzibar and find 25% drop of hours worked for electricity-

dependent jobs, a reduction in income for all sectors (7.8% and 14.2% for non-electricity dependent and electricity dependent occupations respectively), suggesting of inter-dependence of businesses across sectors and spill-over effects from outages on businesses that are not electricity-reliant. In exploration of alternative income generating activities, they do not find evidence that workers substituted labor across earning activities, but instead increase leisure, spending 16% more time at home. [79] specifically focuses on the impacts of power outages on smaller firms. Their preliminary findings suggest that there are 14% losses to revenues and 11% profits due to electricity outage for all tailors, however, those that use more technical inputs lose more. They detect reduction in labor supply on days of outages and a compensating increase on days without outages, however, the increase in hours worked do not fully compensate for the hours lost. Further, there is shift in demand for labor on days of outages and firms switch driven by a switch to cheaper inputs during less productive days.

This study adds to the existing literature in the following ways. First, it paints a landscape of the power reliability in a developing country. Second, it provides descriptive statistics on the effects of power outages on small businesses. Third, it measures the burden of unreliable power supply on small business owners.

2.2.1 Electricity Generation

Over 95% of electricity in Malawi is generated through 7 hydro-electric station, located along the Shire River. The complete process from generation to distribution is owned by a single state company - ESCOM. [3]. In recent years, erratic rain schedules have

[3]The total electricity generation is fairly low 2,069 GWH (Gigawatt Hour), compared to neighboring countries with similar populations Zambia (11,338 GWH) and Zimbabwe (7,297 GWH). Source: BP Statistical

caused a major reduction in the water levels in lakes and rivers across the country, the lowest in the past 15 years, causing only 60% generation of total capacity[4]. The situation is expected to worsen and with unpredictable rain schedules the water level continue to decline. To avoid overload on the electrical grid and prevent system-wide blackouts, the state company frequently implements load sheddings - temporary interruption of electricity supply to reduce the load on generation plants. In addition to load shedding, there is the need for continuous maintenance of decaying equipment and infrastructure which causes either large shut-downs of power affecting large areas within city (in which all connections on the same feeder are affected) or smaller neighborhood-level shut-downs caused by faults of the lower voltage lines (failures at Low-Voltage Transformer level transformers). This results in almost daily interruption in some areas across Malawi. The frequency and intensity of blackouts is subject to seasonality and months following the rain season (April-June) are usually impacted the least, while in September through March, frequency and duration of outages increase.

The extent of the problem is dramatic and administrative data on outages supplied by the power company for a single month in 2017[5] in Blantyre (similar size city), there were 187 load shedding instances, 87% of them were caused by water levels in the rivers, totaling to 670 hours of outages across the city. In addition, there were 50 High Voltage faults lasting in total over 50 hours, 40 forced outages lasting over 100 hours, 31 planned outages lasting over 236 hours. This is excluding small faults reported by customers (over 2400), lasting 18,431 hours. In total, the city experienced 2708 outages as a whole which lasted

Review of World Energy 2017, International Energy Statistics 2016

[4]Source: ESCOM

[5]The same level of details is not available for the time and location of this study

close to 20,000 hours![6]

2.3 Data

The main source of data comes from [2] study conducted in Lilongwe, the capital of Malawi. The study took place in 2016 across 5 urban markets. First, all small scale businesses in the markets (referred to as market vendors or business owners in the paper) were censused. During the census, contact information was collected as well as disqualifying criteria for the study. A final list of 965 businesses was obtained. These include a variety of business types such retail shops, produce sellers, hair styling (salons), welding, carpentry, etc. Next, we classified businesses into three categories: food sellers, non-food sellers (or mixed goods) and services. Business composition and classification is available in appendix in Table 2.1. Four disqualifying criteria was used before the sample selection. First, since the project participation required ability to read questions and keep written records, we excluded illiterate business owners (4.5% of the sample). Second, polygamous business owners were dropped due to complications related to answering questions related to household events (5.5% of business owners). Third, owners were required to be heavily involved in business operations and were excluded if they were not present in the business (11.8%). Those business owners were likely to heavily rely on shopkeepers to run the daily operations. This resulted in 78% of qualified business owners. Summary of selection criteria is available in the appendix Table 2.2. A random sample of 400 businesses was selected stratifying by market, business category and gender; and half of the sample was

[6]Data from the Southern Region of Malawi supplied by the electricity company in June 2017. Data of this level of detail and precision is only available for Southern Region of Malawi where a follow up study took place

selected to take part in the eight-week data collection. A detailed baseline survey containing questions on demographics, business outcomes, savings, cash flow and electricity usage was administered to the full sample.

Next, the logbook training was conducted to every respondent in the logbook group. Each logbook respondents was trained on how to fill out the logbooks by a field officer at owners' shops. The training included detailed guidance on how to fill out the logbooks daily, as well as a practical demonstration during which a sample day record was completed. The logbooks contained detailed questions on business outcomes (labor supply, revenues, profits, business expenditures), personal outcomes (household expenditures, daily savings), economic shocks (illness, money transfers, weddings and funerals), and the exact start and end times of market electricity outages. The respondents were asked to keep the logbooks daily and enter records at the end of each business day. Once a week, an enumerator met with the respondent to check the logbook records, collect the the logbook and give the respondent a clean copy of the logbook for the following week. In the second month, after the respondents were more familiar with the process, the enumerators visited the respondents once in 2 weeks. To encourage participation and minimize attrition, the respondents were compensated \$1/week in telephone credit. In addition, the respondents were promised an analysis page about their businesses at the end of the study based on the information they provided[7].

Attrition during the study summarized in Panel B of Table 2.2 in the appendix. Out of 198 scheduled to keep the logbooks 7.6% were disqualified due to poor vision because

[7] A simple page contained 5 graphs describing business patterns of each owner: average revenue per day of the week, time series graph of revenues and profits during the study period, and break down of business and personal expenditures

they could not see the writing in the books, 5.6% refused prior to logbook data collection, and 4.5% refused during the logbook data collection. 164 kept the logbooks for at least two weeks and 157 had completed the 8 week collection. An average respondent kept the book for 54 days, with a median of 62 days. There was a substantial learning curve associated with filling out the logbooks, and the first week of data is dropped for the analysis. This leaves about 50 observations per respondent (minimum 6, maximum 56, median 55).

The second source of data is the scheduled and actual times of outages that were initiated by the electricity company, consisting mostly of load shedding. The scheduled outage times were obtained from the local newspaper and the actual outage times were provided by the electricity company. The list of outages that company provided are only a subset of all outages that hit the markets, due to a number of local outages that occur on low-voltage lines of which the company does not have a record. Thus, the definition of electricity outage is derived from self-reports recorded by the daily logbook and the company outage times are used to validate the accuracy of the self-reports.

2.4 Summary Statistics

The impacts of power outages on businesses vary based on the degree of reliance on power supply and may be more severe for electricity users than for non-users, in addition service providers (salons, printing shops) may be impacted more due to potential inability to operate during outages. Thus, the sample is broken down into two sub-samples: users and non-users of electricity. Electricity users are the businesses that directly rely on power for income generation such as barbershops, electronic item sellers, salons and tailors who

use electric equipment. The non-users are businesses that do not directly depend on power for income generation. Non-users should not suffer from electricity outages directly but may experience spill over effects if their business depends on operations of users. The classification of businesses into the two categories is obtained from answers to the question whether a business needs electricity to conduct its operations and the description of how exactly the outages affect business owners. While splitting of the sample allows to identify possible channels through which the effects take place, there may still be heterogeneity of responses within a category. For instance, service providers who rely on electricity may have to halt the operations or operated with limited capacity compared to stores that sell perishable products, and only suffer from long outages but continue operations during blackouts.

2.4.1 Selection into Electricity-dependent Business

Low levels of electrification among market vendors is surprising given no barriers to access since transformers are located inside the markets. Thus, consistent with findings by [96], demand for power connections must responsible for low usage of electricity. So, what drives the selection into electricity dependent businesses? While this data does not allow for inspection of determinants of selection into electricity dependent occupations due to the lack pre-electrification data of the users, correlates between electricity collection and business owner characteristics may shed some light on the drivers of the decision.

The comparison of users to non-users in the full sample(N=388)[8] is presented in Table 2.3. First, the majority of businesses in the markets are non-users and the users only

[8]Not just half of the sample that participated in the logbooks

comprise 31% of the sample. Largely, the electricity users are service providers (tailors, barbershops, salons, printing shops), perishable food sellers (meat and dairy shops) or electronic item sellers. Non-users are mostly in retail, selling vegetables, staples, clothes, shoes and other items (Panel A, Table 2.3). Electricity users are 1.63 years younger and have 0.72 more years of education (Panel B, Table 2.3). Electricity-dependent businesses are larger and employ more permanent workers. The revenues and profits are similar, however, the profit margins are higher for users, and for every dollar received in revenue, they generate 13% points more in profits. Value of inventory is twice as high for the users than non-users and value of equipment differs by a factor of five. (Panel C, Table 2.3). The difference in financial access is stark; users are 11.2% points more likely to have a bank account, and a result have accumulated larger bank savings ($246.5 compared to $78.16 for users and non users respectively). Financial security and earning opportunities are also better for the electricity dependent businesses (Panel E, Table 2.3).

The correlates between users and non-users characteristics are parallel to those found in the literature between businesses that grow and those that do not ([106]). Business growth is associated with higher education and younger age firm age[9], those owners are more likely to use new technologies and adopt to the changing business environments. Financial access is instrumental to business expansion, and those with initial capital, access to loans and saving instruments grow more. Since electricity-dependent businesses require a significant initial investment into a connection, equipment and security, only those with sufficient capital can make the investments. Non-users may be more capital constrained

[9]We do not have baseline values for firm age, however, age of owners themselves is likely correated with the age of the firm

both by lack of savings and limited borrowing opportunities.

The decision to run an electricity-dependent business requires long-term commitment to remain in the business more permanently to recover fixed setup costs. This suggests that non-electricity dependent businesses may be temporary enterprises and seek for a more permanent income source. A large fraction of the non-user businesses is characterized by high-competition and low product differentiation (vegetable and staple sellers, prepared foods such as fried potatoes). The choice to run a non-connected business may be partially driven by limited outside employment opportunities. Despite the reliance on self-employment in developing countries, over 90% of parents aspire their children to be hold salaried positions ([20]), suggesting of an intrinsic preference for more stable income sources. Thus, non-users may either select into these occupations due to a lack of choice or view it as a temporary employment between more lucrative opportunities. Additionally, higher education levels observed in users make them more employable in non-business sector, and their choice to pursue an entrepreneurial activity is driven by an expectation of a higher earning potential via self-employment. In fact, users are 9% points more likely to have another business as their secondary earning activity (compared to non-users with 6%), suggesting that they are more "serious" and long-term entrepreneurs.

Lastly, electricity-dependent businesses are subjected to frequent electricity outages, thus, only those who are able to buffer against income shocks select into these volatile occupations. Table 2.4 presents daily income and work hours volatility that the two groups experience. The fluctuations are expressed in deviations in percentage changes away from

the individual mean[10]. The users experience 32%[11] more in revenue fluctuations compared to the non-users; there are no detectable differences in profit fluctuations, however, the daily fluctuations in hours is also higher by 50%, which suggests of unusual working schedules (earning mornings or nights). Moreover, users are 16% points more likely to have supplementary income and their secondary income is 2.7 times higher than that of the non-users. Thus, users hold a more diversified "portfolio" of earning opportunities which makes them better equipped for income fluctuations resulting from unreliable power supply.

Thus, I conjecture that users are likely less capital and credit constrained prior to starting businesses, are more permanent establishments and long term entrepreneurs, who are able to cope with income fluctuations caused by outages via supplementary income sources.

2.4.2 Self-reported effects of Power Outages

At baseline, self reports of the effects of blackouts were recorded. Table 2.5 presents summary statistics on the business (Panel A) and effects of electricity outages reported by the business owners (Panels B-E). The average daily revenue is $19.88, with average daily profits of $3.99, which is not low by Malawian standards, where per median income is close to $1 per day. Only 3% have access to an alternative power source (a generator or a solar panel). Even though load shedding schedule is announced in the newspapers a week in advance, only 10% of the all market vendors are aware of upcoming planned outage in the market. This due to the fact that the announcements are highly unreliable and the majority

[10]The values are obtained from daily logbooks. First, mean value of the variable is calculated for each individual separately. Second, the percentage change is found for each day. Lastly, the standard deviation of the percentage changes is calculated for each person, obtaining a single value for an individual

[11](0.69/0.52)=1.32

of the time the electricity company does not follow its own schedule. 39% of market vendors report to be affected by power outages. This is higher than the percentage of users since some of the non-users experience loss of business during blackouts.

In Panel C-E, self-reported effects on business activities are presented only for those 39% who reported being affected by outages. 32% of respondents report not being able to conduct their daily operations without electricity, 23% only partially depend on power, and 25% report losing customers, potentially due to lower volume of customers in the markets. The self-reported losses constitute about 36% ($7.77 USD) of the daily revenue (Panel B), with high a standard deviation suggesting a wide distribution and the presence of businesses that are highly unprofitable during outages. The losses are partially driven by lower customer volumes, and businesses report a difference of 14.55 customers on days with and without outages. Panel D reports perceived general equilibrium effects from the point of view of a market vendor and whether the sales are permanently lost, transferred spatially (to other markets or other sellers within the market) or inter-temporarily (across time). Out of all the foregone sales, 33% are lost permanently, while the rest are transferred to either another day or another vendor.

Finally, unreliable electricity has long term impacts on the decisions of micro entrepreneurs (Panel E), and 19% of the full sample would invest in new technology in power supply was more reliable; 19% would do a different business, and 25% would increase current operation hours. This suggests that power outages present a binding constraint in the short run and an impediment to growth in the long run.

2.5 Outages

2.5.1 Levels of Outages

There are four possible levels of outages that market vendors experience (1) at the shop level (2) at the transformer level; (3) at the market level (4) at the feeder level. The first one is an outage that is caused by a fault in the shop's wire systems and only affects an individual shops. I do not believe that the self-reported outage information contains many of those reports because (a) it is unlikely that a vendor within a market attributes their individual experience of an outage to the whole market and (b) the electric systems within the markets are fragile, and a small outage for a single shop likely affects other market vendors.

The second level of outage is the transformer-level outage, these outages affect all connections with the transformer and do not affect connections in other transformers. I have mapped out the shops within markets that are are affected by outages simultaneously - a proxy for identifying the transformer connections[12]. The majority of the shops within a market belong to the same transformer and, thus, are affected by outages at the same time. There is often a physical division (such as a road) that splits the markets into these sub-groups, and the majority of the market is located on a single side of the road, with some shops across the street (examples: Markets 1, 2 and 3). Constructing an outage at the transformer level may suffer from several issues: (1) the likelihood of report of an outage is lower due to low number of shops belonging to the 'smaller' transformer groups, (2) the shops belonging to the smaller transformer groups may still report a market outage

[12]De-identified GPS locations of shops are available in Appendix, Figure A.9

if they observe one even if it doesn't affect their own shops (3) it is unlikely that the non-users of electricity report outages that are strictly within the transformer boundaries and as a result report outage that they notice. It is also possible that several transformers are affected at the same time, which is consistent with interviews with the market vendors. Thus, I conclude that the majority of outages affect markets as a whole.

Lastly, the outage may occur at the feeder level (a larger network of transformers). In this case, all transformers within the feeder network experience an outage. Load shedding outages occur at the feeder level. Markets 1-3 belong belong to different feeders and are affected by feeder-level outages at the different times. Markets 4 and 5, are geographically closer to each other(about 2 kilometers away) compared to other markets, and according to the electricity company belong to the same feeder. However, examinations of self-reports across the two markets do not suggest that the outages across the two markets are correlated more than with other markets (Table A.18). The records from the electricity company also indicate low incidences of outages, which does not coincide with self-reported data. Thus, I make an assumption that outages affect all market vendors at the same time and construct treatment effects at the market level.

Methodology of constructing outage times

The reports of outages are very infrequent, and out of all individual-day observation, an outage start or end time is reported only 8.9% of the time. Table 2.6 summarizes the percentage of market vendors reporting an outage on a given day for all days (panel A), only for days on which ESCOM reports an outage (Panel B) and days on which ESCOM does not report an outage. The results are presented separately for all vendors (column 1),

users (column 2) and non-users (column 3). First, it is worth noticing that probability of an outage report is higher for users than non-users, however, the difference is only 2.9% (for all days) but is 6.5% points for days with ESCOM outages (Panel B). This suggests that both users are and non-users pay attention to outages in the markets, with users being more attentive. If there was perfect reporting of outage times, then we should expect close to 100% reporting at least among users on days on which the company reports an outage (Panel B), however, we see an average of 20% of users reporting an outages and a maximum of 60% of users reporting an outage. Thus, on average, 80% of the users do not report outages. This indicates of heavy under-reporting of outages at the individual level. Low reports may be driven by moral hazard since the respondents were well aware that other people in the markets were required to answer the same questions, and, thus may have relied on other people to report information that was known publicly. Therefore, individual treatment variable is incomplete, and construction of an outage at a market level is more appropriate.

I use the following procedure to construct a market-level outage from individual reports obtained from the logbooks. Step 1) I determine the minimum number of reports in order to declare an outage, Step 2) I construct a single market-level binary variable from individual reports, Step 3) I validate the constructed outages against the reports provided by the electricity company to understand the quality of the constructed measure.

The first step is to determine the minimum number of reports per market per day in order to declare an outage. I use the quality of the match between the self-reports and the electricity company reports to derive the definition of an outage and consider three

definitions:

An outage is declared if:

(1) at least 1 person (business) or

(2) at least 2 people report an outage, or

(3) at least 3 people report an outage[13]

There is a risk of a type I error with the first definition - assigning an outage when, in fact, it is a false report. If the second two definitions are chosen, there is a possibility of a type II error - not reporting an outage, when there was one. The third definition is the most conservative, but given the low instances of reports, it may be too restrictive.

In order to determine whether definition (1), (2) or (3) is more accurate, I compare the electricity company reports to the reports obtained from the logbooks. There were in total of 49 outages that the company reported for the relevant markets during the data collection. For each definition, I find the percentage of company reports that were detected in the logbooks. Table 2.7 summarizes the results. In columns 1 through 3, I report the percentage of outages out of 49 that were caught if all the reports are considered (column 1), if only reports from electricity users are considered (column 2), and if only reports from non-users are used (column 3). Each row presents definition of an outage described above. If I adopt Definition 1 and consider reported times by users and non-users, I am able to detect 96% of outages that the company reports. Theoretically, reports from the users should be more accurate and complete. In columns (2) and (3) I consider reports

[13]The median size of market is 27, and 1, 2 and 3 reports represent 3%, 7%, 11% of outage reports in the markets.

by users and non-users respectively, and find that the two-subgroups are equally likely to pay attention to outages. If I adopt a stricter definition of an outage and require at least 2 reports per market, I am able to detect 87% of company outages. A stricter definition reduces the chances of correctly finding the company reports from 96% (in which an outage is declared if only 1 person reports an outage) to 74% with definition 3. If I look at the sub-sample of days, excluding Sundays[14], I am able to improve the accuracy of outages with Definition 2 to 92%. Definitions based on user reports only yield significantly to significantly lower chances of capturing an ESCOM report, and thus, all reports of users and non-users should be used. In columns (7) through (9), I report the percentage of total days that get affected by outages. If definition 1 is adopted, then 81% of total days are declared as outage days. With definition 2, the number drops to 58%. This suggests that on 23% of days, only 1 person reported an outage. A fraction of these reports represents correct outages that were only recorded by one person, however, it is also possible that these are misreported (either recorded on an incorrect day of the week or confused with other neighboring fields which asked the times of outages at home and the closing time of the shop).

I conclude that definition 3 is too restrictive, and if adopted, 26% of ESCOM outages are missed. Definition 1 recovers 96% more reports but is also driven by 23% of days in which one 1 person per market reported an outage, and is a greater subject to misreports. Definition 2, recovers 87% of the reports and excludes potential mistakes on days with only 1 report. Thus, definition 2 is preferred but the results from definition 1 are reported in robustness checks[15]. With either definitions, I clearly see that the company

[14]Sundays are often church days and businesses either don't open or open for a part of a day

[15]Alternative outage construction at transformer level which is based on percentage of users reporting an outage is also presented in robustness checks

reports of 49 outages (which represented 17% of total days) heavily underestimates the extent to which the market vendors experience electricity interruptions.

Figure 2.1 reports outage instances for each market for the duration of the study using the adopted Definition 2[16] . Each row represents the market in the sample and the vertical bar indicates an outage for that date. The height of the bar and the number above it represent the percentage of people reporting an outage on that day out of total number of people present in the market. Grey bars are days on which outage is not declared i.e there is less than 1 report[17]. Black diamonds indicate company reported outages for that particular day. Markets 4 and 5 are located on the same feeder and are affected by the company outages on the same days. The rest of the markets are affected by the company outages on different days. It is important to notice that outages are very frequent for some markets and affect the majority of the days; not all markets are impacted equally (market 4 has an especially high frequency of outages). Outages are more frequent in the beginning of the data collection period and are rarer towards the end. This is due to the monthly seasonality of outages related to the start of the rain season.

2.6 Estimation Strategy

The estimation relies on a fixed effect model, where the effects of outages are found by comparing outcomes within an individual business, across time while removing date specific effects and controlling for day of week, weather shocks and any other date-

[16] A corresponding diagram using definition 1 is available in the Appendix, Figure A.8

[17] The first bar in market 4 is marked grey even though the percentage of people reporting an outage is 33. This is because there were only 3 people present in the market that day and 1 reported an outage. The low number of report is explained to the fact that it was the first day of data collection in that market and only 3 people were visited

specific factors

$$Y_{it} = \beta_0 + \beta_1 * Outage_{mt} + \sigma_i + \delta_t + \epsilon_{it} \tag{2.1}$$

where i is an individual business, t is date. Y_{it} is the outcome variable, $Outage_{mt}$ is the outage binary variable that takes a value of 1 if there was an outage on that day, and 0 if there was no outage. All of the specifications include individual business fixed effects - σ_i, δ_t- date fixed effects to control for date shocks that affect all the markets. All monetary outcomes are converted to logarithmic scale due to presence of outliers. All p-values presented in the tables are constructed using the t-percentile bootstrap method described in [44] for inference with small number of clusters.[18] Finally, I present the results of duration outages on the business outcomes, where duration is derived from self-reports and expressed in hour of exposure.

2.7 Results

I present results of power outages on a full set of business outcomes in Table 2.8 and expenditures/savings/transfers in Table 2.9. All monetary outcomes are converted into logarithmic scale and hours are windsorized at 5% to address outliers. In each table, the results are first presented for the full sample (Panel A), then displayed separately for electricity users (Panel B) and electricity non-users (Panel C) to understand differential impacts on the two sub-groups.

First, there are no effects on the probability of the business owner working on

[18]Since the majority of outages hit the full markets as a whole and outcome variables are likely to be correlated within the markets, market is defined as a cluster

the particular day in response to an outage (column 1, Panel A, Table 2.8), and both electricity users (panel B) and non-users (panel C) are not more or less likely to work on days of outages compared to non-outage days. This is consistent with intuition. Largely the outages are not announced (89% of all outage are unanticipated) and the duration is not known in advance, thus making it hard to adjust labor supply on the extensive margin. Labor supply, conditional on working, (column 2) is unaffected by the electricity outages. It is possible that the effects are not fully captured in hours worked because given a shorter outage, users may adjust labor supply on the same day and work past dark. Next, I examine impacts on revenues, conditional on working. Users of electricity lose about 10% of their daily revenues on days of outages, and the coefficient is highly significant with a p-value of 0.016. There are zero average effects on non-users of the electricity. However, users' market profits are not affected. I suspect that this is partially driven by difficulty of measuring daily profits which requires detailed record keeping, knowledge of profit margins for each item sold and correctly accounting for expenses that are incurred weekly or monthly. However, based on the profit margin reported at baseline 10% loss in revenue is about 4% loss in profits. I do not see any significant effects on total income which includes both market and non-market earnings.

Outages affect inventory purchases and, on days of outages users incur 12% less in inventory expenses. This is not necessarily an intuitive result. Baseline data suggests that 20% of electricity users purchase inventory daily and 40% from 3 times/week to a week. Thus, this effect is either solely driven by daily inventory purchases (20% of the sample) or is a reflection of negative future business outlook if there is an outage in the market.

In column 8, I examine the effects on electricity expenditures on that particular day. This serves as a robustness check that the reported outages are not driven by non-payments of electricity bills. I do not find evidence that the rest of the business outcomes are impacted by outages.

In Table, 2.9 I examine the effects on non-business outcomes outcomes for the full sample (Panel A) and users (Panel B)[19]. Since electricity outage may be a form of an income shock, I am interested in understanding whether there are impacts on consumption, savings, and transfers. For the users, I find a 5% increase in transfers received, suggesting of effects of social networks on income smoothing. These are both household and non-household transfers. In monetary terms, this is a very small increase representing 5% of the 0.43 USD on non-outage days. However, transfers are very infrequent and on 92% of the days, there are no transfers received, so this result is driven by large and rare transfers[20]. I do not find any increases on expenditures for users, however, the effects on the whole sample seems to suggest that the expenditures increase in response to outages. This is a surprising result, as the income shock should have either zero or negative effect on expenditures. In monetary terms, however, this increase represents 0.038*4.11=0.15 USD or 100 MWK. This is not a very large increase in expenditures by Malawian standards and is equivalent to approximately half of a soda drink bottle. It is possible, that as business slows down during outage hours, business owners make small expenditures in the markets that would have otherwise been avoided.

[19]Since effects on the non-users are close to zero for business outcomes, the blackouts do not affect non-business outcomes. The results are available upon request

[20]Average value of transfers received is $4.9 USD, conditioning on receiving a transfer

2.8 Causal Inference

The underlying assumption of this approach is that conditional on control variables, the assignment of outages is as good as random, uncorrelated with any market-level characteristics and exogenous to the individual shops. I believe the assumption is satisfied for the following reasons.

It is unlikely that market-specific characteristics are incorporated into decisions of the power company which areas to shut off and when. These markets are smaller players in the economy and the electricity company only considers actors that are larger such as manufacturing plants. Furthermore, the process by which the company determines which areas to shut off is not likely to be related to any characteristics of the areas themselves (unless they avoid those areas completely). Specifically, on each day the company decides the amount megawatts that needs to be off-loaded. Next, they select a combination of areas that gives them the desired amount. Thus, the probability of each area being selected depends on the total demand for power on a particular day and size in wattage of other areas. This explains why the company does not always follow its own schedule posted in advance, and they find it challenging to form correct predictions on daily demand for power. If the total demand for power is smaller than predicted, load shedding will not be initiated; if the demand for power is greater than anticipated, then more areas will experience black outs.

Second, I do not believe that the majority of the reported outages were a result of the planned effort by the electricity company. The company specifically reported that Market 2 is never affected by load shedding since it's located on the same feeder as the Water

Company. Nevertheless, I observe numerous outages in the markets despite the company's protection of that feeder. This makes us believe that the majority of outages are caused by unanticipated faults in the system, rather than by pre-constructed schedule.

Third, it is possible that overload on the system and the resulting blackout is caused by market-level activities. For example, a greater number of customers using services within a market may overload the fragile system and cause a market-level blackouts. In this case, omitting customer volume in the markets would bias the coefficient because it's correlated with both outage instance and shop-level outcomes. On the other hand, customer volumes may adjust in response to outages, so the direction of the causality is unclear. If large number of customers is the culprit of outages, then estimates on the revenue losses are lower bounds of the true estimates.

Fourth, the results would be difficult to give causal interpretation if the outages are anticipated and the behavior of market vendors is adjusted prior to outages. The reports suggest otherwise. Only 10% of the market vendors pay attention to schedules. Further, in comparing the newspaper announcements with the list of actual outages from the electricity company, I found that given an announcement on a particular day, the probability of a company initiated outage is only 61%. Furthermore, the compliance to the timing of the scheduled outage is also low with correlation coefficient of 0.16 between scheduled and actual outage start time. Thus, the public announcements are not reliable and for the majority of outages are not anticipated in advance.

Fifth, the areas across the city are not equivalent in the blackout exposure and a few selected areas never experience outages for political reasons, such as areas where

the parliament is located. It is, thus, possible that more successful businesses relocate to places that are more conducive to business. However, this is unlikely to be a short-term decision done within 2 months during the data collection and I did not follow businesses if they moved out of the markets[21]. This concern would more valid if the businesses were followed in the long run. Lastly, the location of a business does not seem to be driven by the viability of business and 79% of market vendors live within the same geographical area (usually walking, short bus or a bike-taxi ride away)[22].

2.8.1 Estimation of P-values

In contexts where observations are not independent and are correlated within a cluster, an appropriate adjustment needs to be made. It is an especially a concern in panel data where individuals or group of individuals are tracked over time [29]. In the context of this project, the appropriate cluster is a market since the outcomes are likely to be correlated within market over time. However, due to a small number of markets in the sample using the standard White correction is not appropriate since it produced standard errors that are too large with 30 or fewer clusters ([44]). Thus, I follow the procedure outlines in [44] to construct p-values using wild cluster percentile boostrap-t method, with the null hypothesis imposed as it does especially well with the low number of clusters. The distinguishing feature of this procedure is a random re-assignment of the sign of the error term at the market level, which preserves market-level error structure while generating psudo-samples. The estimating procedure for bootstraped p-values under the method is as

[21] Attrition during the data collection period was not caused by a business moving to a different location
[22] However, the choice of residence in the long run is correlated with the wealth of that household

follows. First, I estimate the main model, clustering at the desired level (market in this case), forming an original t-statistic $t_{stat} = \frac{(\hat{\beta}_{outage} - \beta_{H_0})}{se_{\hat{\beta}_{outage}}}$. Next, I impose the null hypothesis on the coefficient of interest i.e. $H_0 : \beta_{outage} = 0$, and form a residual from the restricted estimation $\hat{u_G^R}$, where G is the number of clusters.

For 999 iterations, I repeat the following steps. Step (1): for each cluster G, I either form $\hat{u}_G^{R*} = w * \hat{u}_G^{R*}$ or $\hat{u}_G^{R*} = -w * \hat{u}_G^{R*}$ The original approach recommends using weight $w = (1, -1)$, each with probability 1/2. Since the reassignment of the errors is done at the cluster level, that means that there are at most 2_G unique combinations, which produces 16 unique positive realizations for 5 clusters. By construction the pvalues cannot be smaller than 0.0625, and if the main t-stat is smaller than any of the 16 values, the only conclusion I can come up with is that the p-value is smaller than 0.0625. To increase continuity of p-values, [129] proposes using 6 weights $w = (-\sqrt{1.5}, -\sqrt{1}, -\sqrt{0.5}, \sqrt{1.5}, \sqrt{1}, \sqrt{0.5})$, and [44] advise using expanded weights for G¡10 . Thus, all the pvalues presented in the paper are calculated using the expansive weights.

In Step (2) I construct $\hat{Y}_g^* = X'\hat{\beta}^R + w * \hat{u}_g^{R*}$, where $g = 1, ...G$. In other words, the wild cluster bootstrap replaces Y_g in each re-sample with either $\hat{Y}_g^* = X'\hat{\beta}^R + w * \hat{u}_g^{R*}$ or $\hat{Y}_g^* = X'\hat{\beta}^R - w * \hat{u}_g^{R*}$. Lastly, I calculate the percentage of times (out of 999) the boostrapped $t - wild$ is larger than the original $t - stat$ in absolute values i.e. we are estimating the percentage of wild bootstrapped t-stats that are more extreme than the original one - which is a p-value. This technique has a main advantages over other bootstrapping methods that particularly suits the context with clustered error structure - it does not assume that errors are independent across observations by preserving the original structure of the variance.

The asymptotic validity and refinement were proven by [101] and extended to the clustered setting by [44].

2.8.2 Is there Evidence of Recall Bias?

The logbooks were given to the respondents to fill out every day, however, if the compliance is poor and the respondents filled out the logbooks before meeting with the field officer, the data quality may suffer from the recall bias. For the first month, the logbooks were exchanged once a week, and during the second month the respondents were given logbooks for 14 days to minimize field costs and inconvenience to the respondents. If the compliance is not perfect, and the respondents only filled out logbooks on day 7 (for the first month) and day 14 (for the second month), there should be systematic differences in reports between days within a logbook. In table A.16, I present evidence of reports controlling for days within the logbook. For weekly logbooks (Panel A), day 7 represents the last day of the logbook; similarly, day 14 is the last day for bi-weekly logbooks (Panel B). All the regressions include date fixed effects and errors are clustered at the individual level. If there is clear non-compliance, I should see systematic differences between reports on last days (7 for the first month and 14 for the second month)and other days. For most of the outcomes in Panel, there no evidence that logbooks were filled out on day 7. For bi-weekly logbooks (Panel B), there is limited evidence that logbooks were filled on day 14 for total hours worked and revenues reported but there are is no consistent pattern across other days. Therefore, I conclude that the compliance with filling out logbooks was acceptable.

2.8.3 Robustness Checks and Data Limitations

While data collection with via self-filled logbooks presents an opportunity to collect high frequency data at lower costs, it is also a subject to potential measurement error introduced by respondents coding the values themselves. The recording errors could come from recording a wrong value (for example missing or adding an extra zero), recording entries either on the wrong line and/or on the wrong date. Despite the extensive training on how to correctly fill out the books, the respondents are more likely to make mistakes than enumerators in a more traditional surveying setting.

The logbooks required significant time commitment from the business owners and took on average 30-40 minutes a day to fill out. Although the majority of the market vendors reported learning from keeping records and felt that they personally benefited from the surveying process, data accuracy is still subject to diligence and neatness of an individual respondent. The correct coding of outage times was especially important since it had to be recorded on the correct date (and not day earlier or later). This is rarely a concern for other estimation strategies that rely on average outcomes across groups and do not require the same level accuracy.

There are several issues which I believe introduce measurement error to the treatment variable (instances and times of outage times). First, due to space limitations in the logbooks, I was not able to separately ask about instances of outages and different times for outages. Instead, I only ask about 1 times of outage. If there was no outage, the respondent was asked to put a dash - which indicated that the field was filled out. Thus, the construction of the binary variable heavily relies on any record being present. Second, it is possible

that the dates of outages were not always recorded correctly. For logistical considerations, I exchanged logbooks with respondents on different days of the weeks and, thus, their own individual logbooks started on different days i.e. for the majority of the respondents the first entry did not correspond to a Monday but rather the day of the week in which they meet with field officer for the logbook exchange. Given the fact that the respondent filled out 10 pages of entries as a part of this data collection process, the days and dates may have been confused. There is weak evidence of such misreporting as indicated in Figure 2.2. The graph reports the average number of self-reports on days of Company Outages (centered at 0), days leading to the outage (negative values), and days following the outages (positive values). If there was no misreporting of outage days, then I should expect to see a spike at zero and relatively flat and similar values on days before and after the company outage. However, there is a small increase in average number of reports after the day of the company outage, which suggests of potential presence of the measurement error and miscoding of days of outages.

Third, in our follow up study in Blantyre, which took place in 2017, I learned that markets get hit with shorter outages several times per day instead of being exposed to a single longer outage. This information was not recorded in the logbooks and makes me believe that the coded start and end times of outages may reflect potentially different outages experienced in the market throughout the day. For this reason, I do not think that the obtained measure of duration of outage is informative. Nevertheless, I present the results in Table 2.11 in the Appendix.

Fourth, I believe that there is under-reporting of outages in the markets since there

are 23% of the days on which one 1 person recorded an outage, which is not possible given that the majority of outages affect several shops within a market.

Given the above limitations, I examine the extent to which the estimate change across various definitions of outages and specifications and present various robustness checks in Table 2.10. The results are only presented for revenues as it is the main outcome of interest[23], and for users of electricity since they are impacted by outages. In columns (1)-(5) I present regression results with varying market-level definitions. In columns (1) and (2) I present the regression results adopting Definition 1 as the regressor. In columns (3) - (4), I present different specifications with the preferred Definition 2. In columns (2) and (3), I exclude Sundays because it is usually a church day, and market vendors are less likely to work and report an outage. In column (4) I exclude the days on which only 1 person reported an outage since there is uncertainty as to whether this is a correct report and the day should be classified as an outage or this is a mistake. Excluding those should eliminate the some of the measurement error introduced by miss-classification. The largest effect sizes are found using the definition least prone to measurement error (column 4), where the control days are the days on which no one reported an outage and treatment days are days on which at least 2 people reported an outage. In columns (6)-(9), I explore a transformer-level definition, only relying on users' reports of outages. Due to a very low number of users located on some transformers, I use the percentage of users reporting an outage as a basis for an outage definition. I explore the definitions in increments of 20, and assign an outage to that particular transformer level if at least X% of users report an outage. There is only 1 day on which more than 60% of the users report an outage at the transformer level, so

[23]Results for other outcomes are available upon request

I do not report results for 100 and 80%. There is general agreement in the estimates and there are negative and significant effects across all estimations, the ranges vary from 5% to 16.8% of sales lost.

2.9 Discussion and Conclusion

I estimate short term effects of electricity outages on market vendors in Malawi. First, I document the frequency of outages and discover that the market vendors experience interruptions on at least half of the days. Second, only 31% of market vendors are electricity-dependent business, which is a reflection of low demand for electrification. I conclude that businesses that self-select into electricity-dependent occupations are less capital contrained and are more permanent establishments than non-connected. Third, I estimate the effects of outages on daily outcomes for the two sub-groups: users and non-users of electricity. I find that users lose from 7.8%-15.8% in revenues on outage days. I find that the daily income is not affected by market outages, which may be driven by measurement error in profits. If I infer expected profit losses from profit margins calculated from baseline, then decreases in revenues by 7.8%-15.8% correspond to 3.12% -6.32% losses in profit. Despite the losses in sales, I do not see any evidence of negative effects on consumption and savings, however, the results may be largely attenuated from measurement error in the independent variable. I document very small increases in transfers suggesting of informal safety net against income shocks. Due to likely presence of measurement error, I believe that these are the lower bound effects. The presence of measurement error does not allow estimation of the intensity of the treatment - duration of outages.

The estimates presented are comparable to those presented in the literature. [39] documents losses in household earnings of 7.8% and 14.2% of income loss for non-users and users of electricity respectively. However, it is likely that household earnings are equivalent to profits in their study. [79] document 11% to weekly profits due to power interruptions, which I do not find. However, their sample is composed of larger firms, all connected to electricity ranging in size between 0.38 and 1.13 (0.49 and 0.14 for users and non-users of this study respectively). Thus, the differences in losses between the two studies may be disproportionally larger for larger firms. Contrary to [79], I do not find any evidence of changes in labor supply in response to outages. This may be explained by the uncertainty of outage schedules, and if an outage hits the markets, it is unclear whether it will be short lived. Thus, the business owners prefer to wait out the outage in the markets in case it returns.

2.10 Tables and Figures

Table 2.1: Sample Composition

Panel A: Business Composition at Census

	Count	**Percentage**
Raw Foods	156	16.17
Salon/cosmetics/barber/boutique	154	15.96
Prepared Food	90	9.33
Shoes/Clothes	87	9.02
Groceries	85	8.81
Raw Fish/Meat	74	7.67
Other Eatable Items	71	7.36
Tailoring	29	3.01
Other Non-eatable Items	28	2.9
Restaurant	19	1.97
Alcohol (not a bar)	13	1.35
Music/movie/game services	13	1.35
Various Other	146	15.1
	965	100

Panel B: Classification of Businesses

	Count	**Percentage**
Food	390	40.41
Non-food	313	32.44
Services	262	27.15
	965	100

Table 2.2: Sample Selection and Attrition

	Counts	Percentage
Panel A: Census		
Censused	965	
Disqualified		
Owner not present full time	114	11.8%
Polygamous	53	5.5%
Not literate	43	4.5%
Qualified businesses	753	78.0%
Panel B: Attrition for the Logbook Group		
Attrition at Baseline		
Completed	198	99.0%
Refused	2	1.0%
	200	
Attrition at Logbook Data Collection		
Finished Logbooks	157	79.3%
Refused prior to Logbooks	11	5.6%
Refused during Logbooks, with partial complete	9	4.5%
Disqualified	15	7.6%
Moved Away/Other during LB	6	3.0%
	198	
Number of Busineses in the dataset	164	
Mean number of Day Records Present per Business	50	
Median number of Day Records Present per Business	55	

Table 2.3: Differences between Users and Non-Users: Baseline Data (Part 1)

	(1) All	(2) Users	(3) Non-users	(4) P-Value
Percentage in the sample (N=388)	100%	31%	69%	
Panel A: Business Types				
Selling Food Items	0.43	0.15	0.55	0.00
Selling non-Food Items	0.33	0.25	0.37	0.02
Services	0.24	0.61	0.09	0.00
Panel B: Demographics				
Age	32.97	31.82	33.45	0.10
	(8.88)	(7.94)	(9.22)	
Education	9.88	10.39	9.67	0.03
	(3.04)	(3.00)	(3.04)	
Male	0.70	0.68	0.71	0.61
	(0.46)	(0.47)	(0.45)	
Panel C: Business Outcomes				
Number of Employees	0.26	0.51	0.15	0.00
	(0.67)	(0.94)	0.00	
Average Monthly Revenue	460.30	390.50	488.80	0.12
	(564.00)	(559.80)	(564.30)	
Average Monthly Profit	111.50	111.80	111.40	0.98
	(124.00)	(123.60)	(124.40)	
Average Profit Margin	0.36	0.45	0.32	0.00
	(0.29)	(0.36)	(0.24)	
Value of Inventory	251.60	380.90	200.30	0.00
	(427.30)	(544.40)	(359.20)	
Value of Tools and Equipment	125.00	222.60	86.38	0.00
	(208.40)	(256.50)	(171.80)	

Note: All values are in USD. Standard deviations of the means are in paranthesis. Pvalues of the difference between the means are presented in column 4

Table 2.4: Differences between Users and Non-Users: Baseline Data (Part 2)

	(1) All	(2) Users	(3) Non-users	(4) P-Value
Panel D: Financial Access				
Uses a Bank Account	0.417	0.496	0.384	0.04
	(0.49)	(0.50)	(0.49)	
Uses Mobile Money Account	0.256	0.372	0.266	0.04
	(0.46)	(0.49)	(0.44)	
Panel E: Financial Security				
Total Value of Assets	773.1	1399	511.8	0.00
	(1974.00)	(3119.00)	(1121.00)	
Total Value of Livestock	88.25	71.88	95.08	0.57
	(368.00)	(326.10)	(384.50)	
Total Cash Savings	212.8	324.0	166.4	0.10
	(846.90)	(1491.00)	(295.00)	
Has a Secondary Earning Activity	0.27	0.39	0.23	0.001
	(0.45)	(0.49)	(0.42)	
Second Earning: Business	0.09	0.14	0.06	0.01
	(0.28)	(0.35)	(0.24)	
Second Earning: Average Monthly Income	56.16	99.96	37.94	0.01
	(222.30)	(343.00)	(142.10)	

Note: All values are in USD. Standard deviations of the means are in paranthesis. Pvalues of the difference between the means are presented in column 4

Table 2.5: Summary Statistics of the Self-Reported Effects of Electricity

	Mean	SD
A. Uses of Electricity		
Daily Revenue	19.88	43.74
Daily Profit	3.99	6.35
Has Alt Energy Source	0.03	0.17
Knows about outage announcements	0.10	0.31
Business is affected by Electricity Outages	0.39	0.49
B. Effects of Outages for Those Affected		
My business cannot function without electricity	0.32	0.47
A Part of my business depends on electricity	0.23	0.42
I lose customers	0.25	0.43
No business past dark	0.08	0.27
Loss of product	0.09	0.30
Daily Losses during Outages in USD	7.77	9.10
C. Customers		
Number of Customers on Regular Days	32.94	45.21
Number of Customers on Outage Days	18.39	32.06
D. Where do the lost sales go?		
Customers buy from someone else in the market	0.12	0.32
Customers go to another market	0.35	0.48
Customers come back later	0.21	0.41
Customers don't buy the good at all	0.33	0.47
E. Effects: Long Term		
Invest in new Technology	0.19	0.39
I would do different business	0.19	0.39
Stay Opened Longer	0.25	0.44
Nothing	0.40	0.49
Observations	164	

Table 2.6: Frequency of Outage Reports in the Logbooks

	(1) All	(2) Users	(3) Non-Users
Panel A: All Days			
Probability of Report	8.9%	10.7%	7.9%
Maximum % reporting an Outage Across Days	42.1%	66.7%	50.0%
Panel B: ESCOM Outage Days			
Probability of Report Given Company Outage	15.9%	20.1%	13.7%
Maximum % reporting an Outage Across Days Given Company Report	42.1%	66.7%	33.3%
Panel C: Non-ESCOM Outage Days			
Probability of Report Given Company Non-Outage	7.4%	8.7%	6.6%
Maximum % reporting an Outage Across Days Given Company Non-Report	33.3%	55.6%	50.0%

Table 2.7: Validation of Self Reports Against ESCOM Outages

	ESCOM Outages found in Logbooks (N=49)						**Total Days Affected By Outages (N=288)**	
	All Days			**No Sundays**				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
	Full Sample	**Only Users**	**Only Non Users**	**Full Sample**	**Only Users**	**Only Non Users**	**Full Sample**	**Only Users**
If X people per Market Report an Outage								
Definition 1 (X¿=1)	96%	87%	87%	97%	89%	89%	81%	58%
Definition 2 (X¿=2)	87%	64%	64%	92%	70%	68%	56%	28%
Definition 3 (X¿=3)	74%	38%	43%	78%	46%	43%	41%	12%

Figure 2.1: Outage Instances per Market using Definition 2

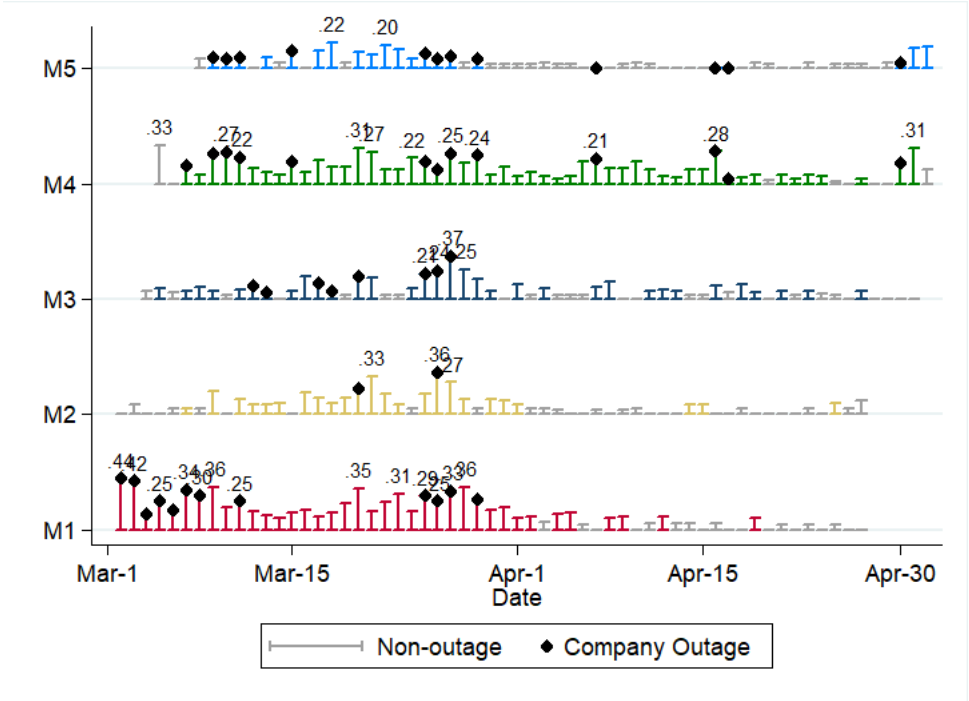


Figure 2.2: Number of Reports of outages in markets around the company outage

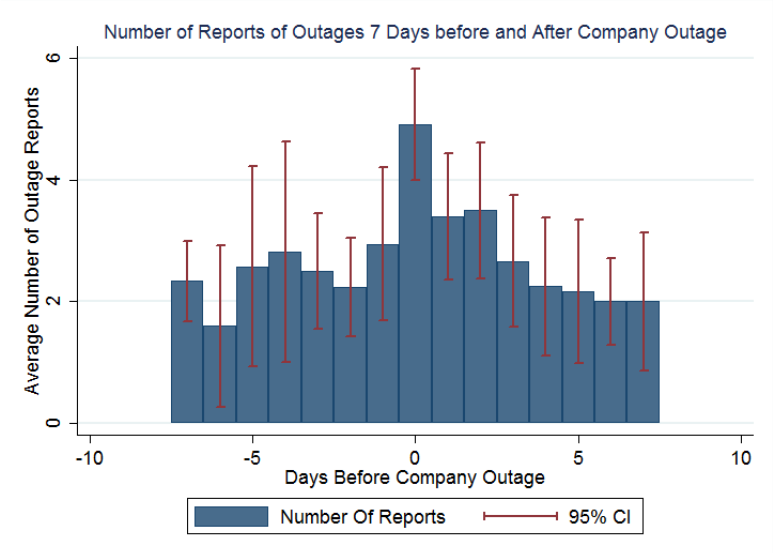


Table 2.8: Effects of Outages on Business Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Business Outcomes					Business Expenditures		
	Labor Supply Today		Logged	Logged	Logged	Logged	Logged	Logged
	= 1 if Worked	Total Hours	Market Revenues	Market Profits	Total Income	Inventory Exp	Electr. Exp	Total Exp
Panel A: Full Sample								
Outage	-0.003	-0.013	-0.027*	0	-0.014	-0.045	0.004	-0.018
	(0.839)	(0.871)	(0.081)	(1.000)	(0.522)	(0.447)	(0.530)	(0.717)
Observations	8,232	6,987	7,332	6,705	6,762	7,838	7,325	7,596
Control Mean	0.92	11.47	19.61	5.82	6.58	9.21	0.15	11.19
Panel B: Users								
Outage	0.021	-0.166	-0.1**	-0.019	-0.048	-0.128*	-0.005	-0.087
	(0.326)	(0.411)	(0.016)	(0.664)	(0.219)	(0.075)	(0.786)	(0.207)
Observations	2,613	2,203	2,316	2,024	2,036	2,478	2,317	2,419
Control Mean	0.89	11.40	14.53	5.94	6.91	6.94	0.38	8.61
Panel C: Non-Users								
Outage	-0.014	0.046	-0.003	0.002	-0.006	-0.014	0.006	0.005
	(0.446)	(0.720)	(0.900)	(0.945)	(0.855)	(0.826)	(0.287)	(0.938)
Observations	5,619	4,784	5,016	4,681	4,726	5,360	5,008	5,177
Control Mean	0.93	11.50	21.78	5.76	6.44	10.20	0.06	12.31

Note: Independent variable is a binary indicator of whether there was an outage on a particular day. Dependent variables are: (1) Binary Indicator of whether a vendor worked today (2) Total Work Hours windsorized at 5% (3) Logged Marked Revenues conditional on working (4)Logged Market Profits conditional on working (5) Lodgged Total Income conditional on working (6) Logged Inventory Expenditures (7) Logged Electricity Payments (8) Logged Total Business Expenditures. All regressions includes individual and date fixed effects. Pvalues from t-wild bootstapped method are in parenthesis.

Table 2.9: Effects of Outages Non-Business Outcome

	(1)	(2)	(3)	(4)	(5)	(6)
	Personal Expenditures		Savings		Transfers	
	Logged Food Exp	Logged Total Exp	Logged Deposits	Logged Withdrawals	Logged Received	Logged Given
Panel A: Full Sample						
Outage	0.009	0.038**	0.016	-0.011	0.013**	0.009
	(0.460)	(0.033)	(0.394)	(0.287)	(0.039)	(0.258)
Observations	8,232	8,232	8,232	8,232	8,232	8,232
R-squared	0.59	0.51	0.59	0.16	0.24	0.46
Control Mean	1.96	4.11	0.68	0.16	0.44	0.94
Panel B: Users						
Outage	0.03	0.078	0.017	-0.027	0.058***	0.001
	(0.141)	(0.157)	(0.310)	(0.562)	(0.002)	(0.973)
Observations	2,613	2,613	2,613	2,613	2,613	2,613
R-squared	0.58	0.53	0.55	0.16	0.34	0.47
Control Mean	1.79	4.25	0.70	0.30	0.43	0.91

Note: Independent variable is a binary indicator of whether there was an outage on a particular day. Dependent variables are: (1) Logged Food Expenditures, (2) Logged Total Expenditures, (3) Logged Deposit Volumes (4) Logged Withdrawal Volumes (5) Logged Total Transfers Received (6)Logged Total Transfers. All regressions includes individual, date fixed effects. Pvalues from t-wild bootstapped method are in parenthesis.

Table 2.10: Robustness Checks. Dependent Variable - Logged Revenue

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Market Level Definitions				Transformer-Level Definition		
	Definition 1		Definition 2		% of Users' Reports		
	Full Sample	Exluding Sundays	Exluding Sundays	Excluding 1 Report	60	40	20
Outage	-0.057*	-0.078**	-0.076*	-0.162**	-0.143	-0.085***	-0.076**
	(0.087)	(0.045)	(0.087)	(0.011)	(0.24)	(0.01)	(0.05)
Observations	2,297	2,020	2,020	1,803	2297	2297	2297
R-squared	0.675	0.694	0.694	0.671	0.675	0.675	0.675
Control Mean	18.42	18.42	17.74	18.42	16.61	16.75	16.5

Note: All regressions includes individual and date fixed effects. Pvalues from t-wild bootstapped method are in parenthesis.

Table 2.11: Effects of Outage Duration on Business Outcomes

	Business Outcomes				Business Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)
				Logged		
	Total Hours	Market Revenue	Market Profits	Total Income	Inventory Exp	Business Exp
Panel A: Full Sample						
Outage	-0.012*	-0.002*	0	0	-0.005	0
	(0.082)	(0.081)	(0.938)	(0.883)	(0.328)	(0.333)
Observations	8,232	6,987	7,332	6,705	6,762	7,838
R-squared	0.232	0.650	0.728	0.552	0.547	0.589
Control Mean	0.916	11.470	19.610	5.815	6.576	9.211
Panel B: Users						
Outage	-0.021	-0.005	0.008	0.006	-0.015	-0.004
	(0.430)	(0.210)	(0.225)	(0.327)	(0.102)	(0.355)
Observations	2,613	2,203	2,316	2,024	2,036	2,478
R-squared	0.226	0.635	0.669	0.497	0.498	0.607
Control Mean	0.894	11.400	14.530	5.943	6.908	6.943

Note: Independent variable is a binary indicator of whether there was an outage on a particular day. Dependent variables are: (1) Total Work Hours windsorized at 5% (2) Logged Marked Revenues conditional on working (3)Logged Market Profits conditional on working (4) Lodgged Total Income conditional on working (5) Logged Inventory Expenditures (6) Logged Total Expenditures. All regressions include individual and date fixed effects. Pvalues from t-wild bootstapped method are in parenthesis.

Chapter 3

Labeling and Liquidity: Evidence from a Savings Experiment in Urban Malawi

3.1 Introduction

A challenge in personal finance is deciding how to divide savings across multiple savings goals. People are often simultaneously saving for large indivisible investments which take many periods to save up for (such as buying land or paying for educational expenses) while also holding smaller balances for day-to-day expenses or for dealing with unforeseen emergencies. While in theory, an entirely rational economic decision-maker should be able to effectively save for multiple purposes in a single savings vehicle, a large literature in behavioral and development economics suggests that many people in the real world may

find this challenging. Among other reasons, this may be because people overconsume due to time inconsistency, because people lose sight of far-off goals, because saving is not always salient in their minds, or because people face demands on their cash from family and kinship networks (see [18], [61], [62], [85]; [117]) . Against this backdrop, savings might be increased by setting up multiple labeled savings accounts, for example one account labeled for the large investment and another for day-to-day expenses. Seminal work by Richard Thaler [126] and many other authors on mental accounting shows how this may work once money is deposited into a labeled savings account, its fungibility for other uses decreases as the saver incurs a psychological cost for withdrawing money for another, non-earmarked purpose. Thus, even without any hard penalties for withdrawing, holding mental accounts can serve as a "soft" commitment device.[1]

We conducted a field experiment with a representative sample of 801 micro-entrepreneurs in the city of Blantyre in Malawi. Treatment groups were given one of two types of savings vehicles: (1) mobile money accounts offered by the telcom company Airtel Malawi, one of the two dominant telcoms in Malawi; or (2) simple metal lockboxes similar to those used in previous recent studies.[2] The mobile money accounts were identical to those commercially available, with one key difference: withdrawal fees were waived.[3] In total, there were four distinct treatment arms: one box, multiple boxes (in which the respondents

[1]Relatedly, the literature on cash transfers shows that the mere act of labeling an unconditional cash transfer while doling it out to the recipient might be enough to activate a mental account and induce spending on the labeled purpose (see [23], [25])

[2]These include [15], [61], [60], [71] and [81], among others.

[3]As discussed in more detail later, withdrawal fees are substantial. Though they depend on the size of the transaction, the average fee is equivalent to about 5%. Please note that the withdrawal fees could not technically be waived as Airtel was not actually set up to do this. The fees were instead reimbursed to the mobile money account at the end of each week by our field staff via mass deposits back to the mobile money account for all withdrawal fees incurred during the the past week.

were offered up to 3 lockboxes), one mobile money account and 2 mobile money accounts. [4] To examine effects, we utilize three main data sources: (1) detailed high-frequency phone surveys with a random subset of respondents, (2) less frequent monitoring surveys (also done over the phone) with all respondents, and (3) transaction-level administrative data from the telco for the mobile money sample for the duration of the study and an unannounced, in-person box check with the lockbox sample at about 4 months after the study began. In order to collect the phone-surveys as well as to facilitate the mobile money treatment, we distributed basic dual-sim cellphones to our entire sample at the outset of the study.

We have four main sets of results. First, take-up of both the lockbox and the mobile money accounts was high. Within the first 4 months of account opening (or accepting a box), nearly all the respondents used a box and 65% of mobile money users had used the account at least once. In 10 months of account opening, the usage of mobile money grew to over 70%. Amounts deposited were high: cumulative deposits after 4 months were \$170 and \$50 for the lockbox and mobile money groups respectively.[5] However, usage of the second mobile money accounts was inconsequential and only 6% of those offered a second account made at least 1 deposit, likely because using the second mobile money account required people to navigate to a second sim card in the phone,[6] which was likely not straightforward for many users, and they may either have chosen to not do so or made mistakes while navigating. For

[4] The two multiple accounts treatments differ in the number of accounts offered because more than 2 mobile money accounts could not be accessed without physically removing the sim card, which we believed to be impractical.

[5] The measurement comes from the self-reported data of deposits during the lockbox check and administrative records from the telco company for mobile money users.

[6] Airtel did not have the backend to provide two mobile money accounts on a single sim, therefore, we provided dual sim phones to all respondents with a separate mobile money account linked to each sim.

this reason, we pool the mobile money account groups. These are substantial given that average income is about $20 per week. We find a total increase in savings of about 20% in the single box group, a 44% in the multiple box group, and about half this big (11%) in the mobile money group (though only the savings in the multiple box groups are statistically significant from zero)[7].

Third, we find a number of downstream effects of the accounts. We find that respondents in treatment groups worked less in their primary occupation (i.e. small business)[8], and worked more on their farms or in secondary occupations. Treatment respondents gave out more credit to customers (the result is driven by respondents with access to a lockbox only). Households given saving accounts changed their spending patterns as well: households spent more money on holidays and were more likely to buy productive assets like land (although, as would be expected, land purchases are rare) and we find some evidence that they increased their spending on education (likely driven by school supplies). Treatment respondents were more likely to give transfers but no less likely to receive them, suggesting positive social spillovers to other households[9]. Interestingly, we find similar effects for the box and mobile money even though, as discussed above, treatment effects in the mobile money account was smaller and in fact total savings are indistinguishable from zero. We note that in a mental accounting framework, there can be downstream effects even if money is simply being shuffled from one account to another, since this shuffling will activate mental accounts and thus induce different spending patterns.

[7]This measurement comes from the monitoring surveys

[8]This result is primarily driven by mobile money and single box holders. While we do not see the probability of working decreasing for multiple box holders, we suspect that it's driven by idiosyncratic factors and other business indicators (such as revenues and profits) show more consistency across coefficients.

[9]However, we cannot rule out that this effect is not driven by airtime compensation and random payouts during the surveying periods.

Finally, we find no evidence of adverse effects of the accounts. We find that treatment households were no less likely to fully treat health shocks and the accounts were used during the weeks on which a respondent or a household member were sick. We do not find increases in medical expenditures or total withdrawals from all saving places, suggesting that there were no significant improvements in treatment of illnesses or improved ability to deal with shocks. However, we find some descriptive evidence that people do indeed report hesitating to withdraw from their accounts for shocks, but this effect appears to have been offset by the overall increase in savings.

Our study also generates several other important ancillary results. We find the mobile money treatment increased mobile money usage, despite the fact that these accounts were already commercially available (aside from the waiving of the withdrawal fees). To our knowledge, this is the first RCT of a mobile money savings account this is because mobile money takes off so rapidly once it is introduced that there is only a narrow window in which it is possible to evaluate its effects since any random assignment will likely get contaminated by individuals adopting mobile money on their own. For instance, in the case of [83] document that within 4 years of the launch of M-Pesa, nearly 70% of Kenya's adult population had adopted it. Our experiment also allows us to benchmark the effect of mobile money against lockboxes. This is of significant value as there is keen policy interest in deepening the penetration of mobile money, especially for making government transfers digitally, but the benefits (and costs) of digital money are not fully understood. Interestingly, all measures show stronger effects of the boxes than mobile money: deposits are 2-3 times higher, and boxes have about twice the effect on total savings than does

mobile money. We find evidence that the difference in usage is due to transactions costs we sought participants' views regarding the downsides of mobile money in a qualitative module that was administered as part of our endline survey, where nearly 50% reported network issues which did not allow them to use the service, 47% reported facing challenges in withdrawing and depositing cash from/into their accounts due to liquidity constraints faced by mobile money agents, and 12% reported distance from the agent as key downsides (multiple responses were allowed). Administrative data on usage also shows that users located farther away from mobile money agents are less likely to use the service.

Our paper is related to several strands of literature. First, our paper is a contribution to a voluminous recent literature examining the effects of providing people with essentially "vanilla" savings products (as opposed to more sophisticated products such as commitment accounts).[10] Our paper is differentiated first in that we are able to conduct an RCT with mobile money rather than more traditional brick-and-mortar banks, and second by the simple fact that usage of the products was extremely high and thus that we are powered to look at downstream effects.[11]

Our ability to look at downstream effects is also bolstered by the detailed high frequency data collection exercise that lends us with three distinct advantages over the current literature: a large number of downstream outcomes, many observations over several weeks of data collection, and high frequency of surveying that led to better recall and low measurement error. This is of great importance in the context of savings as individuals have many different purposes for saving and withdrawing money, and therefore, a diverse set of

[10] Among others, see [15], [61], [62], [42], [38], [108], [116], [60], [59]

[11] A handful of previous studies have found statistically significant effects on downstream outcomes such as business investment ([61], health investment [62], labor supply [42], and self-reported financial well-being [86], [108]

outcomes can potentially be impacted through a savings intervention.[12] This can however, prove to be a disadvantage in a study setting since impacts may get diffused over the many purposes for which savings get used, rendering treatment effects undetectable. Our high frequency data collection exercise was cognizant of this, enabling us to provide evidence on a large number of outcomes. Most notably, we are able to look at labor supply and the spillover effects of savings accounts on individuals' risk-sharing networks, in addition to the standard set of outcomes for savings studies, i.e., savings, expenses, and income.

On the risk-sharing side, an emerging literature in financial inclusion has considered the implications for informal risk-sharing, finding mixed evidence. On the one hand, [99] and [56] show that greater financial access can enable within-person inter-temporal pooling of risk through savings, thus crowding out spatial risk-pooling across people. In contrast, [13], [91], [60], and [49] show that there may be positive spillovers through a sharing of gains.

In addition to precautionary savings that allow inter-temporal consumption smoothing, savings accounts may also bring about labor supply responses by allowing individuals to either adjust their labor supply to the prevailing wage rate (i.e, work more when the wage is high),[13] or by delinking a day's labor supply with that day's cash needs by making it possible to save up instead.[14] For instance, [42] show evidence from Sri Lanka that when households were given access to a deposit collection service, they responded to the improved savings opportunities by working more. Moreover, in the absence of financial

[12]For instance, in the setting of our current project, at least 10 different savings goals were reported across our 800 respondents at baseline (see Web Appendix Table A.20 for details).

[13][57]) show that when individuals face borrowing constraints, labor supply only insufficiently responds to changes in wage rate.

[14]See [63] for evidence that people adjust labor supply on the intensive and extensive margins on days when they have unanticipated as well as anticipated cash needs.

instruments, household labor supply decisions of very poor households may be subject to a subsistence constraint. Indeed, [69] show that alleviating rural Zambian farmers' liquidity constraints during the hungry season enabled them to reduce off-farm agricultural labor and work more on their family farm instead. Our findings are similar: savings accounts enable entrepreneurs to cut back on hours in their primary business and reallocate labor supply to another occupation, including farming.

Our paper was designed as a comprehensive examination of the costs and benefits of limiting liquidity. It is well understood that limiting liquidity with hard commitment devices may have negative consequences either by reducing access to funds in times of unforeseen need, or simply by reducing flexibility, and not allowing for mistakes or for bad realizations of future states of the world.[15] Despite these clearly apparent costs, the literature has found a robust demand for commitment which gets manifested as a willingness to pay non-trivial sums of money for the tying of one's hands (for instance, see [87] for evidence on data-entry workers choosing dominated piece-rate contracts and [46] for evidence on Kenyan dairy farmers accepting lower prices for the option of getting paid infrequently, enabling saving up for lumpy expenses). However, there may also be other downsides of commitment, which are not internalized at the time of making the commitment, for example in terms of saving at very low rates via expensive microcredit loans [22], defaulting partway through a loan contract [71], setting too hard penalties for missing savings goals [84], or signing up for overly ambitious contractual obligations with a high penalty for default (see [53] for gym membership contracts and [31] for early withdrawals from retirement savings

[15] These costs are well-recognized by individuals: in [61] and [85], strict earmarking of funds for a specific purpose reduced take-up.

accounts). Such concerns are also potentially relevant even for softer commitment such as we study here, and to our knowledge there is no work on this topic.

Finally, we are one of the first RCTs about mobile money, which can only be done in country with modest mobile money penetration. Mobile money has been such a success that studying it in an RCT is challenging, since there is only a short window before adoption is near universal. Thus, the best known studies of mobile money in Kenya are based on the rollout of mobile money by the telcom company ([83], [123], [122]). Other more recent studies of mobile money savings accounts in countries with high penetration (namely Kenya) understandably show modest first stages since baseline adoption is already high. Malawi is a unique opportunity to study mobile money at a point in which adoption is far from universal but in which modest interventions (such as waiving withdrawal fees and promoting the product) can induce added uptake. In parallel, we also contribute to an emerging literature on the impacts of mobile money in developing countries. Much of this literature has focused on transfers or payments through mobile money ([6], [33]), and has found positive treatment effects as mobile money reduces transaction costs relative to physical cash disbursals. In contrast, we focus on savings via mobile money, which has several attractive features such as security (relative to cash) and convenience (relative to visiting a bank branch that may be far away). The transaction costs and quality of service, on the other hand, deter frequent usage, and the savings behavior induced by the simple lockboxes is not mirrored with the mobile money accounts.

3.2 Experimental Design and Data

3.2.1 Context and sampling

The study took place in a representative sample of small entrepreneurs operating in Blantyre, the second largest city in Malawi. While Blantyre is an urban center with a population just over 1 million people, the outskirts of the city contain farmland. Blantyre contains 26 wards and 392 enumeration areas (EAs). To construct a sample with coverage across the city, we sought to randomly select three enumeration areas in each ward, ultimately selecting 77 EAs.[16] Within the selected EAs, we aimed to census all small businesses (defined as any business other than a warehouse, wholesale store or supermarket); however, in some areas with high densities of business this was logistically impossible. Therefore, in EAs with more than 100 businesses, we conducted census surveys with only approximately 40% of businesses (chosen randomly). We did, however, count the total number of businesses, which allows us to recover sampling weights.

We counted all businesses in these areas, other than gas stations, clinics, hospitals, banks or microfinance institutions and manufacturing plants. In total, we counted 9,848 businesses and classified 8,078 (82.1%) of these as small businesses.[17] We randomly selected 3,854 of these businesses for the full census interview. Of these, 346 (8.9%) were permanently closed and 51 (1.3%) were not found after 3 visits. Out of 3,457 reached businesses, 3,139 (90.8%) were willing to engage in the initial conversation with our field team, 63 (1.8%) were not interviewed because either owners were under 18 years old or on all 3 visits, the business was being minded by a person (shop-keeper) less than 18 years in age. Out of

[16] One ward had only 1 EA.

[17] Two of the 77 EAs contained no small businesses. One was an industrial area and the other was farmland.

these 3,076 businesses that were reached, 2,842 (92.39%) were willing to participate in the census interview.

After the census, we randomly selected a study sample, subject to several exclusion criteria. We excluded (i) businesses with more than 2 employees, not including the owner (5.5% of the sample); (ii) businesses in which the owner was not actively involved in running operations (where actively involved was defined as working there at least 5 days per week) and thus could not answer business-related questions (9%), and (iii) businesses that were planning to shut down within 6 months, i.e., before the project was slated to end (2.1%). In addition, we excluded (iv) businesses in which the owner reported being polygamous (5%) ; (v) businesses that operated as mobile money agents (2.57%); and (vi) businesses in which the owner reported to be illiterate (14.9%) or whose eyesight prevented them from reading a printed page (9% of the sub-sample which was randomly selected for a baseline interview).[18] In total, 31.3% of businesses were excluded, leaving 1,953 eligible businesses. Of these, 98.9% (1,937) consented to the census

We randomly sampled businesses for inclusion in the study, stratified by financial access (defined by either having a mobile money or a bank account) and self-reported distance to the nearest Airtel mobile money agent (defined as above or below the sample median). We oversampled businesses connected to the electricity grid: while 26% of eligible businesses were connected to the grid, we sampled 35%.[19] Businesses thus sampled were enrolled for a baseline survey. At this survey, vision and literacy was re-checked and respondents who could not read or write (6.3%) or could not see small text (9%) were not eligible

[18]This last criterion was applied because we had originally planned to collect data via printed logbooks. As discussed later, however we ultimately switched to high frequency phone surveys.

[19]This decision was made to improve the power of the related paper, [35], which utilizes some of the data in this experiment to calculate the effect of power outages on business outcomes.

to continue . The remaining respondents were asked to enroll in the study and consent to further surveys. Those who refused were replaced by randomly selected backups. In total, 90% of businesses consented to participate. The baseline sample includes 801 businesses.

3.2.2 Experimental design

The experimental design is summarized in Figure A.10 and the timeline of project activities is shown in Figure A.11. As discussed below, the experiment involved a design in which access to savings products and the frequency of surveying were cross-cut.

Savings products

We offered two types of savings products: metal lockboxes and mobile money accounts offered via the telecom company Airtel. The lockboxes are very similar to those offered in [63] and subsequent projects. These were metal boxes with a deposit slit in the top, and a latch that could be locked. The boxes were produced by a local artisan and cost about $3.4 (at wholesale prices). Respondents were also given a lock and key, worth about $1. These boxes were not commercially available in Malawi at the time. While a sizeable minority of people (22%) had lockboxes at baseline, these were anecdotally more primitive wooden or carton boxes that often had to be broken to be opened, or did not have a lock at all. The project boxes were widely seen as superior, with almost universal takeup rate. Finally, as in [63], respondents were given a passbook to record withdrawals and deposits, and so that they could track the balance without having to open the box.

The mobile money accounts were nearly identical to accounts which were already widely commercially available from Airtel Malawi, with one key difference: we reimbursed

all withdrawal fees for the duration of the project, making withdrawals effectively free for respondents. While these fees are typically not enormous, they can still be an important deterrent, especially if people are not sophisticated about their usage. There is a fixed fee for withdrawals in different monetary ranges (see Web Appendix Table A.19 for the schedule of withdrawal fees). If a respondent makes the maximum allowable withdrawal within a range, the average fee is around 4%; however, if a respondent were to mistakenly withdraw at the bottom of a range, the fee can be as high as 10% (and potentially higher for the lowest range). For our sample, the average fee charged is about 5% of the transaction amount. An important logistical note in interpreting effects is that it was not logistically possible to waive these fees directly; instead, a research assistant received the set of transactions at the end of the week and reimbursed respondents. Finally, though these accounts were already available, we found in initial work that knowledge of mobile money was somewhat limited. We thus developed modules to train people in how to use the accounts. This module, along with surveys, can be found on the authors' websites.

Finally, during baseline, we asked all respondents about their savings goals. At the time of treatment administration and account opening (which was about 2 months after baseline), those in the treatment groups were encouraged to use the project account to save towards their savings goal. Specifically, we asked respondents to write their savings goal on a piece of paper. In the case of the box group, this piece of paper glued to the lid inside of their project boxes. Summary statistics of goals are reported in Web Appendix Table A.20. Columns 1-4 show goals elicited at baseline for the various treatment groups, while the remaining columns show goals elicited at account opening (note that people could pick

more than one goal for the account). Consistent with prior work, at baseline we see a wide variety of goals in the control group, 65% want to invest in a business, 56% want to save up for emergencies, 47% want to save up for regular household expenses, 42% want to save up for the hungry season, and 27% want to save up for durable goods (with smaller percentages on other items). We observe no difference in baseline goals across treatment groups (with the one exception of durables as a goal, which is less common in the mobile money group).

Turning to the project accounts, the percentage of people saving for each goal is lower than in the baseline due to the fact that at baseline we asked about goals across all places that a respondent uses for saving. In the single box group, the biggest goals were investing in business (26%), buying land (25%), education for children (12%) and emergencies (11%). We examine whether these differ between the other treatment groups. Our prior here is that the multiple box group will have more goals but that the mobile money and single box will likely be similar. The pattern of goals looks very similar to this prediction people with multiple boxes are much more likely to mention nearly all of these goals, suggesting that people with a single box chose to set only one goal while those with more than one box set several.

Number of savings accounts

One of the key features of the project is measuring the effect of an incremental account. To this end, we split each of the savings groups (lockboxes and mobile money) into subgroups. In the lockbox treatment, a single lockbox group was only offered one box, while in the multiple lockbox group respondents were offered to receive up to 3 lockboxes. To differentiate the boxes, they were painted different colors (the first was silver, the second

if any was black, and the third if any was brown). For the mobile money treatment arms, respondents in a single Mobile money group were offered only 1 account, while those in multiple account group received were given a choice to get up to 2 Mobile Money accounts with Airtel. These accounts were separate mobile money accounts with a separate sim card and associated phone number due to the fact that the current product does not offer several mobile money accounts within a single phone number. To encourage people to use these with minimal hassle, the respondents were provided a dual-sim phone, discussed below. As with the boxes, the mobile money accounts were labeled as silver or black. In the case of both mobile money as well as boxes, respondents were encouraged to save for their different savings goals in different accounts.

Cell phones

Of course, a cell phone is needed to utilize mobile money. While the vast majority of respondents (94%) already had cell phones, we did not want the lack of a phone to be a deterrent for using mobile money accounts. In addition, anecdotes suggested that some people had outdated or poorly functioning phones that did not work well. Thus, the full sample received a simple phone, including the control groups. The phones were simple feature phones, worth about $12 in Malawi. These phones did, however, allow the inclusion of 2 sim cards, which is important for the multiple mobile money account treatment arm. Though the phones were initially given out to avoid a confound, we used the cell phones to conduct most of the follow-up surveys since the cost of reaching respondents via phone was much lower.

Survey Frequency

We are interested in measuring the effect of savings products on daily economic outcomes. However, in the absence of objective administrative data such as barcode scans to measure sales, the only way to measure these outcomes is through surveys (in fact, most businesses do not keep records). Moreover, for some of the outcomes that we were interested in tracking for this study, such as cash management, labor supply, and coping with shocks, these surveys need to be administered at a high frequency to get around measurement error introduced by imperfect recall.[20] Therefore, we developed a phone-based survey which we administered once or twice per week during 2 post-treatment periods. We call this survey the high-frequency phone survey, or HFPS. The survey, which we discuss in more detail below, is similar to the logbooks used in [61] and includes a battery of questions about personal finance and business outcomes.

It is possible that the survey itself is a treatment, for example by making certain business decisions more salient.[21] To test this hypothesis, we randomly split the sample into two groups: one administered the HFPS and the other not. To measure the effect of surveying at high frequency, we administered two monitoring surveys. These surveys included some of the same questions as the HFPS, but also included a number of other one-time only questions. A detailed analysis of the HFPS can be found in the companion paper [4], which finds that the HFPS had effects on several outcomes (likely through focusing attention on those outcomes) but that the effect of the HFPS was not additive and had

[20] In a field experiment set in Ghana, [80] find that individuals who were surveyed more frequently report working more in self-employment, suggesting better recall.

[21] For instance, in the context of a field experiment on water chlorination based in Kenya,[133] show that the subset of treatment households that was subject to more frequent surveying was more likely to chlorinate their water.

limited impact on estimated treatment effect. For the purposes of this paper, we focus on the results from the HFPS, but use the monitoring survey as a validation of the main results. Since we find no substantial effect of the monitoring in our companion work, we prefer the HFPS results for many of the outcomes, since they are better powered due to expanded time coverage and are more reliable with reduced recall bias.

3.2.3 Data

We utilize 6 main sources of data in this study. First, the census has a limited set of information, including distance to the nearest mobile money agent. Second, the baseline survey contains a host of questions on business outcomes, savings and cash flows, as well as detailed questions about household and demographic characteristics.

Our two main sources of data to measure treatment effects are the high-frequency phone surveys (hence HFPS) and two monitoring surveys. As discussed above, the entire sample was administered the monitoring surveys whereas only half of the sample (randomly selected, stratified by treatment) was administered two HFPS's.

The HFPS was intended to measure business outcomes, labor supply, household expenditures, transfers, savings, credit, shocks and related outcomes at the daily level. The HFPS was conducted in two waves: the first wave was in September-October 2017, approximately 1-2 months after account opening, while the second wave was in February-March 2018. Wave 1 lasted for 8 weeks while Wave 2 lasted for 6 weeks. In Wave 1, respondents were called twice per week, with the 2 calls being 3-4 days apart. In one of the weekly interviews, the respondent was administered a "short" survey which asked about business outcomes over the past 3 days (day by day). The survey took about 15 minutes to

complete. The other "long" call included all the questions in the short call, but also added a recall module over other outcomes that were expected to be rarer or more memorable and thus could be remembered over a week. These included shocks such as household illness and funerals, deposits to savings groups, and transfers given and received. The long survey also included questions about expenditures over the past 7 days. The long survey took about 40 minutes on average.

Respondents were randomized into which days they were to be called; however, once the day of the week was selected, it was kept the same throughout the project (so that respondents could plan on the call at a specific time and day).[22] Respondents were able to pick the time of day when they wished to be called. To encourage compliance, respondents were given US $2 for each week of the survey and were enrolled into a lottery in which there was a 1/4 chance of winning an additional US $3. Airtime was sent directly to the respondents' phones after each phone call was completed.[23] If the respondent was not reached on the original day of the phone call after 3 attempts, a make-up call would be scheduled for the following day in order to reconstruct the lost days. Due to difficulty of recalling information with longer recall periods, only up to 4 recall days were asked at every phone call and some days were lost as a result.

We conducted two monitoring surveys, after each round of the HFPS (in January 2018 and in March 2018). These surveys took approximately 75 minutes to complete, and included a host of questions similar or identical to the HFPS, though over a longer recall period for some outcomes. The surveys also included a number of other questions, such as

[22] Out of 400 respondents, only 2 respondents asked to be called on different days and the rest complied with the original day assignment.

[23] Respondents were enrolled in the lottery even if they were not reached for a specific call, but they did not receive the US $2 payment

questions about self-reported financial security and regret spending. In addition, the second monitoring survey (the endline survey) included some debriefing questions about people's attitudes and experience with the savings products (including upsides and downsides of using mobile money), pressures to share money and experiences with the surveys themselves.

We measure usage of mobile money through administrative data shared by the mobile money operator Airtel. This dataset includes all deposits, withdrawals, transfers received and sent, as well as other uses of mobile money. For the lockbox, we measure usage through a "lockbox check" visit which occurred in December 2017. During this visit we asked a number of questions about usage of the boxes, including the total number and value of deposits and withdrawals since July 2017. Following [62], the one verifiable outcome we could collect was the balance in the box at the time of the visit.[24] We show a regression of the amount in the box on self-reported deposits in Web Appendix Table A.23 (column 1), which shows a coefficient very close to 1 (1.05). We take this as suggestive evidence that self-reporting of box usage was accurate.

The final survey we utilize is a short intake survey, which was administered at the time of account opening. This survey included many of the questions that would later constitute the HFPS, recalled over the previous 3 days. These pre-treatment values are used as controls to improve power of the main regressions [37], so that our baseline specifications are always ANCOVA.

[24]Seventy-nine percent of respondents who we interviewed were willing and able to open at least one of the boxes Of the remaining 21%, 5.4% could not open the box because they did not have the key with them, 9.4% refused to open the box or travel home to show the box to the field officer, 2.4% didn't have access to the boxes at box checks, 3.3% respondents with boxes were not checked because the respondent could not travel home with the field officer on the day of the survey.

3.2.4 Attrition

Attrition for the different surveys is shown in Table A.21. Columns 1-4 show attrition during the HFPS, with the odd columns showing whether a respondent appears at least once and the even columns showing the percentage of calls that were successfully completed. In round 1, 98.8% of respondents completed at least one survey and 88.8% of calls were made; in round 2, this fell 83.8% and 73.5, respectively.[25] While not unexpected, lower compliance in round 2 points to the problem of conducting phone surveys in general. We do not find any differences in attrition across groups, however, the coefficients on the treatment indicators are not significant for any of the surveys. The only statistically significant coefficient is for an indicator for the HFPS sample in round 1: HFPS respondents were 6.5% less likely to complete a survey. However, in round 2, there is no difference between the HFPS and non-HFPS samples.

3.2.5 Summary statistics and randomization check

Summary statistics and a check of randomization balance are presented in Table 3.1 (from the baseline survey) and Web Appendix Table A.22 (from the intake survey). From Table 3.1 Panel A, 46% of the sample is male and the average business owner is 34 years old and has 9 years of education. Ninety-three percent of respondents have an iron roof on their homes and the average value of assets owned by respondents' households is $861. Sixty-eight percent of the businesses are in retail, with the remainder predominantly in services, which includes occupations such as barbershops, tailoring, and welding. These

[25] In round 1, 1.25% of respondents were reached but refused to do the survey; in round 2, this figure 6%. The remaining attriting households were not reached, which could be because people changed numbers, moved, or because they were screening our phone calls.

businesses are very small: average weekly profits are about $18 per week and the average firm has only $154 in inventory.

Panel C shows statistics on savings. Average savings across all sources was $143 at baseline. This was split across multiple locations for the average respondent: the average respondent has 2.5 savings places. The most predominant saving place is keeping cash at home, with 82% of respondents, suggesting that despite access to more formal income sources in an urban environment (such as banks), transactional costs are a major deterrent to usage. Saving groups (VSLAs and ROSCAs) are used by 52% of the business owners, and 30% use mobile money accounts explicitly for saving [26] . Twenty-nine percent of the sample have access to a bank account and 22% save in saving boxes.. Note that sizeable minorities of the sample already save in mobile money and lockboxes, raising the question as to whether our interventions are likely to be effective. As we will show, however, both interventions have a high takeup of both products and show persistent usage throughout the duration of the project. This suggests that the offer products are of superior quality and there may be limited access to information on mobile money which prevents higher adoption rates.Nevertheless, it is important to keep in mind that our experiment consists of providing savings vehicles that are not completely new products.

In columns 2-8, we present regression coefficients from bivariate regressions of dependent variables on an indicator of each treatment arm for the full sample. In columns 6-8, the regressions are replicated for HFPS groups only, where each coefficient represents the effect of being in a particular treatment group relative to the HFPS control group.

[26] Usage of mobile money account for any purposes defined as having own account or using someone else's is 62%, while about half of the respondents have their own mobile money account at baseline.

Reassuringly, we find minimal differences between the groups in the full and the HF sample, with a very few characteristics displaying imbalance by chance. For this reason, all the regression specifications include baseline characteristics We further supplement the balance check with the intake survey data to verify that the groups are similar on measures taken at higher frequency, with the results displayed in Web Appendix Table A.22. While there are some categories that do not perfectly balance between the treatment and control groups (there are different deposits into home savings, boxes and mobile money across groups and treatment groups are less likely to receive non-spousal transfers), the majority of the characteristics suggest comparability. In order to ensure that the treatment effects are not driven by the differences recorded pre-treatment, the HF regression analysis includes the intake survey measures.

3.3 Take-up

3.3.1 Pooled take-up of lockboxes and mobile money

We start by documenting take-up. Figure A.10 shows a CDF of deposits into lockboxes (measured from surveys at the "lockbox check" visit in early December) and the mobile money accounts (measured from administrative data). For comparability between the accounts, Figure A.10 focuses only on deposits made until the month of the lockbox check (December 2017). In Table 3.3, we show summary statistics and the difference between the usage of the accounts through December 2017 (columns 1 through 3). We observe substantial usage of both products: nearly everybody took the product or opened an account, and 97% and 65% of respondents made at least one deposit before December 2017

in the box and mobile money groups, respectively. The total savings measured by the cumulative value of deposits was sizeable in both groups: mean deposits were $130 in the box groups and $44 for the mobile money groups, quite substantial given baseline daily income of about $3-4 per day. The products appear to be used differently: the average respondent made 65 deposits into their box, compared to only about 6 in mobile money. This is not surprising since the predominant saving methods is keeping cash at home, and it seems natural that the box becomes a substitute to home savings. In addition, saving at home has zero transactional costs in contrast to making deposits at mobile money agents, which are 11 minutes of travel time away for an average business owners. In addition, there is a risk of a network failure at the time of the transaction and agents holding inadequate balance to process deposits, which may discourage making a trip to a nearby agent.

Since box deposits are self-reported whereas mobile money transactions are from administrative records, it is useful to compare a verifiable measure of usage – the balances in boxes in December 2017 (i.e. at the date of visit in the lockbox group). We observe that 53% of box respondents had a non-zero balance[27]. While balance is a measure which misses a lot of activity (since the whole point of savings is to withdraw once goals have been reached), we view this data as generally supportive of the notion that the box group used the accounts.

In general, usage of both products in this study was substantially higher than in previous studies, where usage typically features a large number of non-users with a few heavy users.[28] One exception to the norm of low take-up in basic savings access studies is

[27]The corresponding figure is not yet available for the Mobile Money group because we are in progress of obtaining additional data from Airtel.

[28]See [108] and [63] for a compilation of take-up rates in previous work.

[108], where the product provided was the only option available to respondents in Nepalese slums. Thus the high take-up we observe may be an artifact of the limited financial access in Malawi, though it's also likely that mobile money and lockboxes are simply more appealing than other saving tools.[29] In fact, endline qualitative-type questions about saving constraints suggest that the treatment groups are 8-10 percentage points less likely to be deterred from accumulating money because of theft, compared to 14% in the control groups, evidently indicating that the offered products they were superior. Thus even in contemporary urban Malawi, basic questions of security are relevant impediments to savings. The most dominant reason for having trouble saving is not having enough extra cash (46%) and excessive spending on personal expenses (38%), [30]

3.3.2 Usage of multiple accounts

The key test of the effect of multiple accounts is to examine whether usage was increased with the introduction of multiple accounts or not. This is shown in Table 3.4 in which we run the following regression for measures of takeup U_i separately for lockbox and

[29] One of the sites in [63] is in the Liwonde area in Southern Malawi. Only 41% of people used those accounts in the 2 years after account opening, and only 25% used the accounts at least twice. While there are important differences in the samples between studies which preclude direct comparisons as to reasons for differential take-up, is it nevertheless true that higher usage in the current study provides more power for identifying downstream effects.

[30] An important difference with some of the previous work in this space is that many of our respondents already had access to the products in question (lockboxes and mobile money accounts). We examine usage among those who had accounts at baseline and those who didn't in Web Appendix Figure A.13 and Web Appendix Table A.24. Among box respondents, we find minimal differences in usage between those who had boxes at baseline and those who didn't, suggesting that the boxes we provided were a better product than the previous boxes (which indeed they seemed to be). For mobile money, however, usage was clearly higher among those who already had accounts, which may be for several reasons, including (1) the waiving of withdrawal fees, which may encourage people to use mobile money more than they had before or to shift money into project accounts (though this is not the only explanation since total mobile money savings went up); (2) the training about how accounts worked; or (3) the implicit goal-setting. Nevertheless, we still observe substantial usage even among people with no experience with mobile money previously.

mobile money.

$$U_i = \theta_1 Multiple\ Account_i + \beta X_i + \epsilon_i \tag{3.1}$$

where X_i include a variety of baseline characteristics: standard demographic indicators, several measures of baseline wealth and other correlates identified in prior work as possible constraints on saving, including inter-household pressures on sharing income (we also are in the process of adding in baseline time preferences). Results are shown in Table 3.4 and in Figure A.10, which shows a CDF of deposits into the 2 accounts. For both treatments, we observe effects of multiple accounts though the effects are stronger for the box. Since takeup of the box(es) was universal, there is no margin to improve take-up on the extensive margin. However, on the intensive margin, we observe $32 higher deposits for those given multiple boxes (23% of the baseline mean in the single box group). For mobile money, by contrast, the effect is entirely on the intensive margin. In contrast to our prior, groups with multiple mobile money accounts were less likely to use the the product at all. While we do not have a clear explanation of the reason for this result, we speculate that having 2 sim cards may have been confusing for new users which may have caused frustration with trying the account at all. Due to low takeup of multiple mobile money accounts (only 6% of those offered multiple mobile money accounts used it at least once), we pool the multiple and single mobile money groups when presenting the results, and, thus, the effects of mental accounting largerly rely on radnomization within the lockbox groups.

Table 3.5 examines how people decided to split money between multiple accounts, for boxes (in columns 1 through 4) and in mobile money accounts (in columns 5 through 8). . Columns 1 and 5 show mean usage for the group offered 1 box and 1 mobile money account,

while the remaining Columns show usage for those offered multiple accounts. Columns 2 and 6 show usage in the most-used ("dominant") account, and Columns 3 and 7 show usage in the non-dominant accounts (Columns 4 and 8 show the difference between the accounts). While people in the single box group save $151 in their only box, people in the multiple box save somewhat less ($132, i.e. 13% less) in the dominant box. They also save another 1/3 as much in their other boxes ($47), which accounts for the overall increase in savings observed above. We also observe that the average number of deposits for multiple box holders is evenly split between the two groups, while for the median respondent, the usage of dominant boxes is similar to single box holders and the total number of deposits is higher due to having access to another account. The results are not mirrored in the mobile money groups. About 60% of respondents made at least 1 deposit into their first account, and only 6% have used the second account at all. Other measures of usage (values of deposits/number of deposits and withdrawals) also suggest that having 2 accounts with the same telecom company was not useful to the users and the second account as largerly neglected.[31]

3.3.3 Other determinants of take-up

Table 3.4 also shows other correlates of take-up, in addition to the indicator of having multiple accounts. As in many previous studies, we find that wealthier people save more: log assets and years of education (likely correlated with income) are correlated with higher savings. As in [62] people who are taxed by social networks (i.e. who give money but

[31] Anecdotes suggest that respondents preferred having mobile money accounts from two different companies as opposed to 2 accounts within the same company.

do not receive at baseline) also save more. Lastly, women save less in boxes. Interestingly, we see no negative correlation between having a savings box at baseline and savings in the box, suggesting that the boxes were improved technology. Correlates of mobile money are less clear-cut. People with bank accounts and mobile money accounts at baseline save more, whereas though with savings boxes at baseline save less (perhaps because people who save in savings boxes are negatively selected). A final interesting finding is that people save less if they live farther away from a mobile money agent, which could indicate that transactions costs are a deterrent (though there may be other factors correlated with distance).[32]

3.4 Level treatment effects on outcomes from surveys

3.4.1 Specification

For both the HFPS and monitoring surveys, we utilize two main specifications. First, we pool the multiple and single accounts treatment and run the following:

$$Y_{ist} = \theta_1 MM_i + \theta_2 LB_i + \beta X_i + \gamma L_{it} + \mu_s + \epsilon_{ist} \tag{3.2}$$

where Y_{ist} is an outcome for individual i at time t in strata s, μ_s is a strata fixed effect, X_i are individual controls including a baseline measure of the outcome variable (thus making the specification into an ANCOVA), and L_t are lottery airtime winnings. Second, we separately look at the effect of single and multiple accounts, which we estimate in a similar specification:

$$Y_{ist} = \alpha_1 MM_i + \alpha_3 LB_i^1 + \alpha_4 LB_i^{mult} + \beta X_i + \mu_s + \epsilon_{ist} \tag{3.3}$$

[32] This result is similar to [59] who find a negative gradient between distance and usage.

where MM_i is a dummy for being offered a mobile money account, LB_i^1 is a dummy for being offered 1 lockbox, and LB_i^{mult} is a dummy for being offered multiple lockboxes. We pool mobile money based on the prior evidence that suggests no difference in outcomes between single and multiple mobile money accounts. In both specifications, standard errors are clustered at the individual level. All regressions are weighted by population weights.

3.4.2 Treatment effect on savings

In discussion of the treatment effects we focus on results derived from the high frequency surveys since they are superior in measurement of the outcomes to the monitoring checks, however, the monitoring checks are presented as supporting evidence for results found in HF surveys. Effects on savings from HF surveys are presented in Table 3.6. Columns 1 and 2 show a first stage in which the dependent variable is using any box or a mobile money. We find big effects for each treatment: the box treatment increased the probability of making a deposit by about 35-36 percentage points while the analogous figure for mobile money was 37 percentage points. This alone is an interesting result: due to the still limited penetration of mobile money in Malawi, we were able to significantly increase usage through this simple experiment. To our knowledge, this is the first-ever RCT of mobile money – since an RCT is only possible in a narrow window when mobile money is not yet prevalent. Other papers like [83] and [123] are instead differences in differences based on the rollout of the program.

Turning to the other columns in Table 3.6, we look at deposits into the savings product offered in the study (i.e. lockboxes or mobile money). For both groups, we observe a substitution effect from other saving sources into offered product accounts. For the lock-

boxes, we observe a $5 and $11 treatment effect on lockbox saving for single and multiple box holders, respectively. However, usage of home savings and other boxes for the lockbox treatment groups decreases by $3.5-3.8 and about $2. Consequently, the overall treatment effect on deposits is negative (but statically indistinguishable from zero) for single group holders, but positive for multiple box holders. For mobile money, the effects are very similar but more muted. Deposits into mobile money go up by $2.5 but is fully crowded out by other forms of savings. While we observe a much larger savings effect in the multiple box group, we do not make too much of this given that the single box groups do not display the same patterns.

One reason that effects may be muted is that many deposits might occur outside the high frequency surveying window. Table A.24 shows effects on deposits over the past 2 months measured by the 2 monitoring surveys and while the pattern of crowd-out is similar, here we find positive effect on total saving for single box group ($23) and a statistically significant effects of $49, on the base of $110. Similarly, mobile money shows a large treatment effect ($19) but it is not significant.[33]

3.4.3 Treatment effects on business outcomes and labor supply

Table 3.7 shows effects on labor supply and business outcomes from HF surveys. Interestingly, we find that treatment respondents worked *less* in their primary business after receiving accounts: treatment respondents worked 4.8-6.5 percentage points less, on

[33]Effects on withdrawals are shown in Web Appendix Table A.25 and on balances in Web Appendix Table **??** (available in the monitoring check only). Results appear broadly similar.Reasons for withdrawal are in Web Appendix Table A.33. The most common types of withdrawals were for business expenses, general consumption or bills, emergencies or school fees. We observed significant effects on several but not all of these outcomes (notably we find no effect on business outcomes). Consistent with previous evidence, we observe higher withdrawals of all types for the multiple box group.

a base of 83% in the control group. While the effects are driven by one box and mobile money groups only, we attribute the lack of results in multiple box group to noise since (1) it is unlikely that having 1 or more than 1 account induced opposite responses in labor supply and (2) other business indicators (profits and revenues) exibit negative responses in multiple box groups, which is in line with the single box group. Effects are similar in the monitoring survey (Table A.23) though not significant. Respondent appear to switch to farming and other occupations instead: though only 6% of control group respondents did any farming, this increased by 2-8 percentage points in the treatment group. We also observe some switching to secondary occupations, though this is only significant for mobile money respondents in the monitoring survey. On the whole, these labor supply results are very similar to [69] who find that providing hungry season loans to farmers reduces casual off-farm labor and increases labor at home, implying that households work off farm to generate enough cash to meet daily expenses. Similarly, our results imply that entrepreneurs would prefer to work less in their primary business and some would prefer to work more on farms and in other occupations. This implies that entrepreneurial activity is partly designed to meet daily subsistence needs and if anything people work *too much*.

3.4.4 Treatment effects on expenditures

Table 3.8 shows effects on various expenditure categories. We observe a sizeable increase in total expenditures for the box group ($3-4 on a control mean of $26, i.e. 15%). This is driven by an increase across a number of different categories. Though most are insignificant, we find statistically significant increases in education for children and in "other expenses." The results for other categories are confirmed in Table A.25 where we use the

monitoring survey data. The educational results are not mirrored during the monitoring checks. We attribute this finding to either a recall bias (and difficulty recalling aggregate educational expenses for longer time periods) or the results found in high frequency surveys are driven by smaller educational expenses which are harder to detect with questions spanning a larger recall period.

Table 3.9 shows several categories of rare spending that we only measured in the monitoring surveys, including holiday spending, farm inputs, durable goods, and buying and renting land. We find statistically significant increases for several of these, suggesting that the products were helpful for saving for these goals. Treatment respondents spend more on holidays, and are more likely to buy or rent land. In addition, box respondents spend more on farm inputs, consistent with previous results on increases in farming activities.

3.4.5 Treatment effects on loans and transfers

Table 3.10 shows spillover effects on risk-sharing partners. We find that treatment respondents were more likely to give out transfers but no less likely to receive them, suggesting positive spillovers on untreated individuals (a result consistent with [60]). We find no effect on transfers to the spouse. Monitoring Survey results shows less compelling evidence, with no effect on transfers outside the household and a decrease in transfers received from the spouse. The difference in the results may be driven by the different in the recall period. During the HF survey, the respondents were asked to report transfers in the past week, in contrast, during the Monitoring check the transfers were recorded for the past month, which is subject to larger measurement error. We are unable to rule out that the transfers during the HF surveys resulted from the survey compensation during since the HF groups

are not likely to give out transfers during the non-HF periods.

3.5 Measuring Liquidity Costs via Responsiveness to Shocks

3.5.1 Specification

A primary contribution of this paper is to use the high frequency surveys to examine the ability of treatment households to respond to shocks. As discussed previously, effects could go either way. For instance, having access to savings instruments, and therefore, greater savings, may increase resilience. On the other hand, mental accounting may make people *less* likely to withdraw money from accounts in response to shocks.[34] To examine this we will run a fixed effects specification as follows:

$$Y_{ist} = \gamma_1 S_{it} + \gamma_2 MM_i * S_{it} + \gamma_3 LB_i * S_{it} + \lambda L_{it} + \phi_i + \mu_s + \epsilon_{ist} \tag{3.4}$$

where Y_{ist} are outcomes including whether an illness is treated immediately, health and other spending, and labor supply and S_{it} is a health shock (including own or household illness),ϕ_i are individual level fixed effects. [35] We will test whether whether γ_2 and γ_3 are equal to 0.

As previously, we will separately examine effects for multiple and single accounts as follows:

$$Y_{ist} = \theta_1 S + \theta_2 S_{it} * MM_i^1 + \theta_3 S_{it} * LB_i^1 + \theta_4 S_{it} * LB_i^{mult} + \lambda L_{it} + \phi_i + \mu_s + \epsilon_{ist} \tag{3.5}$$

[34]There is also the possibility that having access to savings instruments may make people less likely to fall sick in the first place, for instance, by improving the likelihood of using preventive medicine. We check for this in Web Appendix Table A.34, and find no difference in the likelihood of being sick by treatment.

[35]We have a limited number of other shocks in our dataset but many of these are underpowered because they are rare (i.e. funerals) or don't affect many people (i.e. electricity outages – see Brailovskaya 2018).

and we test whether θ_2 and θ_3 are equal to 0.

3.5.2 Results

Results are shown in Table 3.11 for household illness (Panel A) and own illness (Panel B). Columns 1 and 2 show indicators for whether, conditional on being sick, the illness was treated immediately and fully. In the control, 95% of illnesses were treated immediately while 74% were treated fully (the difference may be attributable to people treating an illness that should require seeing a doctor with medicine from a pharmacy). We do not find any evidence that this was smaller in the treatment groups, in fact, treatment groups in with mobile money and multiple box accounts were more likely to treat the illness fully. In contrast, we observe no effect on medical expenses – while all households increase medical expenses (and other expenses, perhaps to make up for lost purchases of the sick member), we do not observe any difference between the treatment groups and control. Columns 8-12 show effects on withdrawals. We find that treatment respondents do withdraw money from project accounts, whereas control respondents withdraw from home, so that total withdrawals are not differential across treatment groups. Thus if there are liquidity costs of withdrawing money, these are modest for events like health shocks.

3.6 Discussion and Conclusion

In this paper, we examine possible positive and negative effects of mental accounting. We show that people actively use labeled lockboxes and mobile money accounts and that encouraging the activation of additional mental accounts increases total savings. In

contrast to much of the prior savings literature, we find effects on a number of downstream outcomes, including labor supply, certain types of expenditures, and transfers outside the household. However, we find no negative effect on risk-coping. Why is this? One possibility is that there is no liquidity cost and the accounts are indistinguishable from home savings. However, when asked directly in debriefing questions at the end of the final survey, we find that 57% of people ever decided not to take money out of the box because the box was intended for a specific purpose, while 30% reported not being willing to take money out of the box to deal with a health emergency. It may be then that the accounts discouraged more "frivolous" spending, but that liquidity was not reduced for more important emergencies like the health shocks we study. It remains a question for future research to document what types of spending are reduced from the provision of savings accounts.

Another important result we find is that the seemingly more primitive technology of lockboxes were used much more than mobile money. Why is this? In qualitative debriefing interviews, we identified several likely explanations, including higher transactions costs, limited liquidity, and trust. We asked study respondents about the downside of using mobile money, which we tabulate in Web Appendix Table A.35. While 25-30% of people list no downsides, when prompted, 46% of respondents report that the network is often down, 35% report that agents don't have enough cash to make withdrawals, 15% report that agents are too far away, 14% report that agents don't have enough balance to make deposits, 13% report that they don't trust the agents, and 10% report that the system is too complicated. We also observe that usage of mobile money was lower for people living farther away from the agent (Table 3.3). While there are other differences between people that live

closer to agents and people who live farther, this result is also suggestive of transaction costs being a deterrent.

Nevertheless, we do find compelling evidence that mobile money affected many outcomes compared to control, which serves in contrast to other papers including a previous study in Malawi [59]. This is promising since banking in the future is likely going to be in some form of digital platform. Already the number of mobile money agents is orders of magnitude higher than traditional bank branches, and this will only increase over time. A Gates Foundation initiative to map financial service providers in Kenya, Tanzania, and Uganda found 65,569, 45,429, and 41,794 mobile money agents respectively, compared to just 1,254, 579, and 477 bank branches.[36] Mobile money will likely be the vehicle through which developed countries close the gap in financial inclusion with the developed world.[37]

[36] See www.fspmaps.com.

[37] According to the 2017 FINDEX, only 63% of adults in developing countries have a bank account compared to 94% in OECD countries [54]. The statistics are similar for enterprises: according to [120], only about 65% of the MSMEs in non-OECD countries had a deposit account in 2010.

3.7 Tables and Figures

Table 3.1: Summary Statistics and Randomization Check (Part 1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample				HFPS sample only			
		Regression Coefficient for:				Regression Coefficient for:		
	Control Mean	One Box	Multiple Boxes	Mobile Money	Control Mean	One Box	Multiple Boxes	Mobile Money
Panel A. Demographic information and asset ownership								
Age	34.36	2.168	1.008	2.310**	33.58	2.707	2.800*	2.934*
	(10.16)	(1.39)	(1.41)	(1.07)	(9.27)	(1.78)	(1.69)	(1.55)
Married	0.81	-0.09	-0.08	-0.083*	0.79	-0.13	-0.06	-0.07
		(0.06)	(0.07)	(0.05)		(0.09)	(0.08)	(0.07)
Male	0.46	0.04	0.091	0.014	0.56	-0.07	-0.017	-0.095
		(0.08)	(0.09)	(0.07)		(0.11)	(0.10)	(0.09)
Household Farms	0.71	-0.004	-0.021	0.048	0.69	-0.072	0.099	0.045
		(0.07)	(0.09)	(0.06)		(0.11)	(0.09)	(0.08)
Years of Education	9.02	-0.033	0.102	-0.228	9.09	-0.187	0.033	-0.281
	(2.85)	(0.40)	(0.41)	(0.35)	(3.01)	(0.65)	(0.60)	(0.53)
Land Owned (Acres)	0.65	0.04	-0.029	0.084	0.58	0.258	0.016	0.294
	(1.18)	(0.19)	(0.16)	(0.16)	(0.93)	(0.29)	(0.18)	(0.24)
Value of Assets	861.30	-174.302	-7.707	-7.913	960.00	-164.555	182.549	21.111
and Livestock	(1502.00)	(154.07)	(194.08)	(154.28)	(1576.00)	(152.08)	(291.72)	(171.91)
House has iron roof	0.93	0.021	0.019	-0.013	0.93	0.023	0.028*	-0.042
		(0.01)	(0.01)	(0.02)		(0.02)	(0.02)	(0.03)
Uses Own Mobile	0.55	0.045	0.179**	0.066	0.53	0.117	0.156	0.114
Money Account		(0.08)	(0.08)	(0.07)		(0.10)	(0.10)	(0.09)
Owns a cell phone	0.94	0.01	-0.088	-0.027	0.93	0.005	-0.041	0.017
		(0.02)	(0.06)	(0.03)		(0.04)	(0.05)	(0.04)
Distance to closest mobile	11.35	-0.083	-1.211	-0.764	9.84	0.976	1.204	2.033
money agent (minutes)	(14.21)	(1.43)	(1.27)	(1.16)		(1.88)	(1.61)	(1.62)
Pavel B: Business								
=1 if Retail	0.68	0.008	-0.045	0.058	0.66	-0.016	0.083	0.057
		(0.07)	(0.09)	(0.06)		(0.10)	(0.08)	(0.08)
Average Weekly Revenue	65.96	5.051	-8.272	-4.967	66.51	-3.872	12.067	-1.163
	(98.79)	(13.89)	(12.81)	(12.22)	(106.80)	(12.70)	(14.26)	(12.13)
Average Weekly Profit	18.67	0.562	-0.536	-0.008	17.80	0.144	3.008	1.288
	(23.15)	(2.94)	(3.05)	(2.56)	(23.24)	(2.59)	(3.07)	(2.21)
Current Inventory Value	154.00	12.299	-10.3	-57.713	107.90	84.437	120.464	14.338
	(362.20)	(54.43)	(57.99)	(45.02)	(219.60)	(56.50)	(75.35)	(27.46)
Value of Equipment	101.10	-1.043	49.065	17.398	121.90	-15.912	43.975	18.559
	(206.60)	(19.38)	(31.72)	(20.07)	(240.60)	(21.93)	(42.93)	(26.27)

Note: Means are population weighted. Randomization check is performed only for businesses that appear in the analysis sample (i.e. completed a monitoring survey or at least one phone survey). There are 761 observations. Monetary values are winsorized at 1% and expressed in USD.

Table 3.2: Summary Statistics and Randomization Check (Part 2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample				HFPS sample only			
		Regression Coefficient for:				Regression Coefficient for:		
	Control Mean	One Box	Multiple Boxes	Mobile Money	Control Mean	One Box	Multiple Boxes	Mobile Money
Pavel C: Savings								
Number of savings places	2.54	0.07	0.008	-0.004	2.54	0.105	-0.067	-0.033
	(1.17)	(0.17)	(0.16)	(0.15)	(1.19)	(0.24)	(0.24)	(0.21)
Total Cash Savings	143.10	-7.264	-15.098	-5.293	164.30	7.845	19.001	9.11
	(400.90)	(28.80)	(28.99)	(26.42)	(529.00)	(31.65)	(39.03)	(30.48)
=1 if has separate savings	0.65	-0.009	0.024	-0.042	0.60	0.028	0.074	-0.014
places for different purposes		(0.07)	(0.08)	(0.06)		(0.10)	(0.09)	(0.08)
Saves in:								
Mobile money	0.31	0.029	0.112	0.052	0.31	-0.014	0.022	0.043
		(0.06)	(0.08)	(0.06)		(0.08)	(0.09)	(0.08)
Bank account	0.29	0.025	0.11	0.021	0.31	0.048	0.083	0.034
		(0.07)	(0.08)	(0.06)		(0.09)	(0.09)	(0.07)
VSLA / ROSCA	0.52	0.044	-0.054	-0.015	0.49	0.095	-0.005	-0.001
		(0.07)	(0.09)	(0.07)		(0.11)	(0.10)	(0.09)
Secret place at home	0.82	-0.032	0.026	0.01	0.80	0.01	-0.024	0.033
		(0.06)	(0.06)	(0.05)		(0.09)	(0.09)	(0.07)
Savings box	0.22	0.008	-0.093*	-0.046	0.29	-0.076	-0.154*	-0.137*
		(0.06)	(0.05)	(0.05)		(0.09)	(0.08)	(0.08)

Note: Note: Means are population weighted. Randomization check is performed only for businesses that appear in the analysis sample (i.e. completed a monitoring survey or at least one phone survey). There are 761 observations. Monetary values are winsorized at 1% and expressed in USD.

Table 3.3: Take-up of project accounts

	(1)	(2)	(3)
	Take-up by December 2017		
	Boxes	Mobile Money	Difference between (1) and (2)
Opened account / accepted box	0.99	0.99	
Opened more than 1 account if offered	0.76	0.29	
Panel A. Take-up by December 20171			
Ever used account	0.97	0.65	0.32***
			(0.03)
Made at least 2 deposits	0.95	0.53	0.43***
			(0.03)
Total value of deposits	130.73	44.74	85.98***
	(149.29)	(73.57)	(9.60)
	[78.57]	[10.00]	
Number of deposits	65.41	5.75	59.66***
	(91.09)	(9.31)	(5.13)
	[40.00]	[2.00]	
Total value of withdrawals	96.71	44.53	52.17***
	(127.50)	(76.10)	(8.65)
	[50.00]	[7.43]	
Number of withdrawals	11.38	5.38	6.00***
	(27.83)	(8.84)	(1.66)
	[3.00]	[2.00]	
Had a non-zero balance	0.53	-	-
		-	-
Balance	13.77	-	-
	(25.74)	-	-
	[1.43]	-	-
Observations	241	320	

Note: Figures come from administrative data from telecom company for mobile money accounts, and from in-person visits for the lockbox (which occurred in December 2017). Means are presented, with standard deviations in parentheses and medians in square brackets. All monetary values are expressed in USD and winsorized at 5% level. 1Figures only available from mobile money group.

Table 3.4: Determinants of Account Usage

	(1)	(2)	(3)	(4)
	Mobile money		Boxes	
	Used account	Total deposits (USD)	Used account	Total deposits (USD)
Multiple accounts	-0.10**	10.44	0.01	31.94*
	(0.05)	(15.64)	(0.02)	(18.19)
Other Covariates				
Female	0.02	29.32	-0.05	-81.84**
	(0.11)	(34.36)	(0.05)	(40.15)
Married	0.00	13.23	-0.04	-48.10
	(0.09)	(29.58)	(0.04)	(34.19)
Female * married	0.04	-38.69	0.07	45.83
	(0.13)	(40.43)	(0.06)	(44.78)
Age (tens of years)	0.00	-16.11*	0.01	10.35
	(0.03)	(8.95)	(0.01)	(10.61)
Years of education	0.02	-1.89	0.00	7.42*
	(0.01)	(3.29)	(0.01)	(3.78)
Has kids under 18	0.1	39.61	0.02	21.78
	(0.09)	(27.39)	(0.04)	(32.82)
Distance to nearest agent (hours)	-0.17	-67.85*	-0.05*	10.21
	(0.11)	(35.35)	(0.03)	(22.56)
Had savings box at baseline	0.03	-42.94*	-0.04	-1.89
	(0.07)	(22.03)	(0.03)	(25.72)
Had bank account at baseline	-0.01	33.79*	0.03	25.4
	(0.06)	(19.47)	(0.03)	(22.86)
Had mobile money account	0.19***	48.15***	-0.02	36.43*
at baseline	(0.05)	(16.39)	(0.02)	(19.13)
Inverse hyperbolic sine of	0.02	7.06	0.00	5.11
baseline monetary savings	(0.01)	(4.54)	(0.01)	(5.62)
Log assets	-0.01	0.75	0.00	13.63**
	(0.02)	(5.28)	(0.01)	(5.79)
"Taxed" (i.e. gives money but does	0.02	-9.46	0.05*	51.04**
not receive)	(0.05)	(16.54)	(0.03)	(19.99)
Observations	317	317	238	238
Mean of dependent variable	0.79	87.78	0.97	112.20
SD of dependent variable	0.41	122.80	0.18	135.40

Note: Figures come from administrative data from telecom company for mobile money accounts, and from in-person visits for the lockbox (which occurred in December 2017). Monetary values are winsorized at 5%. Standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

Table 3.5: Usage of Boxes and Mobile Money Accounts (Multiple Groups) 4 months after account opening

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Boxes				Mobile Money			
		Group offered 2 or more boxes				Group offered 2 or more accounts		
	Group offered 1 box	Usage of dominant box	Usage of other boxes	Difference between (1) and (2)	Group offered 1 MM	Usage of dominant MM	Usage of other MM	Difference between (5) and (6)
Ever used account	1.00	1.00	0.66		0.71	0.60	0.06	
Total value of	151.40	132.60	46.86	-18.80	49.48	45.10	2.49	-4.38
deposits	(408.14)	(220.76)	(136.33)	(42.90)	(85.42)	(91.06)	(12.11)	(9.87)
	[62.50]	[62.50]	[15.83]		[14.57]	[3.61]	[0.00]	
Number of	61.28	38.56	34.07	-22.72**	5.99	5.14	0.38	(0.85)
deposits	(109.22)	(35.96)	(49.26)	(10.64)	(8.73)	(9.51)	(1.88)	(1.02)
	[30.00]	[25.00]	[14.50]		[3.00]	[1.00]	[0.00]	
Total value of	94.03	106.44	39.32	12.41	49.15	43.72	3.46	-5.43
withdrawals	(144.22)	(199.49)	(135.87)	(23.69)	(81.54)	(93.38)	(19.95)	(9.80)
	[41.67]	[41.67]	[6.94]		[12.89]	[2.43]	[0.00]	
Number of	9.47	6.87	6.78	-2.60	5.66	4.63	0.47	-1.04
withdrawals	(31.71)	(14.59)	(16.79)	(3.24)	(8.22)	(8.70)	(2.25)	(0.95)
	[3.00]	[3.00]	[1.00]		[2.00]	[1.00]	[0.00]	
Balance	21.94	16.80	3.64	-5.14	-6.14	0.11	0.11	6.25
	(47.08)	(50.59)	(7.80)	(7.16)	(56.00)	(62.23)	(62.23)	(6.62)
	[4.17]	[1.25]	[0.00]		[0.00]	[0.00]	[0.00]	
Observations	118		117		160		160	

Note: Figures come from in-person visits for the lockbox (which occurred in December 2017) and administrative transactional data (usage through December 2017). Means are presented in Columns 1-2, with standard deviations in parentheses and medians in square brackets. Standard errors in parentheses in Column 3. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

Table 3.6: Treatment Effects on Deposits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	First Stage		Deposits into project accounts (USD)1		Deposits into other savings sources (USD)				
	Made at least 1 deposit into any Box	MM	Deposits into project Lockboxes	Deposits into project MM	Cash at Home	Other Boxes	Other MM	Other Sources	Total Deposits
Panel A. High frequency surveys									
One box	0.352***	0.045	5.038***	-	-3.592***	-1.993***	-0.641	0.466	-1.352
	(0.07)	(0.09)	(0.81)	-	(1.19)	(0.40)	(0.40)	(1.45)	(2.60)
Multiple boxes	0.366***	-0.022	11.038***	-	-3.807***	-2.091***	-0.681	0.056	4.876*
	(0.07)	(0.10)	(1.93)	-	(1.27)	(0.42)	(0.42)	(1.25)	(2.79)
Mobile money	-0.182**	0.370***	-	2.529***	0.31	-0.809*	-1.083***	-0.60	-0.66
	(0.08)	(0.08)	-	(0.37)	(1.47)	(0.46)	(0.32)	(0.89)	(2.12)
Observations	4516	4516	1775	1852	4513	4514	4514	4516	4516
Number of Businesses	389	389	156	156	389	389	389	389	389
Control Mean	0.59	0.55	0.00	0.00	7.19	2.29	1.66	5.87	16.97
Control SD	0.49	0.50	-	-	12.33	5.04	4.74	20.96	27.30
p-values for F-tests of:									
1 Box = Multiple box	0.70	0.48	-	-	0.84	0.61	0.91	0.81	0.05*
1 Box = Mobile money	0.001***	0.001***	-	-	0.001***	0.001***	0.05**	0.43	0.77
Multiple box = Mobile money	0.001***	0.001***	-	-	0.003***	0.001***	0.17	0.57	0.04**

Note: All regressions control for strata, date fixed effects and baseline controls (see in the text for details). Standard errors clustered at individual level in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. All monetary variables are in USD. Figures in Columns 3-4 are not regression coefficients but are means and standard deviations for the appropriate treatment group only. Other savings include savings in VSLAs, ROSCAs, banks, and other miscellaneous savings places. Deposits are measured over the 7 days prior to the survey, and winsorized at 1%. All regressions include the mean of the dependent variable during the intake survey - see text for details.

Table 3.7: Treatment effects on business outcomes and labor supply

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
	Main Business					Farming		Secondary occupation		
	=1 if Worked	Hours	Revenue	Profits	Inventory Investment	=1 if Farmed	Hours	=1 if Worked	Hours	Income
One box	-0.086***	-0.950**	-0.42	-0.33	1.01	0.02	0.338**	0.05	1.14	0.34
	(0.03)	(0.47)	(0.88)	(0.27)	(1.54)	(0.02)	(0.17)	(0.04)	(1.32)	(1.01)
Multiple boxes	-0.01	-0.07	-0.92	-0.39	0.49	0.080***	0.648***	0.06	3.56	0.35
	(0.02)	(0.41)	(0.99)	(0.29)	(1.08)	(0.03)	(0.24)	(0.05)	(2.32)	(1.03)
Mobile money	-0.066**	-0.55	0.11	-0.451*	1.10	0.035**	0.338***	0.05	-0.11	-0.23
	(0.03)	(0.45)	(0.89)	(0.24)	(0.86)	(0.02)	(0.12)	(0.04)	(0.75)	(0.78)
Observations	31,282	31,281	26,101	25,931	26,098	4,513	4,513	4,516	4,516	4,516
Number of Businesses	390	390	390	390	390	389	389	389	389	389
Control Mean	0.83	8.21	11.86	3.37	7.48	0.06	0.20	0.18	1.96	3.08
Control SD	-	4.67	15.76	3.59	15.92	-	1.20	-	9.11	11.31
p-values for F-tests of:										
1 Box = Multiple box	0.01**	0.08*	0.64	0.83	0.76	0.02**	0.23	0.81	0.28	0.99
1 Box = Mobile money	0.52	0.41	0.54	0.61	0.95	0.40	1.00	0.85	0.25	0.45
Multiple box = Mobile money	0.05**	0.28	0.34	0.83	0.57	0.06*	0.15	0.92	0.1*	0.44

Note: All outcomes are at the daily level, except for farming, secondary occupations (which are measure at the weekly level). All regressions include the mean of the dependent variable during the intake survey - see text for details. All regressions control for strata, date fixed effects and baseline controls (see in the text for details). Standard errors clustered at individual level in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. All monetary variables are expressed in USD. Farming hours and hours in secondary occupations are measured at the weekly level.

Table 3.8: Treatment effects on expenditures (measured only in monitoring surveys)

	(1) Staple Foords	(2) Personal Expenses	(3) Airtime	(4) Household Expenses	(5) Education	(6) All Other Expenses	(7) Total
Panel A. High frequency surveys							
One box	0.915	0.842*	0.169	0.355	0.811**	1.753*	4.684*
	(0.87)	(0.48)	(0.13)	(0.56)	(0.32)	(1.04)	(2.46)
Multiple boxes	0.036	0.267	0.065	0.716	0.919**	1.273	3.263
	(0.87)	(0.54)	(0.13)	(0.69)	(0.39)	(0.83)	(2.53)
Mobile money	0.038	0.348	0.11	0.463	0.366	0.644	1.733
	(0.83)	(0.42)	(0.11)	(0.47)	(0.30)	(0.62)	(1.98)
Observations	4,502	4,516	4,512	4,516	4,516	4,516	4,516
Number of businesses	389	389	389	389	389	389	389
Control Mean	10.28	3.46	0.83	3.66	1.57	6.12	25.90
Control SD	6.26	4.02	1.03	6.29	4.12	8.77	18.01
p-values for F-tests of:							
1 Box = Multiple box	0.28	0.28	0.44	0.63	0.81	0.67	0.62
1 Box = Mobile money	0.21	0.22	0.62	0.84	0.20	0.24	0.21
Multiple box = Mobile money	1.00	0.87	0.70	0.70	0.19	0.39	0.53

Note: All variables are measured over the prior 7 days. All regressions control for strata, date fixed effects and baseline controls (see in the text for details). Standard errors clustered at individual level in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. All variables are expressed in USD and winsorized at 5%.

Table 3.9: Treatment effects on expenditures (measured only in monitoring surveys)

	(1) Holiday Spending	(2) Farm Inputs	(3) Durable Goods	(4) =1 if Bought Land	(5) =1 if Rented Land
Panel A. Monitoring Surveys					
One box	1.88	7.935*	0.09	0.02	0.104**
	(1.35)	(4.48)	(12.76)	(0.02)	(0.05)
Multiple boxes	2.318*	8.28	9.27	0.069**	0.105**
	(1.39)	(5.16)	(13.29)	(0.03)	(0.05)
Mobile money	3.516***	4.44	7.31	0.040*	0.06
	(1.08)	(3.88)	(12.85)	(0.02)	(0.04)
Observations	1308	1287	1308	636	637
Number of businesses	719	715	719	636	637
Control Mean	8.30	27.52	44.35	0.02	0.05
Control SD	13.94	40.47	110.40	0.13	0.21
p-values for F-tests of:					
1 Box = Multiple box	0.78	0.94	0.43	0.09*	0.99
1 Box = Mobile money	0.22	0.33	0.51	0.43	0.29
Multiple box = Mobile money	0.35	0.37	0.86	0.27	0.27

Note: Expenditures are cumulative for the past months (July-January for Monitoring Survey 1 and January-April for Monitoring Survey 2). Conditional on being positive, the average amount spent on land is $386 (mean) and $250 (median); while the amount spent on rent per month is $35 (mean) and $2.3 (median). All regressions control for strata, date fixed effects and baseline controls (see in the text for details). Standard errors clustered at individual level in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. All variables are expressed in USD and winsorized at 5%.

Table 3.10: Treatment effects on loans and transfers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Transfers to / from friends & family				Transfers to / from spouse				Digital credit		Other sources of credit	
	Received Transfer	Amt.	Gave Transfer	Amt.	Received Transfer	Amt.	Gave Transfer	Amt.	Received Loan	Amt.	Received Loan	Amt.
Panel A. High frequency surveys												
One box	0.07	0.19	0.108*	1.19	0.00	-0.66	-0.02	0.10	0.03	0.03	0.02	1.49
	(0.05)	(0.79)	(0.06)	(0.78)	(0.04)	(0.60)	(0.06)	(0.56)	(0.04)	(0.03)	(0.02)	(1.27)
Multiple boxes	-0.01	-0.35	0.135***	0.83	0.00	-0.07	-0.01	-0.21	0.05	0.03	0.00	-0.05
	(0.04)	(0.69)	(0.05)	(0.63)	(0.04)	(0.59)	(0.06)	(0.55)	(0.04)	(0.02)	(0.02)	(0.74)
Mobile money	0.05	-0.02	0.126***	0.78	0.00	-0.20	0.02	-0.02	0.05	0.02	-0.01	-0.36
	(0.04)	(0.59)	(0.05)	(0.49)	(0.04)	(0.69)	(0.05)	(0.39)	(0.04)	(0.02)	(0.02)	(0.53)
Observations	4,516	4,516	4,516	4,516	4,516	4,185	4,516	4,184	4,516	4,516	4,516	4,516
Number of Businesses	389	389	389	389	389	386	389	386	389	389	389	389
Control mean	0.246	2.967	0.456	2.951	0.291	2.761	0.401	2.216	0.293	0.13	0.0523	1.655
Control sd	0.431	10.48	0.498	6.327	0.455	7.223	0.49	4.414	0.456	0.293	0.223	10.91
p-values for F-tests of:												
1 Box = Multiple box	0.05*	0.52	0.61	0.67	0.91	0.33	0.97	0.60	0.52	0.78	0.36	0.24
1 Box = Mobile money	0.55	0.79	0.73	0.57	0.96	0.48	0.51	0.79	0.54	0.66	0.19	0.13
Multiple box = Mobile money	0.08*	0.58	0.83	0.92	0.86	0.86	0.53	0.67	0.89	0.85	0.75	0.63

Note: All regressions control for strata and date fixed effects, and for the mean of the dependent variable (measured during the intake survey - see text for details). Standard errors clustered at individual level in parentheses. Transfers include gifts and loans, and both cash and in-kind payments. All monetary values in USD. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Other credit includes loans from VSLAs, ROSCAs, banks, microfinance institutions, and moneylenders.

Table 3.11: Coping With Health Shocks (Part 1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Conditional on sickness:		Expenditures (USD)					Withdrawals				
	=1 if treated immediately	=1 if treated fully	Medical Expenses	Staple Foods	Personal	Total	Hours Worked	Home	Boxes	Mobile Money	Other	Total
Panel A. Household member sick												
Member sick			2.87***	0.43	0.15	7.20**	-1.14	3.44	-1.13	-0.08	-1.54	1.42
			(0.40)	(0.90)	(0.45)	(3.23)	(3.65)	(2.12)	(0.91)	(0.64)	(1.35)	(3.33)
1 Box*Member sick	-0.11*	0.03	0.45	1.16	2.16**	4.37	-1.87	-2.52	4.98*	0.61	2.40	4.60
	(0.06)	(0.07)	(0.60)	(1.21)	(0.93)	(5.07)	(4.81)	(2.53)	(2.88)	(0.80)	(1.97)	(4.61)
MultipleBox*Member sick	0.00	0.11*	-0.70	1.66	1.90**	3.95	0.75	-5.15**	6.62**	-0.26	1.43	2.49
	(0.04)	(0.06)	(0.54)	(1.35)	(0.83)	(5.16)	(5.11)	(2.38)	(3.13)	(0.71)	(3.83)	(7.23)
MobileMoney*Member sick	-0.06	0.15**	-0.61	-0.21	0.92*	-0.27	-3.19	-2.67	-0.21	0.98	2.92	0.15
	(0.04)	(0.06)	(0.45)	(1.11)	(0.55)	(3.74)	(4.43)	(2.93)	(0.93)	(0.75)	(2.15)	(4.26)
Observations	606	606	4515	4501	4515	4515	4510	4510	4510	4510	4510	4510
Number of businesses	258	258	389	389	389	389	389	389	389	389	389	389
Control Mean	0.95	0.74	0.38	10.08	3.42	24.63	56.62	4.85	0.92	1.04	5.46	12.18
Control SD	0.23	0.44	1.30	6.08	4.03	16.38	22.92	12.93	5.01	4.31	54.65	56.85
p-values for F-tests of:												
1 Box*sick = Multiple box*sick	0.03**	0.21	0.05*	0.70	0.82	0.94	0.58	0.11	0.70	0.14	0.82	0.78
1 Box*sick = Mobile money*sick	0.36	0.04**	0.04**	0.18	0.16	0.26	0.74	0.95	0.07*	0.54	0.79	0.26
Multiple box*sick = Mobile money*sick	0.22	0.44	0.83	0.12	0.22	0.35	0.36	0.31	0.03**	0.02**	0.72	0.74

Note: Standard errors clustered at individual level in parentheses. Transfers include gifts and loans, and both cash and in-kind payments. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

Table 3.12: Coping With Health Shocks (Part 2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Conditional on sickness:		Expenditures (USD)					Withdrawals				
	=1 if treated immediately	=1 if treated fully	Medical Expenses	Staple Foods	Personal	Total	Hours Worked	Home	Boxes	Mobile Money	Other	Total
Panel B. Respondent sick												
Respondent sick			2.08***	-0.21	0.08	1.49	1.17	1.31	-1.13	-0.51	-0.75	-0.87
			(0.47)	(0.91)	(0.62)	(1.91)	(3.76)	(1.63)	(1.08)	(0.43)	(1.21)	(2.53)
1 Box * respondent sick	0.02	-0.11	-0.78	1.14	1.33	2.74	-19.31**	-2.82	1.93	-0.58	-0.90	-2.66
	(0.07)	(0.09)	(0.58)	(1.64)	(0.92)	(3.27)	(7.95)	(3.69)	(2.00)	(0.46)	(1.94)	(5.08)
Multiple Box * respondent sick	0.06	0.05	-0.42	1.68	-0.05	4.35	-6.40	-3.61*	4.98***	-0.08	-1.71	-0.18
	(0.06)	(0.08)	(0.60)	(1.29)	(0.79)	(3.74)	(5.16)	(1.86)	(1.63)	(0.54)	(1.94)	(3.94)
Mobile Money* respondent sick	0.00	0.06	-0.25	-1.11	0.41	0.10	-10.61*	-1.55	-0.78	1.82**	-1.15	-1.29
	(0.06)	(0.08)	(0.62)	(1.28)	(0.77)	(3.08)	(5.62)	(1.77)	(1.22)	(0.71)	(1.47)	(3.16)
Observations	491	491	4515	4501	4515	4515	4510	4510	4510	4510	4510	4510
Number of businesses	247	247	389	389	389	389	389	389	389	389	389	389
Control Mean	0.83	0.78	0.50	10.26	3.50	25.71	57.06	5.21	0.95	1.13	5.44	12.65
Control SD	0.38	0.42	1.66	6.22	4.06	18.24	22.92	13.53	5.09	4.55	54.44	56.79
p-values for F-tests of:												
1 Box*sick = Multiple box*sick	0.47	0.05*	0.49	0.74	0.11	0.71	0.10	0.82	0.18	0.21	0.73	0.66
1 Box*sick = Mobile money*sick	0.83	0.02**	0.31	0.16	0.28	0.49	0.29	0.72	0.18	0.001***	0.90	0.79
Multiple box*sick = Mobile money*sick	0.25	0.85	0.76	0.02**	0.48	0.27	0.42	0.09*	0.001***	0.005***	0.77	0.77

Note: Standard errors clustered at individual level in parentheses. Transfers include gifts and loans, and both cash and in-kind payments. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

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Appendix A

Appendix Tables and Figures

Figure A.1: Illustration of a call schedule

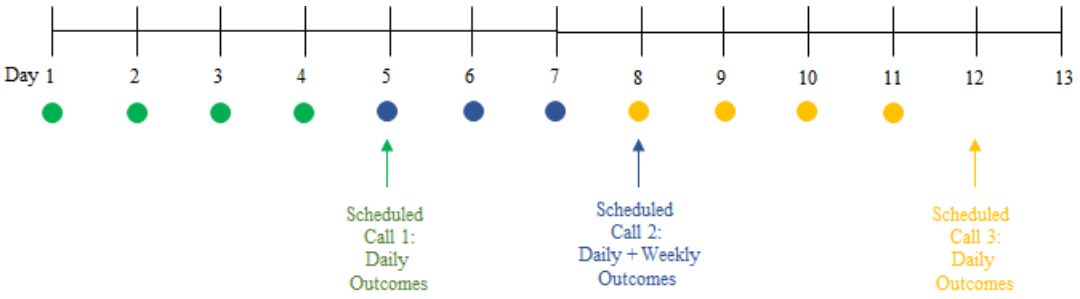


Figure A.2: Illustration of make up calls

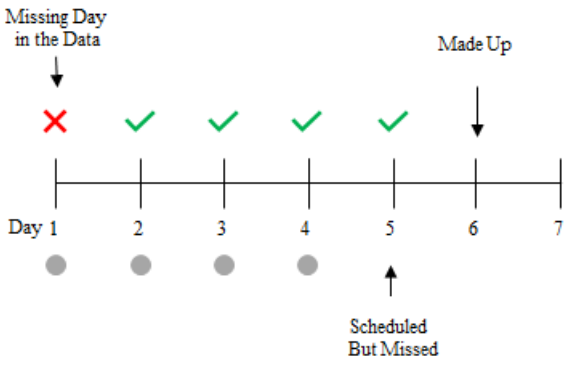


Table A.1: Census, Baseline, Phone Surveys: Summary and Attrition

	Counts	**Percentages**
Panel A: Census		
Total Business Count	9848	
Total Large Businesses not interviewed	1770	18.0%
Total Small Businesses Counted	8078	82.0%
Total Small Businesses Selected to Interview	3854	
Permanently Closed	346	
Owner/Keeper not found after 3 Attempts	51	
Opened Businesses Selected for an Interview	3457	
Business Willing to interview	3139	90.8%
Excluded based on age criteria	63	2.0%
Consented to the survey	2842	92.4%
Interviewed - eligible	1952	68.7%
Interviewed, eligible, consented	1937	99.2%
Panel B: Baseline Attrition		
Visited	602	
Refused	60	10.0%
Not-Eligible	100	16.6%
Enrolled	400	66.4%
Panel C: Phone Surveys Attrition	**Full Sample**	**Connected With Appliances**
Enrolled	400	74
Reached for at least 1 interview	391	70
Average Number of Days/Person	49.04	47.2
Median Number of Days/Person	52	52
Panel D: Missing Scheduled calls due to Outages		
Probability of Skipping a call for non-outage reasons	0.07	0.06
Skipped calls due to outages (regression coefficient)	0.01	0.02

Note: Panel A: Counts of businesses during Census. Panel B: Attrition during baseline. Panel C: Completion rates and attrition during phone surveys.

Figure A.3: Attrition: Number of Surveys Completed through Time

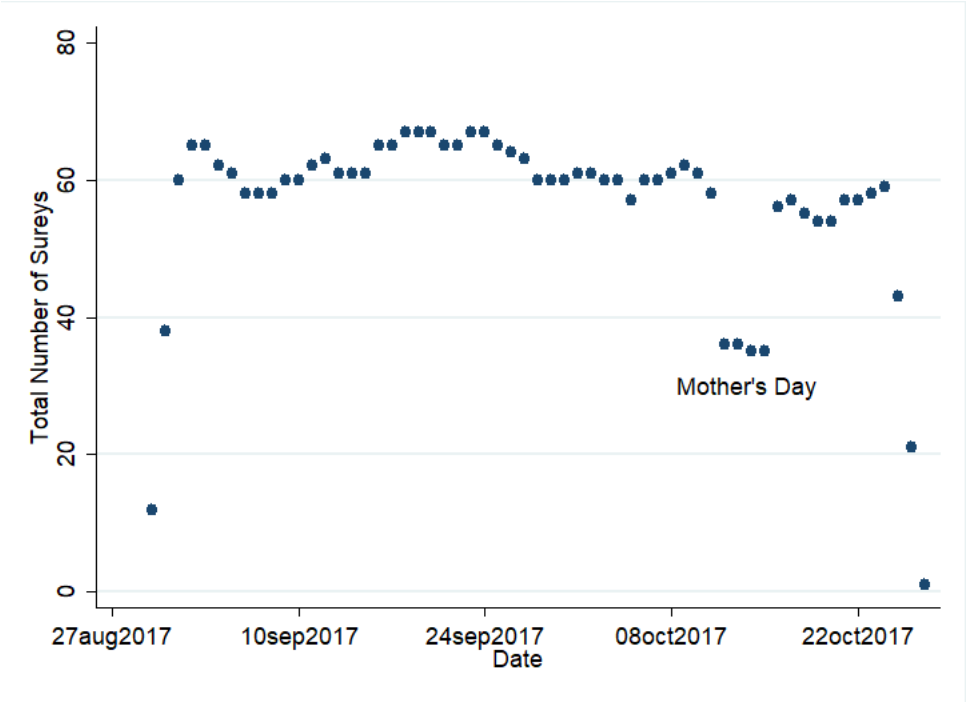


Table A.2: Sample Composition

	(1)	(2) Connected	(3) Not-Connected
	Without Appliances	**"Highly Dependent" (with Appliances)**	
	68	**74**	**258**
Panel A: Food Items Retail			
Staples	1%	0%	6%
Raw Produce	7%	0%	12%
Meat/Fish	1%	0%	5%
Prepared Food	13%	7%	22%
Other Edible	3%	1%	7%
Total	**26.5%**	**8.1%**	**53.1%**
Panel B: Non-Food Items Retail			
Charcoal	0%	0%	7%
Hardware	6%	0%	3%
Household Items	0%	0%	1%
Livestock	1%	0%	0%
Shoes	3%	0%	7%
Convinience Stores	44%	5%	11%
Electronics	0%	8%	0%
Other Non-Edible	1%	3%	6%
Total	**55.9%**	**16.2%**	**35.7%**
Panel C: Services			
Music	1%	14%	0%
Printing	0%	4%	0%
Salon	7%	45%	0%
Tailor	1%	4%	5%
Welder	1%	8%	1%
Bar/Restaurant	1%	1%	1%
Other Services	4%	0%	3%
Total	**17.6%**	**75.7%**	**11.2%**

Note: Dissagregate business categories in the sample.

Table A.3: Systematic Attrition. Number of Surveys Completed

	(1)	(2)
	Number of Surveys Completed	
	Unconditional	Conditional on at least 1 phone call
Male	-3.122**	-1.502
	(1.41)	(1.11)
Age	0.115	0.133*
	(0.09)	(0.07)
Education	-0.046	0.23
	(0.24)	(0.20)
Married	0.309	-1.676
	(1.26)	(1.02)
Uses a Bank Account	1.937	0.948
	(1.25)	(0.98)
Uses Mobile Money Account	1.141	0.163
	(1.48)	(1.20)
Total Value of Assets and Savings (Z)	0.105	0.135
	(0.42)	(0.32)
Monthly Profit	-0.021*	-0.019**
	(0.01)	(0.01)
Market Area	-0.714	-1.208
	(1.21)	(0.99)
Risk Averse (Z)	0.128	0.502
	(0.56)	(0.47)
Loss Averse (Z)	-0.912	-0.109
	(0.65)	(0.57)
Business type 2: Non-food items	4.258*	2.642
	(2.43)	(1.87)
Business type 3: Services	2.177	1.856
	(2.12)	(1.63)
Observations	74	70
Mean	0.23	0.2
Median	13.0	13.7
P-Value from F-test	15.0	0.27

Note: Dependent Variable is the number of surveys completed conditional on being reached once.

Figure A.4: Effect Sizes on Hours Worked For Various Outage Durations, "Highly-Dependent" Sub-sample

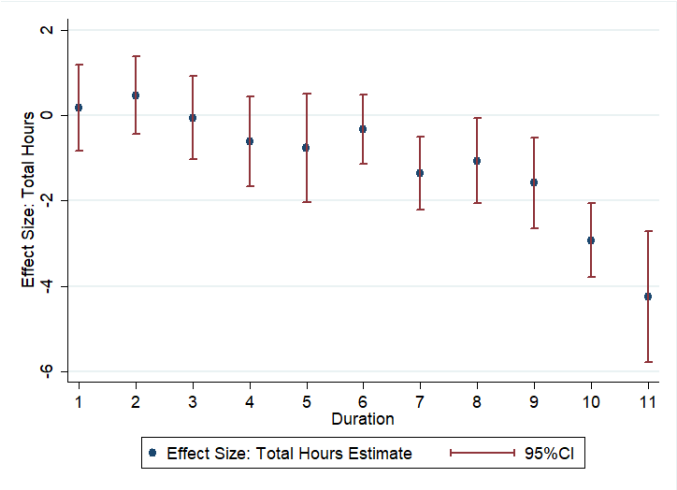
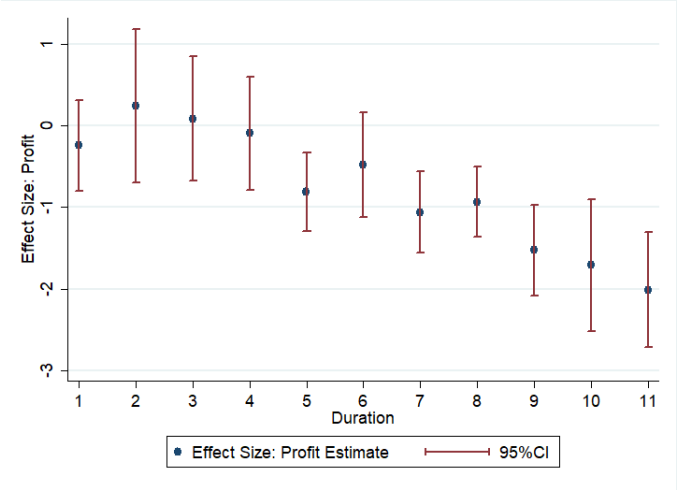


Figure A.5: Effect Sizes on Hours Worked For Various Outage Durations, "Highly-Dependent" Sub-sample



Note: Point estimates for effects of outages varying in intensity on hours worked and profits in the main business. All regressions include individual, date, recall day fixed effects and lottery wins as control variables. Profits are expressed in USD and are winsorized at 5% level. Standard errors are clustered at the transformer level. Red bars represent confidence intervals around the estimate.

Table A.4: Effects of Outages Conditional on Working, "Highly-Dependent" Sub-sample

	(1) Hours	(2) Customers	(3) Revenue	(4) Profit
Outage	-1.101***	-2.214***	-1.557***	-1.068***
	(0.32)	(0.43)	(0.53)	(0.19)
Observations	2681	2714	2719	2714
R-squared	0.046	0.064	0.047	0.061
# of Businesses	70	69	70	70
Control Mean	10.76	21.45	19.91	5.494
Control SD	3.484	19.48	22.47	4.314

Note: Independent variable is a binary indicator of whether there was an outage on a particular day. All regressions include individual, date, recall day fixed effects and lottery wins as control variables. Monetary outcomes are expressed in USD. Monetary outcomes and customers and are winsorized at 5% level. Standard errors are clustered at the transformer level.

Figure A.6: Distribution of Electricity Connected businesses by Business Type At Census

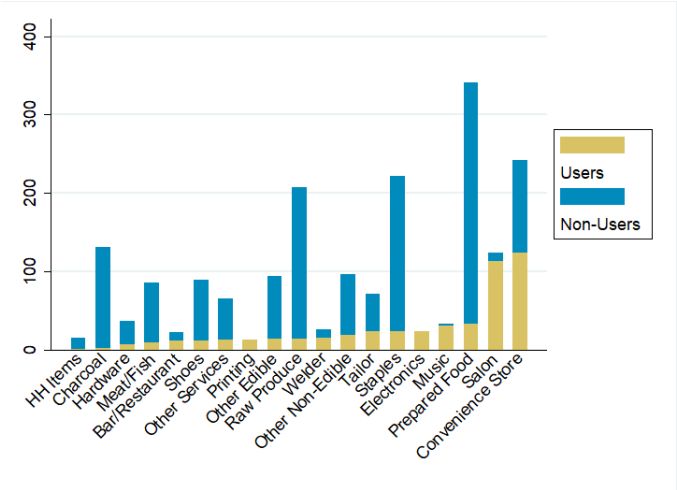


Table A.5: Effects of Outages on Business with Lagged Terms, "Highly-Dependent" Subsample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Worked Today	Work Hours	Num Customers	Main Revenues	Main Profits	2nd Income	Total Income
Outage	-0.096***	-1.817***	-2.699***	-1.508**	-1.145***	0.057	-1.030***
	(0.03)	(0.40)	(0.67)	(0.56)	(0.21)	(0.07)	(0.22)
Outage Yesterday	0.022	0.151	0.7	0.471	0.175	-0.025	0.19
	(0.02)	(0.25)	(0.50)	(0.48)	(0.18)	(0.09)	(0.19)
Outage 2 Days Ago	-0.013	-0.443**	-0.578	-0.595	0.019	-0.004	0.015
	(0.02)	(0.20)	(0.49)	(0.37)	(0.16)	(0.06)	(0.17)
Outage 3 Days Ago	0.009	0.033	-0.022	-0.731	0.026	0.084	0.096
	(0.02)	(0.22)	(0.41)	(0.67)	(0.20)	(0.08)	(0.21)
Outage 4 Days Ago	0.034**	0.331**	0.071	-0.058	0.199	-0.003	0.303**
	(0.02)	(0.15)	(0.29)	(0.42)	(0.14)	(0.08)	(0.14)
Outage 5 Days Ago	0.041**	0.215	-0.067	0.206	0.08	0.036	0.147
	(0.02)	(0.20)	(0.42)	(0.46)	(0.18)	(0.12)	(0.21)
Outage 6 Days Ago	0.012	0.225	-0.413	-1.830**	-0.443*	-0.076	-0.375
	(0.02)	(0.23)	(0.53)	(0.72)	(0.24)	(0.10)	(0.24)
Observations	3,232	3,232	3,225	3,229	3,224	3,229	3,232
R-squared	0.085	0.09	0.049	0.042	0.056	0.026	0.05
# of Businesses	70	70	70	70	70	70	70
Non-Outage Day Mean	0.866	8.663	11.36	11.95	4.575	0.328	4.783
Non-Outage Day SD	0.341	4.515	14	17.85	4.078	1.967	4.192
Pval F-Test: sum of B2-B7=0	0.0278	0.327	0.816	0.139	0.904	0.969	0.438

Note: Independent variable is a binary indicator of whether there was an outage on a particular day, yesterday and 5 days before yesteday. All regressions include individual, date, recall day fixed effects, lottery wins as control variables and controls for missing days. Monetary outcomes are expressed in USD. Monetary outcomes and customers and are winsorized at 5% level. Standard errors are clustered at the transformer level.

Table A.6: Effects of Not Having an Outage Today (good day) and having an outage yesterday (bad day), "Highly-Dependent" Sub-sample

	Labor Supply			Income				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Extensive Margin							
	Worked Today	Worked Second Occupation	Total Work Hours	Primary Busines Customers	Revenue	Profit	Secondary Occupation Profit	Total Income
No Outage Today	0.110***	-0.014	1.677***	2.492***	0.919	1.215***	-0.093	1.069***
	(0.03)	(0.01)	(0.40)	(0.58)	(0.73)	(0.28)	(0.13)	(0.31)
Outage Yesterday	0.032	0.018	0.102	0.601	0.083	0.214	-0.061	0.194
	(0.03)	(0.01)	(0.28)	(0.44)	(0.44)	(0.16)	(0.11)	(0.17)
No Outage Today*	-0.028	-0.006	0.192	0.321	0.975	-0.134	0.069	-0.074
Outage Yesterday	(0.03)	(0.02)	(0.34)	(0.52)	(0.93)	(0.30)	(0.16)	(0.32)
Observations	3,232	3,232	3,232	3,225	3,229	3,224	3,229	3,232
R-squared	0.079	0.026	0.085	0.047	0.035	0.05	0.023	0.044
# of Businesses	70	70	70	70	70	70	70	70
Non-Outage Mean	0.866	0.0549	8.663	11.36	11.95	4.575	0.328	4.78
Non-Outage SD	0.341	0.228	4.515	14	17.85	4.078	1.967	4.192

Note: Independent variables are: a binary indicator of whether there no outage today (good day), and there was an outage yesterday (bad day), yesterday, and the interaction term of non-outage today and outage yesterday. All regressions include individual, date, recall day fixed effects, lottery wins as control variables and controls for missing days. Monetary outcomes are expressed in USD. Monetary outcomes and customers and are winsorized at 5% level. Standard errors are clustered at the transformer level.

Table A.7: Effects of Number of Outages per Week on Business Outcomes, "Highly-Dependent" Sub-sample

	(1) Work Hrs	(2) Number of Customers	(3) Main Revenue	(4) Main Profit	(5) Total Income	(6) Inventory
# of Outages a week	-0.693	-0.63	-3.603**	-0.993**	-1.118	-3.784
	(0.63)	(1.63)	(1.64)	(0.42)	(1.07)	(2.40)
Observations	473	473	473	473	473	473
R-squared	0.366	0.232	0.204	0.258	0.115	0.147
# of Businesses	70	70	70	70	70	70
Dep Var Mean	47.57	58.34	53.74	21.45	28.18	19.39
Dep Var SD	20.27	75.29	74.06	16.56	38.83	51.74
Num Outage per Week	4.531	4.531	4.531	4.531	4.531	4.531
Num Outage per Week SD	2.006	2.006	2.006	2.006	2.006	2.006

Note: Independent variables are: number of outages per week (aggregated from individual reports), aggregated lottery payment wins for the week, and a number of non-missing days in the week. Dependent variables are aggregated to individual week level. Individual and call date fixed effects are included. Errors are clustered at the transformer level. Monetary outcomes are expressed in USD and are winsorized at 5% level.

Table A.8: Effects of Number of Outages for 2 weeks on Business Outcomes, "Highly-Dependent" Sub-sample

	(1) Work Hrs	(2) Number of Customers	(3) Main Revenue	(4) Main Profit	(5) Total Income	(6) Inventory
# of Outages per 2 weeks	-0.87	1.922	-1.591	-1.260*	-2.036	-2.141
	(0.99)	(1.87)	(1.35)	(0.68)	(2.10)	(2.62)
Observations	252	252	252	252	252	252
R-squared	0.718	0.519	0.634	0.472	0.205	0.395
# of Businesses	70	70	70	70	70	70
Dep Var Mean	89.12	109.3	100.7	40.19	52.79	36.33
Dep Var SD	39.73	134.5	131.8	29.48	66.35	82.53
Num Outage per Week	8.504	8.504	8.504	8.504	8.504	8.504
Num Outage per Week SD	3.752	3.752	3.752	3.752	3.752	3.752

Note: Independent variables are: number of outages per week (aggregated from individual reports), aggregated lottery payment wins for the week, and a number of non-missing days in the week. Dependent variables are aggregated to individual week level. Individual and call date fixed effects are included. Errors are clustered at the transformer level. Monetary outcomes are expressed in USD and are winsorized at 5% level.

Table A.9: Robustness Checks. Dependent Variable - Daily Profits

	(1)	(2)	(3)	(4)	(5)
	Not	Transformer Level			Neighbor ¡1 km
	Anticipated Outages Only	% Reports 50%	% Reports 65%	All but 1 or 2 Reports	IV
Outage	-0.800***	-0.820***	-0.823***	-1.055***	-0.697
	(0.19)	(0.24)	(0.23)	(0.23)	(1.27)
Observations	1,670	3,239	3,239	3,283	2,603
R-squared	0.058	0.039	0.039	0.046	0
# of Businesses	70	70	70	70	67
Non-Outage Mean	4.575	4.331	4.306	4.307	4.681
Non-Outage SD	4.078	4.085	4.069	4.071	4.118
First Stage					0.166
First Stage Tstat					4.564
# Business/Transformer		2.91	2.91	2.91	

Note: Dependent variable is profit expressed in USD, winsorized at 5% level. All regressions include individual, date and recall day fixed effects. Sub-sample of only unanticipated outages (column 1); definitions of outages at the transformer level (columns 3 through 4); Neighbors' reports are used as an instrumental variable (column 6); Sub-Sample of respondents who were affected by outages on less than 70% of the days (Column 6)

Table A.10: Evidence of Recall Bias

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Work	Number of			Outages		
	Hours	Customers	Revenues	Profits	Binary	Number	Duration
Recall Day 2	0.087	0.208	-0.033	-0.02	-0.018	-0.048*	-0.365
	(0.19)	(0.36)	(0.45)	(0.14)	(0.02)	(0.03)	(0.25)
Recall Day 3	0.146	0.544	0.451	-0.02	-0.033*	-0.069***	-0.626**
	(0.19)	(0.37)	(0.45)	(0.14)	(0.02)	(0.03)	(0.25)
Recall Day 4	0.163	0.731	1.003*	0.115	-0.025	-0.085***	-0.386
	(0.24)	(0.45)	(0.55)	(0.18)	(0.02)	(0.03)	(0.31)
Observations	3,307	3,299	3,303	3,298	3,232	3,227	3,209
R-squared	0.237	0.678	0.563	0.338	0.2	0.148	0.173
Yesterday (Day 1) Mean	8.109	16.88	13.11	3.149	0.74	0.806	7.596

Note: Monetary outcomes and customers and are winsorized at 5% level. Standard errors are clustered at the transformer level.

Table A.11: Sub-sample with only Yesterday's Values (Recall day=1)

	(1) Hours Total	(2) Number of Customers	(3) Main Revenue	(4) Main Profit	(5) 2nd Income	(6) Total Income
Outage	-1.405***	-1.81	-1.283	-1.380***	-0.132	-1.341***
	(0.41)	(1.14)	(1.27)	(0.32)	(0.21)	(0.33)
Observations	936	935	936	935	935	936
R-squared	0.137	0.105	0.069	0.105	0.054	0.099
# of Businesses	70	70	70	70	70	70
Non-Outage Mean	8.663	11.36	11.95	4.575	0.328	4.783
Non-Outage SD	4.515	14	17.85	4.078	1.967	4.192

Note: Sub-sample with values from yesterday only. Independent variable is a binary indicator of whether there was an outage on that day. All regressions include individual, date, recall day fixed effects and lottery wins as control variables. Monetary outcomes are expressed in USD. Monetary outcomes and customers and are winsorized at 5% level. Standard errors are clustered at the transformer level.

Figure A.7: Matches Agreement Rate between neighbors located within 1 km of each other

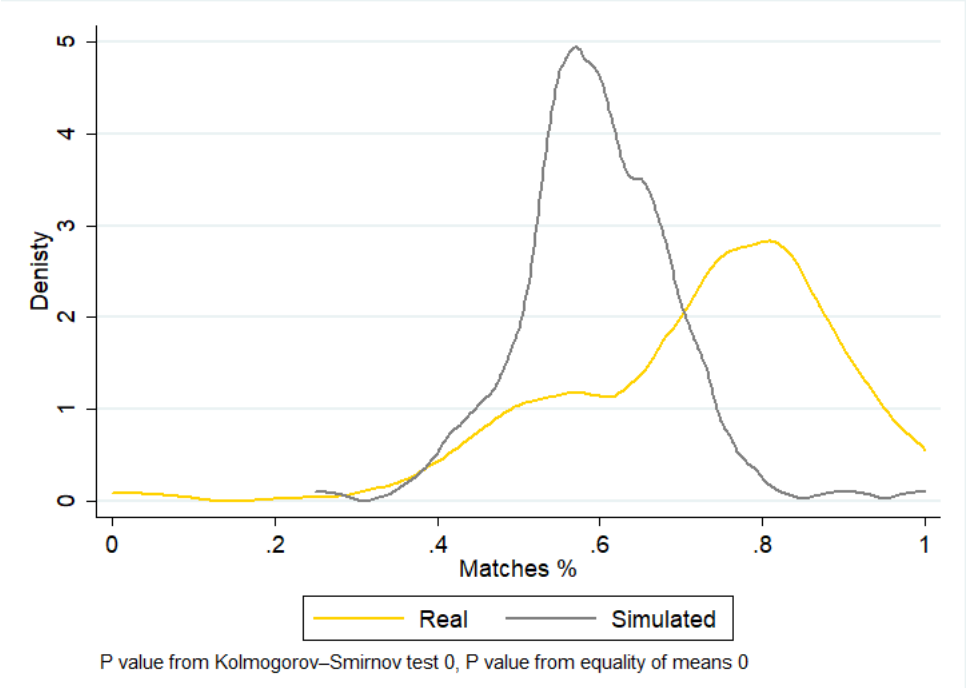


Table A.12: Differences between Businesses Connected and Not Connected to the grid (Part 1)

	(1) Not Connected	(2)	(3)	(4)	(5)
		Connected			
		No appliances		"Highly-Dependent"	
	Mean/SD	Diff.	P-value	Diff.	P-value
Demographics					
Male	0.465	0.12	0.071*	0.27	0.000***
	[0.031]		0.00		0.00
Age	34.279	1.84	0.189	-2.51	0.045**
	[0.619]		0.00		0.00
Education	8.686	0.65	0.084*	1.45	0.000***
	[0.173]		0.00		0.00
House Connected to Electricity	0.523	0.32	0.000***	0.15	0.020**
	[0.031]		0.00		0.00
Business Outcomes					
Number of Employees	0.070	0.15	0.003***	0.31	0.000***
	[0.019]		0.00		0.00
Monthly Revenue	212.220	123.06	0.000***	-76.23	0.010**
	[14.820]		0.00		0.00
Monthly Profit	54.783	33.71	0.000***	4.19	0.553
	[3.289]		0.00		0.00
Value of Inventory	95.148	242.90	0.000***	-31.61	0.445
	[21.587]		0.00		0.00
Value of Tools and Equipment	51.047	126.36	0.000***	585.47	0.005***
	[9.319]		0.00		0.00

Note: Outcome variables are expressed in USD and winsorized at 5% level.

Table A.13: Differences between Businesses Connected and Not Connected to the grid (Part 2)

	(1) Not Connected	(2)	(3)	(4)	(5)
		Connected			
		No appliances		"Highly-Dependent"	
	Mean/SD	Diff.	P-value	Diff.	P-value
Financial Access & Security					
Uses a Bank Account	0.260	0.15	0.014**	0.19	0.002***
	[0.027]		0.00		0.00
Uses Mobile Money Account	0.539	0.08	0.246	0.25	0.000***
	[0.031]		0.00		0.00
Total Value of Assets	478.470	1078.21	0.000***	721.67	0.000***
	[48.772]		0.00		0.00
Total Value of Cash Savings	100.799	60.78	0.020**	96.53	0.017**
	[11.412]		0.00		0.00
Dual Income	0.453	(0.06)	0.406	-0.14	0.028**
	[0.031]		0.00		0.00
Monthly Spouse Earnings	38.184	86.23	0.012**	37.48	0.115
	[5.019]		0.00		0.00
Has a Secondary	0.190	0.12	0.034**	0.07	0.210
Earning Activity	[0.024]		0.00		0.00
Second Earning:	7.922	30.37	0.001***	14.21	0.059*
Average Monthly Income	[2.323]				0.00
Attitutes Towards Risk					
Impatient	0.467	0.03	0.584	0.01	0.897
(1 - the most impatient)					
Loss Aversion	0.155	0.05	0.317	-0.03	0.476
(1 - the most loss averse)					
Risk Attitutes	0.523	0.08	0.100	-0.03	0.525
(1 - the most risk averse)					

Note: Outcome variables are expressed in USD and winsorized at 5% level.

Table A.14: Reasons why business owners chose electricity/non-electricity occupation

	(1) All	(2) N
Connected		
Reason why Business is connected to the grid		
Makes more Money	0.48	120
Can Afford It	0.15	120
Inherited/Has Skills Knowledge	0.10	120
Other	0.09	120
Not Connected		
Reason why Business is not connected to the grid		
Too Expensive	0.33	237
Business doesn't need it	0.35	237
Power Outages	0.09	237
No Electricity in the Area	0.05	237
Wouldn't Make More Money	0.05	237

Note: Reports from follow up surveys during the Phone Surveys. The number of observations is lower due to the fact that not all respondents were reached on the days when the questions were implemented.

Table A.15: Effects of Outages on Businesses Not Connected to Electricity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Labor Supply			Income			
	Extensive Margin			Primary Busines			
	Worked Today	Worked Second Occupation	Total Work Hours	Customers	Revenue	Profit	Secondary Occupation Profit
Panel A: Binary							
Outage	0.014	0	0.02	-0.039	-0.066	0.03	0.008
	(0.01)	(0.01)	(0.02)	(0.34)	(0.32)	(0.08)	(0.03)
Observations	12,170	12,170	3,822	12,058	12,162	12,063	12,167
# of Businesses	253	253	242	253	253	253	253
Non-Outage Mean	0.811	0.0463	0.523	16.76	13.27	2.951	0.218
Non-Outage SD	0.392	0.21	0.869	18.16	17.67	3.447	1.47
Panel B: Duration in hours							
Before 7am	-0.003	0.003**	-0.005	-0.076	0.003	-0.027	0.006
	(0.00)	(0.00)	(0.04)	(0.08)	(0.13)	(0.04)	(0.01)
7am-6pm	0.006***	-0.001	0.050***	0.068*	0.078	0.017	-0.002
	(0.00)	(0.00)	(0.01)	(0.03)	(0.06)	(0.01)	(0.01)
6pm-12am	0.005**	0.001	0.034	0.113*	0.071	0.030***	0.018*
	(0.00)	(0.00)	(0.02)	(0.07)	(0.05)	(0.01)	(0.01)
Observations	12,163	12,163	12,163	12,051	12,155	12,056	12,160
# of Businesses	253	253	253	253	253	253	253
Non-Outage Mean	0.811	0.0463	7.852	16.78	13.28	2.953	0.218
Non-Outage SD	0.392	0.21	4.624	18.16	17.68	3.448	1.471

Note: Independent variable is a binary indicator of whether there was an outage on a particular day (panel A), and broken down into duration expressed in hours before start of the work day, during the work day and after the work day (panel B). All regressions include individual, date, recall day fixed effects and lottery wins as control variables. Monetary outcomes are expressed in USD. Monetary outcomes and customers and are winsorized at 5% level. Standard errors are clustered at the transformer level.

Table A.16: Evidence of Recall Bias

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Logged					
	Total Hours	Market Revenues	Market Profits	Business Exp	Total Deposits	Total Withdrawals	Report of Outage
Panel A: For single week logbooks							
Day 6	0.044	-3.138	-0.944	-0.486	-0.069	-0.135	0.018
	(0.20)	(2.24)	(0.97)	(1.21)	(0.12)	(0.10)	(0.01)
Day 5	-0.005	-0.952	-0.289	4.304	-0.221	-0.114	0.009
	(0.20)	(2.23)	(0.84)	(3.77)	(0.19)	(0.11)	(0.02)
Day 4	0.021	0.223	0.008	-1.234	-0.41	-0.021	0.007
	(0.22)	(2.21)	(0.83)	(2.27)	(0.29)	(0.14)	(0.02)
Day 3	-0.05	-1.879	-0.633	4.465	-0.582*	0.029	-0.006
	(0.27)	(2.91)	(1.46)	(5.06)	(0.33)	(0.23)	(0.02)
Day 2	-0.075	-2.115	-0.98	-0.91	-0.554*	-0.163	-0.026
	(0.28)	(2.95)	(1.29)	(1.63)	(0.32)	(0.14)	(0.02)
Day 1	0.09	-2.76	-0.92	3.406**	1.26	0.19	0.02
	(0.32)	(2.85)	(1.09)	(1.67)	(0.99)	(0.26)	(0.02)
Obs	3655	3837	3496	3990	4329	4329	4329
R-squared	0.01	0.03	0.01	0.01	0.01	0.01	0.04
FE	Date	Date	Date	Date	Date	Date	Date

Note: The presented coefficients are relative to the omitted category - reports on Day 7 for logbooks containing spanning 1 week. All regressions include date fixed effects. Standard errors(clustered at the individual level) are in parenthesis.

Table A.17: Evidence of Recall Bias

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Logged					
	Total Hours	Market Revenues	Market Profits	Business Exp	Total Deposits	Total Withdrawals	Report of Outage
Panel B: For double-week logbooks							
Day 13	-0.439**	-2.335*	-0.817	-0.896	0.799	0.07	-0.045
	(0.19)	(1.31)	(0.50)	(1.72)	(0.76)	(0.40)	(0.03)
Day 12	-0.264	-0.917	-1.322**	5.437	0.118	-0.256	-0.047*
	(0.22)	(1.34)	(0.64)	(4.59)	(0.29)	(0.32)	(0.03)
Day 11	-0.434**	1.261	-0.422	5.117	0.837	-0.286	0
	(0.22)	(1.33)	(0.59)	(5.32)	(0.81)	(0.29)	(0.03)
Day 10	-0.258	-0.56	-0.648	1.325	0.119	-0.307	-0.056**
	(0.25)	(1.11)	(0.73)	(2.96)	(0.38)	(0.32)	(0.02)
Day 9	0.13	0.029	-0.962	3.507	1.077	-0.331	-0.034
	(0.20)	(1.37)	(0.74)	(4.05)	(1.02)	(0.28)	(0.03)
Day 8	0.168	-2.148	-0.035	-1.954	0.081	-0.421	-0.028
	(0.24)	(1.59)	(0.57)	(1.89)	(0.30)	(0.29)	(0.03)
Day 7	-0.073	0.365	-0.964	1.816	5.094	-0.249	-0.035
	(0.30)	(2.94)	(0.62)	(2.90)	(5.04)	(0.31)	(0.03)
Day 6	-0.256	1.592	0.189	0.888	-0.128	-0.28	-0.008
	(0.29)	(2.74)	(0.75)	(2.75)	(0.31)	(0.30)	(0.03)
Day 5	-0.119	-0.205	-1.159*	-0.622	1.021	-0.05	-0.016
	(0.29)	(2.24)	(0.67)	(2.73)	(0.83)	(0.46)	(0.03)
Day 4	0.134	1.43	-1.486*	2.681	0.06	-0.173	-0.047*
	(0.26)	(2.50)	(0.77)	(3.24)	(0.34)	(0.31)	(0.03)
Day 3	0.238	-1.613	-0.632	0.136	0.265	-0.267	-0.019
	(0.27)	(1.97)	(0.75)	(2.93)	(0.37)	(0.31)	(0.03)
Day 2	-0.058	-0.286	-1.114*	13.642	0.173	-0.202	-0.026
	(0.31)	(2.32)	(0.67)	(12.26)	(0.44)	(0.33)	(0.02)
Day 1	0.045	0.115	-0.929	-0.557	0.038	-0.314	-0.015
	(0.30)	(2.63)	(0.60)	(1.96)	(0.33)	(0.29)	(0.02)
Observations	3332	3495	3209	3606	3903	3903	3903
R-squared	0.01	0.01	0.01	0.01	0.01	0.01	0.06
FE	Date	Date	Date	Date	Date	Date	Date

Note: The presented coefficients are relative to the omitted category - reports on Day 14 for logbooks containing spanning 2 week. All regressions include date fixed effects. Standard errors(clustered at the individual level) are in parenthesis.

Figure A.8: Outage Instances per Market. Definition¿=1

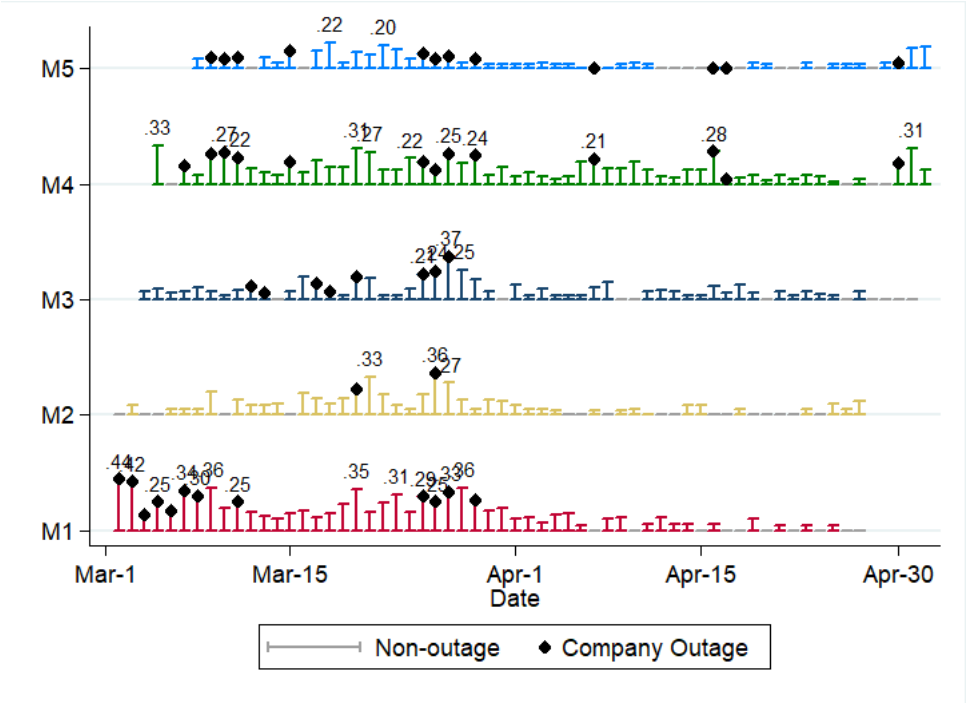


Table A.18: Correlation of Outage Instances Across Markets

	M1	M2	M3	M4	M5
M1					
M2	19%				
M3	19%	2%			
M4	33%	8%	15%		
M5	33%	18%	8%	13%	

Note: Based on Market-Level Outage Definition 2

Figure A.9: GPS Locations of Market Vendors belonging to the same Transformer Groups



(a) Market 1 (b) Market 2 (c) Market 3

(d) Market 4 (e) Market 5

Figure A.10: Experimental Design

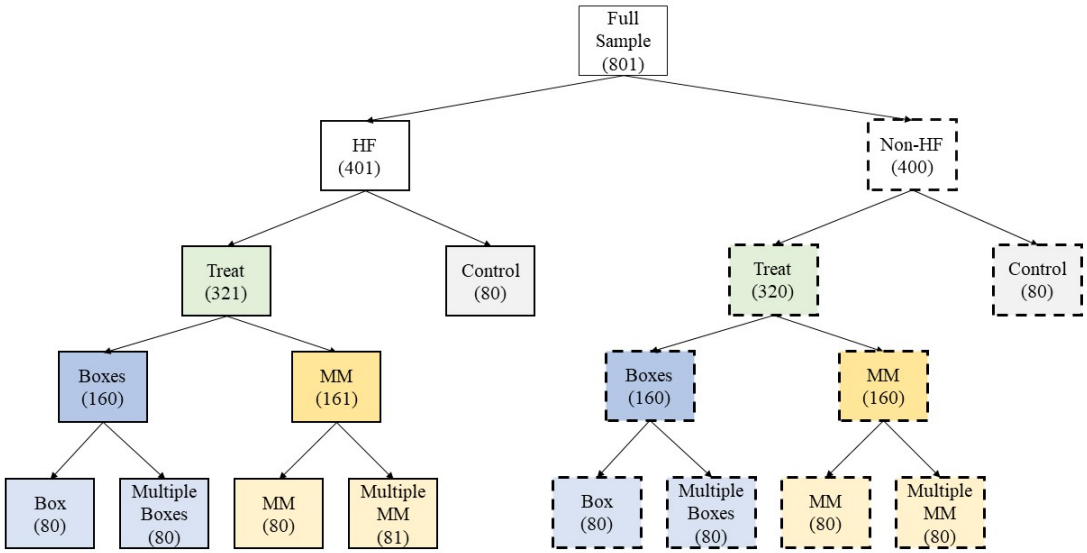
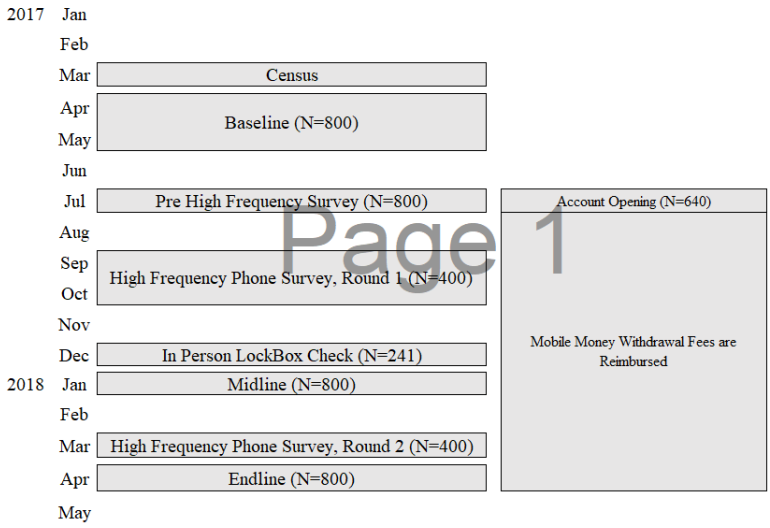


Figure A.11: Project and Survey Timeline



Page 1

Figure A.12: Map of Blantyre with Treatment Assignment

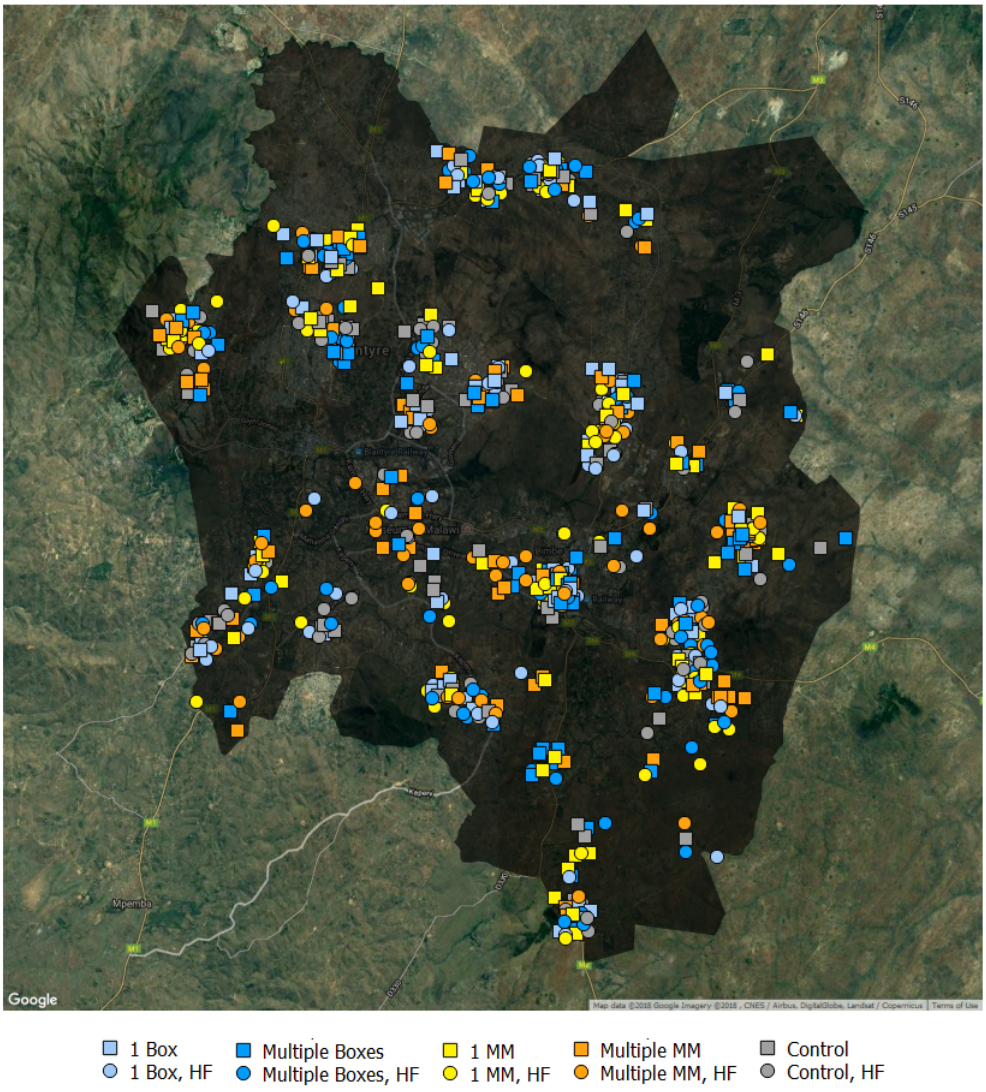
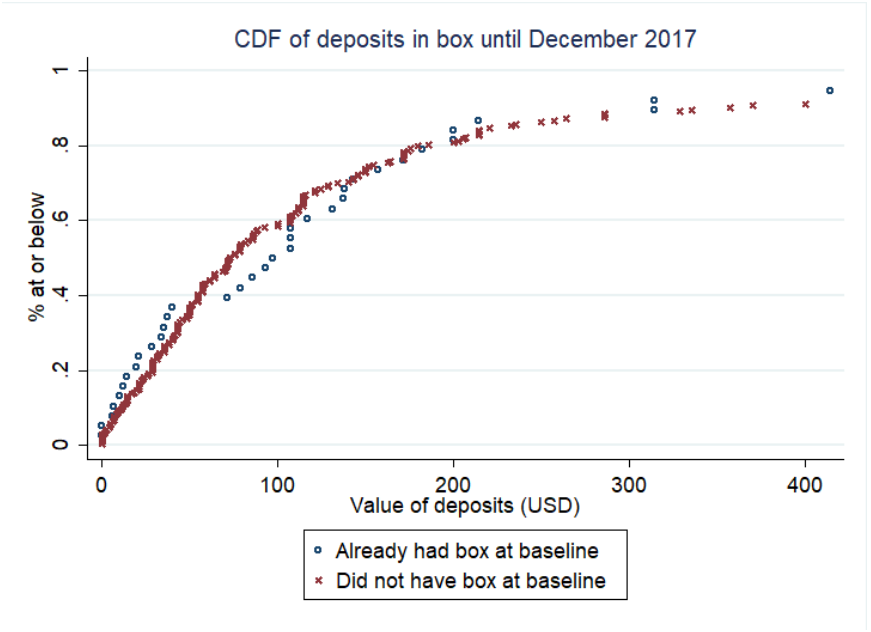


Figure A.13: CDFs of deposits by baseline account status

(a) Boxes



(b) Mobile Money

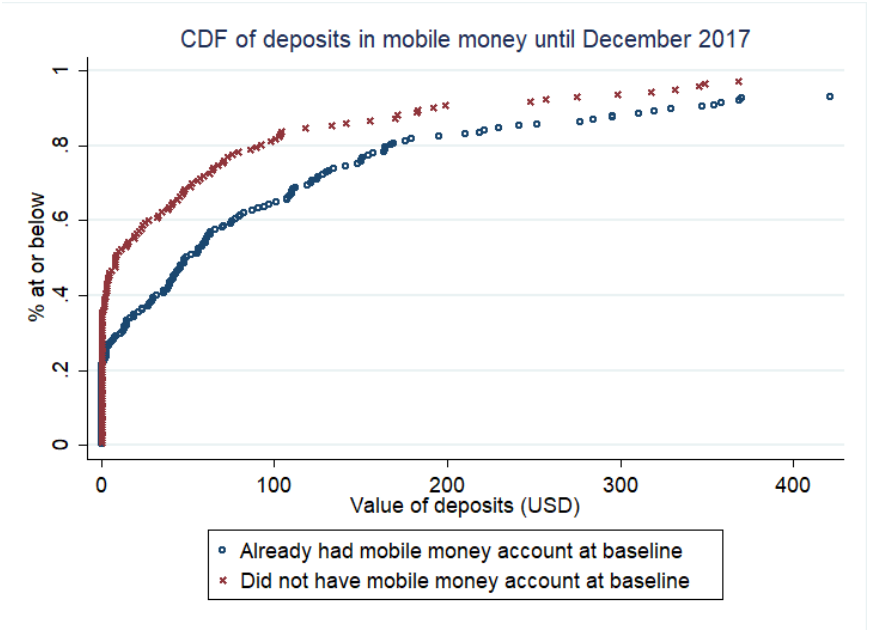


Figure A.14: Project mobile Money Usage over Time

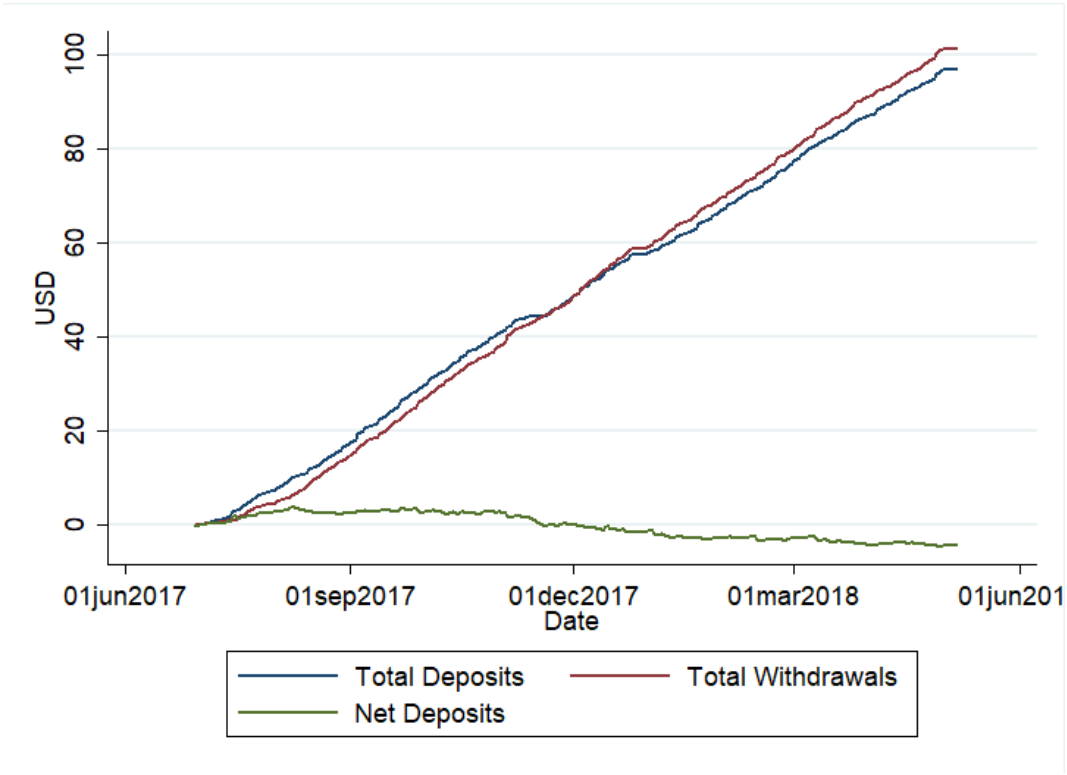


Table A.19: Withdrawal Fee Schedule for Mobile Money Account

	(1)	(2)	(3)
		Fee (percentage) for:	
Withdrawal amount (USD)	Fee (USD)	Minimum withdrawal within range	Maximum withdrawal within range
0.07-0.72	0.029	-	0.040
0.73-1.43	0.060	0.082	0.042
1.44-3.57	0.140	0.097	0.039
3.58-7.14	0.290	0.081	0.041
7.15-14.29	0.540	0.076	0.038
14.3-28.6	1.080	0.076	0.038
28.7-85.7	2.710	0.094	0.032
85.7-142.9	5.360	0.063	0.038

Note: from posted withdrawal fees on Airtel webpage. The maximum withdrawal is $142.9.

Table A.20: Saving Goals

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline				Purpose Listed for Project Savings Account			
		Difference between control and:						Difference
	Control Mean	1 Box	Multiple Boxes	Mobile Money	1 Box	Multiple Boxes	Mobile Money	between (5) and (6)
Invest in equipment/inventory/other expenses in existing business	0.65	0.04	-0.09	0.02	0.26	0.52	0.36	0.301***
		(0.07)	(0.08)	(0.06)				
Health or other emergencies	0.56	0.08	0.06	0.06	0.11	0.44	0.20	0.330***
		(0.07)	(0.09)	(0.06)				
Regular expenses (food, household bills, utilities, etc.)	0.47	0.02	0.03	-0.01	0.03	0.21	0.08	0.220***
		(0.07)	(0.08)	(0.06)				
Hungry Season	0.42	-0.03	0.00	0.00	0.06	0.13	0.03	0.088**
		(0.07)	(0.08)	(0.06)				
Durables for Home	0.27	-0.07	0.00	-0.107**	0.11	0.23	0.07	0.087*
		(0.06)	(0.08)	(0.05)				
Buying land	0.14	0.04	0.119*	0.07	0.25	0.28	0.24	-0.04
		(0.05)	(0.07)	(0.05)				
Education for children	0.14	-0.03	0.04	-0.03	0.12	0.24	0.13	0.155**
		(0.05)	(0.08)	(0.04)				
Starting a new business	0.06	0.03	-0.04	0.01	0.08	0.05	0.07	-0.05
		(0.05)	(0.03)	(0.03)				
Investments in agriculture	0.05	-0.02	-0.01	-0.01	0.04	0.08	0.02	0.05
		(0.03)	(0.03)	(0.03)				
Other goals	0.06	0.02	0.01	0.01	-	-	-	-
		(0.03)	(0.03)	(0.02)				
Observations	801				635			

Note: Other goals include buying a car (or other transport), weddings, adult education, and personal expenditures. The baseline goals were recorded across all saving places for which the respondent reported using.

Table A.21: Attrition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(6)
	High Frequency Phone Surveys (HFPS)				Monitoring Surveys			
	Round 1 (September-October 2018)		Round 2 (March 2018)					
	Completed at least 1 survey	Percentage of surveys completed	Completed at least 1 survey	Percentage of surveys completed	Completed round 1 (January 2018)		Completed round 2 (March 2018)	
Panel A. Pooled								
Box	-0.019	-0.036	0.013	-0.020	0.000	-0.050	0.026	-0.025
	(0.02)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.06)
Mobile Money	-0.006	-0.015	0.031	0.029	0.013	0.044	0.028	0.013
	(0.02)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.06)
HFPS sample	-	-	-	-	-0.065**		-0.028	
	-	-	-	-	(0.03)		(0.03)	
Panel B. Disaggregated groups								
One box	0.000	-0.023	0.000	-0.029	0.006	-0.038	0.000	-0.050
	(0.02)	(0.04)	(0.06)	(0.06)	(0.04)	(0.06)	(0.04)	(0.06)
Multiple boxes	-0.037	-0.049	0.025	-0.01	-0.005	-0.063	0.051	0
	(0.02)	(0.04)	(0.06)	(0.06)	(0.04)	(0.06)	(0.04)	(0.06)
Mobile money	-0.006	-0.015	0.031	0.029	0.013	0.044	0.028	0.012
	(0.02)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.06)
Sample	HFPS	HFPS	HFPS	HFPS	Full	HFPS	Full	HFPS
Observations	400	400	400	400	801	400	801	400
Control Mean	0.988	0.888	0.838	0.735	0.863	0.813	0.775	0.8

Note: See text for discussion of surveys and Figure A2 for a project timeline.

Table A.22: Summary Statistics and Randomization Check based on the Intake Survey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample				HFPS sample only			
		Regression Coefficient for:				Regression Coefficient for:		
	Control Mean	1 Box	Multiple Boxes	Multiple Mobile	Control Mean	1 Box	Multiple Boxes	Multiple Mobile
Panel A: Business and Labor Supply								
=1 if Worked	0.80	0.004	-0.091*	-0.021	0.82	0.008	-0.09	-0.034
		(0.03)	(0.05)	(0.03)		(0.05)	(0.06)	(0.05)
Daily Hours Worked	8.34	-0.415	-1.062	-0.331	8.76	0.072	-0.943	-0.593
	(5.33)	(0.61)	(0.65)	(0.53)	(5.29)	(0.81)	(0.80)	(0.67)
Daily Profits	4.24	-0.252	-0.903	-0.574	4.43	-0.527	-0.148	-0.036
	(6.82)	(0.90)	(0.82)	(0.80)	(6.75)	(0.76)	(0.86)	(0.81)
=1 if Farmed Last Week	0.05	-0.002	0.012	-0.006	0.02	0.026	0.013	0.034
		(0.04)	(0.04)	(0.03)		(0.05)	(0.04)	(0.04)
Hours Farmed Last Week	0.22	0.378	1.117	-0.139	0.09	0.058	-0.049	0.088
	(1.09)	(0.55)	(1.10)	(0.18)	(0.68)	(0.23)	(0.20)	(0.27)
Panel B: Savings, Expenditures and Transfers Last Week								
Home Deposits	5.81	9.897**	-0.881	1.013	5.87	5.503*	-0.903	1.664
	(11.91)	(4.03)	(1.50)	(1.66)	(11.99)	(3.22)	(1.87)	(2.35)
Box Deposits	2.20	-1.738	-2.695*	0.285	1.76	-0.328	-2.246*	-1.653
	(11.97)	(1.62)	(1.56)	(2.69)	(5.47)	(1.47)	(1.21)	(1.29)
MM Deposits	0.78	-0.302	0.025	-0.568	0.74	0.224	0.789	-0.456*
	(3.83)	(0.60)	(0.65)	(0.53)	(2.46)	(0.56)	(0.69)	(0.26)
Total Deposits	16.04	8.979	-1.657	2.331	16.26	6.922	2.661	3.993
	(27.29)	(5.73)	(3.56)	(3.70)	(27.76)	(4.41)	(4.78)	(4.19)
Food Expenditures	5.85	0.082	0.301	-0.014	5.94	0.205	0.125	-0.19
	(4.64)	(0.66)	(0.64)	(0.58)	(4.16)	(0.88)	(0.88)	(0.75)
Amount of Transfers	1.39	2.192*	2.852	0.529	1.22	2.13	1.599	1.141
Given to Family/Friends	(5.10)	(1.15)	(1.93)	(0.67)	(5.31)	(1.40)	(1.06)	(0.94)
Amount of Transfers	2.61	-0.036	-1.316*	-1.161	3.58	-0.479	-1.568	-2.429**
from Family/Friends	(5.65)	(0.87)	(0.75)	(0.71)	(6.68)	(1.44)	(1.19)	(1.10)
Amount of Transfers	2.85	0.719	0.455	0.837	2.28	3.379	2.105	2.291
to Spouse	(9.84)	(1.74)	(1.42)	(1.50)	(8.35)	(2.90)	(1.98)	(2.22)
Amount of Transfers	1.61	1.407*	1.917	1.038**	1.70	1.302	0.161	1.066
to Spouse	(3.73)	(0.80)	(1.83)	(0.49)	(3.08)	(1.03)	(0.58)	(0.74)
Observations	2865				1477			

Note: Means are population weighted. Randomization check is performed only for businesses that appear in the analysis sample (i.e. completed a monitoring survey or at least one phone survey). There are 761 unique businesses. Monetary values are winsorized at 1% and expressed in USD.

Table A.23: Correlations between Self-Reports and Administrative Data

	(1)	(2)	(3)	(4)	(5)
	Independent	Administrative Transaction Data			
	Balance Verification	MM Deposits	MM Deposits	MM Deposits	MM Deposits
	Box Check	HF1	HF2	MS1	MS2
Self Report	1.047***	0.928***	0.517***	0.712***	0.480***
	(0.02)	(0.08)	(0.07)	(0.06)	(0.07)
Sample	Box Groups	HF MM	HF MM	Full MM	Full MM
Observations	190	157	157	272	261
Pvalue (B=1)	0.0426	0.341	0.00	0.00	0.00
Saw at least 1 box	0.793				
Saw all Boxes	0.751				

Note: Regressions are run without a constant term.

Table A.24: Treatment Effects on Deposits - Evidence from Monitoring Surveys

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	First Stage		Deposits into project accounts (USD)1		Deposits into other savings sources (USD)				
	Made at least 1 deposit into any Box	MM	Deposits into project Lockboxes	Deposits into project MM	Cash at Home	Other Boxes	Other MM	Other Sources	Total Deposits
Panel A. Monitoring surveys4									
One box	0.596***	-0.11	45.066***	-	-14.378***	-9.509***	-2.131	1.451	23.112
	(0.06)	(0.07)	(4.60)	-	(3.45)	(2.68)	(2.88)	(10.28)	(14.79)
Multiple boxes	0.593***	-0.028	75.652***	-	-16.765***	-11.861***	-2.568	0.119	49.946***
	(0.06)	(0.08)	(9.26)	-	(3.26)	(2.86)	(3.00)	(11.02)	(18.22)
Mobile money	-0.02	0.281***	-	16.932***	-1.006	0.76	-7.027***	2.084	19.206
	(0.06)	(0.06)	-	(1.71)	(3.59)	(3.17)	(2.51)	(8.40)	(12.25)
Observations	717	717	530	533	1308	1308	1308	1308	1308
Control Mean	0.41	0.48	-	-	20.78	16.96	12.78	56.21	110.10
Control SD	-	0.50	-	-	37.36	39.35	29.92	103.60	135.50
p-values for F-tests of:									
1 Box = Multiple box	0.93	0.27	-	-	0.32	0.13	0.85	0.90	0.12
1 Box = Mobile money	0.001***	0.001***	-	-	0.001***	0.001***	0.008***	0.94	0.75
Multiple box = Mobile money	0.001***	0.001***	-	-	0.001***	0.001***	0.003***	0.82	0.05**

Note: All regressions control for strata, call date fixed effects and baseline controls (see in the text for details). Standard errors clustered at individual level in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. All monetary variables are in USD. Figures in Columns 3-4 are not regression coefficients but are means and standard deviations for the appropriate treatment group only. Other savings include savings in VSLAs, ROSCAs, banks, and other miscellaneous savings places. In Deposits are cumulative deposits in the 2 months prior to the 1st survey (November 2017-December 2017), and the 2nd survey (March 2018-February 2018), winsorized at 5% level.

Table A.25: Treatment effects on expenditures (measured with monitoring surveys)

	(1) Staple Foords	(2) Personal Expenses	(3) Airtime	(4) Household Expenses	(5) Education	(6) All Other Expenses	(7) Total
Panel A. Monitoring surveys2							
One box	1.03	1.255*	0.23	2.684**	1.18	0.84	6.042*
	(0.95)	(0.76)	(0.20)	(1.29)	(7.26)	(1.08)	(3.13)
Multiple boxes	1.982*	0.81	0.13	1.13	-0.37	2.801**	6.780**
	(1.03)	(0.73)	(0.22)	(1.35)	(7.23)	(1.32)	(3.24)
Mobile money	1.08	1.254*	0.26	1.69	-1.11	1.20	5.367*
	(0.90)	(0.67)	(0.18)	(1.18)	(6.43)	(0.97)	(2.82)
Observations	1305	1307	1308	1307	1307	1308	1308
Number of businesses	719	719	719	719	719	719	719
Control Mean	13.14	5.11	1.81	6.93	49.43	7.91	36.97
Control SD	8.85	6.42	1.77	10.65	77.02	9.28	25.62
p-values for F-tests of:							
1 Box = Multiple box	0.35	0.54	0.64	0.19	0.82	0.12	0.81
1 Box = Mobile money	0.96	1.00	0.86	0.34	0.71	0.69	0.81
Multiple box = Mobile money	0.33	0.47	0.49	0.57	0.90	0.15	0.59

Note: All variables are measured over the prior 7 days. All regressions control for strata, date fixed effects and baseline controls (see in the text for details). Standard errors clustered at individual level in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. All variables are expressed in USD and winsorized at 5%.

Table A.26: Treatment effects on business outcomes and labor supply - Evidence from the Monitoring Surveys

	(1)	(2)	(3)	(4)	(5)			(6)	(7)	(8)	(9)	
	Main Business							Farming		Secondary occupation		
	=1 if Worked	Hours	Revenue	Profits	Inventory Investment	Inventory Stock	Credit to Customers	=1 if Farmed	Hours	=1 if Worked	Hours	Income
Panel A. Monitoring surveys												
One box	-0.04	-0.13	-0.27	-0.37	0.26	7.88	1.99	0.05	0.63	0.00	-0.03	-0.47
	(0.03)	(0.43)	(1.13)	(0.33)	(0.81)	(31.93)	(1.31)	(0.05)	(0.61)	(0.02)	(0.64)	(0.48)
Multiple boxes	-0.04	-0.16	-0.19	-0.18	-0.16	33.71	3.922**	0.101*	2.322***	0.05	0.92	0.14
	(0.03)	(0.43)	(1.19)	(0.32)	(0.84)	(35.74)	(1.58)	(0.05)	(0.84)	(0.03)	(1.21)	(0.58)
Mobile money	-0.05	-0.39	-0.92	-0.42	-0.70	25.69	1.09	0.03	1.342**	0.053**	1.476*	-0.23
	(0.03)	(0.38)	(1.04)	(0.29)	(0.71)	(31.52)	(1.10)	(0.04)	(0.59)	(0.03)	(0.78)	(0.43)
Observations	1,308	1,177	1,306	1,297	1,308	1,306	1,308	1,308	1,306	1,308	1,305	1,300
Number of businesses	719	679	719	718	719	719	719	719	719	719	719	719
Control Mean	0.74	8.26	10.00	3.06	7.62	220.20	4.45	0.24	2.21	0.06	0.69	1.71
Control SD	0.31	3.50	13.35	3.51	12.15	399.00	11.45	0.43	6.90	0.24	4.14	4.96
p-values for F-tests of:												
1 Box = Multiple box	0.94	0.94	0.95	0.54	0.61	0.46	0.29	0.34	0.05*	0.17	0.42	0.29
1 Box = Mobile money	0.72	0.42	0.46	0.87	0.18	0.56	0.50	0.69	0.23	0.06*	0.13	0.56
Multiple box = Mobile money	0.68	0.51	0.44	0.37	0.43	0.80	0.06*	0.17	0.25	0.96	0.73	0.49

Note: All outcomes in are at the daily level, except for farming, secondary occupations (which are measure at the weekly level), inventory stock and giving credit to customers. All regressions include the mean of the dependent variable during the intake survey - see text for details. All regressions control for strata, date fixed effects and baseline controls (see in the text for details). Standard errors clustered at individual level in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. All monetary variables are expressed in USD. Farming hours and hours in secondary occupations are measured at the weekly level.

Table A.27: Treatment effects on loans and transfers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Transfers to / from friends & family				Transfers to / from spouse				Digital credit		Other sources of credit	
	Received Transfer	Amt.	Gave Transfer	Amt.	Received Transfer	Amt.	Gave Transfer	Amt.	Received Loan	Amt.	Received Loan	Amt.
Panel A. Monitoring surveys												
One box	-0.01	2.52	-0.03	0.87	-0.06	-1.48	-0.03	0.89	0.06	0.65	0.106**	4.97
	(0.06)	(1.59)	(0.05)	(0.90)	(0.04)	(1.34)	(0.05)	(0.91)	(0.06)	(0.50)	(0.05)	(6.87)
Multiple boxes	-0.06	2.28	-0.04	0.75	-0.090**	-1.33	0.03	0.72	-0.07	-0.23	0.01	2.21
	(0.05)	(1.44)	(0.06)	(0.96)	(0.04)	(1.45)	(0.05)	(0.93)	(0.06)	(0.52)	(0.06)	(8.19)
Mobile money	0.01	0.83	-0.04	-0.16	0.02	-1.07	0.06	1.927**	-0.02	-0.33	0.03	0.71
	(0.05)	(1.11)	(0.05)	(0.75)	(0.04)	(1.17)	(0.05)	(0.79)	(0.05)	(0.38)	(0.05)	(6.39)
Observations	1308	1308	1308	1308	1308	1308	1308	1308	1103	1103	1308	1308
Number of businesses	719	719	719	719	719	719	719	719	698	698	719	719
Control Mean	0.38	5.81	0.42	4.02	0.34	6.91	0.38	3.88	0.70	2.33	0.30	25.33
Control SD	0.49	14.87	0.49	7.59	0.47	15.44	0.49	7.38	0.46	3.83	0.46	66.16
p-values for F-tests of:												
1 Box = Multiple box	0.36	0.89	0.94	0.91	0.52	0.91	0.29	0.86	0.03**	0.11	0.13	0.74
1 Box = Mobile money	0.66	0.24	0.95	0.21	0.05*	0.68	0.04**	0.25	0.12	0.03**	0.13	0.43
Multiple box = Mobile money	0.11	0.28	0.97	0.23	0.008***	0.81	0.45	0.15	0.38	0.82	0.79	0.85

Note: Transfers are measured over the last month prior to each survey. Credit is measured over last several months prior to each survey. All regressions control for strata and date fixed effects, and for the mean of the dependent variable (measured during the intake survey - see text for details). Standard errors clustered at individual level in parentheses. Transfers include gifts and loans, and both cash and in-kind payments. All monetary values in USD. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Other credit includes loans from VSLAs, ROSCAs, banks, microfinance institutions, and moneylenders.

Table A.28: Self-reported reasons it is hard to save

	(1)	(2)	(3)	(4)	(5)
	= 1 if respondent agrees with statement that it is hard to save because:				
	Money might be Stolen	I have many family commitments	I spend more than I like on personal expenditures	I don't genernate enough money to save	I don't have a habit to save
One box	-0.098**	-0.05	-0.09	0.02	-0.01
	(0.04)	(0.04)	(0.08)	(0.07)	(0.06)
Multiple boxes	-0.100**	0.00	-0.03	0.00	-0.03
	(0.04)	(0.06)	(0.08)	(0.08)	(0.06)
Mobile money	-0.085**	-0.03	0.04	0.08	0.00
	(0.04)	(0.04)	(0.07)	(0.06)	(0.05)
Observations	669	669	669	669	669
Control Mean	0.142	0.127	0.381	0.463	0.172
p-values for F-tests of:					
1 Box = Multiple box	0.97	0.34	0.41	0.75	0.66
1 Box = Mobile money	0.66	0.53	0.05*	0.33	0.80
Multiple box = Mobile money	0.50	0.55	0.31	0.22	0.42

Note: All regressions control for strata and date fixed effects. All questions were asked only in the last monitoring survey, so there is 1 observation per respondent. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

Table A.29: Take-up of project accounts by prior ownership of accounts.

	(1)	(2)	(3)	(4)
	Box group		Mobile money group	
	Had box at baseline	Didn't have a box at baseline	Had MM at baseline	Didn't have MM at baseline
Opened account / accepted box	1.00	0.99	0.99	0.99
Opened more than 1 account if offered	0.73	0.76	0.34	0.21
Panel A. Take-up by December 2017				
Ever used account	0.95	0.98	0.72	0.57
Made at least 2 deposits	0.95	0.95	0.61	0.42
Total value of deposits	127.80	131.27	56.46	30.24
	(131.66)	(152.66)	(80.49)	(61.22)
	[102.14]	[72.86]	[24.00]	[2.14]
Number of deposits	64.92	65.50	6.66	4.63
	(70.81)	(94.55)	(8.98)	(9.62)
	[44.00]	[40.00]	[3.00]	[1.00]
Total value of withdrawals	102.17	95.69	54.38	32.35
	(117.99)	(129.45)	(82.62)	(65.41)
	[78.57]	[48.57]	[17.29]	[2.00]
Had a non-zero balance	0.32	0.57	-	-
Balance	8.15	14.83	-	-
	(22.05)	(26.29)	-	-
	[0.00]	[2.29]	-	-
Panel B. Take-up by May 2018				
Ever used account			0.80	0.64
Total value of deposits			112.05	63.44
			(153.63)	(115.73)
			[48.86]	[7.86]
Number of deposits			13.68	9.32
			(17.63)	(19.96)
			[9.00]	[3.00]
Total value of withdrawals			122.91	73.06
			(181.98)	(142.00)
			[53.02]	[7.20]
Observations	38	203	177	143

Note: Figures come from administrative data from telecom company for mobile money accounts, and from in-person visits for the lockbox (which occurred in December 2017). Means are presented, with standard deviations in parentheses and medians in square brackets. All values are expressed in USD and winsorized at 5% level. Long term usage figures are only available from mobile money group. Balance for MM is not currently available as the authors are currently waiting to receive an updated transaction data.

Table A.30: Effects on withdrawals (HF survey results)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Withdrawals from project accounts (USD)		Withdrawals from other savings sources (USD)				
	Withdrawas from project Lockboxes	Withdrawas from project MM	Cash at Home	Other Boxes	Other MM	Other Sources	Total Withdrawals
Panel A. High frequency surveys2							
One box	0.432***		-0.046	0.001	-0.063	0.434	0.676*
	(0.13)		(0.21)	(0.08)	(0.05)	(0.27)	(0.40)
Multiple boxes	1.014***		-0.427***	-0.098***	-0.023	0.142	0.519
	(0.30)		(0.16)	(0.04)	(0.05)	(0.28)	(0.44)
Mobile money		0.263***	-0.01	-0.04	-0.121***	0.41	0.43
		(0.04)	(0.18)	(0.04)	(0.04)	(0.29)	(0.37)
Observations	12321	12807	31287	31290	31290	31290	31290
Number of Businesses	156	156	389	389	389	389	389
Control Mean	0.00	0.00	0.75	0.14	0.16	0.73	1.78
Control SD	0.00	0.00	4.57	1.81	1.72	19.30	20.06
p-values for F-tests of:							
1 Box = Multiple box	-	-	0.06*	0.23	0.32	0.35	0.76
1 Box = Mobile money	-	-	0.87	0.63	0.02**	0.93	0.49
Multiple box = Mobile money	-	-	0.02**	0.01**	0.003***	0.39	0.85

Note: All regressions control for strata, date fixed effects and baseline controls (see in the text for details). Standard errors clustered at individual level in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. All monetary variables are expressed in USD. Other savings include savings in VSLAs, ROSCAs, banks, and other miscellaneous savings places. Withdrawals are measured at the daily level, and winsorized at 5%. All regressions include the mean of the dependent variable during the intake survey - see text for details.

Table A.31: Effects on withdrawals (Monitoring survey results)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Withdrawals from project accounts (USD)		Withdrawals from other savings sources (USD)				
	Withdrawas from project Lockboxes	Withdrawas from project MM	Cash at Home	Other Boxes	Other MM	Other Sources	Total Withdrawals
Panel B. Monitoring surveys							
One box	36.688***	0.508	-8.763***	-3.999*	-1.534	2.426	25.514*
	(5.75)	(0.70)	(3.36)	(2.30)	(2.08)	(9.58)	(13.52)
Multiple boxes	67.441***	-0.262	-10.774***	-5.730***	0.7	3.153	62.349***
	(10.70)	(0.61)	(3.55)	(2.15)	(2.42)	(13.37)	(22.44)
Mobile money	1.887	12.132***	-0.559	1.739	-3.761*	-3.962	14.446
	(3.40)	(1.38)	(3.71)	(2.75)	(2.03)	(8.13)	(11.36)
Observations	1308	1308	1308	1308	1308	1308	1308
Number of businesses	719	719	719	719	719	719	719
Control Mean	0.00	0.00	12.80	9.68	8.84	59.27	89.91
Control SD	0.00	0.00	29.27	31.43	23.50	216.50	232.10
p-values for F-tests of:							
1 Box = Multiple box	-	-	0.25	0.20	0.21	0.96	0.1*
1 Box = Mobile money	-	-	0.001***	0.004***	0.10	0.39	0.34
Multiple box = Mobile money	-	-	0.001***	0.001***	0.003***	0.56	0.02**

Note: All regressions control for strata, date fixed effects and baseline controls (see in the text for details). Standard errors clustered at individual level in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. All monetary variables are expressed in USD. Other savings include savings in VSLAs, ROSCAs, banks, and other miscellaneous savings places. In Panel B, withdrawals are cumulative withdrawals in the 2 months prior to the 1st survey (November 2017-December 2017), and the 2nd survey (March 2018-February 2018), winsorized at 5% level.

Table A.32: Effects on Balances in Monitoring Surveys

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Balance in project accounts (USD)		Balance in other savings sources (USD)				
	Lockboxes	MM	Cash at Home	Other Boxes	Other MM	Other Sources	Total Balance
Panel A. Monitoring surveys							
One box	37.070***		-9.106***	0.965	-3.653	-18.78	7.189
	(5.38)		(2.98)	(1.82)	(2.53)	(13.90)	(17.44)
Multiple boxes	42.256***		-12.389***	0.031	-5.297**	2.788	26.995
	(5.67)		(2.56)	(1.22)	(2.51)	(16.49)	(18.49)
Mobile money		9.822***	-0.492	11.221***	-5.564**	3.62	17.401
		(1.52)	(2.71)	(2.06)	(2.25)	(14.55)	(16.19)
Observations	530	533	1300	1298	1300	1308	1308
Number of businesses	530	533	716	716	716	719	719
Control Mean	0.00	0.00	14.49	1.70	9.33	56.10	81.44
Control SD	0.00	0.00	26.44	10.89	21.86	109.50	119.60
p-values for F-tests of:							
1 Box = Multiple box	-	-	0.15	0.59	0.32	0.15	0.28
1 Box = Mobile money	-	-	0.001***	0.001***	0.19	0.07*	0.52
Multiple box = Mobile money	-	-	0.001***	0.001***	0.81	0.95	0.56

Note: All regressions control for strata, call date fixed effects and baseline controls (see in the text for details). Standard errors clustered at individual level in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively. All monetary variables are expressed in USD and winsorized at 5% level. Other savings include savings in VSLAs, ROSCAs, banks, and other miscellaneous savings places.

Table A.33: Reasons for withdrawal from project accounts

	(1)	(2)	(3)	(4)
		Box groups		
	Overall Mean	One Box	Multiple Boxes	Mobile money
Purposes for withdrawals				
(=1 if at least one of the last 3 withdrawals was for the following purpose)				
Business investment or inventory	0.457	0.383	0.520	0.561
General consumption or household bills	0.435	0.318	0.537	0.512
Emergencies	0.326	0.243	0.398	0.183
School fees	0.283	0.280	0.285	0.201
Durables or home improvement	0.143	0.131	0.154	0.073
Agricultural investment	0.130	0.140	0.122	0.067

Note: Data comes from last monitoring survey, and is conditional on making at least 1 withdrawal. N = 394

Table A.34: Treatment effects on sickness

	(1) =1 if member of household sick	(2) =1 if respondent sick
One box	0.03	0.01
	(0.03)	(0.03)
Multiple Box	0.03	0.01
	(0.03)	(0.02)
Mobile money	0.00	0.00
	(0.02)	(0.02)
Observations	4515	4515
Number of businesses	389	389
Control Mean	0.12	0.12

Note: Standard errors clustered at individual level in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

Table A.35: Downsides of mobile money

	(1) Prompted	(2) Unprompted
Are there downsides of using mobile money?		
No, there are no downsides	0.25	0.30
Of those who say yes, listed downsides:		
Network is often down	0.46	0.38
Agents don't have enough cash to make withdrawals	0.35	0.13
Agents are too far away	0.15	0.04
Agents don't have enough balance to make deposits	0.14	0.03
I don't trust mobile money companies with my money	0.13	0.02
Too complicated	0.10	0.01
Agents will cheat you	0.09	0.01
It is not useful to me	0.09	0.01
I am afraid I may forget my pin	0.05	0.01
I don't know	0.03	0.046
I don't know enough about mobile money	0.00	0.07
Observations	261	224

Note: Responses were first collected unprompted, then enumerators went through a list of precoded reasons. Questions without prompts were not included in the first few days of surveying, which is why the number of observations is lower. Data is restricted to the mobile money treatment group in the experiment.