I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14839

Formalized Employee Search and Labor Demand

Lukas Hensel
Tsegay Gebrekidan Tekleselassie
Marc Witte

NOVEMBER 2021

I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14839

Formalized Employee Search and Labor Demand

Lukas Hensel
Peking University

Tsegay Gebrekidan Tekleselassie
Policy Studies Institute

Marc Witte
IZA

NOVEMBER 2021

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Formalized Employee Search and Labor Demand*

Firms often use social networks to find workers, limiting the pool of potential applicants. We conduct a field experiment subsidizing firms' formal vacancy posting. The subsidies increase non-network employee search and shift vacancies towards high-skilled positions. Post-treatment, firms continue searching for high-skilled workers despite reverting to network-based search. This change in skill requirements does not increase vacancy posting or hiring, suggesting substitutability between workers of different skill levels. Finally, we experimentally show that information asymmetries about applicants' skills do not limit firms' formal search. Our results highlight that exposure to different labor market segments can permanently change firms' labor demand.

JEL Classification: D22, J23, J46, C93

Keywords: firms, hiring, social networks, formalization, field experiments

Corresponding author:
Marc Witte
Forschungsinstitut zur Zukunft der Arbeit (IZA)
Schaumburg-Lippe-Str. 5-9
53113 Bonn
Germany
E-mail: witte@iza.org

* We thank Stefano Caria, Morgan Hardy, Rob Garlick, Clément Imbert, Erin Kelley, Pramila Krishnan, Jeremy Magruder, Mahreen Mahmud, Kate Orkin, Simon Quinn, Climent Quintana-Domeque, Jonas Radbruch, Chris Roth, Chris Woodruff, as well as seminar participants at Applied Development Conference, UC Berkeley, the Ethiopian Economics Association, EEA conference, ESPE conference, Exeter, IZA, Jobs and Development Conference, NEUDC, NYU Abu Dhabi and Oxford for useful comments and help. We thank Abis Getachew Makuria for outstanding research assistance. Financial support from grants by the International Growth Centre (Grant 32425), Private Enterprise Development in Low-Income Countries (PEDL Exploratory Research Grant 6023), and the Centre for the Study of African Economies is gratefully acknowledged. Lukas Hensel gratefully acknowledges financial support by the Wellspring Philanthropic Fund. The research described in this article was approved by the Economics Departmental Research Ethics Committees (DREC) at the University of Oxford (ECONCIA18-19-14).

1 Introduction

A long-standing question in development economics is how the prevalence of informal institutions affects socioeconomic outcomes. Informality has been studied in areas as diverse as credit markets (e.g. Banerjee et al., 2021), insurance (e.g. Mushfiq et al., 2013), and work relationships (e.g. Ulyssea, 2018, 2020). Much of this literature defines formality based on the existence of explicit contracts, registrations, or regulated markets. However, this abstracts from informal processes that underlie the observed market outcomes. Consider a firm that wants to hire a new worker and searches informally through networks rather than advertising a job publicly. On the one hand, this can alleviate information frictions and moral hazard problems (Dustmann et al., 2016, Heath, 2018), leading to more productive employment relationships. On the other hand, sparse networks might limit firms' ability to expand their business, leading to aggregate welfare losses (Chandrasekhar et al., 2020).

In this paper, we investigate the effect of incentivizing firms to advertise their vacancies through formal channels. For this purpose, we offer vacancy posting services to a randomly-selected subset of 625 small and medium-sized firms in Addis Ababa, Ethiopia. As part of these services, we first fully subsidize firms' vacancies in all commonly-used 'formal' channels, including online and offline job boards and newspaper advertisements.[1] Second, we also take care of the actual posting of the vacancy, thus covering logistical costs. Taken together, we subsidize and post all job adverts of the firm, regardless of the posting price.[2] Treated firms are eligible to use the vacancy services for four months. We collect detailed data on all firms' vacancy creation and hiring behavior during and after the treatment period. To ensure that we capture all attempts at finding new employees across treat-

[1] Specifically, we post the vacancies (i) on the five largest physical job-boards in Addis Ababa, (ii) in the largest national biweekly newspaper 'The Reporter', (iii) on the online job-board 'Ezega.com'.

[2] The costs of posting a formal vacancy exceed the average monthly wage of employees in our sample.

ment groups, we also administer five waves of phone surveys during the treatment period in addition to an in-person endline survey. Moreover, we conduct multiple post-treatment phone surveys for two months after the end of the treatment period to capture post-intervention behavior.

Beyond a pure formalization of the hiring process, the treatment could affect firms' hiring behavior in three different ways. First, a reduction in the cost of formal employee search might directly affect vacancy creation: it might become more profitable for firms to create new vacancies and hire additional employees. Second, formalized search exposes firms to a different pool of job-seekers. Given the differential search costs for job-seekers across search channels, this pool is more likely to contain skilled workers looking for relatively well-paid jobs (cf. Rebien et al., 2020).[3] Firms might adjust to the new pool of job-seekers by shifting the skill requirements of vacancies. Finally, the intervention might prompt firms to learn new information about the labor market, which could shift their hiring behavior beyond the treatment period.

Our study has four key findings that speak to our hypotheses. First, while our intervention successfully increases formal vacancy posting in treated firms, we do not observe an average increase in the total number of vacancies created. Treated firms are 3.3 times and statistically significantly more likely than control firms to post at least one vacancy through formal search channels. However, there is no significant difference between treatment and control firms in terms of vacancy creation. Instead, we observe a significant reduction in the fraction of filled vacancies by 20 percentage points. We provide evidence for two mechanisms behind this reduction in successful matches. First, applicants obtained through formal search channels have high wage expectations and high reservation wages relative to the realized

[3]We also find evidence that formally-posted vacancies are disproportionately targeted at higher-skilled job-seekers. Based on all publicly-posted job advertisements in Addis Ababa during our study period, we find that 27% of vacancies require applicants to have a diploma, 37% a BA degree, and only 5% require less than 10 years of schooling. These requirements are high compared to the education levels in the population of job-seekers in Addis Ababa, where only 12% have at least a BA degree, 15% a diploma, and 34% have fewer than 10 years of schooling (according to the 2018 Ethiopian Labor Force Survey).

wages of filled vacancies and average baseline salaries. This makes it more difficult for firms to successfully hire new employees. Second, firm managers update negatively about both the quality and quantity of applicants in formal search channels. This could lead to firms posting overly-ambitious vacancies or having excessively stringent screening criteria.

Our second finding further explains why treated firms struggle to fill vacancies. We find that treated firms are a significant 5.6 percentage points more likely to create at least one white-collar vacancy. This is an increase of 81 percent relative to the control group mean. Additionally, both the overall number of white-collar vacancies and the share of white-collar postings among all vacancies increase by approximately 40 percent compared to control firms. This increase in white-collar vacancies does not lead to an increase in white-collar hires, which instead remain constant, with point estimates close to zero. For non-white-collar vacancies, we observe no significant change in vacancy creation and a relatively large (8 percentage points) but insignificant decrease in the fraction of firms reporting any non-white-collar hires. These results suggest that firms indeed change the composition of their labor demand towards more high-skilled positions, which is consistent with firms anticipating being able to access a higher-skilled applicant pool through formal employee search.[4] However, these expectations are not met and firms do not hire more white-collar employees despite the more formalized search.

Our third finding speaks to how incomplete information about applicant skills might prevent formal employee search. We randomly offer half of the treated firms the option of having all applicants to their vacancies screened for three cognitive or socio-emotional skills, on top of the vacancy posting subsidy.[5] This add-on treatment does not impact the uptake of the intervention, vacancy creation, or hiring outcomes, suggesting that information frictions about applicants' skills are not driving low levels of formal employee

[4] In line with this interpretation, we find that control group firms posted 41% of white-collar vacancies but only 4% of non-white-collar vacancies formally.

[5] While firms could have opted out from this screening service, all firms that used the vacancy posting subsidy in this treatment group also elected to add the screening component.

search. This contrasts with a growing body of literature documenting the importance of such frictions for job-seekers (Abebe et al., 2020a, Carranza et al., 2020, Bassi and Nansamba, 2021, Abel et al., 2020).

Finally, we show that some treatment effects persist beyond the treatment period. On the one hand, we find that once the vacancy subsidy runs out, treated firms do not continue posting job adverts in formal channels. However, on the other hand, we observe a persistent shift from non-white-collar vacancy creation to white-collar vacancies. Moreover, we observe significant decreases in both average vacancy creation and hiring levels during the post-treatment period. This pattern suggests that firms persistently update their beliefs about labor market conditions and maintain their response even after the treatment has ended. We indeed document that treated firms become more pessimistic about the merits of formalized employee search, which might explain the observed decreases in vacancy creation and hiring. We provide suggestive evidence that instead of hiring new employees, managers reduce turnover among existing employees by paying them more.

The persistence of treatment effects points to the importance of firm learning as a key mechanism behind firms' reaction to outside interventions. Interventions can incentivize firms to learn about labor market conditions, but also direct attention to previously-neglected management practices or business strategies. More generally, in line with recent theoretical work by Chandrasekhar et al. (2020), our findings suggest that firms face important information frictions that prevent them from learning about local conditions. While we cannot directly speak to whether the lack of experimentation is optimal given its cost, the sustained treatment effects after the treatment period suggest that firms' ex-ante behavior would not be optimal in a frictionless world. The implication is that providing firms with better information about local labor market conditions could lead to more productive firms.

With these findings, we make four main contributions to the literature. First, we show that informal hiring practices affect the type of positions for which firms search and hire. This provides evidence supporting Rebien et al. (2020)'s finding that firms search more formally for high-skilled workers in Germany. Previous work on the labor constraints faced by small and medium-sized enterprises (SMEs) in low- and middle income countries has found mixed results. Hardy and McCasland (2018) document that alleviating employee search constraints for SMEs in the manufacturing sector in Ghana through a local matchmaking process leads to increases in firm size and profits. Other experiments that use temporary wage subsidies to encourage hiring without alleviating search constraints find no evidence of permanent effects on firm outcomes (de Mel et al., 2019, Groh et al., 2016, Galasso et al., 2004). This is the first paper to study the effect of firm-side vacancy posting costs on vacancy posting behavior. Our results suggest that search frictions can be specific to the worker's position and that firms' labor force composition is endogenous to such search frictions. A paper close to ours set in a developed country context is Algan et al. (2020), who study an intensive bundled intervention for French small and medium-sized firms. Their intervention comprises vacancy drafting and posting services similar to ours, but in addition contains access to the public CV database, pre-screening and interviewing services, as well as matching and post-hiring support (such as contracting). They find a 24% increase in vacancies posted through the employment services and 10% increase in formal hires. In contrast to their bundled intervention, we disentangle the role of two specific frictions: vacancy posting cost and information frictions about work seekers. Moreover, our study is set in a labor market with a much higher prevalence of informal employee search and thus speaks to frictions that might prevent the development of more formal labor market institutions.

Our paper also speaks to the large body of literature on the importance of search frictions for job-seekers by emphasizing the importance of different search channels (Abebe et al., 2020a, Carranza et al., 2020, Bassi and Nansamba, 2021, Abel et al., 2020, Wheeler et al., 2021). In particular, the change in vacancy composition that we observe might also be a response to the selection of job-seekers into different search channels. This is in line with evidence demonstrating an important role of liquidity constraints for application decisions (Abebe et al., 2021).

Second, we also speak to a nascent body of literature documenting unintended long-term consequences of labor market interventions on the beliefs and behavior of firms and job-seekers in developing countries. Abebe et al. (2020b) document long-term changes in search and hiring behavior in response to attending a 'disappointing' job fair that led to firms negatively updating about the average quality of job-seekers in the same context as our study. They also report a shift towards more formal search channels and a reduction in hiring levels. Our results suggest that such firms might also be disappointed by the results of formal employee search channels, leading to a reduction in average hiring levels and providing a potential mechanism behind the results in Abebe et al. (2020b). Moreover, Bandiera et al. (2020) show that lower-than-expected callback rates of a matching intervention have long-term impacts on the beliefs and search behavior of job-seekers, leading to substantially worse labor market outcomes. Importantly, both papers study the impact of being negatively surprised by the quality of newly-created matching interventions.

This paper documents that firms face negative surprises even within existing labor market institutions. This suggests important information frictions during the hiring process and a lack of experimentation by firms, with potentially important consequences for firms' labor demand (Hanna et al., 2014). Focusing on the job-seeker side of the labor market, Kelley et al. (2020) study the impact of using job portals for entry-level job-seekers in India. They find that treated job-seekers have higher reservation wages and end up working less in response to signing up to the platform. Our study emphasizes that unintended consequences of being exposed to existing labor market structures can also affect the demand side of the labor market.

Third, and more broadly, we contribute to the literature studying different hiring channels such as networks (Calvó-Armengol and Jackson, 2004, Beaman and Magruder, 2012, Kramarz and Skans, 2014, Heath, 2018, Witte, 2021) and job fairs (Beam, 2016, Abebe et al., 2020b). We study the advertisement of formal vacancies on online and physical job boards as well as in newspapers. Similar to the literature on job fairs, we do not find strong short-term effects on aggregate hiring numbers. However, we document a marked shift in the composition of posted vacancies and—for a subgroup of firms—hires. This emphasizes the importance of firms' endogenous response to changes in the cost and availability of search methods.

Finally, our paper speaks to a long-standing debate in development economics on the importance of formal versus informal institutions and markets. A nascent strand of literature has emphasized how the introduction of formal institutions can interact with (Comola and Prina, 2021) or even crowd out (Banerjee et al., 2021) pre-existing informal mechanisms; for instance, in credit or insurance markets. Similarly, firms in our study were exogenously nudged towards formal vacancy posting, with the help of randomly-allocated subsidies. However, in contrast to much of the existing research, we do not find that this 'push towards formality' permanently reduces informal behavior. Instead, after the subsidy runs out, the market strongly reverts back to the informal mechanism. Firms even update their beliefs about the usefulness of formal search channels downwards. This suggests that the informal search in our context might be optimal for firms given the current labor market conditions.[6]

The remainder of this paper proceeds as follows. In section 2, we describe the context and our data and we link the labor market in the context of Addis Ababa theoretically to our intervention in section 3. In section 4, we describe the experimental design before we present and discuss the treatment effects of our experiment in section 5. Section 6 concludes.

2 Context

Our study took place between March and November 2019 in Addis Ababa, the capital of Ethiopia. With an average GDP growth rate of almost 10 percent over the last decade, Ethiopia is one of the fastest-growing countries in

[6] This does not mean that formal search processes in aggregate are not efficient. It is possible that, once a larger fraction of job-seekers use formal search channels, formal employee search will become profitable for firms.

Sub-Saharan Africa (World Bank, 2020). At the same time, most of the country's young urban population is out of permanent or formal employment, while rural areas are traditionally dominated by subsistence agriculture. Unemployment rates are particularly high for young people who graduate from high school or higher education institutions, despite widely-reported shortages of qualified employees by Ethiopian firms, suggesting a problem with matching job-seekers to vacancies. Similar to many other urban labor markets in low- and middle-income countries, the labor market in Addis Ababa is characterized by a large degree of network-based job and employee search (Serneels, 2007). In principle, this could both be a response to and a cause of the matching problem described above: firms might rely on social network search to overcome information asymmetries vis-à-vis the job candidates, but at the same time suffer from a restricted pool of applicants with a limited distribution of skills or abilities.

Job search in developing countries is often very expensive for both job-seekers and firms. For example, Carranza et al. (2020) document that active job-seekers in Johannesburg spend more than 30 percent of their weekly expenditure on job search-related expenses. By contrast, firms face the dual problem of extremely high numbers of applications for certain jobs on the one hand, while on the other hand they struggle to hire qualified personnel for positions demanding more extensive skill-sets. Employee search via formal channels such as newspapers and job boards in particular is relatively costly for firms. For example, in the context of our study, posting a single job ad in a newspaper in the smallest available format costs about 3,800 ETB (105 USD), which is more than the average monthly salary that firms in our sample pay their workers.

Perhaps as a consequence of the substantial costs involved, formal vacancy posting is not very common among firms in Addis Ababa. To illustrate this, we create a database of publicly-posted vacancies in Addis Ababa, comprising 29,312 job advertisements posted over 36 weeks.[7] In February 2019,

[7]This database covers almost 100 percent of all posted vacancies in Addis Ababa. We collect data on job advertisements from the four main sources of job advertisements in the

there were 438,747 formally registered firms in Addis Ababa. If we conservatively assume that every firm hires one employee per year, we should expect on average 438,747/52 $\simeq$ 8,437 posted vacancies per week. Instead, we find approximately 814 unique vacancies posted in the city per week. This means that only a small fraction of approximately 10 percent of firms post vacancies.[8] At the same time, vacancy posting is skewed towards higher-status jobs, with almost half of vacancies requiring a university degree and and only 14 percent requiring high school education or less.

The matching problem described suggests that finding suitable employees is not a trivial task for a firm, and that the skills required for firms' production processes are not always easily understood. For instance, while some firms might require skills that are frequently distributed in formal applicant pools, other firms or type of jobs require rare or difficult-to-observe skills. In these latter cases, formal vacancy posting might not yield a distribution of applicants with a larger density of such skills compared to—say—network-based search. This means that there are good reasons to expect heterogeneity in the treatment effects of our intervention.

2.1 Sample recruitment

For our study, we recruited SMEs in Addis Ababa in two ways. First, we obtained a list of registered firms in Addis Ababa from the municipal authorities. Second, our field team went to recruit firms face to face in well-known business areas. To participate in our study, firms had to meet the following criteria. First, they had to in principle express interest in a generically-described service that would help their firm with job advertising. Second,

city: i) the ten largest physical vacancy boards located across the city, ii) vacancies in the three major newspapers, iii) the four largest online job boards (www.employethiopia.com, www.ethiojobs.com, www.ezega.com, www.mjobs.com), and iv) the largest social media job channel (on the messaging service 'Telegram'). We collect the data on a weekly basis between March and October 2019.

[8] If we compare this number to similarly-sized cities in rich countries, we note that on the single platform `indeed.com` alone, there are approximately 6,500 unique vacancies per week in Berlin, 7,000 in Birmingham, and 14,000 in Madrid.

they had to have between 5 and 50 employees. Third, they could not exclusively hire through employment agencies. Finally, they had to not rule out hiring a new worker over the next three months. We chose these screening criteria to identify firms that were likely to use our intervention to increase statistical power.[9] In total, we recruit 625 firms that meet our criteria during the screening survey. These firms are spread out across Addis Ababa (see Online Appendix Figure A2).

2.2 Summary statistics

Summary statistics are presented in Table 1. Firms in our sample have on average 14.5 employees, of which 14 percent are highly-educated white-collar workers.[10] 51 percent of firms are in the manufacturing sector and 27 percent in the hospitality or retail sectors. 26 percent of our firm respondents are female and the average age is 35 years.[11] They are well educated, with 45 percent of respondents having a university degree.

Existing employee search channels are largely informal and network-based. 79 percent of sampled firms use network-based search for employees, and 50 percent of firms exclusively rely on network-based employee search. Only 9 percent of firms post their vacancies through formal channels (i.e., in newspapers or on job board; no firm uses online job boards at baseline), closely mirroring our city-wide back-of-the-envelope calculations.

Overall, the firms in our sample are relatively optimistic about their business. 62 percent and 77 percent of firms have a positive business outlook for the next three and twelve months following the baseline survey, respectively. Furthermore, in the three months after the intervention firms expect to hire

[9] This sampling strategy limits the external validity of our findings. We discuss the generalizability of our findings in section 5.5.

[10] Two firms report more than 50 employees due to changes between the screening and baseline survey.

[11] For all of our data collection activities, we asked to speak with *the person in charge of hiring decisions*. In the vast majority of cases, this is the firm manager or owner. In the few cases where there was managerial turnover during the study period, we instead interviewed the new person in charge of hiring decisions.

on average 3.46 new workers.

We observe stark heterogeneity in the firms' current application volumes. At baseline, 37 percent receive about the appropriate number of applications, while 23 percent of firms state that they receive too few applications and 40 percent receive too many. 11 percent of firms do not know. On average, firms think that by posting their vacancy on one (additional) job board in the city center, they would receive eleven more applications.

[Table 1 about here.]

3 Conceptual framework

Theoretically, employee search through networks can have ambiguous effects. While networks can improve worker-firm matches by alleviating information frictions and moral hazard problems (Dustmann et al., 2016, Heath, 2018), sparse networks might constrain firm growth, lowering aggregate welfare (Chandrasekhar et al., 2020). By reducing the cost of formal employee search to almost zero, firms are likely to switch to using more formal employee search, in particular for vacancies where networks are sparse or less useful in reducing moral hazard. This is particularly likely for high-skilled white-collar jobs, as firms have few existing white-collar workers, which limits the scope of their network. We discuss four potential dimensions of how the vacancy posting subsidy might affect firms' behavior.

First, the subsidy might affect firms' level of vacancy creation. As the subsidy reduces the (marginal) cost of employee search, it reduces the required level of expected productivity for new hires, and hence could lead to an increase in vacancy creation. However, the decision to hire employees is discrete (in particular for the SMEs in our sample) and a decrease in marginal costs does not necessarily make an additional hire more profitable. The predicted effect crucially depends on the magnitude of the reduction in search costs and the marginal profit associated with an additional hire.

Second, making formal search more affordable exposes firms to a different pool of job-seekers. Job-seekers who look for jobs in formal channels generally invest more resources—such as transport costs and mobile internet—in the hope of securing well-paid, formal jobs. Given the relative costliness of formal search for both sides of the labor market, jobs with higher potential matching surpluses should be more likely to be filled through formal channels. The vacancy posting subsidy reduces the cost for firms while keeping the potential pool of applicants constant. Hence, one could expect that firms shift the composition of their vacancies towards high potential surplus vacancies as they have easier access to suitable job-seekers for these positions.

Third, vacancy posting subsidies could lead to firms learning about the labor market. In the framework of Chandrasekhar et al. (2020), firms might—even in the long-run—not realize that formal employee search is profitable. If there are different types of jobs, firms might not even learn about the profitability of hiring different kinds of workers. The vacancy subsidy could induce firms to learn about both dimensions and change their long-term behavior accordingly. We explicitly test this theory using two months of post-treatment data.

Finally, firms might shift forward planned vacancies and hires so that they take place within the treatment period and are covered by the intervention services. The post-treatment data enables us to detect such behavior.

4 Experimental design

Our field experiment is designed to study how subsidizing formal employee search channels affects vacancy postings and employment flows. Figure 1 displays the experimental design of our study. We randomly allocate firms to one of two groups. Randomization happens at the firm level at the end of the baseline survey. Firms in the treatment group are offered the opportunity to post their job adverts on up to five physical vacancy boards, one major online job board (`www.ezega.com`) and the major newspaper "The Reporter" at no cost. To facilitate take-up of the intervention, firms are offered to send an electronic copy of the job advert or alternatively research staff would pick up a hard copy at the firm's premises. This offer covers all vacancies during the four-month treatment period.[12] We additionally randomize 50 percent of firms in the vacancy subsidy group to receive an applicant screening intervention in addition to the vacancy subsidy. Firms in this group are offered a screening of all applicants to their vacancies for cognitive and socio-emotional skills. The results of this screening are then passed on to the firm. We use this additional treatment to test whether a lack of information about job-seekers' skills affects firms' vacancy posting and hiring. For most of the analysis, we use the pooled treatment group to focus on the effect of the vacancy posting subsidy. This reflects the fact that there are few significant differences between the two arms. Where there are differences, we note them explicitly.

[Figure 1 about here.]

4.1 Data collection

We survey 625 eligible firms before, during, and after the treatment period, for a total of 6,068 interviews. After the screening survey, we conduct an in-person baseline survey to capture manager and firm characteristics, expectations, as well as pre-existing hiring practices. The end of the baseline survey also marks the beginning of the four-month intervention period. During the intervention period, we carry out regular phone-based surveys to capture vacancy postings and hiring.[13] On average, we conduct more than five phone surveys during the treatment period per firm.

At the end of the treatment period, we conduct an in-person endline survey to capture employment flows and levels, firm-level characteristics, and manager beliefs on the effectiveness of different hiring channels. For the main analysis of vacancy posting and employment flow data, we aggregate

[12] The timeline of the study is shown in Appendix figure A1.

[13] For firms that were not responsive to the phone calls, we conduct the survey in person.

the phone surveys and the endline survey at the firm level to facilitate interpretability (McKenzie, 2012). After the end of the treatment period we conduct further phone surveys with sampled firms to assess whether our intervention changes behavior in the two months following the treatment period.

4.2 Experimental integrity

To check whether the randomization successfully achieves balance on baseline observable characteristics, we present Appendix Table A1. Out of sixteen tested variables, we only observe one significant baseline imbalance (at the ten percent level), which suggests that the randomization worked as intended.[14] Controlling for this variable does not affect the results in systematic ways.

Attrition levels are generally low and mostly balanced across treatment groups (Appendix Table A2). We manage to reach 96 percent of firms to conduct at least one phone survey (5.6 surveys per firm on average). Furthermore, we successfully reach 97 percent of firms for our in-person endline survey. For our main analysis we pool both data sources, which means that we have outcome data for 100 percent of control group firms and 99 percent of treatment firms (four firms in the treatment group could neither be reached during phone surveys nor the endline survey, meaning that they also did not take up the intervention). While the latter difference is significant at the 5 percent level, it is very small and very unlikely to influence our results. Finally, we manage to contact 88 percent of firms at least once during the two-month post-treatment period (for an average of 2.6 surveys). Reassuringly, there is no statistical difference between the treatment and control group attrition rates during the post-treatment period.

[14]The exception is the gender of the interviewed firm manager.

5 Results

In this section, we describe the treatment effects of the vacancy subsidy intervention on our main outcomes. We use the following equation to estimate treatment effects:[15]

$$y_i = \beta_0 + \beta_1 vacsub_i + \varepsilon_i \tag{1}$$

where y_i is the firm-level outcome of interest. y_i is aggregated across phone surveys and the endline survey whenever possible. $vacsub_i$ is a dummy variable equal to one if firm i is eligible for the vacancy posting subsidy treatment. We use heteroskedasticity robust standard errors throughout the analysis.[16]

Take-up and formalization of employee search We find a large and highly significant increase in the use of formal vacancy posting and a decrease in the use of networks for employee search (columns (4) to (9) of Table 2). In particular, we find a 17-percentage-point (331 percent of the control mean) increase in the fraction of firms posting vacancies through formal channels for at least one of their vacancies (p<0.01). This goes hand in hand with a substantial increase in the absolute number of formally-posted vacancies (by 0.46 vacancies per firm or 320 percent, p<0.01) and the fraction of va-

[15]We registered a pre-analysis plan for this project in which this is the main specification. We deviate from the pre-analysis plan in the following main ways. First, we expand the number of outcomes, as we consider studying both the extensive and intensive margins and the success ratio of vacancy creation. To account for this we include all variables in the multiple hypothesis test correction. In line with these changes, we do not normalize outcomes over time to be able to use extensive margin outcomes. Second, we do not normalize by treatment duration as we do no observe differential attrition by treatment group. Third, we use pooled treatment effect estimation instead of separate effects for a screening add-on intervention as our main specification. Finally, we do not show hire- and vacancy-level specifications and outcomes for which the data quality is insufficient. More details can be found in Online Appendix Section D.

[16]In Online Appendix Section B we estimate treatment effects controlling for observable firm and manager characteristics. Specifically, we control for pre-specified covariates by using the post-double LASSO method for each outcome separately (Belloni et al., 2013). The results remain quantitatively and qualitatively unchanged.

cancies posted through formal means (31 percentage points or 447 percent, $p<0.01$). These large effect sizes suggest that our intervention succeeds in increasing the formalization of vacancy posting among treated firms.

Furthermore, firms are selective in using our intervention to post job adverts. Column (3) of Table 2 shows that on average firms in the treatment group post 0.56 vacancies through our intervention, which amounts to 73 percent of vacancies of firms that use the intervention at least once (or 35 percent of all posted vacancies in the treatment group). In total, among firms that posted any vacancy during the treatment period, 48 percent use the vacancy subsidy at least once.[17] This suggests that despite initially being interested in using the intervention, firms are selective in their use of formal search channels. Moreover, this indicates that the returns to formal employee search might vary substantially across firms and vacancies.

[Table 2 about here.]

5.1 Impact on vacancy creation and hiring decisions

The vacancy subsidy intervention was designed to reduce the marginal cost of posting vacancies through formal channels. This decrease in marginal costs should make it more attractive for firms to post vacancies. To test this hypothesis, we estimate treatment effects on vacancy creation in Table 3.

We find no significant treatment effect on vacancy creation on either the intensive or intensive margin of vacancy creation. On average, we observe an increase in the total number of vacancies by 0.12, although this effect is not statistically significant (column 2). Interestingly, the treatment group also exhibits a 4.8-percentage-point (10 percent of the control mean), non-significant decrease in the fraction of firms posting any vacancy (column 1).

[17] Overall vacancy posting levels are lower than anticipated, at least partly due to external events. In May and June of 2019, there were frequent power cuts due to nationwide electricity shortages that negatively affected the operations of firms in our sample. Around 35 percent of baseline firms reported that they changed their business activities in response to the electricity outages, with 20 percent of firms postponing hiring. Furthermore, there was a coup attempt on June 22, 2019, which lead to a nationwide internet shutdown and slowed down or stopped business activities for about two weeks.

[Table 3 about here.]

Hiring outcomes We observe a significant reduction in the fraction of vacancies successfully filled (Table 3, columns 4-6). Specifically, we observe a reduction of 20 percentage points in the fraction of successfully-filled vacancies (down from a control group mean of 88 percent and significant at the 1 percent level even after MHC, column 6). Similarly, the fraction of firms filling any vacancy (and thus making any hire) falls by 8 percentage points, which is significant at the 10 percent level before MHC and marginally insignificant afterwards. This could be due to various factors, including a shift in the nature of posted vacancies or the observed shift in employee search channels. This pattern translates into a sizable (0.21 hires or 17 percent of the control group mean) but insignificant decrease in the number of hires. This decrease is driven by a reduction of firms successfully hiring any candidate rather than by the number of hires of actively-hiring firms. Put differently, we observe marginally significant treatment effects on the extensive but not the intensive hiring margin.

5.2 The role of information frictions about worker skills

Is the usefulness of our intervention constrained by firms' inability to prescreen applicants obtained through formal networks? Information frictions have been found to be an important aspect in many labor markets in developing countries—including in Ethiopia—and could limit the effectiveness of formal employee search (Carranza et al., 2020, Abebe et al., 2019, Bassi and Nansamba, 2021). To test whether limited information about candidate skills constrains the use of formal search channels, we offer half the firms in the treatment group the option to have all applicants screened for three cognitive or socio-emotional skills of the firm's choice.[18] We invite all applicants to a screening center in downtown Addis Ababa for a screening

[18]To ensure that the screening is relevant for firms, we let them choose from a list of ten skills that are commonly associated with labor market success.

session. When then pass on their test results (grouped in terciles among all applicants) on to the hiring managers who are then free to arrange interviews according to the results.

Overall, we find very little heterogeneity based on whether firms receive an additional screening intervention (Appendix Table A8). Columns (1) to (3) show no difference in the formalization of employee search by treatment group. Similarly, columns (4) to (8) show that there are no statistically significant differences in vacancy creation or hiring numbers. The screening add-on also does not affect the skill composition of created vacancies and hires (Appendix Table A9). This suggests that even if firms face more severe information frictions when using formal search channels, these frictions do not seem to limit firms' use of formal vacancy posting or affect their vacancy creation when posting costs are subsidized.

5.3 Composition effects

Beyond the effect on the number of vacancies and hires, the vacancy subsidy could also affect the type of employees for whom firms search. The subsidies incentivize firms to search in a different and potentially previously-unknown labor market segment. This formal segment of the labor market is likely to predominantly contain jobs that are difficult to fill through networks (cf. Rebien et al., 2020). These presumably include positions that do not make up a large share of the firms' existing workforce, such as high-skilled white-collar jobs.[19] Indeed, we find that, among firms in the control group, white-collar vacancies are more than four times as likely to be posted formally compared to non-white-collar vacancies (42 vs 4 percent, respectively). This suggests that the returns to formal employee search are higher for white-collar vacancies, which could affect how firms use the vacancy posting subsidy.

Hence, we study how the intervention affects the composition of vacancies created during the treatment period. Table 4 shows the impact on the

[19] We define white-collar vacancies as "Professional, Managerial, or Administrative" workers. Typical white-collar job titles include manager, accountant, and supervisor. Typical non-white-collar jobs are cooks, waiters, or carpenters.

skill composition of posted vacancies. We observe a significant increase in the level of white-collar vacancy creation at all margins, (columns (1) and (2)). On average, the number of white-collar vacancies increases by 0.094, which is equivalent to an increase of 78 percent of the control group mean. We observe no significant changes in the number of non-white-collar vacancies (columns (5) to (6)). If anything, we observe a relatively sizable but insignificant decrease in the likelihood of posting any non-white-collar vacancy by 5.3 percentage points. The two results combined yield a significant increase in the fraction of white-collar vacancies by 6.4 percentage points on average. This suggests that the treatment leads firms to use white-collar vacancies as substitute for non-white-collar vacancies.

This composition effect does not translate into different hiring outcomes. We observe that the negative effects on the fraction of filled vacancies is present for both white-collar and non-white-collar vacancies (columns (3) and (7) of Table 4). However, the point estimate for white-collar vacancies is almost twice as large in absolute terms (-36.1 percentage points for white-collar vacancies vs -19.3 percentage points for non-white-collar vacancies). Table 5 explores hiring numbers and shows no significant impacts and small effect sizes for white-collar hires. This is true for the fraction of firms hiring any white-collar workers, the total number of white-collar hires, and the fraction of white-collar hires. We observe a marginally significant (before MHC) decrease of 7.8 percentage points in the number of firms conducting any non-white-collar hire, in line with the results on overall hiring numbers. In line with the negative hiring effects being driven by non-white-collar workers, we show in Appendix table A12 that at endline the share of white-collar employees is a significant 2.4 percentage points (or 24%) higher in treated firms.

Impact on hire characteristics The change in type of vacancies created could also lead to a change in the quality or type of worker hired, even without affecting overall hiring numbers. To study this, we estimate the impact of the intervention on indicators that measure the match quality of new hires, namely the salary and the satisfaction of the manager with the new hire. Appendix Table A13 shows no impact on any of these outcomes. We also find no effect on the share of female hires, among all hires.

Why do firms struggle to fill vacancies? We find evidence for two potential reasons. First, applicants to formally-posted job advertisements have unrealistically high wage expectations. Second, firm managers receive fewer and worse-than-expected applicants, which might lead them to leave vacancies unfilled.

We observe a strong mismatch between applicants' expectations and realized salaries for the position to which they applied. Table A3 uses the applicant data we collect for firms in the screening treatment group to show applicants' reservation wages, wage expectations, realized salaries, and average baseline salaries. Three facts emerge. First, wage expectations and reservation wages are on average significantly higher than both the realized wage (when the vacancy is filled) and the average salary of vacancy posting firms at baseline. This suggest that applicants are generally over-optimistic about the possible remuneration. Second, applicants to unfilled vacancies have 38% higher reservation wages and 26% higher wage expectations compared to applicants to filled vacancies, despite the fact that average baseline salaries between firms with filled and unfilled vacancies do not strongly differ. Overall, this pattern is consistent with overly optimistic expectations at least partially explaining why firms face difficulties filling vacancies. Finally, the discrepancy between expectations of applicants for filled and unfilled vacancies is larger for white-collar compared to non-white-collar vacancies (59% difference in reservation wages vs 31% difference in reservation wages). Hence, applicants to unfilled white-collar vacancies are more over-optimistic than those to unfilled non-white-collar vacancies. This pattern is further in line with the observed heterogeneity in the decrease in filling rates across vacancy type.

We also find that managers negatively update their beliefs about the quality and quantity of applicants obtained though formal search channels.[20] This negative updating compered to baseline beliefs could mean that managers either posted overly-ambitious vacancies or conducted a too stringent screening of applicants, which in turn might have led to the observed increase in unfilled vacancies.

To document belief updating, we ask firm managers at endline whether they think that applicants obtained through the different formal channels are of better quality than those obtained through networks. We then construct a normalized belief index, with higher values indicating a higher expected quality of applicants through formal search channels relative to network-based search.[21] We find that on average firm managers in the treatment group have significantly lower expectations about the quality of applicants obtained through formal channels (-0.17 standard deviations, significant at the 10 percent level after MHC) than the control group (see Appendix Table A6). The impact on expectations for white-collar and non-white-collar applicants are of a similar size (columns (2) and (3)), although only the effect on non-white-collar applicants is statistically significant even after correcting for multiple hypothesis testing.

We also find negative average treatment effects on the expected number of applicants after posting a vacancy in formal channels (0.21 standard deviations, significant at the 10 percent level after MHC), with no discernible differences between expectations about white-collar and non-white-collar applicants. These effects are more noisily estimated due to the unbounded nature of the variable. Overall, this implies that managers in the treatment group update negatively about the prospects of formal employee search.

The fact that our treatment affects endline beliefs about the usefulness of formal search channels indicates that firm managers had incomplete infor-

[20]This is despite the fact that 75% of applicants fulfill all required criteria for the posted vacancy and that formally-posted vacancies attract substantially more applicants than informally posted vacancies (see appendix figure A3). In principle, formal search thus yields ample and qualified applicants. Hence, the negative updating is only relative to managers' prior beliefs about the usefulness of formal employee search.

[21]The index summarizes answers across different formal search channels (online, job board, newspaper) and vacancy types (white collar, non-white collar).

mation about the properties of such search channels. This in turn suggests that firms experiment little with different types of employee search. In the context of our experiment, such lack of experimentation might well be optimal for firms. However, a broader lack of accurate information might slow down or even prevent firms from adapting formal search channels as these channels improve over time (for example, because more workers start to use them).

Taken together, our results suggest that the vacancy posting subsidy shifts firms' vacancy posting patterns. Firms use the intervention to post white-collar vacancies that they would not have posted otherwise. At the same time, the fraction of firms posting any non-white-collar vacancies decreases. This pattern suggests that firms substitute non-white-collar vacancies with white-collar vacancies when offered the subsidy. However, this shift does not lead to an increase in white-collar hiring, and many vacancies remain unfilled. We provide evidence for two factors that could explain this decrease in filled vacancies. First, we show that applicants to unfilled vacancies have high reservation wages and wage expectations, which plausibly makes it more difficult for firms to fill vacancies. Second, managers negatively update about the quality and quantity of applicants obtained through formal search channels.

[Table 4 about here.]

[Table 5 about here.]

5.4 Belief updating leads to sustained changes in behavior

Changes in beliefs induced by labor market interventions can lead to long-term changes in behavior and outcomes for firms and labor markets. Bandiera et al. (2020) document that the lack of (expected) callbacks can lead to discouragement of job-seekers with long-term negative effects. Abebe et al. (2020b) explore how firms that are exposed to job-seekers of below-expected quality at job fairs update their beliefs and search behavior. They find that firms increase job search through formal channels and reduce overall hiring. Firms that participate in our study are also exposed to candidates who fall short of expectations, through formal search channels. This could lead to post-treatment changes in search methods and labor demand.

To further investigate the impact of our treatment on belief updating, we continue our phone surveys in the two months following the end of the treatment period. We successfully recontact 554 or 88% of participating firms for an average of 2.6 times during the two-month post-treatment period. Attrition in the post-treatment period thus remains relatively low and reassuringly is not related to treatment assignment (see columns 5 and 6 of Appendix Table A2). To analyze post-intervention treatment effects, we aggregate vacancy creation and hiring measures across all successfully-completed phone surveys.

In the following, we present three key results. Our first finding is that treated firms do not continue to use formal search channels after the subsidy period. Appendix Table A15 shows no significant difference between control and treatment firms in the fraction of vacancies that are posted formally. If anything, the treatment had a slightly negative impact on the likelihood of using formal search channels once the subsidy ran out.[22] This is in line with firms negatively updating about the returns to using formal search channels during the treatment period.

Second, we observe that treated firms reduce their hiring levels substantially (see Table 6). Treated firms are 7.5 percentage points (34% of the control group mean) less likely to create at least one vacancy during the post-treatment period. They reduce their total number of vacancies by 0.18 or 47% of the control group mean. These reductions are statistically significant at the 5 percent level even after MHC. They also translate into a reduction in the likelihood of at least one successful hire (-8.1 percentage points; 37% percent of the control mean) and the total number of hires (-0.26 hires; 56% of the control mean). The reduction in hiring is mostly driven by reductions

[22]While the negative treatment effects are large in relative terms, the control group means are small and the effects are far from significant.

in vacancy creation, although we also observe a marginally significant reduction in the fraction of filled vacancies. This is in line with firms' reaction during the treatment period and indicates a permanent shift in beliefs.[23]

Third, we study the post-treatment difference in the composition of labor demand. We find that firms display a persistent increase their demand for white-collar workers. They are 2.3 percentage points more likely to create at least one white-collar vacancy (88 percent of the control group mean) and create an additional 0.026 white-collar vacancies (81 percent of the control group mean). These effects are significant at the 10% level after MHC (also see Appendix Table A16). We also observe a decrease of 10 percentage points in the likelihood of creating any non-white-collar vacancy (48 percent of the control mean). The total number of non-white-collar vacancies also decreases by 0.21 (59 percent of the control mean). These results are significant at the 1% level after MHC. Taken together, these results lead to a highly significant increase in the average fraction of white-collar vacancies across firms by 21 percentage points (135 percent of the control mean).

The average fraction of white-collar hires also increases by 17 percentage points (97 percent of the control group mean, see Appendix Table A17). However, the impacts on white-collar hiring numbers—although still substantial in relative terms—are not significant. The negative effects on non-white-collar vacancy creation translates into a significant decrease in non-white-collar hires.

Put together, while we observe a sustained shift towards white-collar vacancy creation and hiring, this is at least partially driven by substantial decreases in non-white-collar hiring after the treatment period.

What explains the sustained decrease in non-white-collar vacancy creation and hiring? We provide suggestive evidence that the decrease in

[23]In theory, the vacancy posting subsidy could also cause the front-loading of vacancy creation during the treatment period, which is then followed by a reduction in vacancy posting after the treatment. However, given that we do not observe an initial increase in vacancy posting during the treatment period, we think that this is unlikely to drive the results.

non-white-collar hiring is accompanied by an improvement of conditions for existing workers, potentially leading to lower levels of turnover which reduces the need for new hires.

First, at endline, workers in treated firms earn more than workers in control firms (significant at the 10% level after MHC), which is entirely driven by non-white-collar workers' salaries (Table A7). This result suggests that firms substitute the hiring of new non-white-collar workers with higher salaries for their existing non-white-collar workforce.

Second, we find suggestive evidence that after the treatment period, formerly-treated firms manage to keep existing employees for longer (Table A18). Specifically, the number of employees who left the firm is approximately 20% lower in treated firms, although the results are not significant. However, we observe a 6.2 percentage point reduction in the share of firms that saw employees leave for personal reasons and a 2.4 percentage point decline in fired employees (both results marginally insignificant after MHC).

Overall, the evidence presented suggest that firms update their beliefs about the productivity of white-collar workers in particular, which implies that firms learn important information about their own labor-based production function. Firms reduce the hiring of employees with relatively low earnings and instead exert more effort to keep existing employees. This is in line with managers negatively updating about the quality of the applicant pool, which raises the value of existing workers.

[Table 6 about here.]

5.5 Discussion

Put together, our results highlight that exposing firms to new labor market segments—by subsidizing the use of formal employee search channels—can have persistent effects on their personnel strategy.

Most firms do not actually increase average hiring levels as they struggle to fill created vacancies, potentially because they were overly optimistic about the size and quality of the applicant pool. However, firms react by shifting their labor demand towards more high-skilled white-collar jobs. While this shift in the demanded skill composition persists after the treatment period ended, the use of formal search channels does not. This suggests that firms not only learn about the quality of the applicant pool obtained through formal search channels, but they also seem to internalize the profitability of hiring more white-collar employees. Put differently, firms seem to have updated their beliefs about their production function. This indicates that without being exposed to our intervention, firms have incomplete information about the marginal productivity of at least some factors, which can be alleviated by drawing attention to that factor.[24]

Firms indeed respond to the treatment with a reduction in vacancy creation and hiring levels, which persists beyond the treatment period. We provide suggestive evidence that this effect is driven by a reduction of turnover among existing employees, potentially achieved by increasing their salaries. This rather sophisticated firm strategy could be induced by negative updating, in particular about the quality of applicants obtained through formal search channels.

Firms do not keep using formal search channels beyond the treatment period, suggesting that formalized search in the current labor market environment in Addis Ababa is not profitable. However, the fact that firms persistently change their behavior demonstrates that interventions can induce learning in other dimensions, which in turn leads to important effects on firms' personnel decisions.

One important limitation of our experiment is the fact that we only analyze the partial equilibrium effect of moving one side of the labor market to formal search channels. If we induced a substantial share of all hiring firms in Addis Ababa to use formal search channels instead of networks, jobseekers might react and shift towards more formal search channels. This would in turn increase the number and composition of applicants that firms

[24]More broadly, our findings could indicate that firms might have used the vacancy subsidies to experiment with their human resource strategies, leading to updated beliefs and changes in behavior beyond the treatment period.

can attract, which might make formal search more attractive. Hence, our study does not assess the relative merits of formal and informal employee search in a general equilibrium setting, but rather the effect of incentivizing formal employee search conditional on current, mostly informal labor market institutions.

6 Conclusion

We randomly provide vacancy posting subsidies to 625 SMEs in Addis Ababa, Ethiopia, to test whether incentivizing firms' formal vacancy posting changes their hiring practices. We pay for all formal job advertisements of treated firms over a period of four months and survey firms extensively before, during, and after the treatment. The immediate vacancy posting costs that we cover for treated firms are substantial and amount to approximately 120 USD per vacancy, which is more than the average monthly salary firms in our sample pay their workers.

Our intervention successfully increases the share of firms posting in formal channels four-fold. This shift in posting techniques does not lead to more vacancy creation, but rather induces firms to gravitate towards creating higher-skill vacancies. However, not all of these new, high-skill vacancies get filled, with treated firms' probability of filling a given vacancy decreasing by 20 percentage points. While treated firms update negatively about the usefulness of formal search channels and stop using them after the end of the intervention period, the shift towards higher-skilled white-collar workers persists. This suggests that firms change their beliefs about the usefulness of white-collar employees, despite their negative impression of the labor market conditions.

Alleviating information frictions about applicant skills—which has been found to increase job-seekers' labor market outcomes in the literature—does not change the impact of the vacancy posting subsidy. This suggests that the lack of information about job-seekers' skills is not a binding constraint for firms in the context of Addis Ababa. Their difficulty in filling white-collar vacancies suggests that the availability of skilled workers as well as misaligned wage expectations constrain firms' hiring numbers.

When extrapolating from this study, it is important to keep in mind the partial equilibrium nature of our research. In all likelihood, our experiment did not affect the search behavior of job-seekers. As such, this study only speaks to the effect of formalized employee search given current job search habits. If a large fraction of firms were to switch to using more formal search channels, this would also incentivize job-seekers to also rely more on these channels. This in turn might have important consequences for the composition of the applicant pool and the resulting incentives for firms to use formal channels.

References

Abebe, Girum, A. Stefano Caria, and Esteban Ortiz-Ospina, "The Selection of Talent: Experimental and Structural Evidence from Ethiopia," *American Economic Review*, June 2021, *111* (6), 1757–1806.

_ , Marcel Fafchamps, Michael Koelle, and Simon Quinn, "Learning Management Through Matching: A Field Experiment Using Mechanism Design," *NBER Working Paper 26035*, 2019.

_ , Stefano Caria, Marcel Fafchamps, Paolo Falco, Simon Franklin, and Simon Quinn, "Anonymity or Distance? Job Search and Labour Market Exclusion in a Growing African City," *Review of Economic Studies*, 2020.

_ , _ , _ , _ , _ , _ , and Forhad Shilpi, "Matching Frictions and Distorted Beliefs: Evidence from a Job Fair Experiment," *Working Paper*, 2020.

Abel, Martin, Rulof Burger, and Patrizio Piraino, "The Value of Reference Letters -Experimental Evidence from South Africa," *American Economic Journal: Applied Economics*, 2020, *12* (3), 40–71.

Algan, Yann, Bruno Crépon, and Dylan Glover, "Are active labor market policies directed at firms effective? Evidence from a randomized evaluation with local employment agencies," *Working Paper*, 2020.

Bandiera, Oriana, Vittorio Bassi, Robin Burgess, Imran Rasul, Munshi Sulaiman, and Anna Vitali, "Worker Heterogeneity and Job Search: Evidence from a Six-Year Experiment in Uganda," *Working Paper*, 2020.

Banerjee, Abhijit, Emily Breza, Arun G Chandrasekhar, Esther Duflo, Matthew O Jackson, and Cynthia Kinnan, "Changes in social network structure in response to exposure to formal credit markets," Technical Report, National Bureau of Economic Research 2021.

Bassi, Vittorio and Aisha Nansamba, "Screening and Signaling Non-Cognitive Skills: Experimental Evidence from Uganda," *Economic Journal*, 2021.

Beam, Emily A., "Do job fairs matter? Experimental evidence on the impact of job-fair attendance," *Journal of Development Economics*, 2016, *120*, 32–40.

Beaman, Lori and Jeremy Magruder, "Who Gets the Job Referral? Evidence from a Social Networks Experiment," *The American Economic Review*, 2012, *102* (1027), 3574–3593.

Belloni, Alexandre, Victor Chernozhukov, and Christian Hansen, "Inference on Treatment Effects After Selection Among High-Dimensional Controls," *Review of Economic Studies*, 2013, *81* (2), 608–650.

Calvó-Armengol, Author Antoni and Matthew O. Jackson, "The Effects of Social Networks on Employment and Inequality," *American Economic Review*, 2004, *94* (3), 426–454.

Carranza, Eliana, Rob Garlick, Kate Orkin, and Neil Rankin, "Job Search and Matching with Two-Sided Limited Information ," *Working Paper*, 2020.

Chandrasekhar, Arun G, Melanie Morten, and Alessandra Peter, "Network-Based Hiring: Local benefits; Global Costs," *Working Paper*, 2020.

Comola, Margherita and Silvia Prina, "Treatment Effect Accounting for Network Changes," *The Review of Economics and Statistics*, 07 2021, *103* (3), 597–604.

de Mel, Suresh, David McKenzie, and Christopher Woodruff, "Labor drops: Experimental evidence on the return to additional labor in microenterprises," *American Economic Journal: Applied Economics*, 2019, *11* (1), 202–235.

Dustmann, Christian, Albrecht Glitz, Uta Schönberg, and Herbert Brücker, "Referral-based job search networks," *Review of Economic Studies*, 2016, *83* (2), 514–546.

Galasso, Emanuela, Martin Ravallion, and Agustin Salvia, "Assisting the Transition from Workfare to Work: A Randomized Experiment," *ILR Review*, 2004, *58* (1), 128–142.

Groh, Matthew, Nandini Krishnan, David Mckenzie, and Tara Vishwanath, "Do Wage Subsidies Provide a Stepping-Stone to Employment for Recent College Graduates? Evidence from a Randomized Experiment in Jordan," *Review of Economics and Statistics*, 2016, *98* (3), 488–502.

Hanna, Rema, Joshua Schwartzstein, and Sendhil Mullainathan, "Learning Through Noticing: Theory and Evidence From a Field Experiment," *The Quarterly Journal of Economics*, 2014, pp. 1311–1353.

Hardy, Morgan and Jamie McCasland, "Are Small Firms Labor Constrained? Experimental Evidence from Ghana," *Working Paper*, 2018.

Heath, Rachel, "Why do Firms Hire using Referrals? Evidence from Bangladeshi Garment Factories," *Journal of Political Economy*, 2018.

Kelley, Erin M, Christopher Ksoll, and Jeremy Magruder, "How do Online Job Portals affect Employment and Job Search? Evidence from India," *Working Paper*, 2020.

Kramarz, Francis and Oskar Nordström Skans, "When strong ties are strong: Networks and youth labour market entry," *Review of Economic Studies*, 2014, *81* (3), 1164–1200.

McKenzie, David, "Beyond baseline and follow-up: The case for more T in experiments," *Journal of Development Economics*, 2012, *99* (2), 210–221.

Mushfiq, Ahmed, Mark R Rosenzweig, By Ahmed, Mushfiq Mobarak, and Mark R Rosenzweig, "Informal Risk Sharing, Index Insurance, and Risk Taking in Developing Countries," *American Economic Review: Papers & Proceedings 2013,*, 2013, *103* (3), 375–380.

Rebien, Martina, Michael Stops, and Anna Zaharieva, "Formal Search and Referrals from a Firm's Perspective," *International Economic Review*, 2020, *61* (4), 1679–1748.

Serneels, Pieter, "The nature of unemployment among young men in Urban Ethiopia," *Review of Development Economics*, 2007, *11* (1), 170–186.

Ulyssea, Gabriel, "Firms, Informality, and Development: Theory and Evidence from Brazil," *American Economic Review*, August 2018, *108* (8), 2015–47.

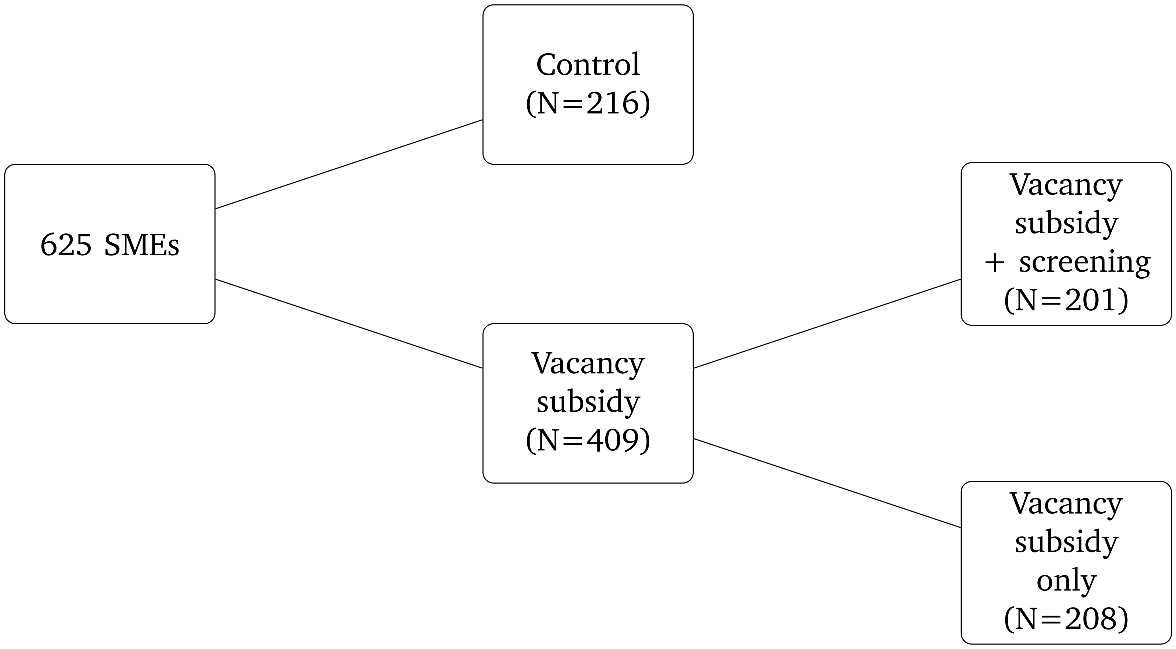
— , "Informality: Causes and Consequences for Development," *Annual Review of Economics*, 2020, *12* (1), 525–546.

Wheeler, Laurel, Robert Garlick, Eric Johnson, Patrick Shaw, and Marissa Gargano, "LinkedIn (to) Job Opportunities : Experimental Evidence from Job Readiness Training," *American Economic Journal: Applied Economics*, 2021.

Witte, Marc, "Why do workers make job referrals? Experimental Evidence from Ethiopia," *Working Paper*, 2021.

World Bank, *World Development Indicators*, World Bank, 2020.

Figure 1: Experimental design



This figure shows the randomly-allocated treatment arms and corresponding sample sizes of our experimental design.

Table 1: Summary statistics

	Mean	SD	Median	Min	Max	# obs
Firm characteristics						
Age of firm (in years)	7.19	7.99	5.00	0.10	63.00	616
# of employees	14.49	11.60	10.00	4.00	88.00	625
Share of white-collar employees	0.14	0.15	0.11	0.00	0.94	625
Share of pink-collar employees	0.18	0.20	0.12	0.00	1.00	625
Share of blue-collar employees	0.57	0.29	0.60	0.00	1.00	625
Share of grey-collar employees	0.11	0.12	0.09	0.00	0.75	625
Manufacturing sector	0.51	0.50	1.00	0.00	1.00	625
Service sector (retail, hospitality)	0.27	0.45	0.00	0.00	1.00	625
Hiring practices						
Uses formal hiring channels	0.09	0.28	0.00	0.00	1.00	625
Uses network hiring channels	0.79	0.41	1.00	0.00	1.00	625
Uses employment agencies	0.39	0.49	0.00	0.00	1.00	625
Manager expectations						
Expected number of hires over the next three months	3.46	5.93	2.00	0.00	90.00	624
Positive bus. outlook next 3 months	0.62	0.49	1.00	0.00	1.00	611
Positive bus. outlook next 12 months	0.77	0.42	1.00	0.00	1.00	584
Optimistic firms	0.60	0.49	1.00	0.00	1.00	598
Manager characteristics						
Female	0.26	0.44	0.00	0.00	1.00	625
Manager age	35.32	10.30	32.00	19.00	84.00	625
Manager has univ. degree	0.45	0.50	0.00	0.00	1.00	625
Raven score (standardized)	-0.00	1.00	-0.11	-2.28	2.07	625

Notes: Table 1 presents baseline summary statistics of firm and firm manager characteristics. The number of observations varies due to "don't know" answers and refusals to answer. The total number of firms is 625.

Table 2: Formalization of employee search

	Take-up			Formal search			Network based search		
	(1) Any	(2) Any | any vacs	(3) # vacs	(4) Any	(5) # vacs	(6) % vacs	(7) Any	(8) # vacs	(9) % vacs
Treatment	0.215***	0.481***	0.558***	0.169***	0.461***	0.313***	-0.078**	-0.055	-0.094*
	(0.020)	(0.037)	(0.081)	(0.025)	(0.111)	(0.039)	(0.036)	(0.063)	(0.054)
	[0.001]***	[0.001]***	[0.001]***	[0.001]***	[0.001]***	[0.001]***	[0.038]**	[0.380]	[0.095]*
Control mean	0.000	0.000	0.000	0.051	0.144	0.070	0.269	0.398	0.462
Observations	621	288	621	621	621	288	621	621	288

Notes: Table 2 displays the impact of the vacancy subsidy on formal employee search. Column (1) shows the fraction of firms posting at least one vacancy through our intervention. Column (2) shows the number of vacancies posted through our intervention conditional on using the subsidy for at least one subsidy. Column (3) shows the number of vacancies for which the vacancy subsidy was used. Column (4) to (6) shows the impact of the vacancy subsidy on formal employee search. Column (7) to (9) shows the impact of the vacancy subsidy on using network-based employee search. Heteroskedasticity robust standard errors are displayed in parenthesis. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table 3: Impacts on vacancy postings and hiring outcomes

	Vacancy creation			Hires	
	(1) Any	(2) # vacs	(3) % vacs filled	(4) Any	(5) # hires
Treatment	-0.048	0.124	-0.203***	-0.078*	-0.210
	(0.042)	(0.171)	(0.041)	(0.042)	(0.171)
	[0.312]	[0.468]	[0.001]***	[0.150]	[0.312]
Control mean	0.495	1.153	0.877	0.454	1.218
Observations	621	621	288	621	621

Notes: Table 3 displays the treatment effects on vacancy creation (columns (1) to (3)) and hiring outcomes (columns (4) to 5)). Heteroskedasticity robust standard errors are displayed in parenthesis. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Composition effects - vacancy creation

	White collar				Non-white collar		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any vac	# vacs	% vacs filled	% vacs	Any vac	# vacs	% vacs filled
Treatment	0.056**	0.094*	-0.361***	0.064*	-0.053	0.027	-0.193***
	(0.024)	(0.053)	(0.114)	(0.034)	(0.042)	(0.149)	(0.042)
	[0.031]**	[0.064]*	[0.007]***	[0.064]*	[0.118]	[0.315]	[0.001]***
Control mean	0.069	0.120	0.827	0.094	0.463	1.032	0.881
Observations	621	621	66	288	621	621	267

Notes: Table 4 displays the effect of our intervention on the skill composition of vacancy postings. Columns (1) to (4) show the impact on white-collar vacancies. Columns (5) and (7) show the impact on non-white-collar vacancies. Heteroskedasticity robust standard errors are displayed in parenthesis. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Composition effects - hiring decision

	White collar			Non-white collar	
	(1)	(2)	(3)	(4)	(5)
	Any hire	# hires	% hires	Any hire	# hires
Treatment	0.009	-0.002	0.033	-0.078*	-0.208
	(0.021)	(0.047)	(0.035)	(0.041)	(0.159)
	[0.989]	[1.000]	[0.861]	[0.423]	[0.628]
Control mean	0.060	0.111	0.088	0.426	1.106
Observations	621	621	250	621	621

Notes: Table 5 displays the impact of the vacancy subsidy on formal employee search during the posttreatment period. Column (1) to (3) shows the impact of the vacancy subsidy on formal employee search. Column (4) to (6) shows the impact of the vacancy subsidy on using network-based employee search. Heteroskedasticity robust standard errors are displayed in parenthesis. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Post-treatment employee search

	Vacancy creation			Hires	
	(1)	(2)	(3)	(4)	(5)
	Any vac	# vacs	% vacs filled	Any hire	# hires
Treatment	-0.065**	-0.155**	-0.047*	-0.070**	-0.222***
	(0.032)	(0.060)	(0.027)	(0.032)	(0.074)
	[0.051]*	[0.026]**	[0.084]*	[0.045]**	[0.013]**
Control mean	0.194	0.333	1.000	0.194	0.403
Observations	625	625	95	625	625

Notes: Table 6 displays the impact of the effects of the vacancy subsidy intervention on vacancy creation and hires in the two months following the four-month treatment period. Columns (1) to (3) show the impact on vacancy creation outcomes. Columns (4) to (5) show the impact on hiring outcomes. Heteroskedasticity robust standard errors are displayed in parenthesis. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix
For Online Publication

The Online Appendix contains additional tables and figures referenced to in the main text. Online Appendix Section A contains additional result tables. Online Appendix Table A1 tests for balance between treatment and control group. Online Appendix Table A2 tests for differential attrition by treatment groups. Online Appendix Table A3 shows wage expectations and realized wages for different vacancy types. Online Appendix Table A4 displays heterogeneous treatment effects by a wide range of observable characteristics. Online Appendix Table A5 displays treatment effects on turnover of existing employees. Online Appendix Table A6 displays treatment effects on managers' beliefs. Online Appendix Table A7 shows the impact on average salaries at endline. Online Appendix Table A8 shows the additional impact of the screening intervention on vacancy posting and creation, and hiring outcomes. Online Appendix Table A9 shows the additional impact of the screening intervention on the composition of vacancy creation and hires. Online Appendix Table A10 shows the impact on downstream business outcomes. Online Appendix Table A11 shows the impact average search inputs. Online Appendix Table A13 show the impacts on characteristics of hired individuals. Online Appendix Table 5 shows the impacts on the skill composition of new hires. Online Appendix Table A15 shows impacts on search channels after the treatment period.

Next, we display additional results for the post-treatment period. Online Appendix Table A16 shows the impacts on the skill composition of vacancy creation in the post-treatment period. Online Appendix Table A17 shows the impacts on the skill composition of new hires in the post-treatment period. Online Appendix Table A18 shows the impacts on employee turnover during the post-treatment period. Online Appendix Table A19 shows the impacts on the characteristics of new hires.

In section B, we show all main results with control variables selected according to the pre-analysis plan. Online Appendix Table A20 shows the main effects. Online Appendix Tables A21 and A22 display the effects on the skill composition of vacancy creation and hires. Online Appendix Table A23 displays the impact on manager beliefs. Online Appendix Table A24 displays the impact on turnover. Online Appendix Table A25 displays the impact on search inputs. Online Appendix Table A26 shows the impacts on the characteristics of new hires. Online Appendix Table A27 shows the main effects during the post-treatment period. Online Appendix Tables A28 and A29 display the effects on the skill composition of vacancy creation and hires during the post-treatment period. Online Appendix Table A30 displays the effects on turnover during the post-treatment period.

Section C contains additional figures. Online Appendix Figure A1 displays the timeline of the experiment. Online Appendix Figure A2 shows the geographical distribution of firms in our sample.

A Additional tables

Table A1: Treatment balance

	Control	Treatment	Δ	p(Control=Treatment)
Firm characteristics				
Age of firm (in years)	7.45	7.05	-0.404	0.548
# of employees	15.12	14.16	-0.952	0.352
Share of white-collar employees	0.13	0.15	0.014	0.271
Share of pink-collar employees	0.17	0.18	0.008	0.635
Share of blue-collar employees	0.59	0.56	-0.024	0.316
Share of grey-collar employees	0.11	0.11	0.002	0.816
Manufacturing sector	0.52	0.50	-0.024	0.563
Service sector (retail, hospitality)	0.27	0.28	0.008	0.836
Hiring practices				
Uses formal hiring channels	0.10	0.08	-0.021	0.391
Uses network hiring channels	0.81	0.79	-0.018	0.588
Uses employment agencies	0.36	0.41	0.054	0.183
Manager expectations				
Expected number of hires over the next three months	3.06	3.67	0.618	0.159
Positive bus. outlook next 3 months	0.62	0.61	-0.008	0.840
Positive bus. outlook next 12 months	0.79	0.76	-0.028	0.441
Optimistic firms	0.59	0.61	0.018	0.673
Manager characteristics				
Female	0.30	0.23	-0.069	0.068
Manager age	34.98	35.50	0.519	0.565
Manager has univ. degree	0.42	0.47	0.051	0.226
bl_raven_score_m	8.99	8.86	-0.128	0.716

Notes: Table A1 presents tests for equality of means across the treatment and control group.

Table A2: Attrition analysis

	During treatment period				Post treatment period	
	(1) Any highfreq survey	(2) # highfreq surveys	(3) Has endline survey	(4) Has highfreq or endline survey	(5) Any post treatment survey	(6) # post treatment surveys
Treatment	-0.005	0.171	0.003	-0.010**	0.010	-0.002
	(0.017)	(0.193)	(0.015)	(0.005)	(0.027)	(0.123)
Control mean	0.958	5.440	0.968	1.000	0.880	2.569
Observations	625	625	625	625	625	625

Notes: Table A2 test whether attrition rates differ across treatment groups.

Table A3: Expected and realized earnings

	Applicant data		Realized salary data	
	(1) Reservation wage (mean)	(2) Wage expectation	(3) Realized salary	(4) Average salary at baseline
Panel A: All vacancies				
All vacancies	5059	5490	-	2945
Vacancies with hires	4066	4670	3256	2996
Vacancies without hires	5601	5907	-	2804
Panel B: White collar vacancies				
All white collar vacancies	6281	7695	-	2993
White collar vacancies with hires	4678	6127	4314	2940
White collar vacancies without hires	7454	8740	-	2895
Panel C: Non white collar vacancies				
All non white collar vacancies	4532	4463	-	3045
Non white collar vacancies with hires	3729	3898	2955	3010
Non white collar vacancies without hires	4874	4702	-	2956

Notes: Table A3 compares average reservation salaries, salary expectations to realized salaries and average baseline salaries. All values are in Ethiopian Birr per month (at the end of 2019 100 Birr were worth around 3.5 USD). Samples are restricted to to the vacancy subsidy plus screening treatment group because reservation salary and salary expectation data is only available for applicants applying to vacancies posted in the screening group. Columns (1) and (2) are applicant level averages (applicants to vacancies with hires but without salary information are excluded to make results comparable to column (3)). Column (3) three is the average salary of newly hired employees for vacancies posted during the treatment group (at the vacancy level). The sample in column (4) displays firm level averages with the sample defined to be comparable to columns (1) to (3).

Table A4: Heterogeneous impacts by observable characteristics

	Firm characteristics			Sector			Hiring practices		Expectations			Manager characteristics			
	(1) Bus. age	(2) % wc employees	(3) Manufactoring sector	(4) Service sector	(5) Health	(6) Formal	(7) Network	(8) Exp. hires (3m)	(9) Pos. bus. outlook (3m)	(10) Pos. bus. outlook (12m)	(11) Female	(12) Age	(13) Univ. degree	(14) Raven's score	(15) col15
Panel A: Impact on number of vacancies posted															
Treatment	0.008	0.190	-0.327	0.250	0.124	-0.007	0.141	-0.523	0.018	0.486**	0.545**	0.040	0.493	0.106	-0.366
	(0.226)	(0.217)	(1.064)	(0.287)	(0.192)	(0.179)	(0.162)	(0.540)	(0.216)	(0.228)	(0.275)	(0.220)	(0.753)	(0.206)	(0.453)
Treatment × hetero. var	0.018	-0.517	0.517	-0.291	-0.033	1.274**	-0.081	0.791	0.020	-0.509	-0.469	0.242	-0.011	0.011	0.055
	(0.026)	(1.185)	(1.185)	(0.340)	(0.392)	(0.522)	(0.902)	(0.563)	(0.053)	(0.327)	(0.349)	(0.312)	(0.022)	(0.357)	(0.043)
Hetero. var	0.006	0.672	-0.672	-0.673***	0.828***	-0.594	0.538	-1.259***	0.045	0.777***	0.825***	-0.395*	0.014	0.249	-0.045
	(0.015)	(1.021)	(1.021)	(0.260)	(0.292)	(0.365)	(0.783)	(0.460)	(0.031)	(0.221)	(0.230)	(0.226)	(0.019)	(0.277)	(0.035)
Control mean	1.153	1.153	1.153	1.153	1.153	1.153	1.153	1.153	1.153	1.153	1.153	1.153	1.153	1.153	1.153
Observations	613	621	621	621	621	621	621	621	620	607	580	621	621	621	621
Panel B: Impact on number of hires															
Treatment	-0.229	-0.106	-0.836	-0.348	-0.129	-0.234	-0.278	-0.507	-0.274	0.183	0.225	-0.243	0.221	-0.203	-0.535
	(0.230)	(0.243)	(0.991)	(0.262)	(0.193)	(0.181)	(0.174)	(0.418)	(0.200)	(0.206)	(0.277)	(0.210)	(0.661)	(0.196)	(0.389)
Treatment × hetero. var	0.004	-0.730	0.730	0.242	-0.325	0.425	0.824	0.352	0.011	-0.541*	-0.491	0.064	-0.012	-0.045	0.036
	(0.024)	(1.148)	(1.148)	(0.342)	(0.393)	(0.552)	(0.744)	(0.456)	(0.048)	(0.319)	(0.351)	(0.350)	(0.018)	(0.360)	(0.041)
Hetero. var	0.003	0.135	-0.135	-0.642**	0.928***	-0.520	-0.040	-1.030***	0.039	0.773***	0.761***	-0.267	0.003	0.289	-0.014
	(0.019)	(1.056)	(1.056)	(0.280)	(0.318)	(0.481)	(0.534)	(0.372)	(0.036)	(0.249)	(0.279)	(0.288)	(0.015)	(0.301)	(0.033)
Control mean	1.218	1.218	1.218	1.218	1.218	1.218	1.218	1.218	1.218	1.218	1.218	1.218	1.218	1.218	1.218
Observations	613	621	621	621	621	621	621	621	620	607	580	621	621	621	621

Notes: Table A4 displays the heterogeneous treatment effects on number of posted vacancies and number of hires by observable firm and manager characteristics. Columns indicate heterogeneity variable. Dependent variable in Panel A is the number of posted vacancies. Dependent variable in Panel B is the number of hires. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Effects on employee turnover

	Employees left		Leaving reasons		
	(1)	(2)	(3)	(4)	(5)
	Any	#	Personal	Better opportunities	Fired for performance
Panel A: Pooled					
Treatment	-0.002	-0.361	-0.075**	-0.012	-0.018
	(0.041)	(0.292)	(0.035)	(0.022)	(0.019)
	[0.920]	[0.767]	[0.096]*	[0.643]	[0.513]
Control mean	0.597	2.435	0.241	0.079	0.060
Observations	621	621	621	621	621

Notes: Table A5 displays the effect of our intervention on employee turnover. Column (1) shows the impact on a dummy variable indicating any turnover during this period. Column (2) shows the impact on the number of employees who left the firm (winsorized at the 99th percentile). Columns (3) to (5) show the impact of mentioning the respective reasons as important for employee turnover at least once during phone or endline surveys. Column (3) displays the impact on mentioning personal reasons, column (4) displays the impact on mentioning leaving for better opportunities, and column (5) shows the impact on mentioning firing workers for bad performance. Heteroskedasticity robust standard errors are displayed in parentheses. Minimum q-values from two-stage false discovery rate correction within families (columns (1) and (2) and (3) to (5)) are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effects on managers' beliefs about formal employee search

	Applicant quality			Applicant numbers (standardized)		
	(1) Index	(2) WC	(3) Non-WC	(4) Index	(5) WC	(6) Non-WC
Treatment	-0.169**	-0.133	-0.183**	-0.214*	-0.198*	-0.203*
	(0.084)	(0.084)	(0.084)	(0.111)	(0.115)	(0.110)
	[0.072]*	[0.072]*	[0.072]*	[0.091]*	[0.091]*	[0.091]*
Control mean	0.110	0.087	0.120	0.141	0.131	0.134
Observations	605	605	605	561	553	560

Notes: Table A6 displays the treatment effects on beliefs about the quality and number of applicants obtained through formal search channels. Columns (1) to (3) show the impacts on beliefs of beliefs about applicant quality. Applicant quality is measures by binary variables equal one if managers believe that they can obtain better quality candidates through different formal search channels relative to network-based hiring. Columns (4) to (6) show the impacts on beliefs of beliefs about absolute applicant numbers. All variables normalized sums of non-missing normalized beliefs across different formal search channels (online, job board, newspaper) and vacancy type (white collar, blue collar, pink collar). Number of observations varies for beliefs about applicant numbers due to "don't know" answers. Heteroskedasticity robust standard errors are displayed in parentheses. Minimum q-values from two-stage false discovery rate correction within families (columns (1)-(3) and (4)-(6)) are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effects on average monthly salaries

	Averages salaries at endline (ihs)		
	(1) Pooled	(2) White collar	(3) Non-white collar
Treatment	0.120*	-0.015	0.121*
	(0.063)	(0.070)	(0.062)
	[0.094]*	[0.381]	[0.094]*
Control mean	8.412	8.944	8.327
Observations	597	418	596

Notes: Table A7 displays the effect of our intervention on average monthly salaries at endline (transformed using the inverse hyperbolic sine). Column (2) shows impact on white-collar wages conditional on having white-collar employees. Column (3) shows impact on non-white-collar wages conditional on having non-white-collar employees. Heteroskedasticity robust standard errors are displayed in parentheses. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: The effect of the worker screening add-on on vacancy creation and hires

	Vacancies posted formally			Vacancy creation		Hiring outcomes		
	(1) Any	(2) # vacs	(3) %	(4) Any vacancy	(5) # vacs	(6) Any hire	(7) # hires	(8) % vacancies filled
Treatment	0.152***	0.446***	0.297***	-0.065	0.142	-0.082*	-0.140	-0.178***
	(0.032)	(0.135)	(0.050)	(0.049)	(0.197)	(0.048)	(0.200)	(0.049)
Treatment × screening	0.034	0.032	0.031	0.035	-0.037	0.007	-0.143	-0.049
	(0.041)	(0.168)	(0.065)	(0.050)	(0.225)	(0.048)	(0.197)	(0.058)
Treatment effect screening	0.186***	0.478***	0.328***	-0.031	0.105	-0.075	-0.283	-0.227***
	(0.034)	(0.145)	(0.051)	(0.049)	(0.212)	(0.048)	(0.195)	(0.051)
Control mean	0.051	0.144	0.070	0.495	1.153	0.454	1.218	0.877
Observations	621	621	288	621	621	621	621	288

Notes: Table A8 displays the treatment effects of the screening add-on on vacancy posting and hires. Columns (1) to (3) show impacts on formal vacancy posting. Columns (4) and (5) show impacts on vacancy creation. Columns (6) to (8) show impacts on hiring numbers. Heteroskedasticity robust standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: The effect of the worker screening add-on on skill composition of vacancy creation and hires

	Vacancies					Hires				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Any wc vac	# wc vacs	% wc vacs	Any non-wc	# non-wc vacs	Any wc hire	# wc hires	% wc hires	Any non-wc hire	# non-wc hires
Treatment	0.066**	0.121*	0.073*	-0.067	0.021	0.007	-0.010	0.024	-0.078*	-0.131
	(0.029)	(0.073)	(0.041)	(0.048)	(0.164)	(0.024)	(0.052)	(0.041)	(0.047)	(0.188)
Treatment × screening	-0.019	-0.055	-0.017	0.033	0.018	0.003	0.015	0.018	0.001	-0.158
	(0.033)	(0.076)	(0.045)	(0.049)	(0.197)	(0.025)	(0.051)	(0.047)	(0.047)	(0.183)
Treatment effect screening	0.047	0.066	0.055	-0.034	0.038	0.011	0.005	0.042	-0.077	-0.288
	(0.029)	(0.056)	(0.040)	(0.049)	(0.192)	(0.024)	(0.056)	(0.043)	(0.048)	(0.179)
Control mean	0.069	0.120	0.094	0.463	1.032	0.060	0.111	0.088	0.426	1.106
Observations	621	621	288	621	621	621	621	250	621	621

Notes: Table A9 displays the heterogeneous treatment effects of the screening add-on on the skill composition vacancy posting and hires. Columns (1) to (5) show impacts on the composition of vacancy creation. Columns (6) and (10) show impacts on the composition of hires. Heteroskedasticity robust standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Effects on downstream firm outcomes

	(1) Profit	(2) Revenue	(3) Outlook 3m	(4) Outlook 12m	(5) # of employees
Treatment	-0.266	-0.025	-0.011	-0.024	-2.460
	(0.228)	(0.217)	(0.049)	(0.089)	(1.606)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Control mean	4.128	5.563	4.602	5.595	16.818
Observations	580	580	619	551	605

Notes: Table A10 displays the effect of our intervention on downstream firm outcomes. Heteroskedasticity robust standard errors are displayed in parentheses. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Effects on candidate search inputs

	Index	Days	Hours			Cost (ETB)		
	(1) Search costs	(2) Search duration	(3) Screening	(4) Non-screening	(5) Total	(6) Screening	(7) Non-screening	(8) Total
Treatment	-0.019	-1.055	-1.051	1.277	0.168	22.323	19.060	48.872
	(0.144)	(1.170)	(0.860)	(0.843)	(1.295)	(118.531)	(17.208)	(120.154)
	[0.897]	[0.736]	[0.717]	[0.717]	[0.897]	[0.897]	[0.717]	[0.897]
Control mean	0.000	4.951	4.410	1.046	5.448	228.528	17.512	241.887
Observations	240	234	227	226	227	236	233	234

Notes: Table A11 displays the effect of our intervention on candidate search inputs. The outcomes are calculated as firm-level averages and are only defined for firms that posted at least one vacancy during the treatment period. Heteroskedasticity robust standard errors are displayed in parentheses. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Effects on employee numbers and shares

	(1) # of employees	(2) Share of WC employees
Treatment	-2.488	0.024**
	(1.605)	(0.011)
	[0.121]	[0.068]*
Control mean	16.818	0.099
Observations	606	600

Notes: Table A12 displays the effect of our intervention on the number of employees and the share of white-collar employees. Heteroskedasticity robust standard errors are displayed in parentheses. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Effects on characteristics of hires

	(1) Salary (ETB, IHS)	(2) Satisfaction	(3) Share female
Treatment	0.035	-0.034	-0.023
	(0.083)	(0.126)	(0.057)
	[1.000]	[1.000]	[1.000]
Control mean	8.165	0.020	0.586
Observations	232	236	250

Notes: Table A13 displays the effect of our intervention on the characteristics of new hires. The outcomes are only defined for firms that hired at least one person during the treatment period. Heteroskedasticity robust standard errors are displayed in parentheses. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Effects on willingness to pay for services

	(1) Subsidy	(2) Formal posting
Treatment	69.321	0.053
	(61.784)	(0.078)
	[0.972]	[0.972]
Control mean	278.565	-0.035
Observations	604	594

Notes: Table A14 displays the effect of our intervention on willingness to pay for the subsidy treatment and formal vacancy posting more generally (winsorized at the 99^{th} percentile). Heteroskedasticity robust standard errors are displayed in parentheses. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Impact on search channels - post-treatment

	Formal search			Network based search		
	(1) Any	(2) # vacs	(3) % vacs	(4) Any	(5) # vacs	(6) % vacs
Treatment	-0.010	-0.028	-0.019	-0.035	-0.053*	0.009
	(0.012)	(0.023)	(0.048)	(0.023)	(0.030)	(0.094)
	[0.673]	[0.673]	[0.812]	[0.673]	[0.673]	[0.858]
Control mean	0.021	0.042	0.075	0.084	0.111	0.335
Observations	554	554	95	554	554	95

Notes: Table A15 displays the impact of the effects of the vacancy subsidy intervention on the composition of vacancy creation in the two months following the four-month treatment period. Columns (1) to (3) show the impact on white-collar vacancy creation. Columns (4) and (5) show the impact on non-white-collar vacancy creation. Heteroskedasticity robust standard errors are displayed in parenthesis. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Impact on vacancy creation composition - post-treatment

	White collar				Non-white collar		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any vac	# vacs	% vacs filled	% vacs	Any vac	# vacs	% vacs filled
Treatment	0.021	0.024	-0.167*	0.205***	-0.087***	-0.178***	
	(0.014)	(0.018)	(0.082)	(0.071)	(0.030)	(0.056)	
	[0.090]*	[0.090]*	[0.043]**	[0.009]***	[0.009]***	[0.009]***	[.]
Control mean	0.023	0.028	1.000	0.087	0.185	0.306	1.000
Observations	625	625	23	95	625	625	80

Notes: Table A16 displays the impact of the effects of the vacancy subsidy intervention on the composition of vacancy creation in the two months following the four-month treatment period. Columns (1) to (4) show the impact on white-collar vacancies. Columns (5) and (7) show the impact on non-white-collar vacancies. All non-white-collar vacancies were filled in either group. Heteroskedasticity robust standard errors are displayed in parenthesis. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A17: Impact on hiring composition - post-treatment

	White collar			Non-white collar	
	(1)	(2)	(3)	(4)	(5)
	Any hire	# hires	% hires	Any hire	# hires
Treatment	0.018	0.015	0.171**	-0.101***	-0.270***
	(0.016)	(0.019)	(0.070)	(0.034)	(0.080)
	[0.153]	[0.203]	[0.016]**	[0.006]***	[0.003]***
Control mean	0.026	0.032	0.087	0.211	0.426
Observations	554	554	93	554	554

Notes: Table A17 displays the impact of the effects of the vacancy subsidy intervention on the composition of hires in the two months following the four-month treatment period. Columns (1) to (3) show the impact on white-collar hires. Columns (4) and (5) show the impact on non-white-collar hires. Heteroskedasticity robust standard errors are displayed in parenthesis. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A18: Effects on post-treatment turnover

	Employees left		Leaving reasons		
	(1)	(2)	(3)	(4)	(5)
	Any	#	Personal	Better opp.	Fired for performance
Treatment	-0.035	-0.167	-0.062*	0.016	-0.024*
	(0.039)	(0.163)	(0.034)	(0.021)	(0.014)
	[0.607]	[0.607]	[0.145]	[0.180]	[0.145]
Control mean	0.266	0.793	0.191	0.053	0.032
Observations	551	551	551	551	551

Notes: Table A18 displays the impact of the effects of the vacancy subsidy intervention on employee turnover after the end of the treatment period. Column (1) shows the impact on a dummy variable indicating any turnover during this period. Column (2) shows the impact on the number of employees who left the firm (winsorized at the 99th percentile). Columns (3) to (5) show the impact of mentioning the respective reasons as important for employee turnover at least once during phone or endline surveys. Column (3) displays the impact on mentioning personal reasons, column (4) displays the impact on mentioning leaving for better opportunities, and column (5) shows the impact on mentioning firing workers for bad performance. Heteroskedasticity robust standard errors are displayed in parenthesis. Minimum q-values from two-stage false discovery rate correction within families (columns (1) and (2) and (3) to (5)) are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A19: Effects on characteristics of hires - post-treatment

	(1) Salary (ETB, IHS)	(2) Satisfaction	(3) Share female
Treatment	0.321***	-0.025	0.082
	(0.116)	(0.210)	(0.089)
	[0.021]**	[1.000]	[0.552]
Control mean	7.959	0.014	0.388
Observations	85	90	93

Notes: Table A19 displays the effect of our intervention on the characteristics of new hires after the end of the treatment period. The outcomes are only defined for firms that hired at least one person during the treatment period. Heteroskedasticity robust standard errors are displayed in parentheses. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Results including control variables

Table A20: Main impacts on vacancy creation and hiring - including control variables

	Vacancy creation			Hires	
	(1)	(2)	(3)	(4)	(5)
	Any	# vacs	% vacs filled	Any	# hires
Treatment	-0.046	0.108	-0.197***	-0.078*	-0.198
	(0.040)	(0.154)	(0.038)	(0.041)	(0.163)
	[0.317]	[0.484]	[0.001]***	[0.133]	[0.317]
Control mean	0.495	1.153	0.877	0.454	1.218
Observations	621	621	288	621	621

Notes: Table A20 displays the treatment effects on vacancy creation (columns (1) to (3)) and hiring outcomes (columns (4) to (5)). Heteroskedasticity robust standard errors are displayed in parenthesis. All specifications include control variables selected from a pre-specified set of variables using LASSO algorithms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A21: Composition of vacancy creation - including control variables

	White collar				Non-white collar		
	(1) Any vac	(2) # vacs	(3) % vacs filled	(4) % vacs	(5) Any vac	(6) # vacs	(7) % vacs filled
Treatment	0.048**	0.092*	-0.361***	0.014	-0.057	0.019	-0.166***
	(0.023)	(0.052)	(0.114)	(0.030)	(0.040)	(0.140)	(0.039)
	[0.059]*	[0.084]*	[0.007]***	[0.277]	[0.141]	[0.341]	[0.001]***
Control mean	0.069	0.120	0.827	0.094	0.463	1.032	0.881
Observations	621	621	66	288	621	621	267

Notes: Table A21 displays the effect of our intervention on the skill composition of vacancy creation. Columns (1) to (4) show the impact on white-collar vacancies. Columns (5) and (7) show the impact on non-white-collar vacancies. Heteroskedasticity robust standard errors are displayed in parenthesis. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A22: Composition of new hires - including control variables

	White collar			Non-white collar	
	(1) Any hire	(2) # hires	(3) % hires	(4) Any hire	(5) # hires
Treatment	0.005	-0.007	-0.014	-0.080**	-0.205
	(0.019)	(0.046)	(0.031)	(0.040)	(0.152)
	[0.875]	[0.875]	[0.875]	[0.229]	[0.446]
Control mean	0.060	0.111	0.088	0.426	1.106
Observations	621	621	250	621	621

Notes: Table A22 displays the effect of our intervention on the skill composition of new hires. Columns (1) to (3) show the impact on white-collar hires. Columns (4) and (5) show the impact on non-white-collar hires. Heteroskedasticity robust standard errors are displayed in parenthesis. All specifications include control variables selected from a pre-specified set of variables using LASSO algorithms. Minimum q-values from two-stage false discovery rate correction are displayed in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A23: Effects on managers' beliefs - including control variables

	Applicant quality			Applicant numbers (standardized)		
	(1) Index	(2) WC	(3) Non-WC	(4) Index	(5) WC	(6) Non-WC
Treatment	-0.156* (0.082) [0.086]*	-0.117 (0.082) [0.154]	-0.201** (0.082) [0.086]*	-0.225** (0.111) [0.086]*	-0.204* (0.122) [0.112]	-0.211* (0.109) [0.086]*
Control mean	0.109	0.086	0.118	0.141	0.131	0.134
Observations	606	606	606	561	553	560

Notes: Table A23 displays the treatment effects on beliefs about the quality and number of applicants obtained through formal search channels. Columns (1) to (3) show the impacts on beliefs of beliefs about applicant quality. Applicant quality is measures by binary variables equal one if managers believe that they can obtain better quality candidates through different formal search channels relative to network-based hiring. Columns (4) to (6) show the impacts on beliefs of beliefs about absolute applicant numbers. All variables normalized sums of non-missing normalized beliefs across different formal search channels (online, job-board, newspaper) and vacancy type (white collar, blue collar, pink collar). Number of observations varies for beliefs about applicant numbers due to "don't know" answers. Heteroskedasticity robust standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A24: Effects on turnover - including control variables

	Employees left		Leaving reasons		
	(1)	(2)	(3)	(4)	(5)
	Any	#	Personal	Better opportunities	Fired for performance
Treatment	-0.005	-0.324	-0.080**	-0.010	-0.014
	(0.040)	(0.277)	(0.033)	(0.022)	(0.019)
	[0.904]	[0.605]	[0.077]*	[0.815]	[0.767]
Control mean	0.597	2.435	0.241	0.079	0.060
Observations	621	621	621	621	621

Notes: Table A24 displays the impact of the effects of the vacancy subsidy intervention on employee turnover. Columns (1) to (3) show the impact on a dummy variable indicating any turnover during this period. Column (2) shows the impact on the number of employees who left the firm (winsorized at the 99th percentile). Heteroskedasticity robust standard errors are displayed in parenthesis. All specifications include control variables selected from a pre-specified set of variables using LASSO algorithms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A25: Effects on candidate search inputs - including control variables

	Index	Days	Hours			Cost (ETB)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Search costs	Search duration	Screening	Non-screening	Total	Screening	Non-screening	Total
Treatment	-0.019	-1.055	-1.051	1.277	0.168	22.323	19.060	48.872
	(0.144)	(1.170)	(0.860)	(0.843)	(1.295)	(118.531)	(17.208)	(120.154)
	[0.897]	[0.736]	[0.717]	[0.717]	[0.897]	[0.897]	[0.717]	[0.897]
Control mean	0.000	4.951	4.410	1.046	5.448	228.528	17.512	241.887
Observations	240	234	227	226	227	236	233	234

Notes: Table A25 displays the effect of our intervention on candidate search inputs. The outcomes are calculated as firm-level averages and are only defined for firms that posted at least one vacancy during the treatment period. Heteroskedasticity robust standard errors are displayed in parentheses. All specifications include control variables selected from a pre-specified set of variables using LASSO algorithms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A26: Effects on characteristics of hires - including control variables

	(1) Salary (ETB)	(2) Satisfaction	(3) Share female
Treatment	-155.282	-0.034	-0.046
	(196.870)	(0.126)	(0.047)
	[0.646]	[0.786]	[0.646]
Control mean	2199.932	0.020	0.586
Observations	232	236	250

Notes: Table A26 displays the effect of our intervention on the characteristics of new hires. The outcomes are only defined for firms that hired at least one person during the treatment period. Heteroskedasticity robust standard errors are displayed in parentheses. All specifications include control variables selected from a pre-specified set of variables using LASSO algorithms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A27: Effects on post-treatment employee search, including control variables

	Vacancy creation			Hires	
	(1) Any	(2) # vacs	(3) % vacs filled	(4) Any	(5) # hires
Treatment	-0.064**	-0.148**	-0.047*	-0.071**	-0.226***
	(0.031)	(0.058)	(0.027)	(0.031)	(0.070)
	[0.048]**	[0.029]**	[0.084]*	[0.033]**	[0.007]***
Control mean	0.194	0.333	1.000	0.194	0.403
Observations	625	625	95	625	625

Notes: Table A27 displays the impact of the effects of the vacancy subsidy intervention on vacancy creation and hires in the two monthw following the four-month treatment period. Columns (1) to (3) show the impact on vacancy creation outcomes. Columns (4) to (5) show the impact on hiring outcomes. Heteroskedasticity robust standard errors are displayed in parenthesis. All specifications include control variables selected from a pre-specified set of variables using LASSO algorithms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A28: Effects on vacancy creation composition - post-treatment, including control variables

	White collar				Non-white collar		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any vac	# vacs	% vacs filled	% vacs	Any vac	# vacs	% vacs filled
Treatment	0.021	0.025	-0.167*	0.169***	-0.088***	-0.183***	
	(0.014)	(0.018)	(0.082)	(0.061)	(0.029)	(0.054)	
	[0.150]	[0.150]	[0.083]*	[0.013]**	[0.007]***	[0.004]***	[.]
Control mean	0.023	0.028	1.000	0.087	0.185	0.306	1.000
Observations	625	625	23	95	625	625	80

Notes: Table A28 displays the impact of the effects of the vacancy subsidy intervention on the composition of vacancy creation in the two months following the four-month treatment period. Columns (1) to (4) show the impact on white-collar vacancies. Columns (5) and (7) show the impact on non-white-collar vacancies. All non-cwhite-collar vacancies were filled. Heteroskedasticity robust standard errors are displayed in parenthesis. All specifications include control variables selected from a pre-specified set of variables using LASSO algorithms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A29: Effects on hiring composition - post-treatment, including control variables

	White collar			Non-white collar	
	(1)	(2)	(3)	(4)	(5)
	Any vac	# vacs	% vacs	Any vac	# vacs
Treatment	0.014	0.011	0.160***	-0.090***	-0.250***
	(0.014)	(0.016)	(0.060)	(0.029)	(0.069)
	[0.383]	[0.516]	[0.016]**	[0.005]***	[0.001]***
Control mean	0.023	0.028	0.087	0.185	0.375
Observations	625	625	93	625	625

Notes: Table A29 displays the impact of the effects of the vacancy subsidy intervention on the composition of hires in the two months following the four-month treatment period. Columns (1) to (3) show the impact on white-collar hires. Columns (4) a nd (5) show the impact on non white-collar hires. Heteroskedasticity robust standard errors are displayed in parenthesis. All specifications include control variables selected from a pre-specified set of variables using LASSO algorithms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A30: Effects on post-treatment turnover - including control variables

	Employees left		Leaving reasons		
	(1)	(2)	(3)	(4)	(5)
	Any	#	Personal	Better opportunities	Fired for performance
Treatment	-0.027	-0.048	-0.053*	0.014	-0.021*
	(0.038)	(0.177)	(0.029)	(0.018)	(0.012)
	[0.587]	[0.784]	[0.221]	[0.587]	[0.221]
Control mean	0.285	0.874	0.168	0.047	0.028
Observations	618	618	618	618	618

Notes: Table A30 displays the impact of the effects of the vacancy subsidy intervention on employee turnover after the end of the treatment period. Columns (1) to (3) show the impact on a dummy variable indicating any turnover during this period. Column (2) shows the impact on the number of employees who left the firm (winsorized at the 99th percentile). Heteroskedasticity robust standard errors are displayed in parenthesis. All specifications include control variables selected from a pre-specified set of variables using LASSO algorithms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Additional figures

Figure A1: Timeline

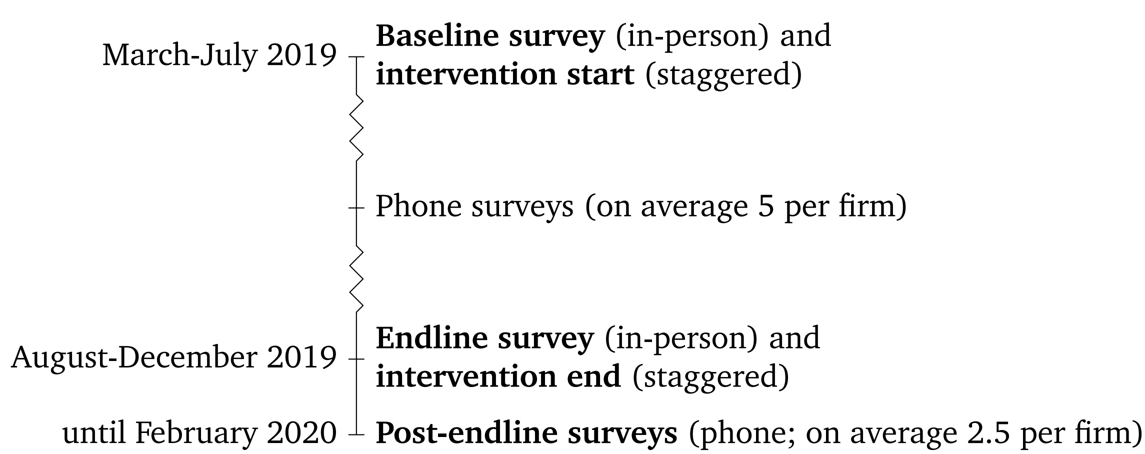


Figure A2: Geographical distribution of firms

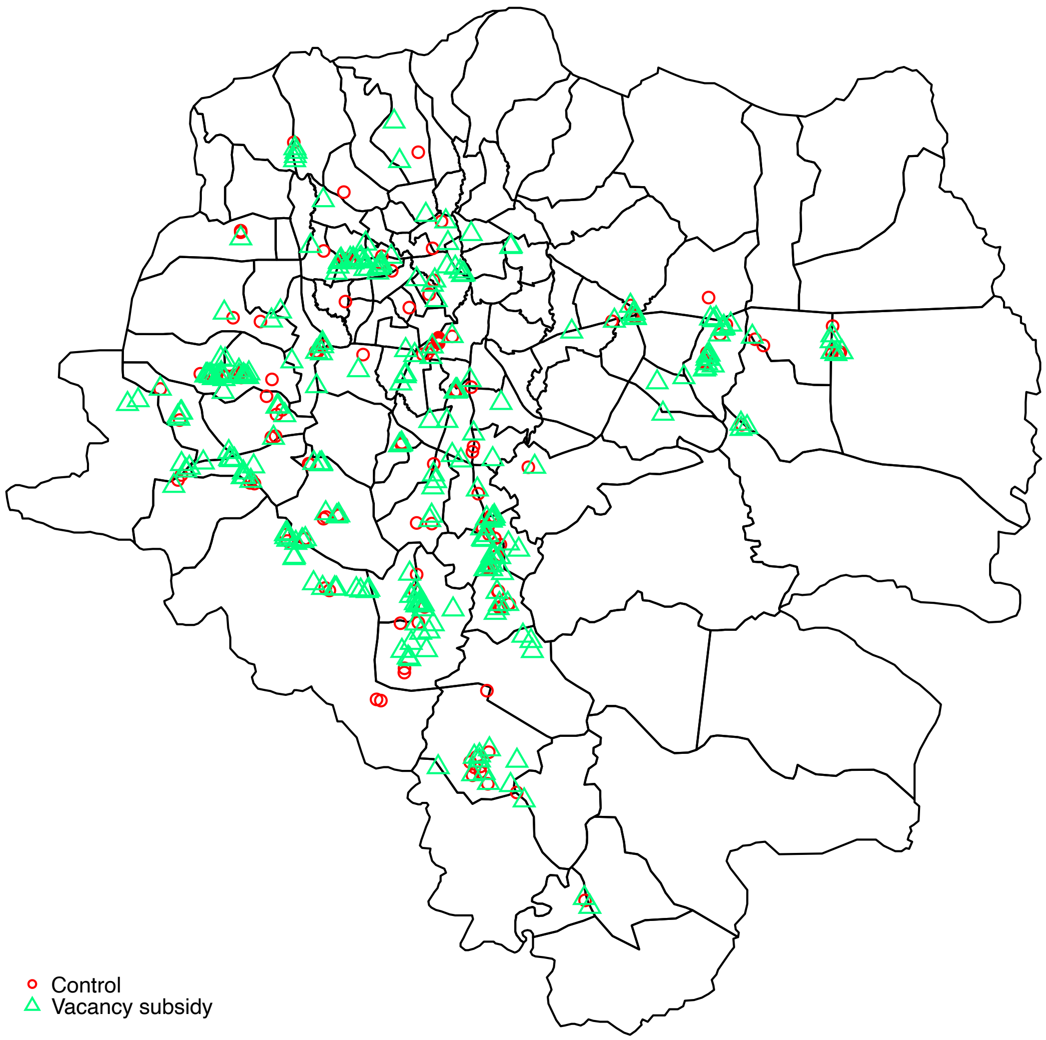
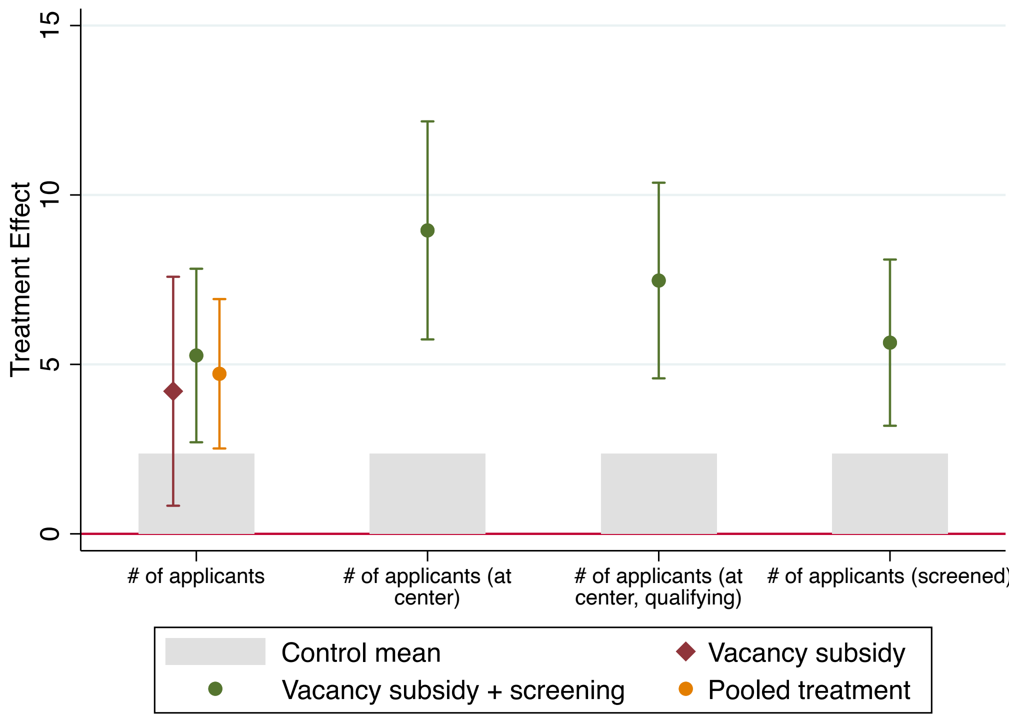


Figure A3: Treatment effects on the number of job applicants



This figure shows the treatment effects on the number of applicants. The grey bars display the control group mean. 90% confidence intervals are displayed. Bar 1 shows the separate effects of the vacancy subsidy treatment, the vacancy subsidy treatment plus screening add-on, as well as the pooled treatment on the number of applicants per firm (as reported by the firms). Bars 2-4 show the effects on applicant numbers as collected by our screening center, for the vacancy subsidy treatment plus screening add-on. The applicant numbers are displayed based on whether a candidate simply called the number specified on the vacancy (bar 2), fulfilled all the criteria specified in the job advertisement (bar 3), and actually completed the screening procedure (bar 4). Bar 4 should be approximately equivalent to the number of candidates actually applying directly at the firm (bar 1).

D Deviation from the pre-analysis plan

In our analysis we make the following deviations from our pre-analysis plan:

- We expand the number of outcomes to study both the extensive and intensive margins of vacancy creation, as well as the ratio of filled vacancies to overall vacancies. To accommodate the larger number of outcomes, we spread these across multiple tables and correct for multiple hypothesis testing across the different margins.

- We collapse all different non-white-collar employment categories (blue collar, pink collar, grey collar) into a single non-white-collar category to improve power. We also collapse managers' expectations in the same way.

- We do not normalize outcomes over time, in order to be able to use extensive margin outcomes.

- We do not winsorize the number of vacancies and number of hires because there are no outliers.

- For our main specification, we estimate pooled treatment effects instead of separate effects for a screening add-on intervention, because we do not find consistent differences across treatment arms.

- We only show firm-level regression specifications. We do not show hire- and vacancy-level specifications as they are subject to selection bias and add little additional information beyond the firm-level specification.

- We drop outcomes for which the data quality is insufficient. This mostly affects hire-level outcomes, where we struggled to get adequate data on variables such as ethnicity and religion of new hires (as well as variables derived from these). In addition, this list includes respondents' knowledge about prices of formal employee search methods.

- We include some outcomes that we did not pre-specify in the online appendix (e.g., number of employees at endline and share of white-collar employees at endline).
- We do not display all pre-specified heterogeneity analyses to simplify the presentation of results. We show a subset of this heterogeneity in Table A4.