Labor Reallocation Between Small Firms: Experimental Evidence on Information Constraints*

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Abstract

We document interest in labor reallocation among small firm owners in Ghana; 60% and 41%, respectively, self-report willingness to hire or work for the average local firm owner. Firm owners also exhibit high willingness-to-pay for information on a random subset of hiring firms and jobseeking firm owners, during a Becker-Degroot-Marschak exercise. Conditionally random variation in access to this information generates immediate labor adjustments within and between firms, though rarely of firm owners themselves, and impacts firm closure 5-months post-intervention. Our findings suggest that labor market information of this kind is both valuable and actionable in our context.

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1 Introduction

Researchers have long argued that labor inputs in low- and middle-income countries are misallocated across firms, with important implications for aggregate productivity (Benjamin, 1992, Hsieh and Klenow, 2014, Lewis, 1954). Firm-level datasets spotlight the motivating empirical puzzle that firms in low- and middle-income countries are small (the modal firm is only the self-employed owner (Hsieh and Olken, 2014)) and spatially-proximate within industry (Banerjee et al., 2022, Bassi et al., 2021). Some of these small firms are labor-constrained; programs that provide wage subsidies or recruitment and matching services have been shown to generate novice hiring in skilled manufacturing and services (De Mel, McKenzie and Woodruff, 2019, Hardy and McCasland, 2023). Worker-level studies have argued that a sizeable share of self-employed people in low- and middle-income countries are *involuntarily* self-employed, preferring wage employment and experiencing meaningful labor slack (Breza, Kaur and Shamdasani, 2021, Hardy and Kagy, 2020, Karaivanov and Yindok, 2022, Schoar, 2010). Why don't labor scarce and labor slack firms consolidate?

We investigate this question by focusing on the universe of firms in a single market in a single industry (garment makers in Hohoe, Ghana), where firm consolidation is theoretically immediately feasible.[1] We collect survey data from firm owners on their willingness to hire or work for "the average garment making firm owner" in town. 60% of firms said they would be willing to hire and 41% of firm owners said they would be willing to work at some wage. Among firms who said they would hire, we collect the maximum wage at which they would be willing to hire under fixed and piece rate contracting schemes. Among firm owners who said they would be willing to work, we collect self-reported reservation wages under both contracting schemes. 40% of our sample reported a willingness to hire "the average garment making firm owner" at the median reported piece rate reservation wage and 18% at the median reported reservation weekly salary. These self-reported hiring and reservation wages suggest meaningful numbers of unrealized matches under both contracting schemes.

[1]Our setting is representative of similar clusters of firms studied in many parts of the developing world (see e.g. Banerjee et al. (2022) and Bassi et al. (2021)). Hardy and Kagy (2018) show that firms in this sample have similar observable characteristics to nationally representative data on self-employed people in Ghana.

Armed with evidence that *labor scarce* firms are operating in close proximity to *labor slack* firms and building on recent work that suggests information may not flow freely among neighboring firms within the same industry (Dalton et al., 2021), we explore whether information may constrain labor reallocation. Using self-reported willingness to hire or work, we generate directories of randomly selected *jobseeking firm owners* and *hiring firms* and elicit firm owners' valuation of the directories. If firm owners are willing to pay to learn about *jobseeking firm owners* or *hiring firms*, that would suggest the information is valuable and not readily available in the absence of intervention. In a Becker-DeGroot-Marschak (BDM) willingness-to-pay (WTP) elicitation exercise (Becker, DeGroot and Marschak, 1964), over 90% of respondents are willing to spend some of their allocation on purchasing a directory and more than half are willing to spend their entire allocation, around 10 percent of mean weekly household income in this sample. We conclude that (1) the directories contain useful new information, and (2) preexisting networks and market structures may not efficiently transmit this information prior to our intervention.

The BDM mechanism generates conditionally random variation in directory acquisition. In our implementation, bids for individual directories could take integer values between zero and three Ghana Cedis (GhC); the randomly selected price could take integer values between zero and four GhC. Bidders received a directory when the randomly selected price was less than or equal to their bid. We exploit this conditionally random variation to study the impacts of providing information, to further test whether information frictions could constrain labor reallocation.

Directory distribution was implemented in early February 2020 and our main results study immediate labor reallocation shortly after implementation. In our first finding, we observe three direct consolidations: three firm owners who conditionally randomly acquired lists of *jobseeking firm owners* hired another firm owner, generating a small but statistically significant impact of *jobseeking firm owner* directory acquisition on firm consolidation of this type. Although we cannot know whether we would have observed more direct consolidation with a longer business-as-usual time horizon,[2] the presence of these three cases implies that the information in our directories was im-

[2] As elsewhere in the world, COVID-19 lockdowns began in Ghana in mid-March 2020.

mediately actionable for some firms and that at least some unrealized consolidations are constrained by missing information.

Secondly, conditionally random acquisition of a *hiring firm* directory leads to labor contraction in firms that acquire these lists. In the sample of employers, the treatment effect is a reduction of about one worker. These effects are large; baseline firm size including the owner is only 3.8 workers in the sample of employers. Data constraints limit our ability to track individual exiting workers from their initial firms, as we do not have pre-intervention worker rosters. However, qualitative discussions with field staff and firm owners suggest that some of these exiting workers were placed (by their current bosses) with firms that appeared on *hiring firm* directories. In other words, vacancy information was conveyed; *labor slack* firms were able to acquire information on *labor scarce* firms via our experiment and used that information to find jobs for their (slack) workforce.

We provide two types of suggestive evidence to support this story. First, we document firm size increases associated with more exposure to the non-owner labor pool, where exposure is defined as a count of workers at firms that acquire a *hiring firm* list with the reference firm's name on it. Second, we document a negative correlation in firm size between firms linked through *hiring firm* lists after dissemination of the lists, but not before, and no such pattern for firms linked through *jobseeking firm owner* directories or firms not linked through our directory experiment. Though both pieces of evidence are indirect, both suggest that *hiring firm* directories generated non-owner labor reallocation towards *labor scarce* firms in our sample.

Our final short-term labor reallocation finding is that firms that acquired *jobseeking firm owner* lists increase their wage bill. Though we are unable to pin down a mechanism, it is plausible that these lists conveyed some type of indirect labor market information that caused these firm owners to adjust their labor inputs. Although longer run impacts of our information intervention are complicated by the onset of the COVID-19 pandemic, we find that recipients of *jobseeking firm owner* lists are more likely to be operational and recipients of *hiring firm* lists are less likely to be operational 5-6 months after list distribution in June and July of 2020. The latter effect is consistent with these firms shedding apprentices after the receipt of *hiring firm* lists.

We make three main contributions. While an extensive literature has found evidence of information frictions for jobseekers and employers in low- and middle-income countries (Abebe et al., 2020, Bassi and Nansamba, 2022, Beaman and Magruder, 2012, Franklin, 2018, Hardy and McCasland, 2023, Witte, 2022, Wu, 2023), to the best of our knowledge, ours is the first paper to experimentally study whether information frictions constrain labor reallocation *between* small firms. The evidence in this paper echoes a literature from high-income countries that argues that gains from agglomeration accrue in part from shared labor pools (Greenstone, Hornbeck and Moretti, 2010).

We also contribute to the broader literature on information constraints faced by firms. Small firms appear to face information constraints in finding suppliers and customers (Aker, Dillon and Blumenstock, 2020, Brooks, Donovan and Johnson, 2018, Jensen and Miller, 2018). Big firms benefit from match-making events that introduce them to other firm owners, with new information being an important mechanism (Cai and Szeidl, 2017, Fafchamps and Quinn, 2016). This paper provides evidence that labor market information concerning the labor slack and scarcity of neighbors may be valuable.

Finally, though we observe limited direct consolidation, ours is a case study in the compelling question: Why don't the productive firms hire the involuntarily self-employed? Here, with a note of caution around the limited time horizon of business-as-usual operation (pre-COVID-19), we provide evidence that a nudge-type intervention can generate (a few) mergers and that information does not appear to be the key friction limiting consolidation of small firms.

2 Experimental Design

2.1 Data

Data collection for the project comes from four sources: a census in September 2019, a baseline survey in October 2019, a BDM elicitation exercise in January of 2020, and a retrospective panel collected in August of 2020 that references February 2020 through

July 2020. The census targeted the universe of garment making firms in Hohoe, Ghana, a district capital with a population of about 70,000 people. Starting from a census of the same population in 2014, and expanding to new firms via snowball sampling and block canvassing, the census captured firms operating in town and in outlying suburbs along an 11km stretch of highway that connects Accra to Togo, all within a limited commuting distance.

In addition to detailed firm and firm owner characteristics, the baseline survey included a network module and self-reports of willingness to hire or work for other garment making firm owners in Hohoe. These self-reports form the basis of the *hiring firm* and *jobseeking firm owner* directories, for which we elicited willingness-to-pay via a BDM exercise over a three-day data collection window in January 2020. The census, baseline survey, and BDM exercise were all conducted in person.

The retrospective panel in the follow-up survey has observations for every month from February 2020 to July 2020 for sales, profits, wages, and hours worked. A full worker roster and measures of income generated outside the reference firm was collected only for February 2020 and July 2020. Name matching from the worker roster is the basis for identifying direct consolidations between firm owners. Unfortunately, neither the census nor the baseline survey have a worker roster, making it impossible to name match movements of non-owner labor across firms. The follow-up survey was conducted by phone.

2.2 Sample

The census identified 569 firm owners, 509 of whom were available during the limited three-day BDM data collection window in January 2020. Of these 509, 464 consented to have their information shared with others and thus enter our experimental sample. We restrict all analysis in the paper to these 464 firms.[3]

[3] Appendix Table A1 characterizes sample selection between the census and the January sample, and between the January sample and the final experimental sample, along the covariates available in the census data. We find that firms available in January 2020 are larger and older than those that were unavailable, which is consistent with the primary reason for attrition from the sample being travel. Owners of younger and smaller firms are more likely to close up shop temporarily for extended holiday travel. We find less evidence of sample selection between January and the experimental sample; the F-test of the joint significance of all covariates predicting selection into the experimental sample is 0.24.

Of the firms in the experimental sample, 455 participated in a baseline survey. The average sample firm owner is 38 years old, went to school for nine years, and has been in the garment making business for 14 years. 78% of garment makers are female, and 68% are of the locally native Ewe ethnicity (See Appendix Table A2).

54% of businesses in the sample employ workers, and these businesses are more profitable than their counterparts without workers. Like small-scale garment making in most low- and middle-income countries, almost all production is bespoke. While some have argued this artisanal production implies limited potential returns to scale in labor (Bassi et al., 2023), the distribution of firms in our sample (and in most similar samples) includes a meaningful share of firms close to the viability threshold (i.e. with very low earnings). In our sample, baseline profits for 60% (27%) of one-person firms were less than the baseline mean (median) wage paid to non-owner workers in firms with workers. In other words, the margin for consolidation remains despite the fact that all firms in our sample are small.

83% of non-owner labor is categorized as apprentices, a broad-based colloquialism that tends to include both novice and skilled workers. This category of workers is ubiquitous in West Africa, and the labor composition of our sample is not dissimilar from most samples of small manufacturing and services firms in the region (Teal, 2016). Apprentices are also the only labor category for which our measurement was consistent across the baseline and endline surveys; for this reason panel analysis focuses on these workers.

2.3 Labor Reallocation Self-Reports

Self-reported willingness to hire or work for other garment makers (and the highest (lowest) amount at which one would be willing to demand (supply) labor) was constructed first as person-specific questions over garment making contacts and then generically for the "average garment making firm owner you do *not* know in Hohoe." For both known and anonymous connections, questions were asked separately for piece-rate contracts (to produce one shirt) and fixed wages contracts (for one week of work).[4]

The mean (median) number of reported garment making contacts was six (five). 60% of firms said they would be willing to hire an anonymous firm owner on either a piece rate or fixed wage contract (or both), and 41% of firm owners said they would be willing to work for an anonymous firm owner on either a piece rate or fixed wage contract (or both).[5] For this reason, the overwhelming majority of implied unrealized reallocation links come from anonymous pairs.

Appendix Figure A2 shows overlapping histograms of anonymous willing-to-hire and willing-to-work reservation wages for the two contract types among people interested in hiring or jobseeking with anonymous firm owners. The figure shows substantial overlap. For example, the median piece-rate willing-to-work reservation wage was 10 GhC; 112 people said they would work at or below this wage and 187 people said they would hire at or above this wage. The median weekly-salary willing-to-work reservation wage was 77 GhC; 58 people said they would work at or below this wage and 85 people said they would hire at or above this wage.

2.4 Directory Construction

Eligible (potentially overlapping) pools for each *jobseeking firm owner* and *hiring firm* directory were composed of firms that self-reported willingness-to-work or -hire under one of four conditions: fixed wage with hypothetical demand, fixed wage without hypothetical demand, piece rate with hypothetical demand, and piece rate without hypothetical demand. Willingness to work or hire included known contact firm-specific self-reports and anonymous "average garment making firm owner you do *not* know in Hohoe" self-reports. 97.3% of firms in these four types of firm-specific list eligibility pools were previously network connected. All firms included in eligibility pools

[4]Baseline survey data collection also embedded a survey experiment that randomized half of the firms into a "hypothetical demand" condition, in which each willingness-to-hire or -work question was preceded by a statement assuming "consistently many garment orders". Hypothetical demand had no impact on the probability of self-reported willingness-to-work or -hire (see Appendix Table A3). See Figure A1 for exact survey question wording.

[5]Piece rate contracts were more popular both as a potential employer (59%) and a potential employee (40%), likely because that contract type is more common (and thus familiar) for skilled garment workers. 36% (26%) were willing to hire (work) on a weekly salary contract, and nearly all of these firm owners also said yes to piece rate contracts.

consented to have their names, firm names, and contact information shared with others. We chose to not use reservation pay responses to restrict list eligibility, as we wanted to allow for potential ex-post wage negotiation, so anyone who self-reported willingness-to-work at any wage is eligible and anyone who self-reported willingness-to-hire at any wage is eligible. Eligibility pools range from about 50 firms (for fixed wage jobseekers) to about 130 firms (for piece rate hiring firms). Each directory included 7 randomly selected names from the relevant eligibility pool.

2.5 Willingness to Pay Elicitation

We measure willingness-to-pay for *hiring firm* and *jobseeking firm owner* directories using the Becker-Degroot-Marschak (BDM) method (Becker, DeGroot and Marschak, 1964). Each respondent was allocated five GhC, and could choose to bid none, some, or all of it on directories during the WTP exercise.[6] Participants were offered two *hiring firm* lists and two *jobseeking firm owner* lists, where one of each type was ranked by sales, and could bid up to 3 GhC on each list. Each firm was randomly assigned one of the four willingness-to-work or -hire conditions listed above, such at that all four offered lists were either piece rate or fixed wage and either with hypothetical demand or without it. All participants went through two practice rounds of the mechanism and enumerators were trained to probe and ensure understanding during these practice rounds.[7]

2.6 Estimating Impacts of Information

In the BDM mechansism, acquiring a directory is determined by whether a randomly drawn price is higher than the participant's bid for a given directory. In our case, the random price generator chose equally integers between 0 and 4 GhC, such that a bid of 0 had a 20% chance of receiving a list because there is a 20% chance that the bid

[6]Five GhC is approximately one US dollar, or 10% of average weekly household income in our sample at the time of the WTP exercise.

[7]This portion of the data collection was conducted in tandem with a development fieldwork class for New York University during which students shadowed enumerators to learn about research, as in Hardy, Kagy and Song (2022). The presence of students was randomized; we find no impact of student presence on the outcomes of interest within the willingness-to-pay exercise. See Appendix Table A4.

meets or exceeds the randomly drawn price. Similarly, a bid of 1 has a 40% chance of receiving a list, a bid of 2 has a 60% chance of receiving a list, and a maximum bid of 3 has an 80% chance of receiving a list. To recover an unbiased estimate of the treatment effect of acquiring a single directory in this conditional randomization, one could simply include indicator variables for each bid. These bid fixed effects are akin to strata fixed effects; within a bid level, the probability of treatment is the same for all firms.

With the four willingness-to-work or -hire conditions and two types of firm name ordering, there are eight categories of *hiring firm* directories and eight categories of *jobseeking firm owner* directories that were auctioned in the BDM exercise. We pool across the four willingness-to-work or -hire conditions and collapse the four lists we offered into two binary treatment assignment variables: receiving any *jobseeking firm owner* list and receiving any *hiring firm* list.[8]

We can extend the logic of bid fixed effects for a single directory auction to our collapsed treatment indicators that include two lotteries each, deriving exact probabilities of acquiring at least one *jobseeking firm owner* list and of acquiring at least one *hiring firm* list. As an example, imagine someone bids 1 in the first *jobseeking firm owner* directory lottery and 2 in the second. The probability of acquiring at least one *jobseeking firm owner* directory is 1 minus the probability of losing both lotteries, or $1 - (0.60 * 0.40) = 0.76$. For all possible bid combinations between 0 and 3, it is possible to generate treatment probabilities over our two collapsed binary treatment indicators. Using indicator variables for these treatment probabilities, like strata in a classic randomized controlled trial (RCT), we extract a simple conditional randomization. That is, in expectation, list receipt is independent of the potential outcomes, conditional on controlling for these treatment probability fixed effects.[9]

Treatment with *jobseeking firm owner* lists and treatment with *hiring firm* lists are distinct conditional RCTs in an overlapping sample. Specifically, one could imagine estimating the effect of conditionally random *jobseeking firm owner* list receipt controlling

[8] Appendix Table A5 fails to detect any relationship between directory categories and WTP for *jobseeking firm owner* lists (Column (1)) and *hiring firm* lists (Column (2)).

[9] Appendix Table A6 lists the eight different treatment probabilities generated by the possible pairs of bids, along with the strata size for each of the two treatment indicators.

for the corresponding treatment probability fixed effects separately from conditionally random *hiring firm* list receipt controlling for the corresponding treatment probability fixed effects, where each firm owner in our sample is participating in both experiments. Here, we analyze them together, controlling for both sets of treatment probability fixed effects, and estimating impacts on each treatment indicator.

We estimate impacts of conditionally random receipt of at least one *jobseeking firm owner* list and/or at least one *hiring firm* list on outcomes of firm i in month t using the following regression specification:

$$\begin{aligned} y_{it} = & \beta_0 + \beta_1 JSList_i + \beta_2 HFList_i \\ & + \gamma_1 \overline{PrHFList}_i + \gamma_2 \overline{PrJSList}_i + \gamma_3 T_i + \gamma_4 B_i + \gamma_4 y_{i,b} + \varepsilon_{it}, \end{aligned} \tag{1}$$

where $JSList_i$ indicates whether firm i obtained at least one *jobseeking firm owner* list and $HFList_i$ indicates whether firm i obtained at least one *hiring firm* list through the WTP exercise. $\overline{PrHFList_i}$ and $\overline{PrJSList_i}$ are the treatment probability fixed effects for firm i described above. T_i are fixed effects for the willingness-to-hire or -work conditions (hypothetical demand or no, piece rate or salary), B_i are imbalanced baseline characteristics (see next sub-section), and $y_{i,b}$ is the baseline value of the outcome variable, where available.

We focus on observations from February 2020, immediately following the list distribution period, and June-July 2020, four to five months after the distribution of the lists. Whenever available, we use two observations per firm for June and July 2020 to improve precision of the results. We use standard errors clustered at the firm level whenever we have more than one observation by firm, and robust standard errors otherwise.[10]

[10] Note that conditional on having observations for both June and July for a given outcome, we have them for each firm in our main sample. We therefore do not need to include months fixed effects into the estimation; the results would remain identical.

2.7 Balance and Attrition

Appendix Table A7 presents tests of treatment balance for our two collapsed treatment indicators, controlling for *jobseeking firm owner* directory treatment probability fixed effects, *hiring firm* directory treatment probability fixed effects, and contract condition fixed effects, mirroring our main specification. Among 30 tests (15 dependent variables times 2 treatment groups), four show significant differences to the control group at the 10 percent significance level, roughly consistent with what should be expected by chance. We control for the imbalanced variables in our remaining analysis, and indicator variables for where baseline data is missing. Note though that these differences appear minor, as F-tests on the two treatment group dummies only show joint significance at the 5 percent level for one of the 15 variables (see column 3 in Table A7).

We were able to interview 437 out of 464 firms that participated in the WTP elicitation during the followup survey, a high tracking rate of 94%. Attrition status is not predicted by treatment (Appendix Table A8). Following Ghanem, Hirshleifer and Ortiz-Becerra (2019), Appendix Table A9 replicates the balance tests from Appendix Table A7 on the sample of non-attrited observation, showing very similar results.. Attrition thus does not seem to have affected balance.

3 Results

3.1 Willingness to Pay for Information

The valuation of the directories was high. Figure 1 Panel (a) shows the distribution of total money offered for information by participating firm owners. The average garment maker offers 3.75 of the total of 5GhC for list purchases, or 75% of the allocated budget. Fifty-one percent are willing to spend their full allocation, and only eight percent do not offer any of their GhC.

Figure 1 Panel (b) shows the total amount bid on the two *jobseeking firm owner* lists and the two *hiring firm* lists. Generally, garment makers bid more for lists of potential workers than for lists of potential bosses. The difference between the combined bids on *jobseeking firm owner* lists and on *hiring firm* lists is statistically significant at the one percent level.[11]

3.2 Short-Term Treatment Impacts on Labor Inputs

Table 1 presents causal impacts of directory acquisition on labor outcomes measured within a month of directory distribution. *Jobseeking firm owner* list recipients increase hiring of other firm owners by a statistically significant 1.4 percentage points. These mergers occur between people who reported no prior network connection and who were connected by the directories. They continue through to July, the end of our data collection window. The point estimates here are quite precise, ruling out large impacts of information on short-term mergers despite high rates of self-reported willingness to merge.[12]

Acquisition of a *hiring firm* directory causes a contraction in firm size among employers. We observe a reduction in firm size of about 1 worker, or a third of the non-owner workforce. In addition, receipt of a *hiring firm* directory causes a reduction in the recipient's wage bill, while receipt of a *jobseeking firm owner* directory generates growth in the recipient's wage bill, driven by employers. [13]

3.3 Descriptive Evidence on Labor Reallocation Between Firms

Qualitative anecdotes from the field suggest that the primary use of *hiring firm* directory information was to place "senior" apprentices working in *hiring firm* list recipient firms with firms appearing on these lists, given their willingness to hire other garment makers in Hohoe. Tables 2 and 3 provide two types of descriptive evidence supporting

[11]More money was offered on *jobseeking firm owners* lists by male respondents, by respondents in Hohoe town, and by respondents of the locally dominant Ewe ethnicity, while more money was offered by firm owners outside of Hohoe for *hiring firm* lists. Appendix Table A10 shows the predictors of the willingness to pay, though we caution over-interpretation as WTP can be driven by many underlying factors.

[12]The fact that self-reported unrealized matches do not materialize into a large number of mergers could be explained by some combination of cheap talk, other external or internal to the firm binding constraints to growth, and a limited time horizon before the onset of the COVID-19 pandemic.

[13]Appendix Tables A11, A12, and A13 present alternative specifications for these main results, controlling for the joint probability distribution over both list types, or estimating impacts of the two list types in separate regressions. Results are qualitatively similar.

this story.

Table 2 documents increases in the firm size of firms that appeared on *hiring firm* directories using a measure of exposure to the non-owner labor force. We parameterize exposure to the non-owner labor force in the market we study using a count of the number of apprentices at baseline in all of the firms that acquired a *hiring firm* directory with the reference firm owner's name. This number varies between 0 and 47 apprentices. The logic is that the *hiring firm*'s vacancy was "posted" to between 0 and 47 potential workers. Column (1) presents results for the full sample and Column (2) presents results from the smaller sample of firms who self-reported a willingness to hire "the average garment making firm owner" and thus have more negligible differences in their probabilities of exposure. Each additional "vacancy posting" is associated with a firm size increase for the average firm owner of 0.015 apprentices just after the intervention; firm size at firms with above median "vacancy postings" is 0.44 workers larger than firm size at firms below median. These coefficients remain quite stable within the sample of firm owners in Column (2).

Table 3 provides further evidence for this phenomenon at the firm-pair level. We split all potential *directed* pairs of firms, i.e. the firm appearing on a list to the firm receiving the list, into three types: (1) firms connected via *jobseeking firm owner* directories, (2) firms connected via *hiring firm* directories, and (3) firms unconnected by our experiment.[14] In column (1), which includes all unconnected pairs, each firm's size is regressed on the firm size of all paired members of this type. For columns (2) and (3), we regress the number of apprentices working at the firm who appears on the respective lists on the number of apprentices at the firm that receives the list, before and after our intervention.[15] The estimated correlation is small for firms connected via *jobseeking firm owner* directories and for firms unconnected by our experiment (albeit the latter being significant given the large size of the sample), both before and after the intervention, and there is no correlation between firms connected by the *hiring firm* directories before the intervention. After the intervention, there is a negative correlation in the apprentice firm sizes of firms connected by the *hiring firm* directories. To

[14]Note that groups (1) and (2) are potentially overlapping.

[15]We cluster errors two-way, within firm i and firm j, to account for the correlation of standard errors within each firm.

put this coefficient size of 0.04 in perspective, we note that the average *hiring firm* list recipient received 8.3 names of potential bosses. If every *hiring firm* directory recipient placed a worker with a firm on the list, this would correspond to a reduction in firm size of 1 from firm owner j leading to an average increase in firm size of firm owner i of around 0.12. If every third recipient of a *hiring firm* directory placed a worker with a firm on their list, that would correspond with our coefficient estimate of 0.04.

Although *jobseeking firm owner* list recipients were the only firms to hire other owners directly, this response to *jobseeking firm owner* list information was quite rare. We do not detect an overall increase in *jobseeking firm owner* list recipient firm size. We also do not detect similar relationships to those documented in Tables 2 and 3 for *jobseeking firm owner* list members nor linked pairs. This leads us to believe that the immediate wage impacts detected in response to *jobseeking firm owner* list receipt may be derived from more indirect information provided on these lists.[16]

3.4 Medium-Term (COVID) Impacts on Firm (Owner) Outcomes

Estimating treatment effects on firm outcomes downstream of labor reallocation is confounded by the COVID-19 pandemic, which disrupted economic activity starting in Mid-March 2020 in Ghana as in the rest of the world. Although an official lockdown occurred for only two weeks, almost two thirds of Hohoe garment making firm owners that operated with positive sales as of February 2020 experienced zero sales during April 2020, and only 80% operated again with positive sales as of July 2020. Owners reported that the COVID-19 pandemic impacted demand, with many large gatherings that traditionally drive garment orders cancelled, as discussed by Hardy et al. (2022).

With this in mind, we turn our attention to our medium-term findings, presented in Table 4. We estimate firm-level outcome impacts on whether firms are open (proxied by reporting any positive sales), their wages, profits, and income from outside the business. We observe two distinct directions for *jobseeking firm owner* list and *hiring firm*

[16] We remain agnostic on the specifics here. Recipients could be updating beliefs about the recipient's own ability type, consistent with some evidence provided in other contexts (Bénabou and Tirole, 2002, Thatchenkery and Katila, 2021). Recipients may also derive some broader (labor) market insights after seeing specific list members as jobseeking, potentially adjusting internal operations in response. High-income country firms have been shown to value this type of information for this reason (Baum and Kant, 2003, Kim, 2022, Thatchenkery and Katila, 2021).

list recipients. *Jobseeking firm owner* list recipients are 8.8 percentage points more likely to be open in June and July 2020. *Hiring firm* list recipients, on the other hand, are 8.5 percentage points less likely to be open. These firm owners also report higher income from alternative sources outside the garment making firm.[17] Impacts on unconditional profits and wages echo these closure patterns.[18] These patterns are broadly consistent with *jobseeking firm owner* list recipients (who had increased labor inputs prior to the pandemic) exhibiting higher resiliency during the crisis, while *hiring firm* list recipients (who had shed workers immediately prior to the pandemic) substituting to other sources of income.[19]

4 Conclusion

We extend the literature on labor misallocation in low- and middle-income countries by testing for information constraints in labor reallocation between small firms. Firm owners self-report a willingness to reallocate labor and are willing to pay for information on the labor reallocation self-reports of their neighbors. We present evidence that a light-touch information intervention can generate some consolidation, but much less than would be predicted by the number of self-reported unrealized matches.

We also present evidence that information on the labor scarcity or slack of neighboring firms generates non-owner labor reallocation from labor slack to labor scarce firms. Because the study was interrupted by the COVID-19 pandemic, we have limited evidence on the efficiency or productivity gains associated with this labor reallocation between firms, but we do find that firm owners who shed workers in response to our experiment reoptimize during the pandemic.

[17] Previous literature has suggested an average firm exit rate of ca. 8 percent per year outside times of crisis (McKenzie and Paffhausen, 2019). It is possible that rates of firm closure in 2020 were accelerated by the COVID-19 crisis, leaving more space for our information to counteract these closure rates. However, the COVID-19 pandemic may have also attenuated the impacts on our outcomes, especially on firm growth, given it was a large and persistent negative demand shock. It is also important to note that the size of the economic shock COVID-19 rendered is not necessarily unique in low- and middle-income countries. A large body of literature has studied similarly meaningful economic shocks, e.g. the 2014 Ebola outbreak in West Africa (Huber, Finelli and Stevens, 2018), election violence in Kenya (Ksoll, Macchiavello and Morjaria, 2021), electricity crises in Ghana (Hardy and McCasland, 2021), and the increasing prevalence of climate disasters (Cavallo et al., 2013).

[18] We do not detect impacts on these outcomes once we condition on closure.

[19] Appendix Table A14 shows gender heterogeneity in impacts for Table 4 consistent with Hardy et al. (2022), with impacts on firm outcomes experienced by both genders, but increases in outside income only experienced for men.

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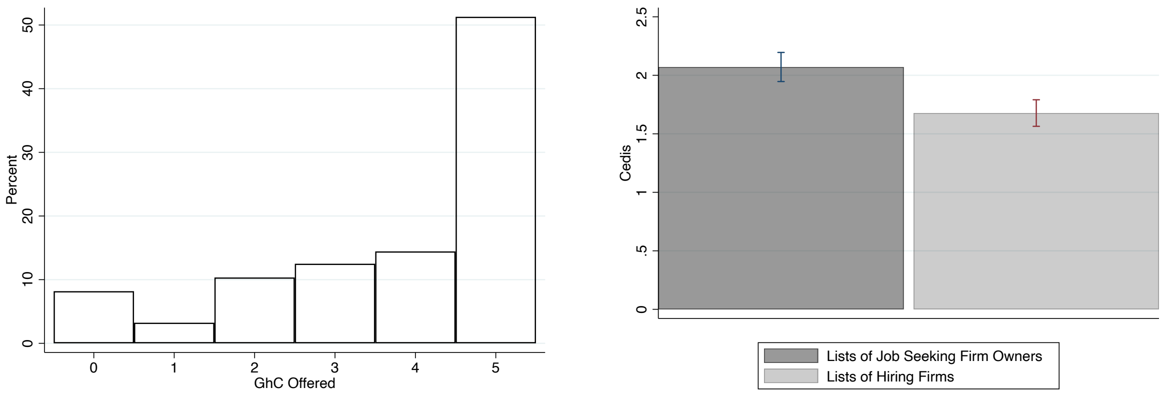
Figure 1: Willingness to Pay for Information



(a) Distribution of Total GhC Offered

(b) Average Total GhC Offered by List Type

Notes: Panel a) shows the distribution of total Ghana Cedis (GhC) offered by firm owners for all available lists, out of the budget of five GhC given to the firm owners during the the willingness to pay exercise (Becker, DeGroot and Marschak, 1964). Panel b) shows the average combined GhC offered by each list type: i) Lists of *jobseeking firm owners* that include other garment making firm owners in Hohoe who stated a willingness to work for the respondent or the average garment maker in Hohoe, and ii) Lists of *hiring firms* that include other garment making firm owners in Hohoe who stated a willingness to hire the respondent or the average garment maker in Hohoe. 95% confidence intervals are shown in panel b).

Table 1: Short-Term Treatment Impacts on Labor Inputs

	Working at Another Firm (1)	Employing Another Firm Owner (2)	# of Workers (3)	Wage Bill (4)	IHS Wage Bill (5)
Panel A: All Firms					
List(s) of *Jobseeking Firm Owners*	-0.010	0.014*	0.075	4.505	0.313**
	(0.010)	(0.008)	(0.204)	(3.078)	(0.135)
List(s) of *Hiring Firms*	0.004	-0.006	-0.384*	-4.664	-0.438***
	(0.004)	(0.010)	(0.203)	(3.399)	(0.165)
Mean Among List Non-recipients	0.000	0.000	1.983	4.333	0.252
Observations	437	437	437	437	437
Panel B: Baseline Employers					
List(s) of *Jobseeking Firm Owners*			0.292	9.209***	0.583***
			(0.357)	(3.526)	(0.190)
List(s) of *Hiring Firms*			-1.029***	-11.823*	-0.978***
			(0.349)	(6.765)	(0.301)
Mean Among List Non-recipients			2.929	7.500	0.375
Observations			234	234	234

Notes: List(s) of *Jobseeking Firm Owners* and List(s) of *Hiring Firms* are binary treatment indicators for acquisition of at least one list of that type. Panel A reports results from the full sample and Panel B restricts the sample to firm owners employing any workers at baseline. All regressions include treatment probability fixed effects, contract type fixed effects, and imbalanced baseline variables. Wages are winsorized at the top one percentile and # of workers includes the owner. Observations are at the firm owner level (February 2020). Robust standard errors are shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: *Hiring Firm* List Inclusion and # of Apprentices

	Outcome: # of Apprentices	
	All Firms	Would Hire Anon.
	(1)	(2)
Panel A:		
# of Apprentices	0.015*	0.011
	(0.009)	(0.018)
Panel B:		
Above Median # of Apprentices	0.430**	0.344
	(0.213)	(0.247)
Baseline # of Apprentices	1.229	1.328
Number of Observations	437	262

Notes: Reported coefficients come from OLS regressions of the number of apprentices working at the firm in February 2020 on two different measures of exposure to the non-owner labor force via the directory experiment. Panel A parameterizes exposure using a count of the number of apprentices at baseline across all of the firms that acquired a *hiring firm* directory with the reference firm owner's name and contact information. This number varies between 0 and 47 apprentices. Panel B is an indicator for being above median on this exposure measure. Column (1) includes all firms participating in the experiment and column (2) restricts the sample to garment makers who were willing to hire the 'anonymous' garment maker. Robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Dyadic Regressions: Evidence for Worker Movements

	Outcome: # of apprentices in firm j		
	(1)	(2)	(3)
	Non-connected firms	Firm j appears on Firm i's Jobseeking Firm Owner List	Firm j appears on Firm i's Hiring Firm List
# of Apprentices Working at Firm i	-0.003***	-0.016	0.016
	(0.000)	(0.012)	(0.018)
Post List Treatment	0.000***	0.001	-0.036**
$\times$# of Apprentices Working at Firm i	(0.000)	(0.006)	(0.018)
Pool of Firms Buying Lists	437	283	278
Pool of Firms Showing up on Lists	437	202	273
Number of Observations	539679	10914	10809

Notes: Reported coefficients come from OLS regressions of number of apprentices in firm j on the number of apprentices in firm i and an interaction with the post-experimental time period, as well as data collection round fixed effects, inclusive of three rounds of data collection: October and December (prior to the experiment) and February (post-experiment). These coefficients are estimated separately for three different samples: i) experimentally non-connected dyads of firms, i.e. cases in which neither firm i appears on firm j's purchased lists, nor vice versa (column 1); ii) dyads in which firm j appeared as a potential worker on a *jobseeking firm owner* list purchased by firm i (column 2); and iii) dyads in which firm j appeared as a potential boss on a *hiring firm* list purchased by firm i (column 3). The standard errors are two-way clustered by firm i and firm j. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Medium-Term Experimental Results

	Open	# of Workers	Wage Bill	IHS Wage Bill	Profits	IHS Profits	Extra Income	IHS Extra Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
List(s) of *Jobseeking Firm Owners*	0.088**	0.030	3.308**	0.237**	3.101	0.386*	-27.873	-0.102
	(0.041)	(0.147)	(1.670)	(0.118)	(9.455)	(0.227)	(25.274)	(0.277)
List(s) of *Hiring Firms*	-0.085**	-0.500***	-3.835*	-0.294**	-9.775	-0.363*	37.452**	0.323
	(0.038)	(0.142)	(2.065)	(0.139)	(10.136)	(0.214)	(17.633)	(0.261)
Mean Among List Non-Recipients	0.692	2.317	3.833	0.244	91.625	3.622	51.667	1.095
Number of Observations	874	437	874	874	874	874	437	437

Notes: List(s) of *Jobseeking Firm Owners* and List(s) of *Hiring Firms* are binary treatment indicators for acquisition of at least one list of that type. All regressions include treatment probability fixed effects, contract type fixed effects, and imbalanced baseline variables. If available, regressions control for the baseline outcome (this is the case in columns (1)-(6), but not in columns (7)-(8)). Wages, profits and extra income are winsorized at the top one percentile. Outcomes in Columns (2), (7), and (8) were only measured in July 2020. Outcomes in Columns (1), (3), (4), (5), and (6) stack June 2020 and July 2020 observations. Robust standard errors in parentheses in Columns (2), (7), and (8). Standard errors are clustered at the firm level where we have two observations for each firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix

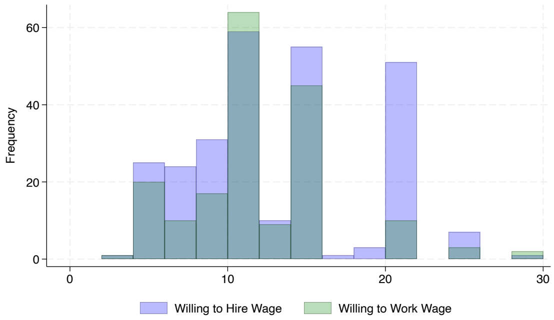
A Appendix Tables and Figures

Figure A1: Labor Reallocation Self-Report Survey Questions

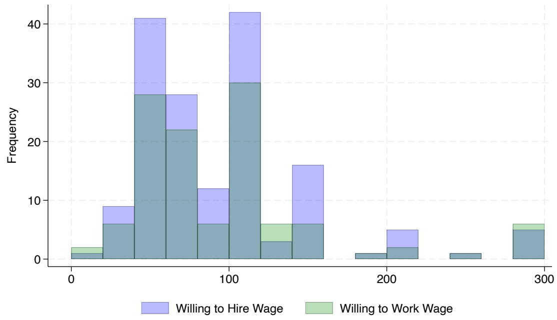
Hiring Piece Rate	*If you know that you will have consistently many garment orders every week for three months, and* if you need an additional piece-rate worker, is there a wage at which you would be open to having the person work for a piece rate in your shop? What is the highest piece-rate amount you would offer this person per child's shirt?
Hiring Fixed Wage	*If you know that you will consistently have many garment orders every week for three months, and* if you need an additional fixed weekly wage rate worker, would you be open to having the person work for a fixed weekly wage rate in your shop? What is the highest weekly payment you would offer this person to work for you?
Working Piece Rate	*If you know that this person will have consistently many garment orders for three months,* is there a wage at which you would be open to work for this person for a piece rate at their shop? What is the lowest amount you would take per child's shirt to work for this person?
Working Fixed Wage	*If you know that this person will have consistently many garment orders for three months,* is there a fixed weekly wage at which you would be open to work for this person for a fixed weekly wage rate in their shop? What is the lowest amount per week you would need to take to work for this person?

Notes: The right column displays the baseline survey questions used to ask respondents about their interest in labor reallocation. These questions were asked for every network contact that the respondent had interacted with in the past year and for the "average garment maker in Hohoe" where "this person" in the question was replaced with either the network contact's name, or the "average garment maker in Hohoe". In addition, firms were randomly assigned to two groups: "hypothetical demand" treatment and control. Firm owners in the "hypothetical demand" group were asked these questions as is, while the control group were asked these questions without the words in italics.

Figure A2: Anonymous Labor Reallocation Self-Reports



(a) Unrealized Matches, Piece Rate



(b) Unrealized Matches, Weekly Salary

Notes: Panel (a) shows overlapping distributions of reported maximum pay for those willing to hire and reservation wages for those willing to work for the average garment making firm owner in Hohoe under a piece rate contract. Panel (b) shows overlapping distributions of reported maximum weekly salaries for those willing to hire and minimum weekly salaries for those willing to work for the average garment making firm owner in Hohoe on a weekly salary. All amounts are in Ghana Cedi.

Table A1: Sample Selection

	(1)	(2)	(3)	(4)	(5)	(6)
	Surveyed in January *Mean*	Attrited in January *Mean*	*Difference* (SE)	Consented to Participate *Mean*	Did Not Consent *Mean*	*Difference* (SE)
Years In Garment Business	14.3	11.3	-3.06**	14.3	14.2	-0.19
			(1.34)			(1.57)
Years In Garment Business, in Hohoe	12.7	9.28	-3.40***	12.7	12.2	-0.48
			(1.27)			(1.49)
Female	0.77	0.83	0.06	0.78	0.67	-0.12*
			(0.06)			(0.07)
In Hohoe Town	0.86	0.68	-0.18***	0.86	0.91	0.05
			(0.05)			(0.05)
# of Workers (Incl. Owner)	2.55	1.47	-1.08***	2.49	3.11	-0.62*
			(0.30)			(0.36)
# Apprentices	1.30	0.40	-0.90***	1.23	1.96	0.72**
			(0.28)			(0.33)
Self-Employed To Be Own Boss	0.52	0.48	-0.04	0.52	0.58	0.06
			(0.07)			(0.08)
Self-Employed For Financial Reasons	0.28	0.30	0.02	0.29	0.18	-0.11
			(0.06)			(0.07)
Self-Employed For Other Reasons	0.20	0.22	0.01	0.20	0.24	0.05
			(0.06)			(0.06)
Observations	509	60		464	45	

Notes: All covariates come from the September 2019 census data. The census captured 569 firms, 509 of which were interviewed in January 2020. *Did Not Consent* includes 44 firms that did not consent to having their information shared and one firm that was dropped from the BDM experimental exercise in error. *In Hohoe Town* denotes firm location, the remaining 14% are located in outlying areas along the main highway leading into and out of town. The F-test p-value for the joint significance of all covariates comparing columns (4) and (5) is 0.24.

Table A2: Summary Statistics

	Mean	SD	Min	Max	Count
Years in Garment Business	14.35	9.78	1	50	463
Years in Garment Business, in Hohoe	12.73	9.51	1	50	463
Female	0.78	0.41	0	1	463
In Hohoe	0.86	0.35	0	1	464
# of Workers (Incl. Owner)	2.49	2.20	1	18	464
# of Apprentices	1.23	2.00	0	14	464
Sales	265.93	254.46	0	2000	454
Profits	163.43	168.70	-210	1200	454
Years of Schooling	8.78	2.56	0	21	455
Ewe	0.68	0.47	0	1	464
Age	38.36	9.18	20	73	455
Ravens Score (of 12)	6.14	2.76	0	11	455
# of Garment Making Network Connections	6.16	6.01	0	33	455
Self-employed to be Own Boss	0.52	0.50	0	1	464
Self-employed for Financial Reasons	0.29	0.45	0	1	464
Self-employed for Other Reasons	0.20	0.40	0	1	464

Notes: Covariates come from the September 2019 census and October 2019 baseline survey. *# of Garment Making Network Connections* is the number of other garment makers in our sample who listed the respondent as a network connection.

Table A3: Labor Reallocation Self-Report on Hypothetical Demand

	(1) Hire PR	(2) Hire Anon PR	(3) Hire FW	(4) Hire Anon FW	(5) Work PR	(6) Work for Anon PR	(7) Work FW	(8) Work for Anon FW
Hypothetical Demand	0.01	-0.01	-0.07	-0.01	0.06	0.05	0.00	0.03
	(0.03)	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)
Constant	0.88***	0.61***	0.72***	0.37***	0.69***	0.38***	0.53***	0.24***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	454	447	454	447	454	447	454	447

Notes: Reported coefficients come from OLS regressions of the outcome listed in the top row on one treatment indicator "Hypothetical Demand", which is a dummy variable that refers to a randomization during the baseline survey data collection in which willingness to hire/to work questions were preceded by a statement assuming "consistently many garment orders." Each column reflects a different type of labor reallocation self-report, where "hire" refers to willingness to hire at least one other garment maker, "work" refers to willingness to work for at least one other garment maker. "Hire Anon" refers to willingness to hire the "average garment maker", while "Work for Anon" refers to willingness to work for the "average garment maker". "PR" refers to whether the firm owner is willing to hire/work for under piece rate contract, and "FW" refers to a fixed rate contract. See Figure A1 for exact wording of the survey question for "Hypothetical Demand". Robust standard errors are in parantheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: WTP by Student Presence

	(1) WTP for List A	(2) WTP for List B	(3) WTP for List C	(4) WTP for List D
Student Present	0.0408	0.0421	0.0131	0.0730
	(0.103)	(0.0964)	(0.0941)	(0.0974)
Observations	464	464	464	464
Adjusted R^2	-0.002	-0.002	-0.002	-0.001

Notes: Reported coefficients come from OLS regressions of different willingness-to-pay measures on a dummy indicating student presence during the WTP exercise. Lists A and B are the two *jobseeking firm owner* directories; Lists C and D are the two *hiring firm* directories. Robust standard errors are in parantheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Willingness-to-Pay by List and Elicitation Characteristics

	(1) Lists of *Jobseeking Firm Owners*	(2) Lists of *Hiring Firms*
Ordered by Sales=0 × High Demand=0 × Fixed Wage=1	0.020	-0.152
	(0.116)	(0.120)
Ordered by Sales=0 × High Demand=1 × Fixed Wage=0	-0.072	-0.050
	(0.116)	(0.121)
Ordered by Sales=0 × High Demand=1 × Fixed Wage=1	-0.063	-0.093
	(0.112)	(0.122)
Ordered by Sales=1 × High Demand=0 × Fixed Wage=0	0.028	0.119
	(0.115)	(0.133)
Ordered by Sales=1 × High Demand=0 × Fixed Wage=1	0.012	0.022
	(0.119)	(0.126)
Ordered by Sales=1 × High Demand=1 × Fixed Wage=0	0.176	0.018
	(0.122)	(0.123)
Ordered by Sales=1 × High Demand=1 × Fixed Wage=1	-0.166	-0.025
	(0.114)	(0.123)
Constant (Mean for Ordered by Sales=0 × High Demand=0 × Fixed Wage=0)	1.046***	0.862***
	(0.081)	(0.093)
Obs.	928	928
Number of firms	464	464
Joint p-value	0.24	0.50

Notes: Reported coefficients come from OLS regressions of willingness to pay for a given list on list characteristics for *jobseeking firm owner* directories (Column (1)) and *hiring firm* directories (Column (2)), where list characteristics are the four contract conditions (with and without hypothetical demand, salary or piece rate) and whether or not the list is ordered by sales. Standard errors are clustered on the firm owner level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Chances of Purchasing a List Given WTP

Chance for List of Either Type		List of *Jobseeking Firm Owners*		List of *Hiring Firms*	
Treatment Probability	GhC Bidding Pattern	Freq.	Share	Freq.	Share
.36	00	88	0.19	116	0.25
.52	01 10	54	0.12	67	0.14
.64	11	78	0.17	93	0.20
.68	02 20	55	0.12	71	0.15
.76	12 21	110	0.24	55	0.12
.84	03 22 30	42	0.09	44	0.09
.88	13 31	18	0.04	11	0.02
.92	23 32	19	0.04	7	0.02
Total		464	1	464	1

Notes: Displayed are the treatment probabilities of purchasing a *jobseeking firm owner* list or a *hiring firm* list, given a firm owner's willingness to pay for each list type, in the leftmost column. The second column shows the Ghana Cedi (GhC) bidding patterns for either list type associated with the treatment probabilities. The maximum bid is 3 GhC per list and 5 GhC in total (over four lists). The four rightmost columns display how the treatment probabilities and bidding patterns are distributed over the 464 firms in our experimental sample, both in terms of absolute and relative frequency. Note that a firm's given bidding pattern for one type of list does not imply the firm's same bidding pattern for the other type of list.

Table A7: Balance by List Treatments

	List(s) of *Jobseeking FirmOwners*	List(s) of *Hiring Firms*	Joint p-value	N
Years In Garment Business	1.375 (0.979)	1.564 (0.956)	0.127	464
Years In Garment Business, In Hohoe	1.142 (0.917)	1.687* (0.924)	0.094	464
Female	-0.072* (0.042)	0.023 (0.043)	0.196	464
Firm Is In Hohoe (As Opposed To Surrounding Area)	-0.012 (0.034)	0.011 (0.034)	0.894	464
# Of Workers	0.113 (0.230)	0.059 (0.214)	0.838	464
# Of Apprentices	0.175 (0.200)	0.061 (0.196)	0.625	464
Sales	28.443 (23.794)	-8.503 (26.984)	0.485	464
Profits	12.886 (16.801)	4.114 (17.012)	0.717	464
Years of Schooling	-0.163 (0.273)	-0.621** (0.269)	0.056	464
Ewe	0.052 (0.051)	-0.068 (0.047)	0.203	464
Age	-0.664 (0.949)	1.535* (0.931)	0.205	464
Ravens Score (Of 12)	-0.057 (0.283)	-0.211 (0.277)	0.727	464
# Of In-Links	0.464 (0.624)	0.665 (0.560)	0.374	455
Self-Employed To Be Own Boss	0.070 (0.053)	0.033 (0.053)	0.329	464
Self-Employed For Financial Reasons	0.036 (0.047)	-0.003 (0.047)	0.747	464

Notes: Reported coefficients come from separate OLS regressions of the covariate of interest on treatment indicators. All regressions include *jobseeking firm owner* directory treatment probability fixed effects, *hiring firm* directory treatment probability fixed effects, and contract condition fixed effects. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Attrition by List Purchases

	Firm is attrited	
	(1)	(2)
List(s) of *Jobseeking Firm Owners*	-0.01	0.00
	(0.02)	(0.03)
List(s) of *Hiring Firms*	-0.01	-0.02
	(0.02)	(0.02)
Treatment Probability FE	No	Yes
Contract Framing FEs	No	Yes
Number of Observations	464	464

Notes: Reported coefficients come from OLS regressions of attrition status on treatment indicators. Column (1) is raw differences. Column (2) includes *jobseeking firm owner* directory treatment probability fixed effects, *hiring firm* directory treatment probability fixed effects, and contract condition fixed effects, mirroring our main specification. Robust standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Balance by List Treatments For Non-Attriters

	List(s) of *Jobseeking Firm Owners*	List(s) of Hiring Firms	Joint p-value	N
Years In Garment Business	1.084 (0.976)	1.453 (0.937)	0.188	437
Years In Garment Business, In Hohoe	0.886 (0.915)	1.554* (0.907)	0.148	437
Female	-0.050 (0.043)	0.016 (0.044)	0.467	437
Firm Is In Hohoe (As Opposed To Surrounding Area)	-0.011 (0.035)	0.004 (0.035)	0.947	437
# Of Workers	0.117 (0.239)	-0.016 (0.224)	0.887	437
# Of Apprentices	0.153 (0.207)	0.010 (0.204)	0.754	437
Sales	26.031 (24.940)	-15.723 (27.985)	0.535	437
Profits	11.184 (17.638)	-1.037 (17.686)	0.818	437
Years of Schooling	-0.167 (0.289)	-0.650** (0.287)	0.061	437
Ewe	0.053 (0.052)	-0.080* (0.048)	0.151	437
Age	-0.921 (0.942)	1.517 (0.922)	0.166	437
Ravens Score (Of 12)	-0.111 (0.288)	-0.275 (0.276)	0.552	437
# Of In-Links	0.510 (0.631)	0.578 (0.559)	0.439	431
Self-Employed To Be Own Boss	0.053 (0.055)	0.033 (0.054)	0.511	437
Self-Employed For Financial Reasons	0.043 (0.049)	0.000 (0.049)	0.687	437

Notes: Reported coefficients come from separate OLS regressions of the covariate of interest on treatment indicators. All regressions include *jobseeking firm owner* directory treatment probability fixed effects, *hiring firm* directory treatment probability fixed effects, and contract condition fixed effects. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Predictors of *Jobseeking Firm Owner* and *Hiring Firm* List Valuations

	Ghanaian Cedis Offered For:			
	(1)	(2)	(3)	(4)
	List(s) of *Jobseeking Firm Owners*	List(s) of *Hiring Firms*	Money Not Offered	Difference (1)-(2)
Years In Garment Business	-0.006	0.007	-0.002	-0.013
	(0.011)	(0.010)	(0.014)	(0.015)
# Of Garment Makers Known In Hohoe	0.003	-0.000	-0.003	0.003
	(0.003)	(0.003)	(0.005)	(0.004)
# Of Workers (Incl. Owner)	0.052*	-0.036	-0.016	0.088*
	(0.031)	(0.030)	(0.036)	(0.049)
Female	-0.439***	0.013	0.426**	-0.451*
	(0.161)	(0.141)	(0.180)	(0.244)
In Hohoe	0.386**	-0.460***	0.073	0.846***
	(0.183)	(0.176)	(0.219)	(0.285)
Ewe	0.398***	0.039	-0.436**	0.359*
	(0.145)	(0.132)	(0.177)	(0.213)
Sales	-0.000	0.000	0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)
Age	0.008	-0.006	-0.002	0.015
	(0.012)	(0.011)	(0.015)	(0.016)
Years of Schooling	0.010	-0.028	0.018	0.038
	(0.025)	(0.023)	(0.030)	(0.038)
HH Monthly Income Per Capita	0.001*	-0.000	-0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Ravens Score (Of 12)	0.025	0.000	-0.025	0.025
	(0.023)	(0.022)	(0.028)	(0.035)
Constant	1.125**	2.470***	1.405**	-1.345*
	(0.467)	(0.437)	(0.582)	(0.693)
Observations	464	464	464	464

Notes: Reported coefficients come from OLS regressions of garment makers' willingness to pay for *jobseeking firm owner* lists (Column 1), willingness to pay for *hiring firm* lists (Column 2), and the amount out of the 5 Cedis given in the willingness to pay exercise that the garment maker decided to keep for herself (Column 3) on a number of baseline characteristics of the firm owners. Standard errors in parentheses. Column 4 shows the difference between amount offered for worker minus hiring lists. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Robustness of Short-Term Treatment Impacts on Labor Inputs

	Working at Another Firm	Employing Another Firm Owner	# of Workers	Wage Bill	IHS Wage Bill
	(1)	(2)	(3)	(4)	(5)
Panel A: all firms					
List(s) of *Jobseeking Firm Owners*	-0.011	0.016*	0.044	4.511	0.310**
	(0.011)	(0.009)	(0.208)	(3.289)	(0.141)
List(s) of *Hiring Firms*	0.004	-0.007	-0.371*	-4.841	-0.445**
	(0.004)	(0.010)	(0.209)	(3.505)	(0.173)
Mean Among List Non-Recipients	0.000	0.000	1.983	4.333	0.252
Number of Observations	437	437	437	437	437
Panel B: baseline employers					
List(s) of *Jobseeking Firm Owners*			0.387	8.098**	0.547**
			(0.401)	(4.012)	(0.214)
List(s) of *Hiring Firms*			-0.962***	-12.457*	-1.036***
			(0.369)	(6.901)	(0.318)
Mean Among List Non-Recipients			2.929	7.500	0.375
Number of Observations			234	234	234

Notes: List(s) of *Jobseeking Firm Owners* and List(s) of *Hiring Firms* are binary treatment indicators for acquisition of at least one list of that type. Panel A reports results from the full sample and Panel B restricts the sample to firm owners employing any workers at baseline. All regressions include fixed effects for the joint treatment probability distribution over both *jobseeking firm owner* and *hiring firm* directories, contract type fixed effects, and imbalanced baseline variables. Wages are winsorized at the top one percentile and # of workers includes the owner. Observations are at the firm owner level (February 2020). Robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Robustness of Short-Term Treatment Impacts on Labor Inputs, only *jobseeking firm owner* lists

	Working at Another Firm	Employing Another Firm Owner	# of Workers	Wage Bill	IHS Wage Bill
	(1)	(2)	(3)	(4)	(5)
Panel A: all firms					
List(s) of *Jobseeking Firm Owners*	-0.010	0.014	0.046	4.081	0.265**
	(0.010)	(0.008)	(0.204)	(2.990)	(0.129)
Mean Among List Non-Recipients	0.000	0.000	1.983	4.333	0.252
Number of Observations	437	437	437	437	437
Panel B: baseline employers					
List(s) of *Jobseeking Firm Owners*			0.262	8.144**	0.497***
			(0.359)	(3.390)	(0.186)
Mean Among List Non-Recipients			2.929	7.500	0.375
Number of Observations			234	234	234

Notes: List(s) of *Jobseeking Firm Owners* and List(s) of *Hiring Firms* are binary treatment indicators for acquisition of at least one list of that type. Panel A reports results from the full sample and Panel B restricts the sample to firm owners employing any workers at baseline. All regressions include treatment probability fixed effects, contract type fixed effects, and imbalanced baseline variables. Wages are winsorized at the top one percentile and # of workers includes the owner. Observations are at the firm owner level (February 2020). Robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Robustness of Short-Term Treatment Impacts on Labor Inputs, only *hiring firm* lists

	Working at Another Firm	Employing Another Firm Owner	# of Workers	Wage Bill	IHS Wage Bill
	(1)	(2)	(3)	(4)	(5)
Panel A: all firms					
List(s) of *Hiring Firms*	0.003	-0.005	-0.403*	-4.618	-0.434**
	(0.004)	(0.009)	(0.208)	(3.481)	(0.168)
Mean Among List Non-Recipients	0.000	0.000	1.983	4.333	0.252
Number of Observations	437	437	437	437	437
Panel B: baseline employers					
List(s) of *Hiring Firms*			-1.070***	-12.074*	-0.965***
			(0.362)	(6.970)	(0.310)
Mean Among List Non-Recipients			2.929	7.500	0.375
Number of Observations			234	234	234

Notes: List(s) of *Jobseeking Firm Owners* and List(s) of *Hiring Firms* are binary treatment indicators for acquisition of at least one list of that type. Panel A reports results from the full sample and Panel B restricts the sample to firm owners employing any workers at baseline. All regressions include treatment probability fixed effects, contract type fixed effects, and imbalanced baseline variables. Wages are winsorized at the top one percentile and # of workers includes the owner. Observations are at the firm owner level (February 2020). Robust standard errors are shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Gender heterogeneity in Medium-Term Experimental Results

	Open	# of Workers	Wage Bill	IHS Wage Bill	Profits	IHS Profits	Extra Income	IHS Extra Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
List(s) of *Jobseeking Firm Owners*	0.078*	0.076	2.246	0.201	-4.575	0.282	-8.635	-0.016
	(0.045)	(0.181)	(1.745)	(0.130)	(10.125)	(0.245)	(7.188)	(0.132)
List(s) of *Jobseeking Firm Owners* × Male=1	0.014	-0.191	1.807	0.093	31.987	0.276	-77.777	-0.739*
	(0.099)	(0.406)	(4.046)	(0.260)	(25.254)	(0.568)	(48.557)	(0.378)
List(s) of *Hiring Firms*	-0.075*	-0.546***	-5.555**	-0.428***	-10.394	-0.333	1.704	0.060
	(0.043)	(0.166)	(2.219)	(0.156)	(10.315)	(0.238)	(7.294)	(0.130)
List(s) of *Hiring Firms* × Male=1	-0.023	0.253	10.988**	0.679**	8.604	0.034	105.092***	0.821**
	(0.084)	(0.332)	(4.602)	(0.306)	(25.509)	(0.515)	(32.307)	(0.323)
Mean Among List Non-Recipients (Male=0)	0.670	2.160	3.600	0.240	87.600	3.509	24.000	0.528
Mean Among List Non-Recipients (Male=1)	0.800	3.100	5.000	0.265	111.750	4.184	35.000	0.645
Number of Observations	874	437	874	874	874	874	874	874

Notes: List(s) of *Jobseeking Firm Owners* and List(s) of *Hiring Firms* are binary treatment indicators for acquisition of at least one list of that type. All regressions include treatment probability fixed effects, contract type fixed effects, and imbalanced baseline variables. If available, regressions control for the baseline outcome (this is the case in columns (1)-(6), but not in columns (7)-(8)). Wages, profits and extra income are winsorized at the top one percentile. Outcomes in Columns (2), (7), and (8) were only measured in July 2020. Outcomes in Columns (1), (3), (4), (5), and (6) stack June 2020 and July 2020 observations. Robust standard errors in parentheses in Columns (2), (7), and (8). Standard errors are clustered at the firm level where we have two observations for each firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.