Journal of Development Economics 108 (2014) 69–86

Contents lists available at ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

Minding small change among small firms in Kenya☆

Lori Beaman [a], Jeremy Magruder [b,*], Jonathan Robinson [c]

[a] Northwestern University, USA
[b] University of California, Berkeley, USA
[c] University of California, Santa Cruz, USA

ARTICLE INFO

Article history:
Received 16 February 2013
Received in revised form 25 October 2013
Accepted 25 December 2013
Available online 22 January 2014

Keywords:
Attention and decision making
Small firms
Kenya

ABSTRACT

Many micro-enterprises in Kenya have low productivity. We focus on one particular business decision which may indicate low productivity: keeping enough change on hand to break larger bills. This is a surprisingly large problem. Our estimates suggest that the average firm loses approximately 5–8% of total profits because they do not have enough change. We conducted two experiments to shed light on why this happens: surveying firms weekly about lost sales, thereby increasing the salience of change, and explicitly informing firms about lost sales. We find that both interventions significantly altered change management and reduced lost sales. This largely rules out many potential explanations such as the risk of theft or the costs of holding change being too high. One explanation consistent with firms' response to the survey and information on their lost sales is that firms were not perfectly attentive to change management prior to the interventions.

1. Introduction

As Sargent and Velde (2002) describe in "The Big Problem of Small Change," having a steady supply of small change and the correct composition of currency denominations is essential for a well-operating economy. Despite the gains in monetary policy described in their book from having small change in rich countries, customers often are unable to complete transactions in developing countries because neither the vendor nor the customer has the correct small change. The "great Buenos Aires coin shortage" made international news in 2009 (Surowiecki, 2009), where a black market arose in which coins sold for a 5–10% premium (Keohane, 2008).

We collected data on this phenomenon among 508 small firms in Western Kenya. These firms are very small: the average firm generates profits of approximately US $20 per week, and 60% of firms have no employees other than the owner. At baseline, the majority of firms reported having lost a sale because they lacked change to break a larger bill in the past 7 days; in addition, the average firm owner spent over 2 hours looking for change while customers waited in that same 7 days. This is wasted time for the customer and also means that firms lose out on sales while they are away looking for change. Using data on weekly profits and hours of work, we approximate that firms lose 5–8% of profits total from these two components.[1] These lost transactions are all the more striking in that many take place while the firms have cash on hand, but in denominations which are too large.

Why is this occurring? There are number of potential explanations in which this behavior is optimal for the firm (taking as given the amount of small denominations readily available in the marketplace).[2] First, there are costs to holding and acquiring small change: for example, holding change – or cash more generally – may mean less inventory. Getting change can be a time consuming and difficult process: anecdotal evidence suggests that it is not always possible to get small change even at local banks. Even when change is available at a bank, acquiring it is a time consuming process. Furthermore, less than half of the business owners have bank accounts. Second, in some environments, the risk of theft may be affected by cash and changeholding. Third, holding low change stocks may be an opportunity to hold-up the customer, selling more or charging higher prices (upselling). Fourth, if there is a fixed amount of demand in the market and change management requires

☆ We thank Conner Brannen, Elliott Collins and Sarah Reibstein for excellent research assistance, and Innovations for Poverty Action — Kenya for administrative support. We thank Rich Akresh, Gadi Barlevy, Jesse Cunha, Pascaline Dupas, Fred Finan, Seema Jayachandran, Dean Karlan, Cynthia Kinnan, three anonymous referees and seminar audiences at CEGA, the BREAD junior affiliate pre-conference, the University of Houston — Rice Empirical Micro Workshop, NYU, the University of Köln, Oregon State University and IZA for helpful comments. We gratefully acknowledge financial support from the University of California, Santa Cruz. All errors are our own.

* Corresponding author.
E-mail addresses: l-beaman@northwestern.edu (L. Beaman), jmagruder@berkeley.edu (J. Magruder), jmrtwo@ucsc.edu (J. Robinson).

[1] As explained in Table 1, we value the time spent fetching change at the average profit per hour as reported in our monitoring surveys (dividing weekly profits by 65 hours per week, the average hours reported in our background survey). This is likely a conservative estimate since we anticipate that changeouts occur most often during busy periods, when profits may be higher. An alternative back-of-the-envelope calculation, using information on firms' reported lost sales while away fetching and average lost profits during changeouts, gives a similar estimate of around 10% of profits.

[2] We do not address the question of overall efficiency costs of changeouts at a market level. An increase in the availability of change may well generate additional transactions, generating market level changes, but our experiment was not designed to answer this question.

0304-3878/$ – see front matter © 2014 Elsevier B.V. All rights reserved.
http://dx.doi.org/10.1016/j.jdeveco.2013.12.010

effort, firms may be colluding together to be in a low effort equilibrium at the expense of consumers.

By contrast, another set of explanations stem from the idea that firms may have imperfect information or imperfect ability to process all available information when making business decisions. For example, the recent literature on limited attention (DellaVigna, 2009; Sims, 2003) suggests that firms may not be perfectly attending to all business decisions or have limited capacity to process all available information. We use two simple interventions to shed light on these two broad sets of explanations.

Our interventions are designed to test if changeholding behavior and profits change in response to interventions which increase the salience of lost sales, but do not directly affect the private profitability of changeholding. In the first intervention, we visited firms once per week to administer a monitoring survey. In this survey, we asked firms about their profits, sales, and, most importantly, the number of times that they ran out of change (which we term “changeouts”), and the profits they lost from changeouts. We also ask how long they were away from the shop in the past week to get change from other businesses, so that we can estimate sales lost during that time away. Finally, we asked about the amount of cash that the entrepreneur brought to work (in total and in coins). As the survey did nothing other than ask these questions of firms, we interpret survey effects as directing the entrepreneur's attention towards the change problem. The survey may have induced firms to then process latent information it already had about the frequency of changeouts and then learn from this new information. To evaluate the effects of surveying, we randomized the date at which we started visiting firms. We estimate the effect of being visited by comparing lost sales at a given period of time between those firms that started the survey earlier to those that started later. Zwane et al. (2011) and Stango and Zinman (2013) both show that respondents in some cases change their behavior simply in response to being asked survey questions.

Second, after following firms for two to three weeks, we calculated the amount of money each firm was losing due to insufficient change, and conducted an information intervention in which we went over the calculation with a randomly selected subsample of firms in the study. During this intervention, we informed firms of their own figures, as well as market averages. We can thus measure whether information has an additional effect, by comparing firms which received information to those that did not, but which both received the same number of reminder visits.

We find that both interventions had large effects on a variety of outcomes, most importantly lost sales and change management. The number of changeouts declined by more than 30% due to the information intervention, and the repeated survey also reduced the number of lost sales due to insufficient change. This translates into less lost revenue and profits directly, and also indirectly from losing fewer sales while away from the shop fetching change during the day. These changes in the incidence of changeouts and associated sales and profits are accompanied by adjustments in change management behavior. Treatment firms report that they visit nearby shops for change less often, and that they give out change to nearby businesses less often. Treatment firms also appear to bring in more cash in the morning. We validate the data using objective measures of cash on hand.

Our estimates also indicate that these interventions increased reported profits for treatment firms. We asked firms about their profits directly in our weekly survey, as suggested in de Mel et al. (2009b).[3] Profits increased by 8–15% (significant at the 5% level in some specifications but not all) in response to the information intervention. The confidence interval overlaps with our estimates from the back-of-the-envelope calculation on lost profits we described above, though in part because the estimates are imprecise. The confidence interval on profits in the previous 2 hours ranges from 3% to 26%. The profit impacts of the effect of reminders from the survey itself are very noisy and never statistically significantly positive. Though profits are estimated with considerable noise (as is typically the case measuring profits in developing countries (de Mel et al., 2009b)), the results suggests that maintaining enough change represents a profitable investment, at least in the short run. However, profits may also be subject to measurement error that causes us to overestimate the effect of the treatments.[4] We also conducted semi-structured interviews at the end of the project, in which respondents reported that the interventions increased the salience of changeouts and increased total profits.

Our empirical analysis has a few important weaknesses to highlight. First, we only measure short-run responses to the survey visits and information campaign (firms are visited on average for 12 weeks). Second, we rely mainly on self-reported measures of outcomes, with the exception of a one-time audit we conducted with some firms. This is typical for studies on business practices of small firms in developing countries, including de Mel et al. (2008), but one may be concerned about reporting given the high frequency of surveying we did.[5] Third, since more local firms are holding change in response to our intervention, control firms may also have been affected either through information spreading or through equilibrium effects. We examine spillovers to control firms' profits using firms' GPS coordinates and do not find evidence of negative spillovers, though there may very well be negative spillovers this test is unable to detect.[6]

Nevertheless, the evidence suggests that being surveyed about changeholding, and being provided information about market-wide changeouts reduces changeouts and increases reported profits, at least in the short run. This is not consistent with much of the set of privately profitable explanations for changeouts put forth earlier. If firms were purposively choosing to hold low change stocks to maximize profits, then surveying them about changeouts and summarizing their survey responses would not induce a behavioral change. If the return to inventory stocks is greater than holding change – even if it means occasionally losing a low mark-up sale – this should be true no matter how many times we survey them. Our interventions did not change the underlying costs and benefits of changeholding, except potentially through equilibrium dynamics which we discuss below.

We instead consider two other classes of explanations for this behavior change. First, we consider the possibility that monitoring induced profitable behavioral change. The empirical results are consistent with a number of models of inattention. This is most clear with the weekly surveys, which provided no information at all. Inattention has been modeled in a number of ways in the literature. Sims (2003) and the related literature on rational inattention puts forth the idea that agents have limited information-processing capacity. They choose to allocate the finite resource of attention optimally to information which has the most benefit. The directed cognition model (Gabaix and Laibson, 2005; Gabaix et al., 2006) argues that cognition is costly and assumes that agents are partially myopic. Agents then focus on some piece of information which they believe to be more useful while ignoring others.

[3] We asked respondents about profits in two ways. First, we asked about profits in the previous two hours using the question “How much profit have you had in the past 2 - hours?” and also profits in the last week with the question “What was the total income the business earned during the last 7 days after paying all expenses including the wages of employees, but not including any income you paid yourself. That is, what were the profits of your business during the last 7 days?” Therefore weekly profits – and most likely profits in the previous 2 hours – do not include the costs of the owner's time. Since getting more change likely increases owner's labor inputs our estimates of the increase in profits is likely upwardly biased. It is however difficult to value an hour of the owner's time as well as know how much time he spent over the week getting change.

[4] Plausible mechanisms which would be consistent with this type of measurement error include if firms inaccurately estimate the costs of holding change or do not properly take into account that they can sometimes increase the price of items when they have insufficient change when reporting profits to us.

[5] McKenzie (2011) discusses that an advantage of frequent interviews to capture firm profits increases power significantly, which is particularly important given how noisy profit estimates are. The potential for misreporting or Hawthorne effects is a disadvantage of the approach, as noted in the paper.

[6] As discussed in the conclusion, many lost sales from changeouts may be recovered by nearby firms.

DellaVigna (2009) in a survey paper puts forth a simple model of inattention where consumers perceive some attributes of a product as being opaque, and where salience affects the amount of attention directed at that opaque attribute. In our setting, the analogue would be that some components of firm profits are opaque and that increasing salience through reminders would induce firms to learn about the costs of changeouts.[7] Karlan et al. (2011) develop an alternative model where future expenditure opportunities are not fully attended to, generating predictions similar to many time inconsistency models. If this model was governing our firms' behavior, they would need to receive ongoing reminders to maintain change stocks. Even with such a narrow intervention and precisely defined business decision as we study here, our results remain consistent with all of these models of inattention and in particular we cannot say whether the firms' inattention is rational or not, nor can we differentiate a number of mechanisms by which inattention may manifest, such as slow learning about the costs of changeouts in the absence of the intervention, or the need for reminders to maintain focus on change stocks.[8] Our results are not, however, fully consistent with the model of inattention in Hanna et al. (2012). In their model firms would not even be able to tell us their lost sales as they would not notice them.

Second, we also consider the possibility that external monitoring led to firms adopting behavior which is ultimately unprofitable in spite of our short run profit results, either because profits are mismeasured or because they would become negative in the long-run. Long-run profit results could be very different in some dynamic equilibrium settings: for example, firms could have been colluding to maintain low change stocks, and monitoring induced a change in this behavior. It may also be the case that there are costs to going into change autarky that firms did not anticipate in the short-run. High turnover among our firms meant that tracking long-run outcome data was impossible (though it also suggests that these dynamic equilibria considerations may be muted). While we ultimately do not view adopting counterproductive behavior as likely, we discuss such possibilities (which may also have implications for other business training interventions) in Section 5.

The paper is organized as follows. The experimental design, sampling, and the data are described in Section 2, and the econometric strategy is discussed in Section 3. The results are presented in Section 4, including evidence from semi-structured interviews. A discussion of how to interpret our results and alternative explanations is in Section 5. We conclude in Section 6.

2. Experimental Design and Data

2.1. Sample

This project intends to assess the impact of increasing the salience of change management among a representative set of small businesses operating in our study areas in Western Kenya. To obtain a representative sample, we conducted several full censuses of markets to draw our sampling frame. More specifically, the project took place in two phases across 10 market centers near the towns of Bungoma and Chwele. The first, larger phase took place in 7 market centers between October 2009 and June 2010, while the second phase took place in 3 market centers between February and April 2011.

In total, we identified 1195 firms in the two censuses (884 in 2009–10 and 311 in 2011). As discussed below, a key aspect of our experimental design is randomizing the date at which we enrolled firms into the study. To do this, each firm identified in the census was given a randomly determined number, stratified by market center and a set of business types.[9] If a firm could not be enrolled (because they refused or could not be found), we replaced it with the firm with the next-highest random number.

Overall, we invited 793 firms (538 in 2009–10 and 255 in 2011) to participate in the project. We successfully enrolled 508 (64%) of these (309 in 2009–10 and 199 in 2011).[10] The firms in the study are fairly heterogeneous in the goods that they sell, as seen in Table 2: our final sample consists of 24% fruit and vegetable vendors, 37% other types of retail (e.g., shops, hardware shops, small vendors) and 34% services (e.g., small restaurants, repair, tailoring, barbers).[11]

The typical business is small: over 60% of firms have only one worker, the owner. Even among retail shops, which are generally larger businesses than the fruit and vegetable sellers, the owner is the only worker in 52% of cases.[12] Moreover, only 16% of businesses have any salaried workers, as shown in Table 2. 56% of firms are operated by women.

The vast majority of transactions are conducted entirely in cash. A small number of businesses have occasionally conducted transactions using mobile money with a cell phone, as described in the next section, but other forms of payment are unheard of (for example, credit or debit card payment). Since payment options other than cash are so limited, it is perhaps not surprising that change management is a much more serious concern than it would be in many developed countries.

2.2. Background on costs of not holding enough change

As described in the introduction, lost sales because of insufficient change is a prevalent problem for these firms. Table 1 shows that over 50% of firms reported having lost at least one sale in the previous 7 days during our first interview with them. Even firms that had not lost any sales in the past week spent over an hour and a half searching for change for customers during that time period. Panel B looks at estimates of total lost profits, including lost sales from changeouts and time spent fetching for change. This back-of-the-envelope calculation suggests that the average firm loses around 5–8% of profits due to inadequate change, and this figure does not include other costs such as credit given out to customers because of insufficient change.

How difficult is it to obtain small change? Keeping small change on hand is challenging but not impossible in this setting. Few firms reported getting change from the bank (15% on average, though in some markets no firm reported getting change from a bank). In the semi-structured interviews, firms reported going mainly to other

[7] The results are therefore consistent with firms learning over time, but that the underlying constraint preventing them from learning without our interventions was inattention.

[8] For example, we cannot say whether firms will revert back to previous behavior without constant reminders or if drawing attention to the problem over one period is sufficient to change behavior in the long-run, since we were unable to track firms after we stopped surveying them. Also, Banerjee and Mullainathan (2008) suggests that owners of small firms must allocate their attention to both business and home decisions, and we are not able to observe the full set of decisions they make.

[9] Since there is a lot of heterogeneity in business type – there were 43 business codes in the census – we stratified by the 3 most common (retail shop, fruit and vegetable vendor, and cereal trader) and a combined residual "other" category.

[10] While we have detail only on why firms didn't participate for part of our sample, a major reason is that there is tremendous turnover among these types of businesses. For example, in March 2010, we re-censused the firms we identified in October 2009 and were able to trace only 50% of the original businesses. This is consistent with Keats (2012), who finds turnover on the order of 40% over 6 month intervals in a nearby part of Kenya.

[11] The remaining 5% are classified as "other."

[12] Note that it is rare for more than one worker to be working at a given time, even in those firms with more than one employee. Many of these employees tend to the business while the owner is away. An implication of the firms having only one worker is that there is not another person available to complete sales while the owner searches for change.

Table 1
Costs of holding inadequate change.

	Mean	N
	(1)	(2)
Panel A. Estimates from weekly survey		
1 if lost sale in previous 7 days	0.54	507
	(0.50)	
Number of lost sales in previous 7 days	3.35	507
	(6.71)	
Value of lost sales in previous 7 days	268.71	504
	(728.70)	
Lost profit in previous 7 days	68.58	496
	(184.90)	
Lost sales when cash on hand (but inadequate change) in previous 7 days	1.88	252
	(2.43)	
Lost sale while fetching change in previous 7 days	0.45	471
	(0.50)	
Total number of lost sales while fetching change in previous 7 days	2.17	467
	(7.09)	
Total time fetching change when could not break bill in previous 7 days (minutes)	135.16	494
	(186.60)	
Profits of business in previous 2 hours (Ksh)	138.82	410
	(165.10)	
Panel B. Estimated lost profit[a]		
Direct lost profits in previous 7 days	49.72	394
	(109.60)	
Total time fetching change when could not break bill in previous 7 days (minutes)	136.73	394
	(185.20)	
Total profits in previous 7 days	1577.33	394
	(1367.00)	
Average estimated lost profits[b]	119.37	394
	(205.80)	
Estimated lost profits as percentage of total profits		
Mean	0.08	394
Standard deviation	(0.09)	
Median	0.05	

Notes: The figures reported are from the first visit with firms. Means are reported in column 1, with standard deviations in parentheses. There were 508 total firms in the sample but data is missing for some firms on some variables in some weeks (both because the surveys changed slightly over time and because of survey errors).

[a] Figures in Panel B are only reported for firms with non-missing data on weekly profits and estimated lost profits.

[b] This is estimated by lost profits plus time spent fetching change (in hours) multiplied by estimated hourly profits (weekly profits/65 hours per week). The average labor hours per week reported in the background survey was 65.

businesses, the M-Pesa (mobile money) shop or the gas station for change.[13] It is not easy to prevent a changeout with a small change in pricing as a typical changeout in our data is a customer wishing to purchase goods for 20 Ksh with a 200 Ksh note.

2.3. Experimental design

To understand whether firms have changeouts in part because they are not aware of the profits they are losing, we conducted two main interventions. First, once firms were enrolled in the study, we visited them on a weekly basis to administer a short “changeout” questionnaire. This questionnaire asked firms a number of questions about change management, including the number of lost sales due to insufficient change in the previous 7 days, the value of these sales, how much time they spent searching for change, how often they gave or received change from nearby firms, and how much cash they brought in the morning. The survey also contained measures of total sales and profits.

The experimental design is based on the idea that the survey itself may serve as a catalyst for firms to start altering behavior, as lost sales and profits due to poor change management become more salient. To measure this, we randomized the start date at which we began to administer the survey to firms. Thus, a comparison of firms which started earlier (and had more reminders through the surveys) to those firms which started later (and had fewer reminders) provides an estimate of the causal effect of the survey on behavior.

To provide variation in the number of times a firm had been visited, we randomly started surveying firms in cohorts. There were 12 cohorts in total (9 in 2009–10 and 3 in 2011). Typically, new cohorts entered the study about every three weeks, though there is some heterogeneity in the gaps between cohorts. There is also heterogeneity in the number of visits firms received across cohorts: the mean number of visits to a firm was 12, with a minimum of 8 and a maximum of 18.

The second intervention more explicitly emphasized the costs of insufficient change. After collecting data for about 6 weeks, we calculated the lost sales for each firm and then visited firms to give them information on their lost sales (the average number of lost sales and associated lost revenue and profit, the frequency and duration of leaving their shop unattended while searching for change, and the amount of goods given out on credit due to insufficient change). If firms were unaware of how individual changeouts aggregate into total lost profits, this intervention may also induce changes in change management behavior. Since we could not provide firms with information without first surveying them, the effect of the information intervention is in addition to the effect of the weekly survey. We also provided information on the average amount of lost profits for firms in their market area. Keep in mind, however, that our sample within each market area has a diverse set of businesses. It is therefore likely that the individual's own information is much more informative than the market averages (which were not disaggregated by business type). Accordingly we view the intervention as primarily providing firms with information they already had but may not have processed.

The empirical strategy to estimate the effect of these two interventions on business practices can be summarized by the hypothetical example below, which is an abridged version of the study design:

	Week						
	1	2	3	4	5		6
Cohort 1 (Information)	v1	v2	v3	v4	v5	Information	v6
Cohort 1 (No Information)	v1	v2	v3	v4	v5	Intervention (Cohort 1)	v6
Cohort 2 (Information)				v1	v2		v3
Cohort 2 (No Information)				v1	v2		v3

In this example, visits to cohort 1 begin in week 1, cohort 2 begins in week 4, and the information intervention for cohort 1 is between weeks 5 and 6 (the information intervention for cohort 2 would be later). In the analysis, we consider the earlier cohort a “veteran” cohort and the later cohort the “novice” cohort.

Since businesses were randomly allocated to cohorts, we can estimate the impact of the reminders by comparing visits 4–6 in cohort 1 to visits 1–3 in cohort 2. The impact of the information intervention is straightforward: we would compare visit 6 for firms which received the information intervention compared to firms that did not among cohort 1 firms (cohort 2 firms would receive the information intervention at a later week not depicted in the figure). Empirically, we implement this in two ways: by comparing simple averages across cohorts or between those who receive information and those who do not; and

[13] While M-Pesa is a promising way to solve the change management problem, only 14% of the sample reported ever using M-Pesa with their customers during this time period — thus, the percentage of firms using it regularly is likely smaller. Indeed, these businesses were just as likely to experience a changeout at baseline as firms that did not use M-Pesa. Given that there are fees associated with M-Pesa use, it is likely not profitable to use it for small transactions.

by running fixed effects regressions which utilize the experimental variation to estimate effects while controlling for secular time trends. These strategies are discussed in more detail in Section 3.

2.4. Data, sampling, balance check and attrition

There are four main surveys used in the study: the “changeout” survey (which was discussed above), a background survey, a debriefing survey after the information intervention, and an endline. The background survey included demographic and background information, as well as risk and time preferences, access to credit, asset holdings, cognitive ability, and entrepreneurial disposition.[14] The debriefing survey, administered to a subset of respondents, was designed to make sure that people understood the calculations. At the end of the survey, we asked firms a few questions about whether they were surprised by the results, whether they intended to change their behavior and, if so, how. The final component of the data is a semi-structured endline survey in which we asked respondents questions about their perceptions of the change problem and how they manage change.

Table 2 demonstrates that characteristics are similar for firms across the 12 cohorts of the intervention, and among firms given the information intervention versus those who were not. The specification in Table 2 is a regression of the variable described in each row on cohort dummy variables, a dummy variable indicating whether the firm was sampled for the information intervention, and the variables used in stratification: market indicators and dummy variables for the three largest business types. Column 1 shows the coefficient on the dummy variable indicating whether the firm received the information intervention. As the table shows, most of the firm characteristics are balanced. Firms that received the information intervention are less likely to have a bank account and less likely to keep financial records, both differences which are marginally significant. Column 3 displays the *p* value from a joint significance test of all the cohort indicator variables. There are two characteristics which differ at the 5% level (risk aversion and whether the home has mud walls), and a third which differs at the 10% level (whether the firm employs salaried workers). These differences are likely due to random chance.

Given that this study involved finding firms at regular intervals to administer a survey, a natural concern would be that the results could be affected by attrition. In practice, we find that 97% of firms had at least 1 visit after the information intervention, and that attrition is not differential between the information and control groups. In addition, though not reported in the table, 98% of firms had at least one visit while they were in a more veteran cohort. The simple mean comparisons, which are described in more detail in the next section, are therefore unlikely to be driven by attrition.

The remaining concern would be that the firms that stay longer are naturally those with fewer changeouts. Note that there is not a tremendous amount of partial attrition: we only missed about 7% out of a total of 5180 visits which should have occurred.[15] Appendix Table A2 indicates that, once firm fixed effects are controlled for, neither of our treatments are associated with missing a particular visit, with point estimates near 0 and statistically insignificant. Veteran cohorts are actually more likely to be surveyed than novice cohorts.[16] Nevertheless, we instrument for the number of visits with the number of visits which should have occurred by that time period (which we call the “ideal” number of visits) in all of our fixed effects specifications.[17] We will also present the results using a variety of specifications, some which we think are less vulnerable to attrition bias than others, and will show graphically a dose response with number of visits and frequency of changeouts.

Table 2
Randomization check.

	Mean	Coefficient on information	*p*-Value of joint test of cohort variables	N
	(1)	(2)	(3)	(4)
Occupation: fruit and vegetable vendor[a]	0.24	−0.02 (0.04)	0.46	506
Occupation: other retail	0.37	0.03 (0.04)	0.69	506
Occupation: services	0.34	−0.02 (0.04)	0.13	506
Age	34.87 (10.47)	1.11 (0.95)	0.49	498
Female	0.56	0.01 (0.04)	0.24	494
Married	0.78	0.05 (0.04)	0.63	496
Number of children	3.29 (2.50)	0.13 (0.23)	0.66	487
Literate in Kiswahili	0.94	−0.03 (0.02)	0.51	487
Years of education	8.85 (3.64)	−0.40 (0.32)	0.28	497
Years business has been in operation	5.68 (5.01)	0.56 (0.47)	0.96	455
Has bank account	0.42	−0.08* (0.04)	0.74	498
Participates in ROSCA	0.70	−0.01 (0.04)	0.53	497
Keeps financial records	0.30	−0.07* (0.04)	0.13	492
Employs salaried workers	0.16	−0.01 (0.03)	0.06*	488
owns land	0.69	−0.01 (0.04)	0.29	498
Value of animals owned by HH (Ksh)	21539 (33718)	−479.31 (3037.16)	0.42	498
Value of household assets (Ksh)	27533 (25562)	−1709.28 (2167.61)	0.09	498
House has mud walls	0.62	0.04 (0.04)	0.02**	498
Ravens score (percentage correct)	0.40 (0.24)	0.00 (0.01)	0.85	498
Risk: amount invested of 100 Ksh[b]	61.39 (17.61)	0.30 (1.57)	0.01***	497

Notes: Each row (other than the occupation codes) is the regression results of the characteristics in the title column on: indicator variables for treatment cohort, a dummy for whether the firm received the information intervention, market fixed effects and business type controls (used in stratification). For the occupation variables, the business type fixed effects are omitted. Column 1 shows the coefficient on the indicator for the information intervention, and column 3 shows the *p* value for the joint test of significance of all the cohort dummies. ***, **, and * indicate significance at the 1%, 5%, and 10% level respectively.

[a] The remaining occupation category not shown in the table is “other.”

[b] Any amount invested in the asset paid off 4 times the amount invested 50% of the time and was lost completely the other 50% of the time.

[14] The survey was administered during the study and not prior to the changeout survey. Therefore we use time invariant characteristics or characteristics which are very unlikely to be affected by the two interventions to confirm balance was achieved through the randomization in Table 2.

[15] A comparable study with panel data with small firms is de Mel et al. (2008), where they interviewed 64% of all firms in all 11 rounds and 93–95% of firms with whom they completed at least 3 waves.

[16] The reason for this was that it was easier to find firms after establishing a regular schedule with them — once enrolled, firms essentially completed the survey in all weeks. Thus, if weeks in which firms are not interviewed are weeks in which firms are especially likely to have changeouts, we expected our veteran–novice comparisons to be biased downwards. However, given that attrition is so low, we do not expect this effect to be substantial.

[17] A related concern would be non-compliance with the information treatment. We check this in column (2) of Appendix Table A3. Ninety-five percent of those sampled for the information intervention actually received it.

3. Econometric strategy

We estimate the effects of our intervention using three different specifications. The first two show specifications which pool multiple rounds together to focus on simple mean comparisons which address the impact of the changeout survey and the information intervention in a transparent way. Our preferred specification uses the full nature of the randomization and the panel data (with firm fixed effects) to identify the effect of each visit and the information intervention.

3.1. Timing of cohorts

The changeout survey was administered to each cohort starting at a different time. Therefore, at any given time, we have a group of veteran cohorts who we have been following for some time and a group of novice cohorts who are new to the study. As a simple mean comparison, we divide each firm's observations into two groups: when it was in the "novice" cohort (when its cohort was the most recent to join the study), and when it was in a veteran cohort (and there were other firms who were newer to the project). We then average all observations for a given firm when it was novice and when it was the veteran cohort, and test whether these mean outcomes are different using the following specification:

$$\overline{y_{it}} = \beta_0 + \beta_1 veteran_{it} + \delta X_i + \epsilon_{it} \tag{1}$$

where $\overline{y_{it}}$ is the mean value for an outcome, such as the number of lost sales due to insufficient change, for firm i with tenure $t \in \{novice, veteran\}$ and X_i are controls for stratifying variables (the interactions of market identifiers and business type controls). β_1 tells us the effect of the survey and therefore the impact of making the change problem more salient to firms. In constructing these means, we exclude all firm-week observations after a firm has received the information intervention, so that we can be sure that the mean differences are not reflecting the fact that veteran firms are also more likely to have received the information intervention. Standard errors are clustered at the firm level.

There are several important issues to note. First, not all firms were interviewed in all weeks, nor interviewed for the maximum number of visits in a given cohort as described above. In fact, some firms were never interviewed during the "novice" or "veteran" period for their wave (in the period in which information had not yet been shared with treatment firms). [18] Second, some waves (those at the end of the two sampling periods) can by definition never appear as veteran cohorts (there are 100 such firms). We are therefore left with 866 observations in these regressions. Since Section 2.4 reveals no evidence of differential attrition by treatment status, a strength of this specification is that some types of selective attrition is unlikely to be driving the results since outcomes are averaged over multiple weeks.

3.2. Information intervention: means

The information intervention, which provided firms with information on the average amount of money they lost over the previous weeks due to insufficient change, was administered once among randomly selected firms. All selected firms in a given cohort received the information at approximately the same time. As a first look at the impact of the information intervention, we examine only the period after a particular cohort has received the intervention, and ask whether firms that were randomly selected to receive that intervention lose less sales than firms that were not. Similar to McKenzie (2011), we control for the mean pre-intervention outcome to reduce noise in the specification. Specifically, we regress

$$\overline{y_i^{POST}} = \gamma_0 + \gamma_1 I_i + \gamma_2 \overline{y_i^{PRE}} + \delta X_i + \epsilon_{it} \tag{2}$$

where $I_i = 1$ if the firm was sampled for information, and $\overline{y_i^{POST}}$ and $\overline{y_i^{PRE}}$ are firm i's mean value for the dependent variable, measured over the period following and preceding the information intervention, respectively. X_i are defined as above. As before, we lose some firms who did not complete surveys after the date of the information intervention (a total of 11 firms).

3.3. Fixed effects

The previous two specifications, while transparent, do not allow us to fully take advantage of the power inherent to the full design. Specifically, at any point in time, our randomization creates two random intensities of treatment: the number of visits that firms should have received, and whether or not that firm has received information. In principle, then, we can regress outcomes on how many visits a firm has received and whether it has received the information intervention, and both of these variables are exogenous, up to sufficiently flexible time trends. We can also include business fixed effects to limit the high degree of inter-business variability in the data. However, there is a small amount of non-random variation in actual visits — some of the firms we follow are quite mobile and could not be found every week (details discussed in Section 4). Thus, we instrument the actual number of visits with the assigned number of visits, and estimate the instrumental variables equivalent of the following specification:

$$y_{iw} = \gamma_1 N_{iw} + \gamma_2 I_{iw} + \mu_i + \lambda_m + \epsilon_{ivw} \tag{3}$$

where N_{iw} is the number of visits that firm i had received by week w, and I_{iw} is whether we had given the firm information by week w. μ_i are firm fixed effects and λ_m are month indicator variables[19]. Standard errors are again clustered by firm. The first stage, predicting actual visits with assigned visits, is highly significant with a minimum t-statistic over 90. Identification of the number of visits is coming from exogenous variation across cohorts in the number of assigned visits, while identification of the information effect (separately from time trends) stems from exogenous variation across cohorts as well as changes within cohorts.[20]

4. Results

In Sections 1–3, we discuss the impact of the two interventions on outcomes relating to changeouts, behavioral adjustments in change

[18] For example, for our measure of changeouts, 22 firms never appear as a novice and 28 never appear as a veteran firm in the periods in which information had yet to be delivered.

[19] We include month indicators since week dummy variables would be collinear with firm fixed effects and the visit count variable

[20] An important point is that this specification supposes a linear form in the effect of visits on outcome variables for simplicity. Given that changes in outcomes within a cohort before and after the information intervention also contribute to the estimation of that coefficient, it is difficult to interpret the coefficient on the information intervention as a "pure" effect of information versus the effect of information plus any non-linearity on the effect of repeated reminders. An alternative specification, omitted for brevity, which includes a non-parametric effect of repeat visits in order to isolate the information intervention effect by itself is available from the authors. The results are quite similar. Also see Fig. 1 and Appendix Fig. A1. Of course, even those estimates are hard to interpret as a "pure" information intervention effect because the intervention occurs within the context of firms that were already enrolled in the study, which is why we focus on the joint effects here.

Table 3
Changeouts and related lost sales.

	Had changeout			Number of changeouts			Lost sales while fetching change		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Veteran cohort	−0.06***			−0.87***			−0.99***		
	(0.02)			(0.19)			(0.23)		
Visit count			−0.02***			−0.14***			−0.08*
			(0.01)			(0.05)			(0.05)
Sampled for information		−0.08***			−0.54***			−0.19***	
		(0.03)			(0.15)			(0.07)	
Given information			−0.10***			−0.65***			−0.27*
			(0.03)			(0.20)			(0.14)
Pre-information average of dependent variable		0.48***			0.28***			0.26***	
		(0.04)			(0.03)			(0.02)	
Observations	866	497	4828	866	497	4823	857	495	4537
# firms	508	497	508	508	497	508	508	495	507
Control mean	0.52	0.42	0.54	2.60	1.52	3.37	1.51	0.52	2.19
Control std. dev.	0.39	0.36	0.50	4.08	2.18	6.73	4.22	1.14	7.12
Specification	Mean	Mean	FE-IV	Mean	Mean	FE-IV	Mean	Mean	FE-IV
Sample	No info	Post-info	Full	No info	Post-info	Full	No info	Post-info	Full

Notes: Columns 1, 4 and 7 are regressions of averages of the dependent variable on an indicator for being in a "veteran" cohort (i.e. a cohort that had started in an earlier wave and that had received more visits). The control group are firms that had started being interviewed later, i.e. the novice cohort. Averages are constructed over intervals in which both were being interviewed. Market identifiers and business type controls, variables used in stratification, are also included. The number of observations reflect the fact that the firm-week observations since a firm received the information intervention are excluded. Standard errors are clustered at the firm level. Columns 2, 5 and 8 present differences between firms that were sampled for the information intervention and firms that were not, after the information had been given (controlling for averages before the information had been given). Market identifiers and business type controls, variables used in stratification, are also included. Columns 3, 6 and 9 are regressions with firm fixed effects. To deal with partial attrition during the survey period, the number of visits is instrumented with the number of visits a firm should have received by that time. The first stage regression is strong, with a t statistic of over 90. Standard errors are clustered at the firm level. The mean and standard deviation are for a control group (novice cohorts in Cols. 1, 4 and 7, firms that were not sampled for information in Cols. 2, 5 and 8, and the firms' first visit in Cols. 3, 6 and 9). Standard errors are in parentheses.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

management, and profits/sales. The corresponding tables, Tables 3, 4, 7 and 8, show the three econometric specifications described above side-by-side for each outcome.

4.1. Changeouts

Columns (1) through (3) of Table 3 present results from our three empirical specifications where the dependent variable is an indicator for having experienced a changeout in the past week. Column (1) reveals that firms that have experienced our survey for some time are, on average, 6 percentage points less likely to experience a changeout in the last week than firms that we have just enrolled in the project. Since the likelihood of experiencing a changeout among novice firms (the control) is 52%, this is a 12% change. Column (2) reveals that firms that were randomly selected for our information intervention are similarly 8 percentage points less likely to experience a changeout than those that were not selected, after the intervention has taken place. This represents a 20% change compared to the control group of firms that never received the information.

Finally, column (3) demonstrates similar trends when business fixed effects are taken into account, utilizing the time series on each firm. The mean novice firm has been visited about 3 times, while the mean veteran firm has been visited about 7 times; our fixed effects estimates are thus broadly similar to the mean comparisons (though slightly larger in this case). We also show these results graphically in Fig. 1 and Appendix Fig. A1 for a host of outcomes. These figures show coefficients and associated standard errors for regressions of changeouts on dummies for the visit number. The dose–response relationship between the number of visits and the frequency of changeouts is striking, as it also is for many of the other key outcomes.

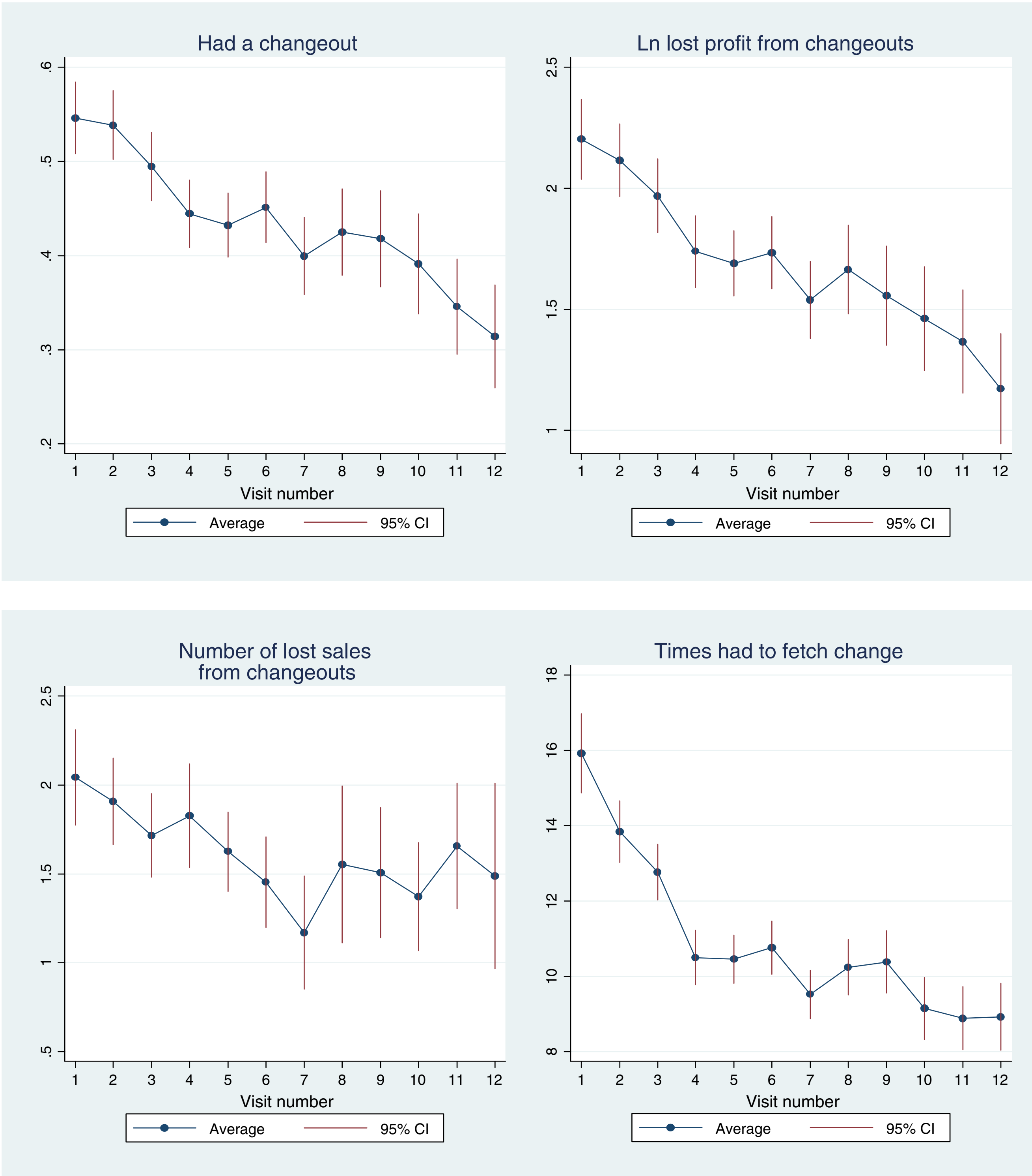
Columns (4) through (6) of Table 3 look at the number of lost sales experienced in the past week. Once again, we see that veteran firms in our study and firms that have received the information intervention both experience fewer lost sales than those that are newly enrolled or that have not received the information intervention. In particular, column (4) shows that veteran firms experience almost one fewer (0.9) changeout during this time, a 33% reduction, and firms that received the information intervention experience a 36% reduction in the number of changeouts. Moreover, this effect is similar whether we examine only mean differences or use the full fixed effects specification. Finally, treatment firms also lose fewer sales while away from the shop fetching change as a result of both interventions. These results are in columns (7)–(9), and the finding is robust across all three specifications.

Treatment firms also lose less income due to these lost sales, the direct effect of changeouts. Additionally, they also lose fewer sales

Table 4
Cash management.

	Ln Ksh brought in			Ln Ksh change brought in		
	(1)	(2)	(3)	(4)	(5)	(6)
Veteran cohort	0.13*			0.25		
	(0.08)			(0.16)		
Visit count			0.01			0.05
			(0.02)			(0.07)
Sampled for information		0.06			−0.12	
		(0.10)			(0.18)	
Given information			0.11			−0.09
			(0.08)			(0.20)
Pre-information average of dependent variable		0.71***			0.29***	
		(0.04)			(0.06)	
Observations	865	497	4805	326	199	1203
# firms	508	497	507	199	199	199
Control mean	5.25	5.41	5.19	3.14	3.55	3.13
Control std. dev.	1.50	1.53	1.87	1.54	1.45	2.08
Specification	Mean	Mean	FE-IV	Mean	Mean	FE-IV
Sample	No info	Post-info	Full	No info	Post-info	Full

Notes: See Table 3 for notes on specifications. The exchange rate was rougly 80 Ksh to $1 USD during sample period. There are fewer observations in columns 1–3 than in other tables because we only asked about the amount of change brought in to work in the latter part of the sample. Standard errors are in parentheses and clustered at the firm level in columns 1, 3, 4 and 6. ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.



Notes: The figures show plots of regressions of the variable described in the figure title on dummy variables for visit count from 1 to 12. The figures were restricted to 12 visits since that was the mean number of visits to a firm - there are fewer observations with visits >12 - and for space considerations.

Fig. 1. Trends in changeouts by visit number. Notes: The figures show plots of regressions of the variable described in the figure title on dummy variables for visit count from 1 to 12. The figures were restricted to 12 visits since that was the mean number of visits to a firm – there are fewer observations with visits >12 – and for space considerations.

while away from their shop to get change during the day, an important indirect effect. Appendix Table A1 shows in columns (1)–(3) that the information intervention reduced the value of lost sales (revenue) and lost profits from changeouts in columns (4)–(6). The impact of the information intervention is to reduce lost revenue by 42% and lost profits by around 33%. Both the veteran firm specification and the visit count coefficient in the full specification show that being surveyed reduced reported lost revenue and profits.

Table 5
Audit data quality.

	(1)	(2)	(3)	(4)
	Cash on hand in audit		Change on hand in audit	
Reported cash on hand	1.02***	1.01***		
	(0.02)	(0.03)		
Reported change on hand			0.97***	1.01***
			(0.04)	(0.05)
Firm fixed effects	No	Yes	No	Yes
Observations	522	522	519	519
# Firms	199	199	199	199
Mean of dependent variable	1254	1254	130	130
Std. dev. of dept variable	2352	2352	217	217

Notes: Data from audits conducted in March–April 2011. Standard errors are in parentheses and clustered at the firm level. ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table 6
Audit.

	Ln change on hand	Ln cash on hand
	(1)	(2)
Veteran cohort	0.39**	0.66***
	(0.18)	(0.22)
Given information	−0.15	0.02
	(0.16)	(0.20)
Observations	522	522
# Firms	199	199
Control mean	3.70	5.41
Control std. dev.	1.97	2.45

Notes: Data from audits conducted in March–April 2011. See Table 4 for notes on definitions of independent variables. Standard errors in parentheses.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

4.2. Behavioral adjustment

Firms can adjust their behavior in a number of ways in order to limit the frequency of changeouts. They can bring more cash into work in the morning; they can monitor their change flows over the day; they can prioritize change for higher profit sales; or they can choose to participate or withdraw from change-sharing relationships among nearby entrepreneurs.

Columns (1) through (3) of Table 4 look at the log quantity of Kenya Shillings in cash brought into work in the morning, while columns (4) through (6) examine the log quantity of change (coins) brought in the morning. In all cases in which we take a log transform, we account for zeros in the underlying measurement by adding one to the measurement before taking the log. Both dependent variables demonstrate weak evidence that our intervention precipitated an increase in cash brought into work. In particular, on average, veteran firms bring in about 13% more cash and 25% more change than newer firms, though only the former coefficient is statistically significant. There is no evidence, however, that the information intervention affected the amount of change brought in, as the standard errors are large. Columns (7)–(9) of Appendix Table A1 shows that the number of changeouts which occur while firms have cash on hand – just not the right bill denominations – also declined, consistent with improved change management. All in all, we can conclude that there is some weak evidence that firms are bringing more cash into work.

A concern with the results described above is that the outcomes are all based on self reports, which may be affected by reporting bias. In order to address this, we "audited" all firms to provide an objective measure of behavioral adjustment. After asking them how much cash and change they had on hand as done in the changeout survey, we paid firms a small amount to show us all the money they had on hand, by denomination. Change is defined as coins, which are denominations up to 20 Ksh. Table 5 shows reassuring evidence that firms tend to report cash on hand truthfully: the objective measures of cash on hand and change on hand are both significantly correlated with reported cash/change on hand.

We also use this as an objective measure of cash management to look at the impact of the two interventions on the amount of change on hand as measured during the audit. As we have only a small number of audits per firm, we focus on the specification

$$y_{it} = \beta_1 veteran_{it} + \beta_2 info_{it} + \delta_t + \varepsilon_{it}$$

where standard errors are clustered at the firm level. This specification compares cohorts which started earlier in a given interval to firms which started the survey later. Table 6 shows that the changeout survey led to both more cash on hand and more change on hand.[21] This provides additional evidence that firms are providing fairly accurate information on cash on hand.

A second dimension of possible behavioral adjustment is in how often firms share change with other market members. Table 7 examines this possibility, where columns (1) through (3) examine how frequently the business received change from other businesses and columns (4) through (6) examine how many times the business gave change to other businesses over the past week. Veteran firms in our intervention receive change on average 2.4 fewer times per week and share change with other businesses on average 1.1 fewer times per week. Similarly, upon receiving the information intervention firms begin receiving and giving out change an additional 1.7 fewer times per week. The fixed effect specifications in columns (3) and (6) provide qualitatively similar results. Therefore, we observe firms adjusting their sharing behavior in response to our intervention. Logically, as these firms are sharing change primarily with other nearby firms, we may anticipate the presence of spillovers in these variables in particular. In Appendix A, we document that the number of times the business received change from nearby firms is one of few variables with precisely estimated spillover effects. Table A4 shows that having more treated firms nearby *increased* the number of times control firms were receiving change from nearby firms. This suggests that our estimates on sharing change can be interpreted as both a reduction in sharing behavior by treatment firms and an increase in sharing behavior by non-treated firms. For example, if treatment firms are refusing to share change because of the salience of treatment, the overall pool of potential firms who serve as change sources would decrease, which could contribute to the higher frequency change sharing of untreated firms.

4.3. Profits

The above evidence suggests that firms became more productive, as they were losing fewer sales – and associated profits – from changeouts and while away fetching change. We look here at whether total reported profits have risen. We measured profits through direct elicitation, following de Mel et al. (2008, 2009b) that calculating profits ourselves using revenue and expenses is less reliable than asking firms directly for profits. In theory, this measure should capture a wide variety of costs including various costs of holding change, and any change in the ability to increase the price of an item when the customer has insufficient change. It does not, however, capture the owners' own time but

[21] The information intervention is not correlated with this objective measure of change or cash on hand in this sample (standard errors are large in both cases). Part of the reason for this may be due to differences between the 2011 and 2009–10 sub-samples. Across the board in our data the association between the information intervention and changeholding is much stronger in the 2009–10 sub-sample. Since these are different markets and different time periods, we cannot interpret these differences in a meaningful way.

Table 7
Change sharing.

	Times received change from other business			Times giving change to other businesses		
	(1)	(2)	(3)	(4)	(5)	(6)
Veteran cohort	−2.42***			−1.08***		
	(0.48)			(0.34)		
Visit count			−0.44***			−0.17**
			(0.11)			(0.08)
Sampled for information		−1.68***			−1.66***	
		(0.50)			(0.52)	
Given information			−1.51***			−0.76**
			(0.44)			(0.33)
Pre-information average of dependent variable		0.49***			0.53***	
		(0.04)			(0.04)	
Observations	808	495	4483	815	495	4449
# Firms	505	495	507	507	495	507
Control mean	13.09	9.51	14.31	9.72	9.51	10.42
Control std. dev.	8.94	7.55	10.59	6.31	7.55	7.47
Specification	Mean	Mean	FE-IV	Mean	Mean	FE-IV
Sample	No info	Post-info	Full	No info	Post-info	Full

Notes: See Table 3 for notes on specifications. Standard errors are in parentheses and clustered at the firm level in columns 1, 3, 4 and 6.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

should capture wages paid to other workers. We will discuss below potential shortcomings to our measure. We capture profits in two ways: firms were asked about total profits in the past 2 hours and over the previous 7 days. We also asked firms about the quantities sold in the past 2 hours. As in the rest of the literature, our data on firm profits is very noisy. We anticipate the reports from the previous two hours to have less measurement error than the entire last week, though at the cost of actual profits being more variable at the finer interval. Our firms are quite heterogeneous in profit scale and variability. In an effort to prevent large outliers in profits from driving our estimates, we use log profits[22]. We also estimate firm-specific means and variance across our visits and trim profit and sales observations where logs of the dependent variable are more than two standard deviations above or below the mean. This procedure omits between 2.6 and 3.1% of observations across the three variables.[23]

Columns (1) through (3) of Table 8 examine log sales over the past two hours, while columns (4) through (6) examine log profits in the past 2 hours and columns (7) through (9) examine weekly profits. In all the cases, the data are too noisy for the simple mean comparisons to deliver significant and robust differences across treatment and control firms, with additional visits even being associated with a small but negative effect on weekly profits which is marginally significant (at the 10% level). However, once firm-specific heterogeneity is controlled for through fixed effects, we identify a positive and significant (at the 5% level) increase in profits (both weekly and over 2 hours) and sales associated with the information intervention. Firms that received our intervention report a 15% increase in sales and profits over the past 2 hours, and an 8% increase in profits over the past week.[24] This is a large estimated effect, and the standard errors are also large. The confidence interval on profits in the previous two hours, for example, includes estimates from 3 to 27%. Our back-of-the-envelope calculation of the total volume of profits which are lost due to the changeout problem was around 5–10% including the direct losses from changeouts and indirect losses from time away from the store while fetching change. There is substantial overlap in the confidence intervals of the two estimates, in part because both are quite noisy. In the fixed effect specification, the relationship between additional visits and profits is insignificant across the board with uniformly small point estimates. As a result, we conclude that responding to the information intervention led to a positive effect on profits and suggests that responding to the survey did not lead to large negative impacts on profits. It also suggests that firms were not holding low change stocks in order to increase profits through upselling, at least in our context.[25]

It is important to emphasize that these profit results derive from self-reported, short-run profit measures. We have already discussed how classical measurement error may hinder inference in this context, and indeed this problem is pervasive in the entrepreneurship literature. Very few business training papers have found significant effects on profits (see McKenzie and Woodruff (2012) for a review of this literature). Nonetheless, there are several aspects of profit mismeasurement which may be particularly germane to our context. First, these are short term profit estimates rather than long run. In Section 5, we discuss the potential of equilibrium behavior change in the long run to change the fundamental profitability of change management. It is also possible that some behaviors which improve change management negatively impact profits in the long-run, for example if there are benefits to visiting fellow shop keepers, such as learning about new products or

[22] We take the log of (profits + 1) to avoid losing observations in which profits are zero. Appendix Table A7 shows additional specifications.

[23] It is well established in this literature that profits of micro-enterprises are measured with a significant amount of noise, as discussed in de Mel et al. (2009b), Bruhn et al. (2012). Untrimmed results are qualitatively similar, though sometimes less precisely estimated (for example, the point estimates on 2 hour sales and profits are similar in magnitude and the information intervention is significant at the 10% level). Our procedure is analogous to the approach in de Mel et al. (2008, 2009a), who trim observations where changes in the profit measure are in the top 1% in levels or logs. That approach has the advantage over our approach that long-run steep trends in profits do not become trimmed, which could happen with our trimming. However, our approach has the advantage that negative and positive outliers are treated symmetrically — in the simple differenced approach, if there is a severe negative outlier, it remains in the sample (the difference is not in the top 1% in levels or logs) but, presuming mean reversion in the following observation, that following (less aberrant) observation is trimmed. While we anticipate meaningful effects from solving the changeout problem, we do not anticipate these effects to be of comparable magnitude to long-run trends in profit growth. As a result, we prefer the symmetric trimming approach.

[24] Since these variables are measured over the last 2 hours and there are time-of-day trends in profits and sales, there is the potential for time of day to be an important omitted variable. However, similar fixed effects regressions find no significant relationship between visit count and information intervention in the time of day we visited firms, and these results are if anything stronger including hour of day fixed effects.

[25] This is not surprising since a typical changeout is a customer attempting to purchase a 20 Ksh item with a 200 Ksh bill. It is unlikely that having no change (or claiming not to) could raise the price that dramatically. Moreover, Appendix Table A6 also indicates that according to the endline survey, only 28% of firms report upselling and, even for them, this occurs only 7% of the time.

Table 8
Profits.

	Ln sold last 2 hours			Ln profits last 2 hours			Ln profits last week		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Veteran cohort	−0.04			−0.04			−0.06*		
	(0.07)			(0.07)			(0.03)		
Visit count			0.01			0.00			−0.01
			(0.02)			(0.01)			(0.01)
Sampled for information		0.02			0.02			0.03	
		(0.08)			(0.07)			(0.05)	
Given information			0.15**			0.15**			0.08**
			(0.07)			(0.06)			(0.04)
Pre-information average of dependent variable		0.65***			0.64***			0.81***	
		(0.03)			(0.03)			(0.03)	
Observations	862	497	4651	813	497	4369	811	497	4422
# Firms	508	497	508	507	497	507	506	497	506
Control mean	5.54	5.64	5.57	4.28	4.43	4.30	7.00	6.92	7.01
Control std. dev.	1.44	1.35	1.67	1.21	1.13	1.40	0.84	0.80	0.94
Specification	Mean	Mean	FE-IV	Mean	Mean	FE-IV	Mean	Mean	FE-IV
Sample	No info	Post-info	Full	No info	Post-info	Full	No info	Post-info	Full

Notes: See Table 3 for notes on specifications. The exchange rate was rougly 80 Ksh to $1 USD during the sample period. Standard errors are in parentheses and clustered at the firm level in columns 1, 3, 4 and 6.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

changing market conditions, which only bear fruit over long time intervals. If firms do not account for the present value of these interactions – and indeed, question wording may have led even the most sophisticated firms from accounting for these benefits – then profits are mismeasured in the short run. Second, it is possible that with limited attention, our measure of profits is inaccurate as firms overlook some other aspect of the business. If, for example, firms were not internalizing the carrying costs of holding more cash, then we may overstate the impact of the interventions on profits[26].

We cannot speak directly to either of these mismeasurement possibilities, except through the more qualitative evidence presented in detail in Section 4. The specificity of our intervention does, however, allow us some leverage in interpreting these data. Unlike studies which focus on more general business training, this intervention targeted a very specific behavior and finds strong evidence that people carried more change as a result. Respondents, therefore, drew an inference from our interventions that it would be profitable to hold more change, which is consistent with the reported data on profits[27] and inconsistent with the idea that exposure to changeout risk is profitable. These mismeasurement issues fundamentally also suggest the presence of limited attention. Suppose that firms are mistaken about the profitability of changeholding in response to our intervention because they do not account for other costs of holding more change. This means we cannot say that holding more change increases profits for firms, but the results would still suggest an inattention problem: if firms were fully attentive, they would not mistakenly think profits had gone up.

4.4. Supporting evidence from semi-structured interviews

To supplement our main results, we performed two "debriefing" surveys during the experiment. The first was administered to a subset

Table 9
Evidence from information debriefing and semi-structured endline.

	(1) Mean	(2) N
Panel A. Information debriefing		
After going through this calculation:		
Would you say that change is a bigger problem than you thought?	0.83	48
Would you say that holding more change has higher returns than the next-best alternative?	0.84	45
Do you plan to change your behavior?	0.85	48
If yes, how:		
Get change in the evening before going home	0.23	43
Get change in the morning before work	0.09	43
Get change in advance of running out during the day	0.28	43
Hold more cash in change (and invest less in something else)	0.12	43
Get change more regularly during day	0.28	43
Panel B. Endline survey		
Have the visits changed your thinking?	0.86	189
Have the visits changed your behavior?	0.75	189
If yes, how?		
Get change in the morning	0.25	142
Get change in the evening	0.12	142
Bring more change to work	0.47	142
Stock less inventory	0.00	142
Manage change better	0.06	142
Give out less change	0.42	142
Give out less credit because don't have enough change	0.11	142
Are you less likely to give out change now?	0.36	188
Do you think you made more money after changing behavior?	0.62	186
If so, how?		
More sales	0.50	116
More big sales	0.22	116
Lose less customers leaving shop unattended	0.60	116
Greater customer loyalty	0.07	116
Give out less credit	0.28	116
Do you think you made more money immediately?	0.70	116
Did you lose some money in adjusting behavior?	0.03	116
Is deciding how much change to bring difficult?	0.23	186
Have you started paying attention less to something else?	0.01	188

Notes: The data in Panel A were collected at the endline at the conclusion of the project. The data in Panel B were from the debriefing after information intervention. Note that the debriefing was conducted only with firms in the later part of the sample.

[26] While it is hard to measure all other aspects of the business, our semi-structured endline surveys suggest that this did not happen. While we discuss those results in more detail in Section 4.4, we note here that 62% of firms reported that profits increased and only 1% reported that paying attention to change crowded out attention on something else.

[27] The fact that firms report higher profits also help to rule out the possibility that respondents were managing their business optimally *ex ante* and changed behavior due only to Hawthorne effects at some cost to their business. It may be the case that these higher profits can only be accessed in the presence of surveying, consistent with rational inattention models such as Sims (2003). In that case, firms could react to the newly processed information by re-optimizing and increasing profits even if they were correctly optimizing given the information and beliefs about the returns to changeholding they had prior to our interventions.

of individuals after they had received the information intervention. The second was administered to a subset at the very conclusion of their participation, and included both those who received information and those who did not.

Panel A of Table 9 presents results from the debriefing interview after the information intervention. Generally, the results are supportive of our main findings. Interestingly, nobody reports that they do not keep change because the returns are higher to some other investment (such as inventory). People were much more likely to say it was because it was hard to get change or that it takes a long time to get it. Also consistent with our results, people seemed to be "surprised" by the results of the study — 83% reported that the amount they lost due to changeouts was more than they had anticipated, and 84% said that holding more change had higher returns than the next-best alternative investment. Finally, 85% reported that they planned to adjust their behavior after the intervention, with most saying that they would be sure to get change either after they finished work in the evening or before they started work the next day. Only a minority reported that they would have to take money out of other investments.

Panel B of Table 9 paints a similar picture in regard to the monitoring visits themselves. Eighty-six percent of individuals report that the visits changed the way they think about change, and 75% report that they modified their behavior. As with the information intervention, the most common adjustments were to get more change in the evening or the morning, and to bring more change to work from home. Forty-two percent of individuals also say they are now less likely to give out change. Perhaps more importantly, the responses are supportive of our finding of an increase in profits: 62% report making more money ultimately from these behavioral changes, and only 3% of these individuals report that there was any adjustment period to higher profits. At the same time, relatively few people (23%) report that deciding how much change to bring is difficult, potentially suggesting that it does not require major changes in business practices to adjust change behavior.

An important question we do not answer in this paper is whether the increased attention on change leads to declines in attention in other aspects on the entrepreneur's life, as is suggested in Banerjee and Mullainathan (2008). What we can say is that the evidence that the information intervention increased profits suggests that declines in attention in other aspects of the business did not come at significant business cost.[28]

Finally, the endline survey gives some insight into other questions raised by this research. First, Appendix Table A5 shows that most individuals save their profits for their day at home (at least for a few days), and that many people report difficulty saving. Only 64% of people separate their business and personal cash, and 87% report spending more on good days. These figures (which are very similar to those reported in Kremer et al. (2011)) show how difficult it is for people to save money when profits are entirely in cash and must be saved at home. It seems conceivable that people may find it hard to adequately set aside money for change for the next day.[29]

5. Discussion

Firms may choose to not hold large quantities of change for a variety of reasons, many of which would be consistent with standard firm models of optimizing behavior, some of which are profitable in the short run (for example, because the costs of holding change outweigh the benefits); others profitable in the long run (for example, through dynamic equilibrium effects or risk-sharing equilibria), and others which are unprofitable at any horizon. In this discussion, we want to consider seriously the possibility of mismeasurement in our profit estimates, so that the estimates in Section 3 do not rule out explanations which are unprofitable. Indeed, an inherent limitation in this study is its reliance on self-reported outcomes. This is a common problem among papers evaluating the impact of various treatments such as business training (Berge et al., 2011; Field et al., 2010; Karlan and Valdivia, 2011; Klinger and Schuendeln, 2011), microfinance (Banerjee et al., 2010; Crepon et al., 2011; Karlan and Zinman, 2010), and the returns to capital (de Mel et al., 2008, 2009a; McKenzie and Woodruff, 2008). As discussed in Section 2, we were able to use an objective measure of behavioral change using an audit of firms to capture their change and cash on hand. This suggests that changeholding did change, at least in response to multiple visits, and we consider only explanations which are consistent with increased changeholding.

We devote this section to discussing potential reasons that behaviors may have changed in response to our intervention, and what that suggests about the underlying motivations for changeouts.

5.1. Unprofitable behavior change

There are two classes of unprofitable behavioral change which merit comment: behavioral change which is profitable in the short run but unprofitable in the long run (which, given the short-run nature of our survey, would be consistent with all results presented thus far), and behavioral change which is unprofitable even in the short run.

5.1.1. Behavioral change which is unprofitable in the short run

While we were able to objectively validate self-reports of changeholding, profits are not easy to objectively verify. As we discussed in Section 3, there are a number of factors which firms may not consider, such as the cost of additional labor or unanticipated future costs of opting out of change sharing arrangements, which may have lead to overestimates of profits among treated firms. This suggests that the potential remains that firms adopt potential changeholding despite it being privately unprofitable in the short run. Many of the potential explanations for the changeout problem are consistent with changeouts being profitable in the short run. These include the possibility that scarce resources are better utilized on other investments, for example inventory; that the costs of acquiring change outweigh the gains from reducing changeouts; and that the potential for theft leaves firms averse to holding change. In the event that firms were not holding change due to a careful consideration of the costs and benefits associated with changeholding, our interventions should not have affected this decision under conventional economic modeling. Our interventions did not change the costs of change acquisition, nor did they change the potential benefit to avoiding a changeout except through equilibrium effects, which should not increase the potential benefits of changeholding.[30] For these explanations to be the sole cause of changeouts, then, our results are only consistent with a model of firm behavior where being surveyed induces firms to adopt behaviors that they know to be unprofitable. While this sort of firm behavior is difficult to reconcile with most conventional economic modeling, we cannot rule it out.

5.1.2. Behavioral change which is unprofitable in the long run

A second interpretation of our results is that solving the changeout problem is only profitable in the short run and costly in the long run, and that our interventions induced myopia among sample respondents. We should thus think about changeouts in dynamic equilibrium. One

[28] Attention declines at home are harder to assess. When we asked our entrepreneurs directly whether they diverted attention from other considerations, and only 1% responded that they did (see Table 9). Of course, this question may be difficult to answer, and as a result should be interpreted with caution.

[29] Panel B of Appendix Table A6 shows that inventory management problems are not limited to change — shops commonly stock out of products as well.

[30] If more nearby firms hold change, then there are fewer local sales which result in a changeout, shrinking the number of potential sales available to a firm with adequate change stocks. We discuss the role of potential dynamic equilibrium effects below.

explanation which would be consistent with this is if profit-loss to changeouts are shared within the marketplace, so that all lost sales caused by changeouts are absorbed by nearby firms. Our profit results, then, would indicate that increased changeholding allows treatment firms to capture more sales from their neighbors in the short run.

In Appendix A we discuss our spillover analysis, which does not show direct evidence of negative spillovers on nearby firms' profits. We suspect that we lack sufficient statistical power to detect such spillovers. Semi-structured interviews with respondents suggest that most customers who experience a changeout simply purchase the item from another vendor (see Panel A of Appendix Table A5). Firms may therefore respond by holding more change to capture more of total aggregate sales available. However, if their neighbors began holding additional change as well, then revenues would presumably decrease as treatment firms lose their competitive advantage.

In this light, one can consider the decision to hold change to be analogous to the classic prisoner's dilemma problem: if firms in a marketplace all hold change, then on average they receive the same revenues as if none of them do but pay the additional cost of maintaining change stocks. Firms therefore would prefer to collude with each other to avoid holding change. Given that changeouts impose costs on customers too, collusion in low changeholding may be a means of collectively taking some consumer surplus. However, firms each have an incentive to deviate from this equilibrium to maximize their share of industry-wide profits. Seen in this light, there are several potential interpretations of our result: first, it is possible that we suggested a new deviation to firms which had not previously occurred to them which is profitable in the short-run. We hold this explanation to be consistent with the role of limited attention: they were not being sufficiently attentive to devise a strategy for gaining the largest share of available industry-wide profits possible. An implication is that inducing behavior change among some firms may push the market towards the unpleasant equilibrium where everyone must hold change in the long-run. Second, it is possible that firms purposely maintain the preferred equilibrium through a system of guaranteed punishments for firms that hold ample change, for example through the threat of moving to the no-changeout equilibrium for some time. In that case, firms may have been well aware of the potential short-run gains to changeholding but preferred not to risk long run punishments to achieve them. If so, given that our interventions were effective at reducing changeouts, we would infer that being surveyed and provided information induced study firms to value short run profits at the cost of a long run risk of punishment from other nearby firms in a way they had not previously.

Our experiment did not track firms over the long-run to observe the eventual equilibrium. Unfortunately small firms in Kenya exhibit high turnover (Keats, 2012), and our sample is no exception. As a result, collection of long-run profit data were not possible, and we cannot make a definitive statement as to how changeholding equilibria responded to our intervention. However, it is not clear why being surveyed or provided information would induce sample firms to expose themselves to punishments from other firms if that deviation was not profitable in present value, given that study interventions did not change the underlying incentive structure.[31] Moreover, the same turnover that prevented collecting long-run data also suggests that cooperation is likely to break down among firms in our setting: if firms are unlikely to stay in a particular marketplace over time, the range of potential punishments must lessen. Still, while we have little reason to expect that our interventions changed firms' intertemporal preferences, we cannot rule out this possibility.

[31] One explanation which would be consistent with this is if orienting attention towards changeouts made firms forget about the equilibrium punishment structure, which would again be consistent with a model of limited attention at work.

5.2. Profitable behavioral change

The final set of explanations we consider is under the assumption that our measured behavioral change and profits are accurate. We interpret these results as most consistent with our interventions drawing entrepreneurial focus towards the changeout problem, which may be profitable. The results are then evidence of a set of explanations consistent of learning, aggregation cost and inattention models.

One interpretation of the results is that firms are learning over time about the returns to holding change in response to the survey and the information treatment. The survey in particular provided no new information to respondents but instead just elicited the information that firms already had. If the increase in salience about change leads firms to learn about its importance, we believe that this type of learning story falls within the broad class of models of inattention as firms had previously failed to learn because they were inattentive towards change. While the information intervention may be providing new information on market averages, the survey itself is hard to square with a learning story that doesn't have attention at its core as the underlying constraint.

The hypothesis that we find most consistent with the observed patterns of behavior change is that the interventions made the change management decision more salient and induced the owners to at least partially process the information that was already available to them or incorporate that information into their decision process. This hypothesis is consistent with both the results on behavior change and profits, and a broad literature on limited attention. An inability to process all available information is at the core of many models of inattention, including models of rational inattention (Sims, 2003) and the bounded rationality model of Gabaix and Laibson (2005) incorporates a cognition cost when agents choose to devote time to reducing uncertainty about a given decision problem. It is difficult if not impossible for us to determine whether our study firms were optimally allocating attention, particularly given Banerjee and Mullainathan (2008) argues that the owners of the firms are also choosing whether to use their finite attention on business decisions versus important matters at home. Our results are also with consistent with other empirical evidence on the role of salience (Chetty et al., 2009; Finkelstein, 2009), which support models of shrouded attributes DellaVigna (2009). These models are in principle difficult to distinguish empirically (which DellaVigna (2009) notes in his survey piece), and our analysis is no exception.

6. Conclusion

In this paper, we focus on a simple business decision that must be made on a regular basis by small firms in Western Kenya — how much change to keep on hand to break larger bills. We document that firms consistently lose a number of sales, both directly and indirectly, by having insufficient change stocks. Using two simple interventions, we provide evidence which is consistent with the idea that some business decisions may not be fully attended to. The interventions make the change management decision more salient and induce the owners to at least partially (cognitively) process the information that was already available to them, but do not change the financial costs or benefits of changeholding behavior, except potentially through equilibrium responses. The results show that both interventions increased changeholding and resulted in fewer lost sales due to insufficient change and a reduction in lost profits. We interpret this behavioral change as most consistent with a model where limited attention impacts business profitability in negative ways. Though having change is a relatively straightforward aspect of running a business, it may be one that is overlooked if owners are constrained in the amount of attention they can dedicate to the management of their business.

There are some key limitations to the study, including our inability to track firms over time to see if the changeholding behavior persists beyond a few months. We primarily rely on self-reports on change management practices and sales/profits, and our measure of profits is very noisy, though we confirm changes in changeholding behavior through an objective audit measure.

Despite these limitations, our focus on change management provides some insights into underlying constraints facing small firms in a developing country context. The particular business decision that we study is surely not the most important factor affecting micro-enterprises' profitability. It is, however, a nontrivial contribution to profits and a good candidate for a decision affected by limited attention, since other constraints seem less relevant and improvements are not difficult to ascertain and implement (in contrast to, for example, inventory management). While we cannot definitively determine that limited attention, much less the precise model of limited attention, is causing firms to lose out on profits, our results suggest that directing attention towards particular business practices (which is a component of all business training interventions) may increase firm productivity.

Appendix A

A.1. Spillovers and equilibrium effects

We anticipate that our treatments may have had two types of spillovers, leading to equilibrium effects and potentially affecting the interpretation of our estimates. First, firms that are being treated may discuss our changeout survey with other nearby firms. To the extent that discussions which take place between entrepreneurs serve to remind untreated firms of the changeout problem, we may expect to see attention constraints slacken market-wide when our enumerators conduct interventions. Second, changes in treatment firm behavior may directly affect firms that are not being treated. Specifically, if firms are capturing a larger fraction of potential sales due to improved change management, then that may result in other nearby firms experiencing fewer sales. These two types of spillovers should have opposite effects on our estimates: learning spillovers would result in conservative estimates; in contrast, spillovers due to behavioral changes of treated firms would indicate that our treatments affected market equilibria, so that our estimated differences are averages of the improved outcomes for treatment firms and the worsened outcomes for control firms.

In the 2011 wave of the survey, we collected GPS coordinates of study firms, allowing us to determine the effects of being near treatment firms on various outcomes. We focus on differences in reported changeouts and behavior during firms' first visit as a function of neighbors' treatment status since these visits serve as the cleanest control group.[32] The experimental design ensures that on each firm's first visit there is a random fraction of the other nearby firms who have already been enrolled into our study. For different outcome measures y_{i1}, we therefore regress:

$$y_{i1} = \beta_1 NumCloseFirms_{i1} + \beta_2 ShareTreated_{i1} + \lambda_t + \varepsilon_{i1}$$

where λ_t are month indicator variables as before, $NumCloseFirms_{i1}$ indicates the number of firms within 100 m of firm i, and $ShareTreated_{i1}$ indicates what fraction of those firms are already enrolled in our study.[33] Appendix Table A4 presents the results of this estimation. We use Conley (1999) standard errors to allow for spatial correlations, with a cutoff threshold of 200 m. Standard OLS errors yield qualitatively similar conclusions.

There are some important spillovers from our treatment on other nearby firms. First, columns (1) and (2) reveal that firms experience fewer changeouts when other nearby firms are treated, and the effect on the number of changeouts is significant at the 5% level. This would lend support to the learning channel for potential spillovers, and suggest that our estimates underestimate the effects of treatment on changeouts.[34] There are no reported differences between firms in more or less dense treatment areas on the amount of cash or change that entrepreneurs are bringing into the shop, suggesting that this learning does not affect behavior on that dimension. However, columns (5) and (6) reveal how these firms are nonetheless able to reduce changeouts: they are sharing change more frequently (by both giving and receiving change more times per day, though only the latter is statistically significant).

This suggests that our estimates on sharing change can be interpreted as both a reduction in sharing behavior by treatment firms and an increase in sharing behavior by non-treated firms. The results are consistent with the following interpretation: the learning effects appear to be insufficient to change practices outside of the work place (since firms do not adjust the amount of cash brought in), but result in a higher frequency of transactions at the work place. We note that if treatment firms are refusing to share change because of the salience of treatment, the overall pool of potential firms to get change from would decrease, which could contribute to the higher frequency change sharing of untreated firms. Finally, columns (7) and (8) reveal that there are no spillover effects on profits that we can estimate.

A.2. Functional form choices on profits

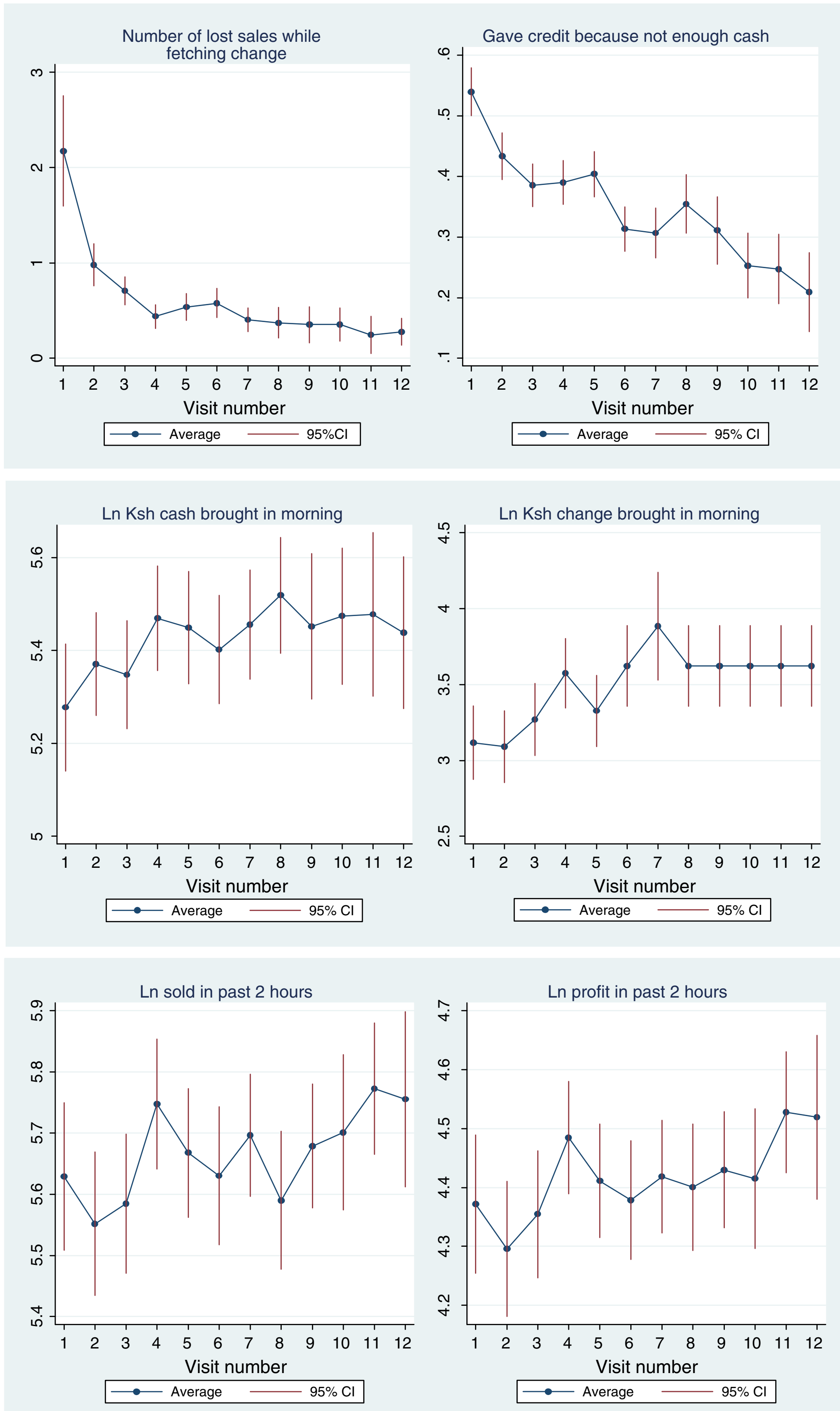
In the main text of the paper, we measure profits in logs rather than levels. Some of the literature on entrepreneurship in development countries prefers a level effect for structural reasons, e.g. de Mel et al. (2008, 2009a) argue they expect a level change in response to an exogenous increase in capital. In our case, we anticipate that better change-holding practices would increase the fraction of small-value sales completed at many shops, making the levels versus logs choice much less clear. We prefer the logged specification for statistical reasons: sample firms are quite heterogeneous in size, and in some ways we anticipate particularly important effects on the small firms. In such a heterogeneous sample, OLS will put relatively more weight on larger-scale businesses in a levels specification (as those businesses have larger daily and weekly variance in profits), but treat firms of different sizes equally in a logged specification. Nonetheless, for comparability to previous results, columns 1–3 of Table A7 repeat the profit estimates using levels on the full sample. Estimates show that firms increase sales in 2 hours by 90 shillings, 2 hour profits by 20 shillings, and weekly profits by about 100 shillings in response to the information intervention, though these estimates do not quite reach conventional significance thresholds. Columns 4–6 demonstrate that some of the reason for the discrepancy between these results and the logged results is indeed due to these larger firms: once we omit the top 2% of firms in terms of average weekly income, 2 hour estimates become marginally significant and weekly estimates again hover at the threshold of significance.

[32] This restriction prevents us from being able to reliably separate spillovers from the information intervention to those from the changeout survey intervention. Only one cohort of firms in 2011 was enrolled after any had received the information intervention, rendering the share of nearby firms that had received the information intervention almost completely collinear with the share of nearby firms that were treated, the number of nearby firms, and month dummies (the R^2 from a regression of share of information firms on those variables at the time of the first visit is 0.91).

[33] Results do not change when the radius around firms is increased to 200 m.

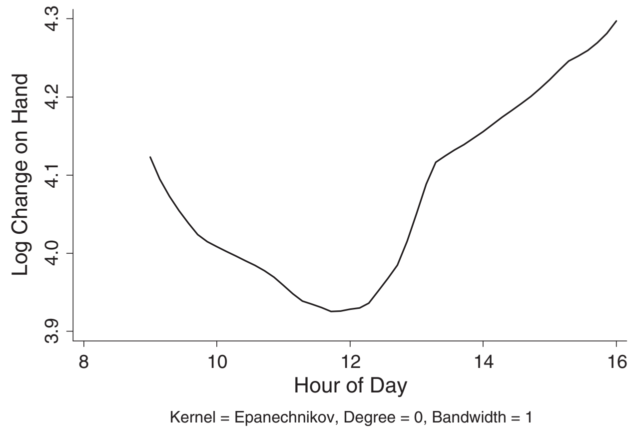
[34] Firms might also experience fewer changeouts because they make fewer transactions, since nearby treatment firms are losing fewer customers. However, we see no reduction in profits or sales in the last 2 hours.

(See Appendix Fig. A2 and Appendix Fig. A3.)

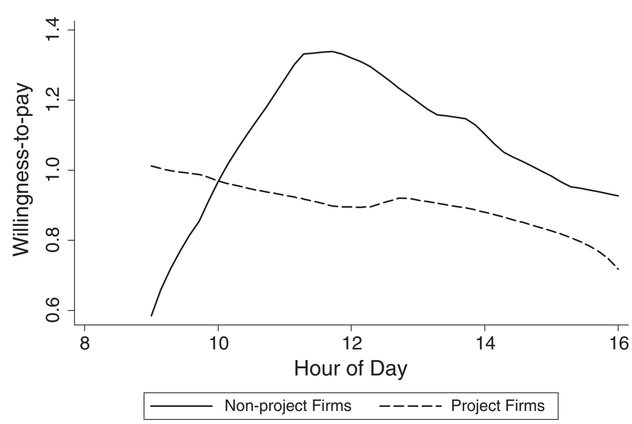


Notes: See Figure 1 notes.

Appendix Fig. A1. Additional outcomes by visit number.



Appendix Fig. A2. Time-of-day trends in change stocks.



Appendix Fig. A3. Time-of-day trends in willingness-to-pay.

Appendix Table A2
Attrition.

	(1)
	Successfully surveyed
Panel A. Fixed effects	
Planned visit number	0.01
	(0.02)
Given information	0.01
	(0.01)
Observations	5180
# Firms	508
Mean of dependent variable	0.93

	(1)	(2)
	Successfully surveyed at least once	
Panel B. Simple means		
Sampled for information	0.01	
	(0.01)	
Veteran cohort		0.02^{*}
		(0.01)
Constant	0.97^{***}	0.96^{***}
	(0.01)	(0.01)
Sample	Post-information	Full
Observations	508	916

Notes: The dependent variable in Panel A is whether the firm was successfully surveyed that week. Regressions include week $*$ month fixed effects. In Panel B, column 1 is restricted to weeks after the information intervention was given and column 2 includes all weeks. There are 916 observations in column 2 because all 508 firms could have appeared as novice cohorts but only 408 could appear as veteran cohorts (since 100 firms were enrolled at the end of the study windows). Standard errors are in parentheses and are clustered at the firm level in Panel A and column 2 of Panel B.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Appendix Table A3
First stage for information intervention.

	(2)
	Received information
Sampled for information	0.95^{***}
	(0.01)
Constant	0.00
	(0.01)
Observations	508
R-squared	0.92

Notes: The dependent variable is whether the firm actually received the information intervention.
Standard errors are in parentheses. *** indicates significance at 1%.

Appendix Table A1
Value of lost sales.

	Ln value lost sales			Ln value lost profits			Lost sales cash on hand		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Veteran cohort	-0.32^{***}			-0.25^{***}			-0.21^{*}		
	(0.12)			(0.09)			(0.12)		
Visit count			-0.10^{***}			-0.08^{***}			-0.01
			(0.03)			(0.03)			(0.04)
Sampled for information		-0.42^{***}			-0.32^{***}			-0.34^{**}	
		(0.14)			(0.11)			(0.17)	
Given information			-0.54^{***}			-0.42^{***}			-0.43^{**}
			(0.14)			(0.11)			(0.21)
Pre-information average of dependent variable		0.52^{***}			0.50^{***}			0.37^{***}	
		(0.04)			(0.04)			(0.05)	
Observations	866	497	4814	863	497	4792	648	293	1981
# Firms	508	497	508	508	497	508	436	293	388
Control mean	2.69	2.09	2.89	2.02	1.56	2.14	1.61	1.56	1.89
Control std. dev.	2.19	1.97	2.83	1.73	1.51	2.22	2.00	1.76	2.44
Specification	Mean	Mean	FE-IV	Mean	Mean	FE-IV	Mean	Mean	FE-IV
Sample	No info	Post-info	Full	No info	Post-info	Full	No info	Post-info	Full

Notes: See Table 3 for notes on specifications. The exchange rate was roughly 80 Ksh to $1 USD during the sample period. Standard errors are in parentheses and clustered at the firm level in columns 1, 3, 4, 6, 7 and 9.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Appendix Table A4
Spillovers.

	Lost a sale	Num lost sales	Ln Ksh brought in	Ln Ksh change brought in	Times receiving change	Times giving change	Ln sold last 2 hours	Ln profits last 2 hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of firms within 100 meters of business	0.00	0.05**	−0.01***	0.00	−0.07	−0.068**	0.00	0.00
	(0.00)	(0.03)	0.00	(0.02)	(0.04)	(0.03)	(0.01)	(0.01)
Share Treated Firms within 100 meters of business	−0.23	−5.13**	0.10	0.26	7.71**	4.41	0.48	0.54
	(0.25)	(2.11)	(0.09)	(0.43)	(2.93)	(3.26)	(0.48)	(0.50)
Mean of dependent variable	0.59	3.52	5.07	3.15	16.28	10.31	5.07	4.05
Std. dev. of dependent variable	0.49	7.11	1.80	2.08	11.30	1.09	2.04	1.69
Observations	197	198	198	197	187	194	198	198

Notes: Data were restricted to the first visit with a given firm. The sample is restricted to firms from the second phase of data collection, in 2011 as we do not have GPS coordinates for the other firms. The regressions also include month dummies. Conley (1999) standard errors are in parentheses, though results are qualitatively unchanged using robust standard errors. ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Appendix Table A5
Cash management.

	(1)
	Mean
How do you save money from the business?	
Bank	0.31
ROSCA	0.56
Home	0.57
M-Pesa	0.22
Other	0.03
What do you do with the cash at the end of the day?	
Bring home	0.76
Put in bank	0.05
Put in ROSCA	0.31
Put in mobile money account	0.13
Buy items	0.58
Restock	0.31
Other	0.05
When do you deposit money into the savings product?	
End of day	0.41
Next day	0.05
Later in week	0.46
Later in month	0.08
Do you find it hard to save money?	0.58
Do you have full control over your money?	0.90
Do you mentally separate business and personal money?	0.64
Do you physically separate business and personal money?	0.05
Do you consume more when your business has a good day?	0.87
Do you have another source of income?	0.29
Do you have a mobile money account?	0.77

Notes: Data are from semi-structured endline interview. There are 238 observations for most variables though the exact sample size differs from question to question.

Appendix Table A6
Other information from endline.

	(1)	(2)
	Mean	N
Panel A. Costs of changeouts		
Do customers ever buy more when you run out of change?	0.28	60
What percentage of the time do customers buy more?	0.07	17
When you run out of change, do you think the customer ever goes home without buying the item?	0.02	60
Panel B. Other inventory decisions		
Value of inventory (trimming top and bottom 1%)[a]		
Wholesale	11,655	455
	(23617)	
Retail	17,220	456
	(36437)	
Did you have a stockout last week?	0.32	237
If yes, total value of lost sales from stockouts	1241	76
	(4623)	

Notes: Data are from the semi-structured endline interview.
[a] There are many more observations for this variable since the other questions were only asked for a subset of the endline surveys.

Appendix Table A7
Additional Profit Estimates.

Dep. var.	Full data			Omitting top 2% of firms		
	Sold last 2 hours	Profit last 2 hours	Profit last week	Sold last 2 hours	Profit last 2 hours	Profit last week
	(1)	(2)	(3)	(4)	(5)	(6)
Visit count	12.73	−0.28	−14.51	−6.09	−0.82	−18.58
	(21.75)	(2.38)	(12.65)	(15.39)	(2.37)	(12.40)
Given information	89.53	21.64	97.01	158.10*	26.03*	90.92
	(92.26)	(14.60)	(59.84)	(82.37)	(14.37)	(57.33)
Observations	4801	4488	4522	4690	4407	4444
# firms	508	507	507	497	497	497
Control mean	681.49	139.15	1566.64	627.44	136.83	1532.72
Control std. dev.	1308.14	165.43	1361.77	1122.72	164.23	1317.81
Specification	FE-IV	FE-IV	FE-IV	FE-IV	FE-IV	FE-IV

Notes: See Table 3 for notes on specifications. The exchange rate was roughly 80 Ksh to $1 USD during the sample period. Standard errors are in parentheses and are clustered at the firm level.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively. The top 2% of firms is relative to mean weekly profits. The top 2% of firms make over 5500 KSH/week.

References

Banerjee, A., Mullainathan, S., 2008. Limited attention and income distribution. Am. Econ. Rev. 98, 489–493.

Banerjee, A., E. Duflo, R. Glennerster, and C. Kinnan (2010). The miracle of microfinance: evidence from a randomized evaluation. Mimeo, MIT.

Berge, L., K. Bjorvatn, and B. Tungodden (2011). Human and financial capital for microenterprise development: evidence from a field and lab experiment. Mimeo, Norwegian School of Economics.

Bruhn, M., D. Karlan, and A. Schoar (2012). The impact of consulting services on small and medium enterprises: evidence from a randomized trial in Mexico. Mimeo, Yale University.

Chetty, R., Kroft, K., Looney, A., 2009. Salience and taxation: theory and evidence. Am. Econ. Rev. 99, 1145–1177.

Conley, T.G., 1999. Gmm estimation with cross sectional dependence. J. Econom. 92 (1), 1–45.

Crepon, B., F. Devoto, E. Duflo, and W. Pariente (2011). Impact of microcredit in rural areas of Morocco: evidence from a randomized evaluation. Mimeo, MIT.

de Mel, S., McKenzie, D., Woodruff, C., 2008. Returns to capital in microenterprises: evidence from a field experiment. Q. J. Econ. 123, 1329–1372.

de Mel, S., McKenzie, D., Woodruff, C., 2009a. Are women more credit constrained? Experimental evidence on gender and microenterprise returns. Am. Econ. J. Appl. Econ. 1, 1–32.

de Mel, S., McKenzie, D., Woodruff, C., 2009b. Measuring microenterprise profits: must we ask how the sausage is made? J. Dev. Econ. 88, 19–31.

DellaVigna, S., 2009. Psychology and economics: evidence from the field. J. Econ. Lit. 47, 315–372.

Field, E., Jayachandran, S., Pande, R., 2010. Do traditional institutions constrain female entrepreneurship? A field experiment on business training in India. Am. Econ. Rev. 100, 125–129.

Finkelstein, A., 2009. Ez-tax: tax salience and tax rates. Q. J. Econ. 124, 969–1010.

Gabaix, X. and D. Laibson (2005). Bounded rationality and directed cognition. Mimeo, NYU.

Gabaix, X., Laibson, D., Moloche, G., Weinberg, S., 2006. Costly information acquisition: experimental analysis of a boundedly rational model. Am. Econ. Rev. 96, 1043–1068.

Hanna, R., Mullainathan, S., Schwartzstein, J., 2012. Learning through noticing: theory and experimental evidence in farming. BREAD Working Paper, p. 359.

Karlan, D., M. McConnell, S. Mullainathan, and J. Zinman (2011). Getting to the top of mind: how reminders increase saving. Mimeo, Yale University.

Karlan, D., Valdivida, M., 2011. Teaching entrepreneurship: impact of business training on microfinance clients and institutions. Review of Economics and Statistics 93 (2), 510–527.

Karlan, D. and J. Zinman (2010). Expanding microenterprise credit access: using randomized supply decisions to estimate the impacts in manila. Mimeo, Yale University.

Keats, A., 2012. Occupational choice in rural Kenya: using subjective expectations data to measure credit and insurance constraints. Working Paper. UCLA.

Keohane, J., 2008. Yes, we have no monedas! Slate (December 3).

Klinger, B., Schuendeln, M., 2011. Can entrepreneurial activity be taught? Quasi-experimental evidence from Central America. World Dev. 39, 1592–1610.

Kremer, M., J. Lee, J. Robinson, and O. Rostapshova (2011). The return to capital for small retailers in Kenya: evidence from inventories. Mimeo, Harvard University.

McKenzie, D., 2011. Beyond baseline and follow-up: the case for more T in experiments. World Bank Policy Research Working Paper.

McKenzie, D., Woodruff, C., 2008. Experimental evidence on returns to capital and access to finance in Mexico. World Bank Econ. Rev. 22, 457–482.

McKenzie, D., Woodruff, C., 2012. What are we learning from business training and entrepreneurship evaluations around the developing world? BREAD Working Paper, p. 358.

Sargent, T.J., Velde, F.R., 2002. The Big Problem of Small Change. Princeton University Press, Princeton, New Jersey 08540.

Sims, C., 2003. Implications of rational inattention. J. Monet. Econ. 50, 665–690.

Stango, V. and J. Zinman (2013). Limited and varying consumer attention: evidence from shocks to the salience of bank overdraft fees. Mimeo, Dartmouth College.

Surowiecki, J., 2009. Change we can't believe. The New Yorker (June 8).

Zwane, A., Zinman, J., Dusen, E., Pariente, W., Null, C., Miguel, E., Kremer, M., Karlan, D., Hornbeck, R., Gine, X., Duflo, E., Devoto, F., Crepon, B., Banerjee, A., 2011. Being surveyed can change later behavior and related parameter estimates. PNAS 108, 1821–1826.