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Search Frictions, Belief Formation, and Firm Hiring: Evidence from Ethiopia*

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Abstract

How do search frictions affect firm hiring decisions? We conduct a randomized control trial among 799 private firms with an active job vacancy in Addis Ababa, Ethiopia. A random subset of these firms are provided subsidized access to a new type of employment agency, which provides additional applicants with college diplomas or degrees. In our first main finding, we show that treated firms are 17.5% more likely to fill the vacancy within one month, but the effect is not driven by hiring workers provided by the agency. Instead, having had more interactions with college educated applicants, treated firms become less optimistic about the average productivity of college graduates. Among those firms requesting a college graduate at baseline, treated firms are significantly less likely to hire a college graduate and more likely to hire a non-college educated worker. There are no significant treatment effects on worker turnover, performance, or effort for the worker hired for that vacancy. These findings demonstrate that search frictions can distort firm hiring behavior by affecting learning and belief formation about the labor market, a potentially important but understudied barrier to firm growth in low- and middle-income countries.

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1 Introduction

There is growing evidence that search frictions have a significant impact on the urban labor markets in low- and middle-income countries. Many burgeoning cities in these countries have few search platforms for firms and job seekers to meet and share information (Franklin, 2018; Kelley et al., 2022; Carranza et al., 2023). For job seekers, they only have limited access to a subset of job posts and potentially miss out many opportunities. Recent research shows that such search frictions prevent job seekers from conducting job search and gaining enough information to develop accurate beliefs of the wage distribution, distorting employment outcomes (Banerjee and Sequeira, 2022; Alfonsi et al., 2023). For firms, similar search frictions may apply — they may also only have limited access to a subset of job seekers and potentially miss out many skilled workers. However, little is known about the impact of such search frictions on firms. Do search frictions prevent firms from matching with skilled workers? Does the lack of interaction with skilled workers lead to inaccurate beliefs of workers' productivity and sub-optimal hiring behavior?

In this paper, we conduct a randomized controlled trial (RCT) on 799 private firms with an active job vacancy in Addis Ababa, Ethiopia. We focus on the hiring of workers with college-level diplomas or degrees (henceforth college graduates) because firms use educational attainment as a heuristic to find skilled workers (Gigerenzer et al., 2022). A random subset of firms are provided subsidized access to a new type of employment agency, which gives access to a larger number of college educated applicants within a short amount of time, effectively reducing the search frictions of matching with college graduates. We show that treated firms, who had more interactions with college educated applicants, become less optimistic about the average productivity of college graduates. Among treated firms requesting a college graduate at baseline, we observe a significant shift from hiring a college graduate to a non-college educated worker. Our findings emphasize that reducing search frictions can induce learning and belief formation of workers' productivity, a potentially important but understudied mechanism to improve firm hiring in low- and middle-income countries.

The city of Addis Ababa, Ethiopia, exemplifies the high search frictions in the labor market. On average, firms in our sample only receive 1.9 job applicants over the course of five months after posting a vacancy, and 64% do not receive any college educated applicants. In addition, although the estimated attendance rate in tertiary education in Ethiopia jumped from less than 1% in the early 1990s to around 12% in 2018 (Ethiopian Socioeconomic Survey), it is unclear whether the quality of college education remains at the same level. Without frequent interactions with college educated applicants, firms may not obtain enough up-to-date information of the productivity of college graduates to form accurate beliefs.

In recent years, we observe a new type of employment agency in Addis Ababa that specializes in the recruitment service for high-skill formal jobs. They manage to form an applicant pool featuring college graduates and match them with firms at a much faster pace. Given that these employment agencies are still new to firms in Addis Ababa, we leverage 11 employment agencies in hope to reduce the search frictions of matching with college graduates, and further examine the effects of reducing search frictions on firm hiring.

We sample 799 private formal firms that are actively hiring in Addis Ababa. We first delineate 88 geographical business areas where most firms cluster and operate. For each business area, the survey team conducts a firm census, randomly selects firms that are actively hiring, and collects one vacancy from each firm. With this sampling method, we enlist a large sample of formal firms and vacancies within a short period of time. 36% firms are in manufacturing and construction sector, 39% in hospitality sector, with the median number of employees 20. We also observe a high demand for college graduates: 35% firms request a college educated worker for their vacancies at baseline.

We then implement the following RCT. We randomly match 41% vacancies with one of the 11 employment agencies at the end of the baseline. Each agency is requested to provide one or two extra applicants for the matched vacancy within two weeks. We prevent direct communication between agencies and firms. If firms hire the recommended applicants from the agency, we pay a conventional commission fee to the agency without incurring extra costs on the firms. As such, we leverage employment agencies to increase the number of college educated applicants for firms, and any learning would only occur through the interaction with the applicants. We collect detailed information of all applicants for the sampled vacancies one month (midline) and five months after baseline (endline), including i) applicant's demographics, education, and experience, and ii) firms' perceptions and hiring decisions on each applicant. We further collect personnel records at endline, including worker turnover, performance, and effort for the workers hired for the sampled vacancies. Using this dataset, we verify that 80% applicants recommended from the agencies have a college diploma or degree, compared to 43% among non-agency applicants, confirming that the intervention successfully increases the likelihood of treated firms being matched with a college graduate.

We first examine whether treated firms are more likely to interview or hire at least one worker by midline, using the initial treatment assignment to obtain intention-to-treat (ITT) causal effects. Firms initially assigned to treatment are 14.2 percentage points more likely to interview at least one applicant (23.5% increase compared to control, p-value 0.006) and 10.1 percentage points more likely to hire at least one applicant (17.5% increase compared to control, p-value 0.055), suggesting reduced search cost and faster hiring decisions. However, the treatment effects are not fully driven by the applicants provided by the agency. Although mechanically, treated firms are 3.07 percentage points more likely to hire any agency applicant, such a magnitude can only explain a small proportion of the increased hiring. Instead, treated firms are 9.07 percentage points more likely to hire any non-agency applicant (p-value 0.079). These results cannot be explained by a simple decrease in the search cost because treated firms should not have hired more non-agency applicants if hiring preferences remained unchanged. The results on interviewing and hiring non-agency applicants are robust to different inference techniques and unaffected by the concerns of attrition, matching strategies of employment agencies, demand effect, or negative spillover on the control firms.

The surprising treatment effects above may reflect changes in hiring preferences due to increasing interaction with college graduates. We first confirm that treated firms indeed receive 29% more college educated applicants over the course of five months, especially for those requesting a college graduate at baseline. However, despite the increased exposure to college educated applicants, treated firms are 11.1% less likely to consider average college graduates to be more productive than non-college educated workers (p-value 0.051). We further elicit firms' perceptions of the productivity of each job applicant and find that college educated applicants from treated firms are 41.6% less likely to be considered productive (p-value 0.063). The evidence implies that treated firms obtain more information from the extra college educated applicants, but what they learn makes them less optimistic of the average productivity of college graduates.

We use a simple model to illustrate how lower search frictions can induce such an update on beliefs and derive testable predictions on hiring behavior. Suppose college graduates possess a productivity premium, or college premium. Firms are uncertain of the college premium. By creating a new search platform featuring college graduates, employment agencies effectively increase the arrival rate of college graduates and reduce the search cost of matching with a college graduate. In addition, from a large class of learning models including Bayesian learning, firms may have more accurate beliefs of the college premium as they observe more signals of productivity from matching with more college graduates. If firms are initially over-optimistic of the college premium, increasing the arrival rate of college graduates may sufficiently decrease the beliefs of the college premium, lower the net benefit of hiring a college graduate, and hire fewer college graduates despite lower search cost.

Following this prediction, we examine the treatment effects on the hiring of college graduates. On average, treated firms tend to interview and hire fewer college graduates and more non-college educated workers by endline, although insignificantly. The average effects, however, can be masked by the heterogeneity regarding the baseline request for college graduates: for firms that request a college graduate at baseline, the decreased beliefs of college premium may render hiring a college graduate to be less profitable, prompting more firms to switch to hiring a non-college educated worker. Indeed, we find that treated firms requesting a college graduate at baseline are significantly less likely to interview and hire any college graduates (27.3% and 33.7% decrease compared to control firms requesting a college graduate, p-values 0.024 and 0.008), and instead more likely to interview and hire at least one non-college educated worker (82.9% and 109% increase compared to control firms requesting a college graduate, p-values 0.070 and 0.049). For firms not requesting a college graduate at baseline, we do not find significant treatment effects on hiring a college graduate or a non-college educated worker, consistent with the interpretation that for firms whose net benefit of hiring a college graduate is already below the search cost initially, further decreasing the beliefs of college premium does not affect their hiring behavior.

A second prediction derived from the model is that for firms with less exposure to college graduates, the information obtained from the extra college educated applicants would lead to larger updates in the beliefs and stronger effects on the hiring behavior. We use the percentage of current employees with a college diploma or degree (henceforth college share) as a proxy of exposure to college graduates. We find that among firms requesting a college graduate at baseline, treated firms with below-median college share are significantly less likely to interview and hire any college graduates (40.1% and 42.8% decrease compared to control firms with below-median college share, p-values 0.070 and 0.041), and more likely to interview and hire at least one non-college educated worker (106% and 113% increase compared to control firms with below-median college share, p-values 0.147 and 0.167). We do not find significant treatment effects for firms with above-median college share. We thus establish causal empirical evidence supporting the hypothesis that employment agencies induce learning about the productivity of college graduates and sufficiently shift the hiring preferences towards non-college educated workers, especially for firms requesting a college graduate at baseline and with less *ex ante* exposure to college graduates.

What signals do firms observe from the college educated applicants that lead to such negative updates in beliefs? We provide descriptive evidence by comparing the characteristics of all college educated versus non-college educated applicants for the same position. We do not find that college educated applicants have more relevant past experience for the position, have more outside options, or are more likely to have a better-paid outside offer. We further find that firms perceive college educated applicants to be equally productive as non-college educated applicants, suggesting that firms do not observe other signals from college educated applicants that may imply a high college premium, which possibly explains the negative updates on the average productivity of college graduates.

We rule out four alternative mechanisms that may explain some of the empirical results. First, firms might hire fewer college graduates because college graduates are more likely to reject the offers. We do not find that college graduates systematically reject more interview invites or hiring offers. Second, we discuss other potential hypotheses on the search cost and benefit. In particular, providing agency applicants may lower the marginal benefit of searching for one more applicant and speed up the hiring process. This cannot explain why we observe a shift in hiring preferences among treated firms that request for a college graduate. Third, firms may perceive college educated applicants to be negatively selected if they do not expect college graduates to apply. This cannot explain why the treatment effects are the strongest among firms requesting a college graduate at baseline; we also do not observe that treated firms perceive college educated applicants to be less productive. Last, treated firms might hire fewer college graduates because they can afford to make sub-optimal hiring decisions and resort to the agencies for future replacement. We do not find evidence suggesting that treated firms plan to hire more applicants from the agencies in the future.

What are the implications on salary and match quality if employment agencies induce treated firms to hire fewer college graduates? First, although we do not find significant ITT effects on the monthly salary, we find suggestive evidence that among complier firms that switch from hiring college graduates to non-college educated workers, they reduce monthly salary by 55.4% because of lower salary ladder for non-college educated workers. Second, for firms requesting a college graduate at baseline, we examine the treatment effects on worker turnover, performance, and effort for the workers hired for the sampled vacancies, as proxies for match quality. We do not find that hired workers are more likely to voluntarily quit or be fired by the firms. We also do not find significant decrease in different measures of on-the-job performance, absenteeism, or overtime work. Together with lower search cost, we conclude with a potential increase in the profit for complier firms.

Our paper makes three key contributions. First, we demonstrate the complex influence of search frictions in the labor market. Current literature has documented the existence of prohibitive search frictions in the low- and middle-income countries (Alfonsi et al., 2023; Vitali, 2023; Kelley et al., 2022; Abebe et al., 2021; Franklin, 2018), but the interventions on simply alleviating search cost, *e.g.*, transportation subsidy, seem to have limited impact on the final employment outcomes of job seekers. Our findings suggest that search frictions may exacerbate the cost of learning, which produces more profound implications in countries where severe information asymmetry exists regarding workers' productivity (Carranza et al., 2023; Bassi and Nansamba, 2022; Abel et al., 2020), job preferences (Banerjee and Chiplunkar, 2022), or trustworthiness (Fernando et al., 2022; Heath, 2018; Beaman and Magruder, 2012).[1] Reducing search frictions, therefore, may generate greater impact on the labor market through facilitating information exchange between different participants.[2]

Second, we provide more empirical evidence to the scant literature on firm hiring practices in low- and middle-income countries. The growing literature on hiring in high-income countries rely on detailed personnel data from large corporations (Haegele, ming; Méndez and Van Patten, 2022; Li et al., 2023) or administrative data (Caldwell and Danieli, 2022; Jäger et al., 2023), both almost non-existent in sub-Saharan African countries. In low-income countries, researchers usually apply RCTs to understand the hiring constraints faced by small firms (Hardy and McCasland, 2023; Banerjee et al., 2023; Hensel et al., 2021). We

[1]In particular, there are two papers that discuss the interplay of search cost and learning cost. Banerjee and Sequeira (2022) incentivize job seekers in South Africa to conduct more job searches and find that job seekers adjust their beliefs of the labor market. Abebe et al. (2023) conduct a job fair in Addis Ababa and find that both firms and workers update their beliefs of the labor market through more mutual interactions. Our paper focuses on the impact of search frictions on firm hiring, and we exploit existing labor market intermediaries to lower the search frictions for firms without engaging in direct information exchange, from which we can design clear mechanism tests on how lower search frictions induce learning of workers' productivity.

[2]Our findings also echo with the issue of hiring minority workers (Cullen et al., 2023; Li et al., 2023), where increasing the exposure to minority workers alleviates statistical discrimination.

manage to combine the two methods in a low-income country by collecting detailed hiring outcomes and personnel records from a large sample of formal firms, and conduct an RCT to rigorously disentangle the effects of search frictions on hiring.

Third, this paper contributes to a small branch of literature in labor economics about labor market intermediaries (Autor, 2008). Autor (2001), Stanton and Thomas (2016), and Cowgill and Perkowski (2020) find evidence of labor market intermediaries inducing positive selection of workers. We find that in addition to positive selection, labor market intermediaries can facilitate information exchange between different participants. This potentially provides policymakers with a cost-effective solution to addressing information asymmetry in low- and middle-income countries.[3]

The paper proceeds as follows. Section 2 discusses more context of the labor market and employment agencies in Ethiopia. Section 3 introduces the sampling method, intervention, and data collection. Section 4 discusses the main specification and the main results on hiring outcomes. Section 5 presents detailed empirical evidence on how employment agencies induce learning. Section 6 presents a cost-benefit analysis. Section 7 concludes.

2 Context

Providing quality education is one of the 17 sustainable development goals by United Nations. Indeed, the last two decades witness a rapid growth in the number of people receiving tertiary education. UNESCO estimates about 9% of young population aged 18-25 are enrolled in tertiary education in Sub-Saharan Africa, compared to 5% in the early 2000. In Figure 1, Panel A, we utilize the dataset from International Labor Organization (ILO) from 2000–20, comparable across countries and over time, and compute the average percentage of labor force aged 25–54 who receive tertiary education in low- or middle-income countries. Compared to 6% in year 2000, the percentage of labor force with tertiary education increases almost three-fold by year 2020, a rise that will continue for the foreseeable future.

Less is certain, however, about the quality of education. For non-tertiary education,

[3] Many programs designed to correct labor market frictions require large-scale third-party effort to overcome coordination cost or provide costly information to labor market participants (Abebe et al., 2023; Algan et al., 2020; Bloom et al., 2013). Policymakers can potentially leverage the existing labor market intermediaries, driven by their own financial interests, to facilitate matching and learning in the labor market.

researchers find mixed effects of investment in schools on education quality (Evans and Mendez Acosta, 2021; Kremer et al., 2013; Kremer and Holla, 2009).[4] For tertiary education, Martellini et al. (2022) investigate the labor outcomes of workers in United States with college degrees from various universities in 48 countries, arguably controlling for the same labor market, and estimate the return to college for each institute. They find that college graduates in the richest countries have 50 percent more human capital than college graduates in the poorest countries, suggesting a large gap in education quality despite the rapid growth in the quantity of tertiary institutions in low- and middle-income countries. We further examine the ILO data, use the unemployment rate of college graduates as a proxy of the return to college, and present the time trend of the unemployment rate in Figure 1, Panel B. The average unemployment rate of college graduates in low- or middle-income countries fluctuates around 5.6% before 2012, but since then has steadily increased to 8.8% in 2020. We do not observe such an increase among non-tertiary educated workers in low- or middle-income countries, nor among tertiary educated workers in high-income countries as shown in Figure B1. Evidence depicts an ambiguous, if not deteriorating, return to college in low- and middle-income countries.[5]

Under such uncertainty of the quality of college education, it is unclear how firms may adjust their hiring practices to the new reality, especially in low- and middle-income countries where the labor market frictions are also more severe. Many firms use education as a major heuristic to evaluate job seekers' quality and are in demand for higher-educated workers (Gigerenzer et al., 2022). Yet, many firms are not able to interact with many college educated applicants, both because there are not many college graduates in the labor market, and

[4]Development economists conduct various interventions on education, mostly targeting primary and secondary schools, to understand how to enhance the quality of education through various pedagogy tools and teacher incentives (Brown and Andrabi, 2023; Duflo et al., 2020; Muralidharan et al., 2019; Piper et al., 2018; Muralidharan and Sundararaman, 2011). Less is understood on how to improve, or simply estimate the quality of tertiary education in low- and middle-income countries. On the other hand, there is a substantial literature in labor economics on the return to college education in developed countries such as United States (Card, 2001; Dale and Krueger, 2002; Carneiro et al., 2011; Zimmerman, 2014; Smith et al., 2020). With the drastic growth in tertiary education in low- and middle-income countries, similar methodologies may be applicable to rigorously estimate the return of tertiary education in low- and middle-income contexts.

[5]The ILO database harmonizes the unemployment statistics across countries and time according with one standard of unemployment: not in employment, seeking employment, and currently available to take up employment given a new job opportunity. The standard of employment includes part-time, informal, temporary, seasonal or casual employment. A modification to the standard took place in 2013 which confines employment to be engagement in producing goods or providing services for pay or profit (ILO, 2013). The modification, however, does not affect most classifications, and we believe it cannot solely explain the increase in unemployment rate among tertiary educated workers in low- and middle-income countries.

because there are not enough platforms for firms to post jobs and find college graduates. In fact, according to Enterprise Surveys by World Bank, 41% firms agree that inadequately educated workforce constitutes at least moderate obstacle (World Bank, 2022), suggesting that the lack of interaction with educated workers is prevalent for firms in many countries. It is thus not difficult to imagine the challenges for firms to obtain information of college graduates and develop accurate beliefs of their productivity.

2.1 Labor Market in Ethiopia

The labor market of Addis Ababa, Ethiopia exemplifies such issues. In the early 1990s, there were only three public universities across the whole country enrolling 1% of all young people aged 18–25. In 2018, the gross attendance rate in tertiary education in Ethiopia jumps to 11.7% (Ethiopian Socioeconomic Survey).[6] The quality of tertiary education, however, is unclear. Anecdotes suggest that the quality of college education seems to decrease in recent years with the rapid expansion of private colleges.[7] Abebe et al. (2021) followed 510 young job seekers in Addis Ababa with a college diploma or degree, among whom 21% were still unemployed three years after graduation, suggesting that college graduates are having difficulty finding jobs in the current labor market.

This seems at odds with the high labor demand for college graduates we observe from our sample of 799 firms, of which we will discuss the sampling method in the next section. Figure 3, Panel A presents a simple comparison between the demand and supply of college graduates. 34.9% firms from our sample are looking for college graduates, much higher than the estimated attendance rate in tertiary education by Ethiopian Socioeconomic Survey. Indeed, most firms value college education. We ask firms in the baseline whether they think college graduates are more productive and have more job opportunities than non-college educated workers. Figure 3, Panel B shows that 70.2% of the firms agree that college graduates are more productive than non-college educated workers, and 61.4% believe there are more job opportunities for college graduates in the current labor market. It is consistent with the common heuristic that higher educational attainment is correlated with higher

[6] Roughly speaking, 11.7% of people aged 18–23 in Ethiopia attended any tertiary institution in 2018.

[7] An article on Guardian in 2015 discusses relevant issues of the recent development of Ethiopian higher education: https://www.theguardian.com/global-development-professionals-network/2015/jun/22/ethiopia-higher-eduction-universities-development.

productivity, either through the value-added to human capital (Becker, 1964) or through the selective procedure of tertiary education (Spence, 1978) .

One explanation to reconcile these two opposing facts is high search frictions. Given the 11.7% gross attendance ratio in tertiary education, by chance, firms may not match with many college graduates during hiring seasons. Besides, there are not many platforms for firms to post jobs. The most common job platforms are three major notice boards located in the city center of Addis Ababa, clearly not enough to facilitate matching in a city of 5 million people.[8] Figure 4 shows the distribution of the number of applicants received for our sampled vacancies over the period of five months (excluding those from the employment agencies in our intervention). The median number of applicants is merely one, the average 1.90, with 12.1% of firms having no applicants at all. Panel B focuses on the distribution of college educated applicants. 64.0% of these vacancies do not receive any college educated applicant. Figure B6 shows that even among firms requesting college graduates, 38.1% still do not receive any college graduate over the course of five months. The descriptive evidence confirms the severity of the search frictions in this labor market, under which firms may not be able to obtain enough information of college graduates' productivity and develop accurate beliefs.[9]

2.2 Employment Agencies

Can labor market correct search frictions itself? We observe a new type of labor market intermediary, employment agencies, that might act as a market self-correction. Responding to the increasing gap between unemployed college graduates and firms' demand for skilled workers, some former job brokers in informal sectors register as an employment agency and tailor the recruitment service for educated job seekers.[10] By strategically locating at the city

[8] In the baseline, we ask firms how they usually post jobs. 46% firms post jobs on notice boards, 45% ask for recommendations through personal networks, and 35% find workers through informal brokers. Only less than 13% post jobs on any online job platforms, and 8% seek help from employment agencies.

[9] Furthermore, Figure B7 shows college educated applicants are mostly concentrated among larger firms and firms with a larger share of employees with a college diploma or degree, which implies an unevenly overwhelming burden of the search frictions on smaller firms and those with little exposure to college graduates.

[10] In 2018, the new Ethiopian government issued an initiative to encourage qualified brokers to register in the government in hope for boosting private and formal employment. To qualify for registration, an employment agency should obtain a business license for taxation purpose, hire at least one expert with professional license in human resources, have at least 4 employees, have a physical office, and deposit 200,000 Ethiopian birr in a security account. Addis Ababa Labour, Enterprises, and Industry Development Office

center, these employment agencies are able to attract a large group of job seekers with a college diploma or degree as well as firms with higher-paid formal jobs, effectively acting as a new job platform that matches firms and college graduates at a much faster pace. Figure B2 shows a representative employment agency. Figure B3, Panel A shows that the number of new registered employment agencies in Bole sub-city after 2018 increases drastically.[11] They are still very new to firms in Addis Ababa, and thus we are able to design a randomized control trial to leverage these employment agencies to lower search frictions for a random subset of firms.[12]

We interviewed the owners of 25 employment agencies between July and August 2021, in Bole sub-city where most recruitment services locate, to observe their daily operations and interactions with job seekers. Table C2, Panel A summarizes the qualitative description of the functions of employment agencies. In general, employment agencies do not seem to provide sophisticated recruitment services. Most employment agencies only check applicants' basic documents such as IDs and education certificates. Some may recommend vocational training facilities to job seekers or check previous employers' recommendation. Most do not provide additional training that potentially enhances workers' productivity, or conduct additional grading test that potentially improves the signals of workers' productivity. This setting stands in contrast with what labor economists have found about labor market intermediaries in other contexts, which provide temporary training or better signals of productivity (Autor, 2001; Stanton and Thomas, 2016).

In addition, we ask 539 job seekers in our sample about their perceived benefits from employment agencies. Table C2, Panel B presents the summary. Job seekers mostly agree that employment agencies may provide advice on which jobs to apply to, but do not help

appoints local officials to specifically regulate and audit all the registered employment agencies. Upon successful matches, employment agencies usually charge 10–20% first-month salary from firms, although informally they also charge job seekers an entry fee between 100–500 Ethiopian birr.

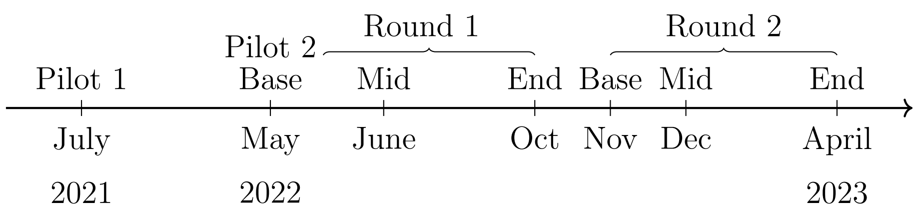
[11] There is another form of labor market intermediaries, outsourcing companies, that are more prevalent in Addis Ababa prior to 2018. Firms outsource low-skill occupations to these companies such as janitors and security guards, similar to Goldschmidt and Schmieder (2017) and Dorn et al. (2018) in the context of Germany and US. Instead, we see a downward trend of registered outsourcing companies post 2019, which may imply an increase in the demand for high-skill instead of low-skill workers.

[12] The trend of employment agencies is also observed in many other low- and middle-income countries. Figure B3, Panel B shows a time series of newly established employment agencies observed from one of the largest online business-to-business platforms. Despite omitting many employment agencies not able to be observed online, there has been an increasing number of new employment agencies since 2005 across low- and middle-income countries providing recruitment services to private firms.

with networking, interview preparation or CV writing. This corroborates our observation that employment agencies do not increase the human capital or provide better signals of productivity. We thus believe that qualitatively, the main function of employment agencies is reducing the search frictions and facilitating matching between firms and college educated job seekers.

3 Data and Intervention

We first conducted a pilot survey during July 2021 of 25 employment agencies to collect qualitative evidence of the functions of employment agencies. We then conducted two rounds of data collection: May–October 2022, November 2022–April 2023.



3.1 Sampling

We conduct a new sampling approach to collect a representative sample of active job vacancies. First, we consult with local government officials from five sub-cities (Bole, Akaki Kality, Yeka, Nefas Silk-Lafto, Lemi Kura) to understand where most businesses are located within the sub-cities. We then delineate 88 business areas in total where most firms conduct businesses; each business area has about 50–100 formal firms. In each business area, enumerators conduct a census and list as many formal firms as possible. Enumerators will then select 10 firms from each business area following three criteria: (1) at least 4 employees; (2) currently hiring or planning to hire within 1 month; (3) respondents agree that hiring is challenging. Figure 2 shows the geographic distribution of 88 sampled business areas and 799 firms selected for the baseline survey.[13]

[13] We managed to enlist 3,369 firms in the census. 958 firms have at least four employees and currently hiring or planning to hire within 1 month. We include the third selection criterion to target firms in need for recruitment service; however, among these 958 firms, 97% agree hiring is challenging, and thus this criterion is not as binding.

This sampling method has a few unique advantages. First, we are able to observe currently operating firms in a much faster way. An alternative sampling method is to obtain a firm registry from the Ministry of Trade. Such registry, however, may have outdated information. During our pilot, we obtained a firm registry from Bole sub-city and only succeeded in contacting less than 20% of the listed firms. Table C1, Panel A compares the sampling of firms to that of Hensel et al. (2021), who sampled from the firm registry. Our firm sample includes more firms from hospitality sector and of more current employees in general. Other existing firm surveys of Ethiopia, such as Large Manufacturing and Electricity Industries Survey, mostly focus on manufacturing firms with at least 10 employees.

Second, we are able to observe firms that do not post jobs on public platforms, such as notice boards or online job search platforms. Franklin (2018) discusses potential sampling bias from only using notice boards in the city center. During our pilot, we collected 150 job posts from 3 major notice boards of Addis Ababa; we also collected 2,073 job posts from a major online job search platform of Ethiopia from 2019–22. Table C1, Panel B compares the posted salary distribution between the three different samples. Our vacancy sample is able to capture more lower-paid jobs, particularly those with salary between 2,000–4,000 Ethiopian birr (ETB) per month. Notice boards and online platforms select higher-paid jobs, possibly because these firms are able to afford higher job-posting costs on these public platforms.

Third, we specifically target formal firms with at least 4 employees. The median firm size in our sample is 20 employees. Such firms may have a higher labor demand that cannot be met through internal network, hence more likely to hire externally.

3.2 Intervention

During the baseline, enumerators collect basic information of sector, workforce structure, and hiring practices. We then select one active job vacancy from each firm and collect vacancy details including minimum requirements on education and experience, job descriptions, and highest salaries that firms are willing to pay, or reservation wage. We use "firm" and "vacancy" interchangeably in the main analysis.[14]

At the end of the baseline, we implement the following intervention. We first select 11

[14] 80% firms in our sample post only one vacancy during the baseline survey. For those who post more than one vacancy, we avoid low-skill positions such as janitors, or positions requiring many years of experience such as executive managers.

employment agencies that are actively operating and have a large labor pool. Most firms in our sample have not worked with any of the 11 employment agencies before.[15] Among firms with reservation wage at least 2,000 ETB (henceforth eligible firms), we randomly select 326 firms into treatment group, stratified by business areas. Firms that are not willing to pay more than 2,000 ETB are not considered for the intervention.[16] To examine the extent of spillover effect, in Round 2, we randomly select 21 business areas, and randomly assign 75% eligible firms per business areas to the treatment; the other 20 business areas in Round 2 are not selected for the treatment.

The matching process follows three steps. First, enumerators match each treated firm quasi-randomly with one of the 11 employment agencies.[17] Second, the employment agency is requested to select 1–2 qualified applicants within two weeks for each matched vacancy. We do not interfere with the selection process. Following conventions, we guarantee 20% first-month salary for employment agencies on behalf of treated firms if the match is successful. No extra costs are incurred to treated firms. We thus preserve the main function of employment agencies, that is, increasing the number of job applicants, without altering monetary incentives for both employment agencies and treated firms.

Third, we deliberately prevent direct communication between the employment agencies and treated firms. We only inform the employment agencies of the job descriptions and rough

[15]In fact, although 25% of the sampled firms have used any external recruitment services in the past, most firms only hire informal or low-skill workers from job brokers and are not aware of the new type of employment agencies that provide skilled workers. Only 8.3% of all firms have worked with the new type of employment agencies observed in the city administration registry. Precisely zero firm reports any of these 11 employment agencies to have been their main recruitment service provider.

[16]We implement the 2,000 ETB threshold to ensure the cooperation with the employment agencies because some specifically mention they would not provide applicants for jobs with too low salary. We use the first two weeks of survey to pilot the treatment. During the pilot, we did not enforce the 2,000 ETB threshold and faced backlash from the employment agencies. As a result, the survey team decided to match some firms initially assigned to control group to the employment agencies. After the pilot, we strictly implemented the initial random assignment and the additional threshold of 2,000 ETB. In the main analysis, we include the pilot sample and use initial random assignment to obtain causal effects.

[17]It is less important whether the matching between firms and the 11 employment agencies is strictly random for two reasons. First, all 11 employment agencies function similarly. All agencies check personal identification and educational certificates, some check previous recommendations, and none provide additional grading or training. Second, in reality, firms may consult with multiple agencies at the same time and select the best recruitment service. The initial match with a particular employment agency matters less to firms than actually receiving a qualified applicant from anywhere. During the implementation, the initial matching between firms and employment agencies is random. However, when the initially matched agency could not find some specific types of workers (e.g., coffee tasters), very occasionally, the survey team might rematch the vacancy to a different agency to increase the likelihood of finding a qualified worker.

locations of treated firms; as such, agencies do not know to which firms they are providing the job seekers. Once employment agencies complete the selection process, the survey team collects the selected CVs and directly delivers to the treated firms in-person; treated firms only know whether the applicant is recommended from an employment agency, without knowing which agency exactly. We thus prevent any direct information exchange between firms and employment agencies, and any learning would only happen through interacting with the applicants. The survey team does not interfere with any hiring process that follows.

3.3 Hiring Data

We conduct two follow-up surveys for each firm. One month after the baseline, enumerators visit each firm, ask for a list of all applicants for the sampled vacancy, and record the following information for each applicant: (1) skill indicators (education, experience), (2) hiring decision (whether the applicant is invited to the interview, whether the applicant passes the interview and gets an offer), (3) perceptions of productivity.[18] In addition, enumerators conduct a phone survey of up to 6 job seekers selected from the applicant list and record the following information for each applicant: (1) demographics (age, gender, residential district), (2) current employment status and salary if employed.[19] For firms that successfully hire at least one worker, we further record the negotiated salary.[20]

Five months after baseline, enumerators visit each firm again. We first collect applicant details for firms that did not make the final decision in the last survey but have hired anyone for the sampled vacancy since then. We then observe following outcomes of the hired worker: (1) whether the worker still stays on the job, quits voluntarily, or has been fired by the firm, (2) performance records (whether firm thinks the worker is more productive compared to similar workers, and performance record from the firm), (3) effort (absent days in the last

[18] Perception questions are only asked in Round 2.

[19] If the firm has no more than 6 applicants, enumerators conduct phone surveys on all applicants. If the firm has more than 6 applicants, enumerators randomly pick 2 job seekers from 3 categories: (i) applicants who pass the interview, (ii) applicants who are invited to the interview but do not show up, (iii) applicants not invited to the interview. 78% job applicants observed in our sample participate in the phone survey.

[20] The survey team strives to collect as many applicants as possible. Enumerators ask firms to go through all printed CVs, applications through online platforms such as Telegram, and personal recommendations, and record information of each applicant by enumerators themselves. Our survey protocols potentially omit some informal applications (for example, workers directly showing up and asking for jobs without any paper records), which are not the majority among applications in the formal sector.

30 days and overtime hours in the last 7 days). We further collect firms' perceptions of the average productivity of college graduates in the current labor market and future hiring plans.

We predominantly use firm-reported data in the main analysis. To validate the accuracy of the data especially on applicants, in Figure B4, we focus on 683 workers who are sampled in the worker survey and hired by firms for the sampled vacancy, of which we are able to compare firms' reports and workers' reports on the same set of labor outcomes. We observe high cross-validation rate: 98.0% workers confirm that they are indeed hired, 95.8% report the same job description. Half of the workers report exactly the same amount of salary as firms do, and 84.3% of the worker-reported salaries are within 0.3 standard deviation. We thus believe that most firms do not systematically misreport information on applicants.

Figure 5, Panel A shows the number of firms that eventually receive extra applicants after the intervention. Among eligible firms, 45.7% of the treated firms receive at least one extra applicant. Zero eligible control firms receive any extra applicant; almost none of the non-eligible firms receive any extra applicant.[21]

We then examine what types of applicant are provided by the employment agencies. We first look at whether the applicants are more likely to have a college diploma or degree. Figure 5, Panel B shows that 80.0% applicants recommended from employment agencies have a college diploma or degree, significantly higher than the average rate 42.8% observed among other applicants in our sample. This supports our qualitative observation that these employment agencies mainly provide college graduates. We further compare agency applicants to non-agency applicants applying to the same job in Figure B5 regarding observable demographics, clustered at the firm level. Having a college diploma or degree remains the most outstanding feature of agency applicants. Agency applicants do not look significantly different regarding experience, gender, and age. We thus establish the evidence that employment agencies effectively reduce the search frictions of matching with college educated applicants.

[21] The main reason why only 45.7% eligible firms receive extra applicants is because some firms hire in the off-season, for example, firms hiring teachers during the school year. We discuss relevant caveats to the estimation in Section 4.3 and alternative mechanisms in Section 5.6.

4 Effect of Employment Agencies on Hiring

4.1 Specification

We use the following specification for the firm-level analysis:

$$Y_{jc} = \alpha_c + \beta T_{jc} + \delta X_{jc} + \epsilon_{jc} \tag{1}$$

T_{jc} is the initial treatment assignment of firm j in business area c. X_{jc} is a vector of baseline characteristics of firms and the posted vacancies. The main outcome of interest Y_{jc} is whether firm j interviews or hires any applicants of certain characteristics. β is the parameter of interest, that is, the effect of being matched to an employment agency on outcome Y_{jc}. Since we stratify the treatment by business area, we include business area fixed effects α_c for all regressions to obtain within-cluster comparison. ϵ_{jc} is the idiosyncratic error clustered at the level of the business area. We only include firms with reservation wage at least 2,000 ETB (eligible firms) in the regression because non-eligible firms are not considered for the treatment implementation. Appendix E replicates all main results by including non-eligible firms in the control group. Table 1 shows the balance between eligible firms initially assigned to treatment and control groups across all baseline characteristics.

Given that not all firms assigned to treatment receive extra applicants, Specification 1 obtains an intention-to-treat (ITT) estimate of the effect of receiving extra applicants from the employment agencies. In addition, the actual treatment status is not exactly equal to the initial treatment assignment during the first two weeks of piloting due to logistical constraints.[22] To address the potential bias caused by the non-compliance, we conduct two additional replication exercises in Appendix E: i) using the initial treatment assignment T_{jc} as an instrument to the actual treatment status, and ii) by excluding the pilot sample. All regressions control for all baseline characteristics listed in Table 1.

[22]Table C3 shows a simple comparison between eligible firms that are eventually selected for treatment and control group, clustered at the business area level. Although these two groups are largely indistinguishable regarding sector, current employee structures, and hiring practices, eligible firms in the treatment group are more likely to require applicants to have at least vocational training, and more likely to post jobs involving skilled, less manual, and less routine work, which imply that firms in the treatment group may provide different types of vacancies. We further address the caveat of firms selecting vacancies in response to the treatment in Section 4.3.

4.2 Effects on Successful Matches

We first confirm the treatment effect on receiving extra applicants from the employment agencies in Table C4, a replication of Figure 5. Panel A shows that on average, firms initially assigned to treatment (henceforth treated firms) receive 0.37 more agency applicant by midline. The number of non-agency applicants are unaffected. Eventually, we observe a significant increase in the total number of applicants. If the employment agencies only reduce search frictions, one would expect treated firms to interview and hire more workers recommended from the employment agencies by the time we conduct the midline survey.

Table 2 presents the main results on whether firms interview or hire any worker by midline. Panel A, Column (1) compares eligible firms initially assigned to treatment group to those in eligible control group, controlling for all baseline characteristics and business area fixed effects. Treated firms are 14.2 percentage points more likely to interview at least one worker for the vacancy when observed one month after the baseline, a 23.5% increase compared to the control mean at 1% significance level.[23] Column (2) includes the non-eligible sample into the control group. The magnitude slightly decreases to 11.8 percentage points with a slightly increased p-value. Column (3) uses the initial assignment as an instrument to the actual treatment status. The F-statistic of the first stage is 124.8, well above the threshold where the normal asymptotic of the estimates is preserved (Lee et al., 2022). The magnitude increases to 19.1 percentage points, but the p-value remains very similar, suggesting that the logistical constraints during the pilot do not impose threat to the estimation. Column (4) excludes pilot sample and obtains higher magnitude (17.9 percentage points) and higher precision.

Panel B shows the results on hiring. Firms initially assigned to treatment group are 10.1 percentage points more likely to hire at least one worker when observed one month after the baseline, or 17.5% increase compared to the control mean (p-value 0.0547). Using the other three different specifications does not affect the magnitudes (8.42–13.6 percentage points) nor the statistical inference (p-value 0.0139– 0.0629). These results consistently show

[23]The control mean also reflects that 40% control firms simply do not conduct any interviews when observed one month after the baseline, among which 68% have at least one applicant. 61% of firms that do not interview any applicants postpone the hiring because of lack of market demand or in hope for better applicants. 14% cancel the vacancies because of budget shortage or other administrative reasons. 21% mention that they do not receive any qualified applicants. Table C5 looks at the treatment effect on additional hiring decisions, and finds that treated firms are less likely to postpone or cancel the vacancies by midline.

a significant positive effect of employment agencies on the match success rate by the time we conduct the midline survey. For the rest of the main analysis, we only show the main specification in Column (1) and report the replication results in Appendix E.[24]

However, we find that the treatment effect is mainly driven by applicants from non-agency hiring channels. Table 3 presents the results. Although mechanically, treated firms are more likely to interview and hire at least one agency applicant, the effect on hiring agency applicants is merely 3.07 percentage points, which can only explain at most 30.4% of the treatment effect on the increased successful matches (10.1 percentage points). In fact, only ten firms eventually give an offer to the applicants provided by the employment agencies. Instead, treated firms are more likely to interview and hire non-agency applicants by 9.76 and 9.07 percentage points. The results cannot be explained by a simple decrease in the search frictions. Figure E1 replicates the results on non-agency applicants using different samples and treatment status and finds robust estimates. In addition, in Table C7, we observe that treatment effects on the match success rate become insignificant by endline, suggesting that search frictions are not as binding a constraint because eventually control firms can afford to wait for at least one applicant and fill the position.

4.3 Robustness

Before we investigate the mechanism further, we examine the robustness of the main results on interviewing and hiring non-agency applicants in the following five ways. First, we examine the robustness of statistical inference in Table C8. Column (2) does not cluster the standard errors at the level of business area. The standard errors are slightly higher than Column (1), which suggests potentially negative correlations within cluster but does not affect the inference. Concerned about statistical inference from a small number of clusters, we use bootstrapping to compute clustered standard errors in Column (3) and conduct a permutation test in Column (4). Standard errors do not vary much. Concerned with the efficiency of the estimates due to heteroskedasticity, in Column (5), we weight the observations

[24]In Table C6, we examine whether our definitions of outcome variables capture the main treatment effect, considering that firms may also create more positions to accommodate more applicants from the employment agencies. The intervention slightly increase both the number of interviewees and that of new hires, albeit insignificantly. We then increase the threshold of the indicator (for instance, whether firms interview at least two applicants); treatment effects are not significant for most of the specifications. We thus believe that our main outcomes, whether firms interview and hire at least one applicant, capture the main treatment effects.

with the inverse of the total number of applicants because vacancies with more applicants may conduct interview or hiring decisions faster. To avoid the potential bias induced by the correlation of treatment status and the number of applicants, Column (6) weights the observations with the inverse of the total number of non-agency applicants. Results from both weighting methods remain similar. Column (7) further imposes an assumption that the outcome variables follow a binomial distribution, under which a binomial logit regression provides the most efficient estimates.[25] The estimates from the binomial logit regressions remain significantly positive.

Second, we examine whether attrition of firms affects the main results systematically. Table C9, Column (1) regresses attrition of firms on the treatment status. Although on average more than 98% of firms are successfully followed up, treated firms have a slightly higher attrition rate by 2.4 percentage points (p-value 0.128). To examine whether attrition affects the main result, in Column (2) and (5), we predict attrition likelihood from the entire set of baseline characteristics, and control for the interaction of treatment status and whether the attrition likelihood is above average. The treatment effects on interview and hiring non-agency applicants remain significantly positive among firms with low attrition likelihood. In addition, we conduct sensitivity analysis in two hypothetical scenarios where no attrited firms interviewed (hired) any worker or all attrited firms interviewed (hired) at least one worker. The extreme estimates are about only 1–2 percentage points away from the main estimates, suggesting very limited influence of attrition, even if potentially endogenous to the intervention.

Third, we examine whether the main results can be explained by the strategic matching behavior of employment agencies. From qualitative interviews, employment agencies express their preferences for higher-paid jobs from which they may get a higher commission fee. It is likely that employment agencies select vacancies that may have a higher chance of hiring. We first compare the reduced-form effects of receiving agency applicants to the IV estimates using initial treatment assignment as an instrumental variable; the difference between the two estimates implies the direction of the selection bias. Table C10 conducts this exercise. Column (1) and (5) present the reduced-form estimates and show that firms receiving agency

[25]Under this assumption, when firms make interview and hiring decision, firms consider each applicant independently, and each applicant has the same probability of getting interviewed or hired. This merely serves a robustness check of the estimation efficiency. We do not use this assumption in any other analysis.

applicants are not more likely to interview or hire any non-agency workers. Column (2) and (6) present the IV estimates and show significant causal effects of receiving agency applicants. We follow Hausman's test (Hausman, 1978) and confirm the two estimates are significantly different. This suggests a *negative* selection bias: employment agencies may have targeted firms that are *less* likely to interview or hire. In Column (3) and (6), we examine whether treatment effects are different for firms with above-average reservation wage. We find negative, although insignificant, heterogeneous treatment effects regarding reservation wage, confirming that the potential strategic matching regarding salary does not drive the main results. We conduct another exercise where we predict the likelihood of receiving agency applicants from the employment agencies using all baseline characteristics, and examine the treatment effects on firms with below-average likelihood. Column (4) and (8) show that if anything, firms with low likelihood of receiving agency applicants are less likely to interview or hire non-agency workers, instead of driving the main hiring patterns.

Fourth, we examine whether demand effect explains the main hiring patterns. It is likely that in response to the intervention, treated firms may provide one out of several vacancies that may benefit the most from the employment agencies, which may explain the imbalance regarding vacancy characteristics in Table C3. In Table C11, Column (1) and (3), we find that treatment effects are smaller among firms with more than one vacancy at the same time, certainly not driving the main empirical patterns. Another possibility is that treated firms may hope to engage less with the survey team to decrease hassle from employment agencies. From the discussion with the survey team, when the respondent is the owner of the firm, this situation is more likely to happen due to less time availability. In Column (2) and (4), we find that treatment effect diminishes among firms where respondents are the owners, suggesting that if anything, firms that wish to engage less do not interview or hire more non-agency workers.

Fifth, the interpretation of main result might differ if there is a spillover effect to non-treated firms. To examine potential within-cluster spillover, we leverage the clustered treatment design in Round 2. Table C12, Column (1) and (4) examine whether non-treated firms (including non-eligible firms) in intensely treated areas are affected by the treatment regarding the interview and hiring outcomes, controlling for local district fixed effects. We find that non-treated firms are slightly less likely to interview or hire in intensely treated areas, but not significantly. Column (2) and (5) examines whether the treatment effects differ in intensely treated areas. Although the estimates are less precise, we do not find such heterogeneous treatment effects, suggesting that within-cluster spillover does not affect the interpretation of our main results.

We further look at whether the spillover effects extend beyond clusters. Within each business area, firms in different locations may be subject to different levels of spillover from outside of the cluster. Using the geo-coordinates of firms, we compute the percentage of treated firms within a given radius, excluding firms in the same business area. Table C12, Column (3) and (6) examine whether the treatment effects are stronger among firms with above-average beyond-cluster treatment intensity within two-kilometer radius; we do not find supportive evidence of such spillover. Figure B8 further varies the length of radius and replicates this exercise. We do not find differential treatment effects in any specification.

5 Learning Mechanism

From Section 4, we find that treated firms conduct hiring decisions faster but do not hire more workers provided by the employment agencies, which cannot be explained simply by the decrease in search frictions. In this section, we examine our hypothesis that employment agencies induce learning by allowing firms to observe more college educated applicants.

Table C13 shows the treatment effect on the number of college educated applicants by endline. On average, treated firms receive 0.329 more college educated applicants, a 29%

5.1 Update on College Graduates' Productivity

Do treated firms update beliefs about the productivity of college graduates? We conduct the following two data collection exercises on firms' beliefs. First, in the endline survey, we ask all firms whether they think college graduates are more productive compared to non-college educated workers in general. Table 4, Column (1) shows that treated firms are 8.67 percentage points less likely to consider college graduates as more productive in general, a 11.1% decrease compared to control mean (p-value = 0.0505). Column (2) breaks down the effect by whether firms request a college graduate at baseline. We observe a larger treatment effect among firms that request a college graduate at baseline (p-value 0.0852), consistent with the fact that these firms receive more college educated applicants from employment agencies. For those who do not request a college graduate at baseline, we observe similar decrease in the perception with lower level of significance (p-value 0.150), possibly because these firms also receive more college educated applicants from the employment agencies, although less significantly.

One may worry if the previous perception question is subject to different reference groups, that is, firms may interpret "general" college graduates in different contexts. In Round 2 midline, we directly elicit firms' perceptions of each applicant's productivity. For each firm, we compute the percentage of non-agency college educated applicants considered with good productivity, a similar metric of firms' perception with a clearly defined reference group.[26] Table 4, Column (3) shows that among treated firms, college graduates are 32.1 percentage points less likely to be considered with good productivity, a 41.6% decrease compared to control firms. Column (4) further shows that such decrease is more significant among firms requesting a college graduate at baseline, less so among those not requesting a college graduate. Figure E2 replicates the results using different samples and treatment status and finds robust estimates. We thus establish that treated firms update negatively on college graduates' productivity after receiving extra college educated applicants from the

[26] For each applicant, we ask the employer, "How productive do you think this applicant would be if hired on the job, very productive, somewhat productive, somewhat not productive, not productive at all?" In the main analysis, an applicant is considered productive if the employer answers "very productive" or "somewhat productive". One caveat is that such metric can be only computed among firms receiving at least one college educated applicant, and employment agencies introduce more college educated applicants to treated firms. Given that Table C13 suggests treated and control firms are balanced in the total number of non-agency college educated applicants, we exclude college educated applicants provided by the employment agencies when computing the metric so it is less subject to such selection bias.

employment agencies.

5.2 Conceptual Framework

We outline a simple model to formalize how employment agencies may affect beliefs of college graduates' productivity through lower search frictions, and generate testable predictions on firms' hiring behavior.

Suppose in a one-period model, firm j opens a vacancy for one worker. Firm j's production function is $\theta_{ij} = \mu_i \theta_j$, θ_j is a firm-specific parameter following a given distribution, and μ_i is the productivity of the matched worker. There are two types of workers in the market: Non-college educated workers with productivity $\mu_i = \mu$, and college graduates with productivity $\mu_i = \mu + a_i$, where a_i is the college premium drawn from a given distribution with mean $a_0 > 0$. Firms observe types perfectly but face the uncertainty of the college premium; denote firm j's belief of average college premium as $\widetilde{a}_j$.[27]

Firm j decides to search for one worker for the vacancy. For non-college educated workers, firm j pays zero search cost. For college graduates, firm j pays a search cost $c(q)$ up front, a decreasing function of arrival rate q.[28] Once the search cost is paid, firm j matches with a college graduate and observe her true productivity μ_i. We further assume that firm j and worker i engage in Nash bargaining and determine the wage $w_{ij} = \beta \mu_i \theta_j$; worker i always takes up the offer.[29]

Firm j calculates whether it is more profitable to search for a college graduate or a non-college educated worker. Firm j compares the search cost $c(q)$ and the net benefit of hiring a college graduate versus a non-college educated worker, which depends on firm j's perception of the average college premium $\widetilde{a}_j$. Appendix D shows that from a large class of learning

[27] One can impose that firm j has a prior of college premium that follows a certain distribution $a \sim F_j(\cdot|I_j)$, where I_j is the set of college graduates that firm j observes in the past, and the mean is $\widetilde{a}_j = \mathbb{E}_j[a|I_j]$.

[28] The search cost can be micro-founded in a simplified Diamond-Mortensen-Pissarides model. Specifically, assume the cost of opening vacancy is k. The Bellman equation of opening a vacancy is $rV = -k + q(J - V)$, where q is the match rate between firms and workers, J is the value of filled position, and V is the value of vacancy. Assuming free entry in the equilibrium and setting $V = 0$, one gets $J = k/q$. One may interpret k/q as the search cost in our model $c(q)$: Firm needs to wait $1/q$ periods to match with a worker, and each period firm needs to pay k to keep the position open. In the equilibrium, the value of filled position equals search cost, although in our simple model we do not require the equilibrium condition.

[29] In general, as long as workers are not the sole claimer of the college premium, all the following predictions follow. We assume wage bargaining because the solution is much simpler, and that more than 70% of firms in our sample engage in wage bargaining after the offer is made.

models, $\widetilde{a}_j$ can be a function of arrival rate q, with the intuition that as firm j has a higher likelihood of interacting with college graduates, firm j observes more signals of the college premium. We thus have the following condition:

$$(1-\beta)\widetilde{a}_j(q)\theta_j \geq c(q) \tag{2}$$

Essentially, by creating a new applicant pool consisting of mainly college graduates, employment agencies are able to lower search frictions and increase the arrival rate of college graduates q. Suppose there is no uncertainty of the college premium, *i.e.*, $\widetilde{a}_j \equiv a_0$. Firms with $\theta_j \geq c(q)/[(1-\beta)a_0]$ would choose to search for a college graduate and eventually hire one. Firms below the threshold would instead hire a non-college educated worker. When employment agencies reduce the search cost c by increasing the arrival rate q, we should see *more* firms hire college graduates and *fewer* firms hire non-college educated workers.

Suppose now firms are over-optimistic of the average college premium, *i.e.*, $\widetilde{a}_j > a_0$. If agencies also induce firms to obtain information of college graduates' productivity, we may observe *fewer* firms hire a college graduate and *more* firms hire a non-college educated worker if $\widetilde{a}_j(q)$ decreases sufficiently. The following proposition summarizes this intuition.

Proposition 5.1. *Suppose firm j has an over-optimistic belief of average college premium $\widetilde{a}_j > a_0$. Define the decreases in c and $\widetilde{a}_j$ due to employment agencies as Δc and $\Delta\widetilde{a}_j$. Firm j is less likely to hire a college graduate if $|\Delta\widetilde{a}_j/\widetilde{a}_j| > |\Delta c/c|$.*

Based on Proposition 5.1, we can characterize complier firms that switch their hiring preferences due to the new search technology. Suppose $|\Delta\widetilde{a}_j/\widetilde{a}_j| > |\Delta c/c|$. For firms that would have hired a college graduate absent employment agencies, given a sufficient decrease in the perceived average college premium, some firms would stop hiring a college graduate because the net benefit of hiring a college graduate drops below the search cost. For firms that would not have hired a college graduate, hiring a college graduate is already less profitable than a non-college educated worker, and thus we should not expect to see any changes in their hiring behavior if employment agencies further lower the beliefs of the average college premium. Therefore, we have the following two predictions if $|\Delta\widetilde{a}_j/\widetilde{a}_j| > |\Delta c/c|$:

Prediction 1. For firms that request a college graduate at baseline, firms matched with an employment agency are *less* likely to hire a college graduate and *more* likely to hire a non-college educated worker.

Prediction 2. For firms that do not request a college graduate at baseline, employment agencies have no effects on hiring behavior.

Another common feature of a learning model is the heterogeneous effects regarding past exposure. With an additional assumption regarding the learning models outlined in Appendix D, firms with more exposure to college graduates in the past would not benefit much from observing an extra college graduate. For firms with less exposure to college graduates, however, matching with an extra college graduate may lead to larger update on beliefs and more significant shift in hiring preferences. Combining the implication from Prediction 1, we have a third prediction:

Prediction 3. For firms that request a college graduate at baseline, employment agencies have stronger effects on those with initially less exposure to college graduates.

5.3 Hiring of College Graduates

We now examine the effects of employment agencies on the hiring of college graduates and non-college educated workers, with a particular focus on the heterogeneity regarding baseline request for college graduates, as a test for Predictions 1 and 2. We use endline hiring outcomes for the analysis hereafter.

Table 5, Panel A first presents the ITT effects on hiring a college graduate or a non-college worker. Column (1) and (2) show that on average, treated firms are less likely to interview any college graduates and more likely to interview any non-college educated workers by endline, although both estimates are not significant. Column (3) shows the two estimates are not significantly different. Column (4) to (6) further show similar yet insignificant pattern on the hiring of college graduates and non-college educated workers. The average ITT effects, however, are potentially masked by heterogeneity. As discussed in Section 5.2, only firms that request a college graduate at baseline would shift their hiring preferences given a sufficient decrease in the belief of college graduates' productivity.

We test the heterogeneous treatment effects regarding baseline request in Table 5, Panel B. Among firms that request a college graduate at baseline, we observe drastic shift in hiring behavior. Treated firms are 16.4 percentage points less likely to interview any college graduate (p-value 0.024), a 27.3% decrease compared to control firms requesting a college graduate; Instead, they are 9.7 percentage points more likely to interview at least one non-college educated worker (p-value 0.070), almost double compared to control firms requesting college graduates among which only 11.7% interview any non-college educated worker. The difference between the two estimates is statistically significant (p-value 0.011). Similarly, compared to control firms requesting a college graduate, treated firms requesting a college graduate are 19.5 percentage points less likely to hire any college graduates (p-value 0.008, 33.7% decrease), and 10.5 percentage points more likely to hire at least one non-college educated worker (p-value 0.049, 109% increase); the difference between the two estimates is statistically significant (p-value 0.004). Among firms that do not request a college graduate at baseline, however, we do not observe any meaningful treatment effects on any interview or hiring outcomes. This is unlikely to be explained by the lack of statistical power, as the majority (65%) of firms do not request a college graduate at baseline. These findings are thus consistent with Predictions 1 and 2 where employment agencies sufficiently reduce firms' beliefs of college graduates' productivity. Figure E3 replicates the results using different samples and treatment status and finds robust estimates.

One can also examine the job descriptions of the posted vacancies to understand whether it is optimal to request a college graduate at baseline for some of the positions. For example, a local car dealership in our sample is hiring a receptionist and requires applicants to have a Bachelor degree. A local garment company is hiring a tailor with a minimum requirement of college diploma and initially only agrees to pay up to 2,000 ETB per month (about 40 USD, the median monthly salary in our sample is 3,000 ETB). In fact, 39% of the jobs that request a college graduate involve mostly routine tasks, 29% involve manual tasks, and 9% are not considered involving skilled tasks. One can imagine that non-college educated workers can compete, and excel, in some of these positions, yet might be neglected by firms that screen out non-college educated workers at the first place. In Table C14, we further break down the treatment effects by types of tasks. We observe the most salient shift in hiring preferences among treated firms that request a college graduate and whose job descriptions feature non-skilled, routine, and manual tasks, consistent with our qualitative observations that college degrees may not be necessary for some of the less-skilled positions.

5.4 Heterogeneity by Exposure to College Graduates

We now examine the third prediction from Section 5.2. For firms with less exposure to college graduates, an extra college educated applicant from the employment agencies may lead to larger updates in beliefs, hence larger treatment effects on hiring outcomes especially among those requesting college graduates at baseline.

We use the percentage of current employees with a college diploma or degree, or college share, as the main proxy for exposure to college graduates. We first verify that lower college share is correlated with larger updates on the beliefs of college graduates' productivity. Table C15 shows that indeed, treatment effects on firms' beliefs of college graduates' productivity are stronger and more significant among firms with below-median college shares, suggesting that college share can be a valid proxy for exposure to college graduates.

We then examine the heterogeneous effects on hiring outcomes and only focus on firms requesting a college graduate at baseline. We first show the bin-scatter plots in Figure 6, Panel A between the college share and the percentage of firms hiring at least one college graduate. Treated firms with lower college shares are less likely to hire any college graduates compared to control firms. Panel B further shows that treated firms with lower college shares are instead more likely to hire at least one non-college educated worker compared to control firms. Such differences disappear as the college share increases.

We replicate this exercise in Table 6 for firms requesting a college graduate at baseline. Compared to control firms with below-median college shares, treated firms with below-median college shares are 23.6 percentage points less likely to interview any college graduates (p-value 0.070, 40.1% decrease), 13.0 percentage points more likely to interview at least one non-college educated worker (p-value 0.147, 106% increase), and the difference between the two estimates is significant (p-value 0.039). The effects on hiring outcomes show very similar pattern: Compared to control firms with above-median college share, treated firms with below-median college shares are 24.6 percentage points less likely to hire any college graduates (p-value 0.041, 42.8% decrease), 12.4 percentage points more likely to hire at least one non-college educated worker (p-value 0.167, 113% increase), and the difference between the two estimates is significant (p-value 0.030). For firms with above-median college share, we do not observe treatment effects on any interviewing or hiring outcomes, consistent with the interpretation that firms with above-median college share have more exposure to college graduates and respond less to the treatment. Results are very similar if we choose different cutoffs of college share.[30] Figure E4 replicates the results using different samples and treatment status and finds robust estimates.

We further examine the heterogeneous treatment effects using other proxies for the exposure to college graduates. Table C16 replicates the results using two different proxies: total number of current employees with a college diploma or degree, and whether firms receive at least one non-agency college educated applicant from other hiring channels. Although less distinctive, we observe more salient shift in hiring preferences among firms with below-median number of college employees, and firms with zero non-agency college educated applicant. We thus provide supportive evidence of the third prediction: Treated firms with less exposure to college graduates are more likely to shift their hiring preferences from college graduates towards non-college educated workers.

5.5 Descriptives of College Educated Applicants

What signals do firms observe from the extra college educated applicants that lower their beliefs of the productivity of college graduates? We are not able to provide causal evidence because the selection of workers by employment agencies is not random. In this subsection, we provide qualitative description of how college graduates may look different from non-college educated workers regarding experience that firms can observe before interviews, as well as other characteristics that firms potentially observe during interviews.

From our qualitative discussions with firms, the most important factor they consider before the interview stage is past experience. In particular, firms care more about the relevance of past experience than years of experience. Table C17, Panel A compares college graduates and non-college educated workers who apply to the same job, cluster at the firm level, controlling for estimated years after graduation and gender presumably also observed by firms before the interview stage. We find that controlling for the years after graduation and gender, college graduates have 2.6 more years of experience and are more likely to have at least two years of experience, but they do not have more relevant experience for the position, suggesting firms may not necessarily consider college graduates to be more productive.

[30] In Figure B9, we replicate the results on hiring a college graduate or a non-college educated worker among firms that request a college graduate at baseline using different cutoffs of college share (50–90 percentile). The patterns remain largely similar regardless of which percentile is selected as cutoff.

We further compare college graduates and non-college educated workers regarding characteristics potentially observed during the interview. During the worker phone survey, we collect information of the education level of workers' fathers as a proxy of family background, as well as workers' outside offers. Among applicants who attended interviews, we do not find significant differences regarding fathers' education, number of outside offers, or whether any outside offer pays a higher salary. Results suggest that college graduates may not differ much from non-college educated workers even if more information is revealed after the interview.

One may wonder if employers obtain other signals that are not captured by the previous measures, for instance, workers' motivations. We are able to compare employers' perceptions of the productivity between college educated and non-college educated applicants using Round 2 midline data. Table C17, Panel B show that employers perceive college educated applicants to be equally productive as non-college educated applicants, suggesting employers do not obtain signals in favor of college graduates. We further conduct an exercise where we predict employers' perceptions of workers' productivity using all the measures above except education, generate a productivity score for each worker, and compare the average scores between college educated and non-college educated applicants. We do not find significant difference regarding the productivity scores.[31]

We thus present qualitative evidence suggesting that firms might not observe signals of high college premium from the college educated applicants. For firms with previously positive beliefs of college premium, observing more college educated applicants from the employment agencies may thus have a negative impact on college graduates' productivity.

5.6 Alternative Mechanisms

We formally discuss four alternative hypotheses that may explain the main empirical findings. First, one may wonder if college graduates are more likely to reject the offers than non-college educated workers. This hypothesis would not affect the effects on whether firms make any interview invite, but if college graduates are less likely to attend the interview, firms may be less likely to hire college graduates as a result. We are able to observe whether each applicant rejects an interview invite or an offer to test this hypothesis; Table C18 shows

[31] We also do not find meaningful differences regarding all measures by whether the college educated applicants are provided by the employment agencies, which rules out a possibility that employment agencies negatively select job applicants.

the results. On average, only 4.7% applicants reject the interview invite, 3.0% reject the offer. We do not find evidence suggesting college graduates are more likely to reject the interview invites or the offers within the same firm.

Second, we examine whether other hypotheses of search cost and benefit may explain the main findings. Suppose firms choose to stop searching when the marginal benefit of having one more applicant is equal to the marginal cost. When employment agencies provide more applicants to treated firms, the marginal benefit of having one more applicant may decrease, thus speeding up the hiring process. This hypothesis may be able to explain the results on faster hiring, but cannot explain why treated firms switch to hiring non-college educated workers, especially when employment agencies provide mostly college graduates. We also rule out another possibility that employment agencies may disproportionately lower the search cost of finding non-college educated workers, as we do not see significant difference in the number of non-college educated applicants in Table C13. One potential alternative mechanism is that when employment agencies are not able to find a match, firms may obtain a signal of high search cost of finding college graduates and stop the search earlier. We already show in Table C10 that treated firms with low likelihood of receiving agency applicants are not more likely to interview or hire any non-agency applicants by midline. We further examine the heterogeneous effects on interviewing and hiring college graduates by the likelihood of receiving agency applicants in Table C20. Among firms that request a college graduate at baseline, firms with low likelihood of receiving extra applicants are not significantly less likely to hire a college graduate or more likely to hire a non-college educated worker, suggesting such a hypothesis on search cost does not drive the empirical patterns.

Third, one may wonder if treated firms hire non-college educated workers because they observe other negative signals from college educated applicants. For example, if a firm posts a position in a certain occupation that does not usually see college educated applicants, the firm may interpret college educated applicants as negatively selected. This explanation is at odds with our findings where the treatment effects are the strongest among firms that request a college graduate at baseline, as they actually expect college graduates to apply. Our previous findings in Section 5.5 also suggest that firms do not perceive college graduates to be less productive than non-college educated workers applying to the same position.

Last, one may impose a different assumption on firm's hiring behavior: firms may resort to employment agencies in the future to find a replacement for the current position, and as a result they can afford to make a sub-optimal decision now. In this hypothesis, we interpret the faster decision making and the decreased hiring of college graduates as a deliberate "error" because making an optimal hiring decision is costly. We find such hypothesis difficult to explain why the treatment effects concentrate among firms that request a college graduate at baseline as these firms are not inherently more prone to sub-optimal decision making. We further ask firms at endline what hiring channels they plan to search for workers in the future. If the hypothesis of lower future replacement cost holds true, treated firms should prefer to continue using the cheaper search technology, *i.e.*, employment agencies. Table C19 shows that treated firms are only slightly more likely to plan to use employment agencies and less likely to use other formal hiring channels in the future; none of the effects is statistically significant. We thus fail to provide substantial evidence to believe that firms' sub-optimal decision making drives the main findings.

6 Cost-Benefit Analysis

Do employment agencies affect firms' profit by switching their hiring preferences towards non-college educated workers? We are not able to answer this question by directly measuring firms' profit, both because profit is a sensitive question in Ethiopia and because employment agencies only affect hiring decisions for one position, and thus the effects may not manifest in the total firm profit. In this section, we discuss the effects on agencies on salary and match quality separately to provide an estimate of the treatment effect on profit.

6.1 Salary

We first apply the same specification in Equation 1 to estimate the treatment effect on monthly salary among firms requesting a college graduate at baseline. Table C21, Column (1) and (2) show that treated firms seem to increase salary by around 15 USD per month, but the difference is not significant. This estimate, however, potentially combines three different effects. First, we only observe salary for firms that hire at least one person by endline. We are not particularly concerned with this potential selection bias, however, because we do not observe significant treatment effect on the match success rate by endline in Table C7. Second, for firms that do not change their hiring behavior, employment agencies may also affect firms' beliefs of workers' productivity and thus affect the salary, an intensive margin of the treatment effect. Third, firms that switch their hiring preferences may generate a compositional effect if the salaries paid for college graduates and non-college educated workers are significantly different.

We are most interested in the third component, that is, for firms that comply to the intervention and switch from hiring a college graduate to a non-college educated worker (henceforth compliers), whether they pay different salaries for hired workers. We first describe the average monthly salary paid to college graduates and non-college educated workers. Among firms that hire a college graduate, treated firms pay 102 USD per month on average. Among firms that hire a non-college educated worker, treated firms pay 61 USD per month on average, 41 USD lower than that of hiring a college graduate, which implies a salary ladder regarding educational attainment. Figure B10 further shows that such a salary ladder is not altered by the intervention. Treated and control firms pay similar salaries for non-college educated workers (63 USD vs. 58 USD). For college graduates, treated firms pay slightly higher salary (112 USD vs. 95 USD, p-value 0.139), but such difference does not stay significant when controlling for baseline characteristics or accounting for potential selection bias.[32] Therefore, when complier firms switch to hiring a non-college educated worker, they may take advantage of the salary ladder and lower the monthly salary for hired workers.

We further provide descriptive evidence of such a salary decrease for complier firms, using the framework of local-average treatment effects (LATE) from Angrist and Imbens (1995) and the technique of estimating potential outcomes of compliers from Abadie (2003). The endogenous variables are whether firms hire a college graduate or a non-college educated worker. We use the interaction of initial treatment assignment and whether firms request a college graduate at baseline as the instrumental variable. Table C22, Column (1) shows that the average salary for complier firms is 124.1 USD before the treatment when they would have hired a college graduate, but the salary drops down to 55.4 USD when they switch to hiring a non-college educated worker after the treatment, a 55.4% decrease. Our findings thus suggest that complier firms pay a lower salary because of hiring a non-college educated

[32]Table C21, Column (3) and (6) show the raw salary comparison between treated and control firms that hire a college graduate and a non-college educated worker, respectively. Column (4) and (7) include all baseline characteristics and do not find significant effects. In Column (5) and (8), we further compute Lee bounds following Lee (2009) to account for potential selection bias of observing salary for college graduates or non-college educated workers. None of the estimates of Lee bounds are significantly distinctive from zero. The results of Lee bounds also indicate the lack of intensive margins of treatment effects on salary.

worker.[33]

6.2 Match Quality

We collect three sets of data in endline to measure the match quality of the hired workers. (1) Turnover: whether the hired workers voluntarily quit or get fired by the firm. (2) Performance: we first directly ask firms whether the hired workers perform better than average workers on the similar positions in the same firm. We then collect the performance records of the hired workers in the last month, as well as the performance record of another 1–3 workers on the similar positions in the same firm, and measure whether the hired workers have better performance records than the other similar workers.[34] (3) Effort: we measure whether the hired workers have any absent day in the last 30 days, and whether the hired workers perform any overtime hours in the last 7 days. Similar to the discussion on salary, given that we do not observe treatment effect on the match success rate by endline, we simply show the ITT effects on the match quality among the 179 eligible firms that request a college graduate at baseline and fill the positions by endline.

Table 7 presents the results. Column (1) shows that hired workers in treated firms are not more likely to quit the job voluntarily. Column (2) shows that treated firms are no more likely to fire the new hires. These two estimates suggest that hired workers in treated firms are equally likely to remain on the job at least by endline. Column (3) shows that treated firms are equally likely to perceive hired workers with above-average productivity. Column (4) replaces the outcome with whether hired workers have higher performance record than average workers on the similar positions and finds no treatment effect as well. Column (5) shows no significant treatment effect on the likelihood of absenteeism. Column (6) suggests that hired workers in treated firms are no more likely to work overtime. We thus do not find substantial treatment effects on any of the measures of the match quality. Figure E5 replicates the results using different samples and treatment status and finds robust estimates.

[33] We exclude salary above 95 percentile to estimate the potential outcomes. The estimates on pre-treatment potential outcomes are as high as 212 USD if not excluding outliers, but the estimates on post-treatment potential outcomes are not subject to outliers.

[34] About 95% firms in our sample use "efficiency" to measure performance, that is, the percentage of targeted production met in the last month. The average efficiency measure is 78.8% in our sample. By comparing to other similar workers in the same firm, this measure is less subject to different occupations or how firms set the production targets within firm.

We further conduct complier analysis on all the six measures of match quality in Table C22, Column (2) to (7). We find no difference on compliers' potential outcomes before and after the treatment, further confirming no treatment effect on match quality among compliers.

Last, we conduct a simple accounting exercise to understand the effect on profit for complier firms that shift towards hiring non-college educated workers. On the costs, treated firms are more likely to make hiring decisions by midline and reduce search cost; we also find suggestive evidence of lower salary for complier firms. On the revenue, treated firms are equally likely to fill the position by endline, with no treatment effects on turnover and match quality among complier firms, suggesting no substantial decrease in revenue. This is potentially surprising given that treated firms hire more non-college educated workers who are presumably less productive than college graduates. Our findings on the heterogeneous treatment effects by tasks in Table C14 significant treatment effects on the match quality but potential decrease in salary for complier firms who switch from hiring a college graduate to a non-college educated worker, suggesting a net increase in firm profit for complier firms.

We thus provide evidence that existing labor market intermediaries can alleviate the cost of learning through lower search frictions. In many cases, treated firms do not interview college graduates but simply read their application materials to infer their potential productivity, suggesting that it may not be as costly to increase the exposure of firms to the labor market. In a broader sense, this paper echoes with Li et al. (2023) who emphasize the benefit of exploring workers in categories such as minority workers with whom employers are less familiar. We show that some labor market intermediaries may help lower the cost of exploration, eventually to the benefit of employers.

We do not discuss whether it is in the best interest of employment agencies to continue the strategy of supplying college graduates. One may conclude that this strategy is not profitable for employment agencies especially when firms correct the perceptions of college graduates' productivity and stop hiring college graduates. This reasoning is, however, incomplete because employment agencies can provide other essential value-added to firms, such as providing additional grading and training to workers. We observe one particular employment agency in Addis Ababa specializing in providing skilled workers to healthcare facilities, along with a full assessment of workers' qualifications and basic training for certain occupations in healthcare sector. We believe that our findings do not necessarily belittle the necessity of employment agencies, but point out the potential decreasing profit margin if employment agencies only facilitate matching without providing other more essential functions, such as enhancing the signals of workers' productivity or providing skill training to workers.

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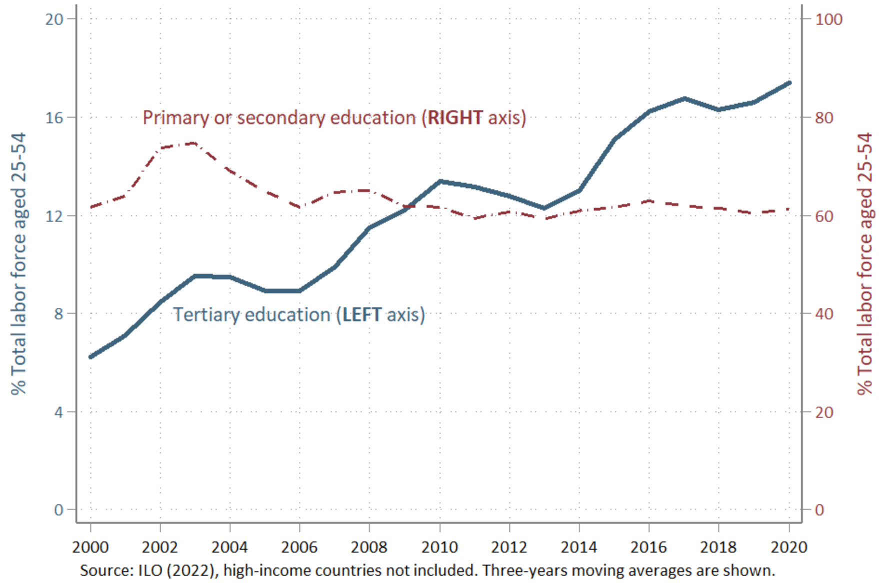
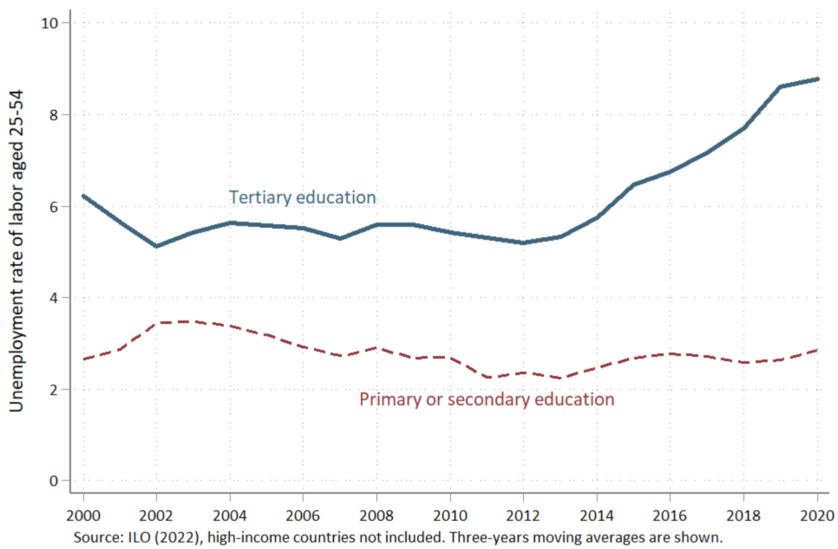
FIGURES

Figure 1: Tertiary Education in Low- and Middle-Income Countries, 2000–20

Panel A. Percentage of tertiary educated workers, aged 25–54

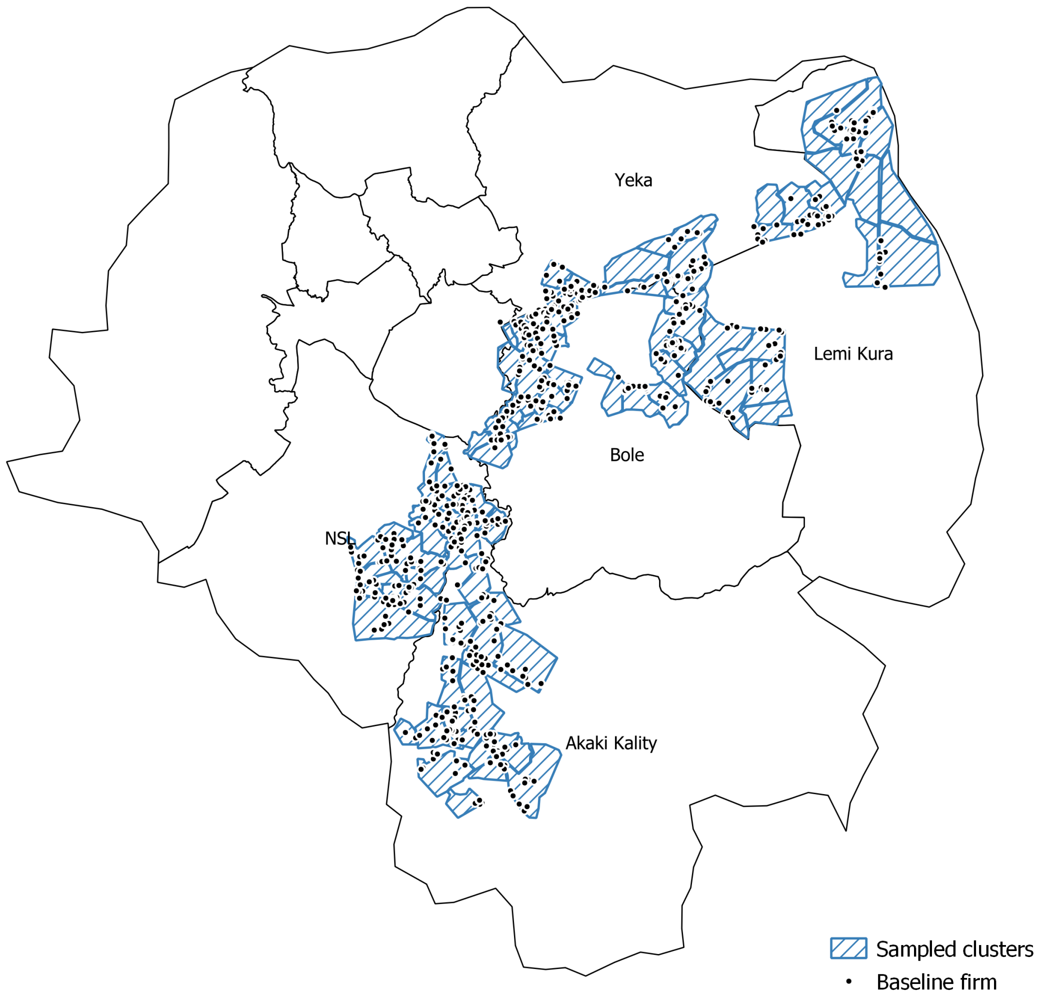


Panel B. Unemployment of tertiary educated workers, aged 25–54



Notes: This figure shows the time series of the percentages of labor aged 25–54 with tertiary education and unemployment rates in low- and middle-income countries, following the classification by World Bank. The labor force and unemployment data are from International Labor Organization database. We compute the three-year moving averages of yearly unemployment rates weighted by the total labor force aged 15–54 in the same year. Blue solid line shows the time series of labor with tertiary education. Red dashed line shows the time series of labor with non-tertiary education.

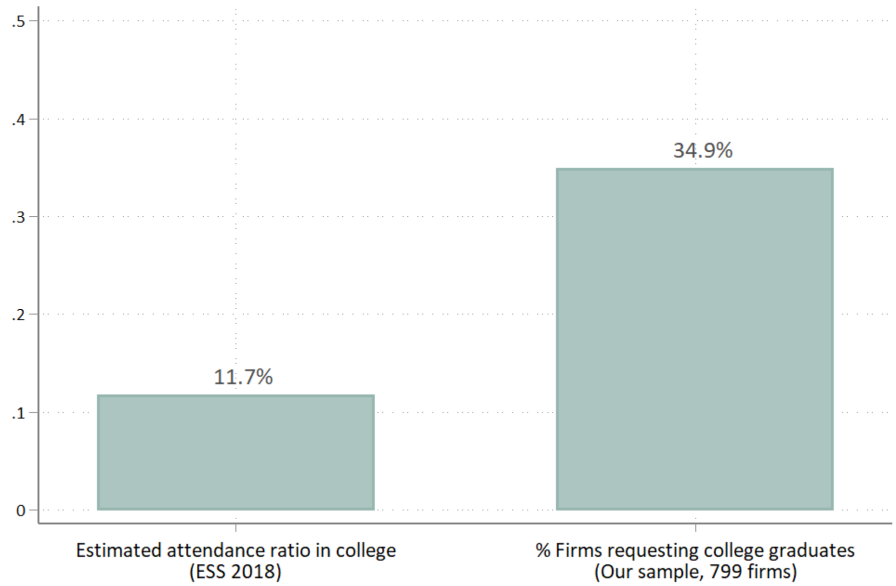
Figure 2: Sampling Map



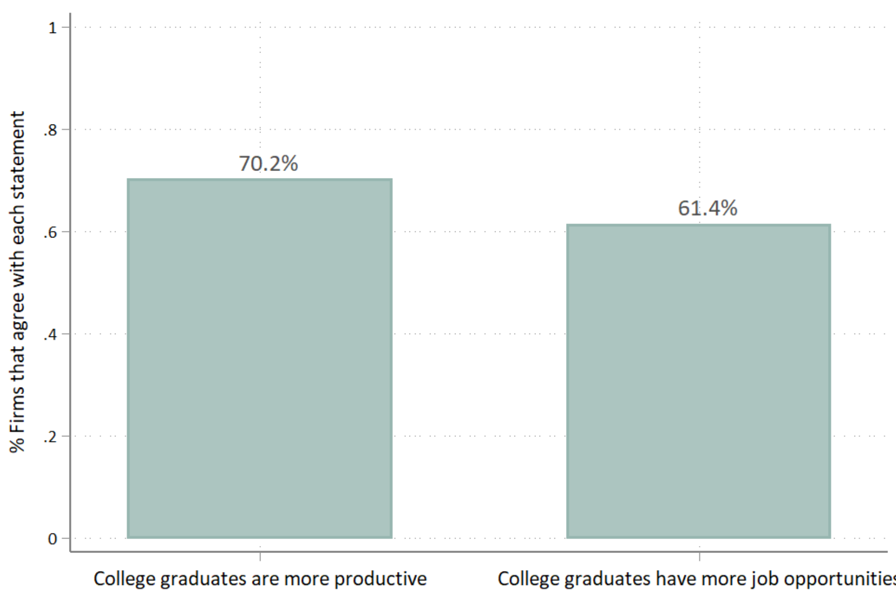
Notes: This figure shows the geographical distribution of 88 business areas from five sub-cities and 799 firms selected in the baseline survey.

Figure 3: Demand for College Graduates

Panel A. Percentage of firms requesting a college graduate



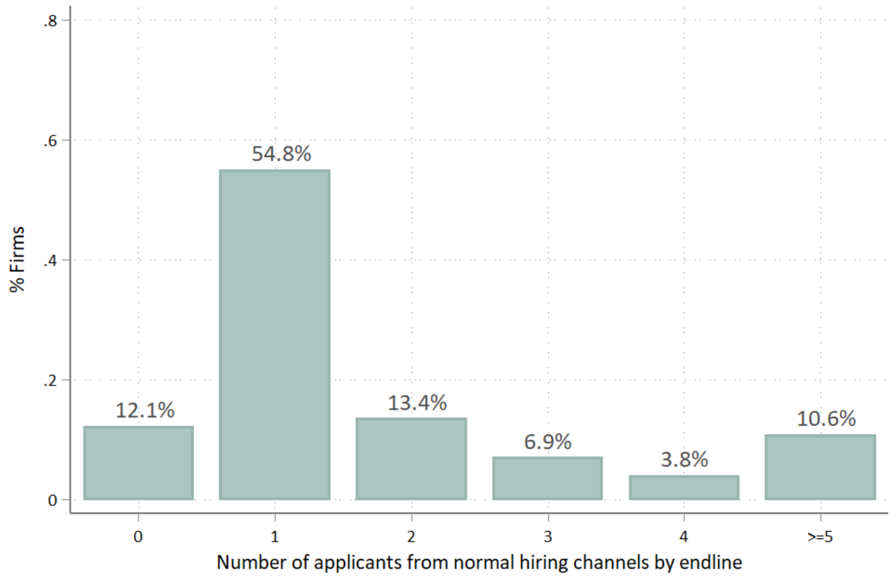
Panel B. Perceptions of college graduates



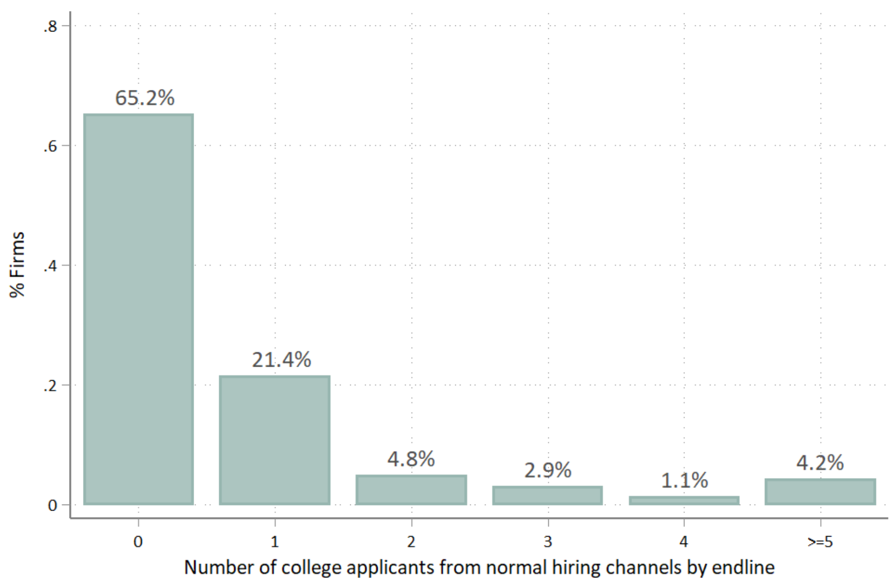
Notes: This figure presents firms' demand for college graduates. Panel A shows the estimated attendance ratio of tertiary education from Ethiopian Socioeconomic Survey in 2018, as a proxy for the percentage of labor force with a college degree, and the percentage of firms that request a college graduate at baseline in our sample. Panel B shows the percentage of firms that agree at baseline that college graduates have better productivity than non-college educated workers, and that college graduates have more job opportunities than non-college educated workers.

Figure 4: Search Frictions

Panel A. Distribution of the total number of applicants



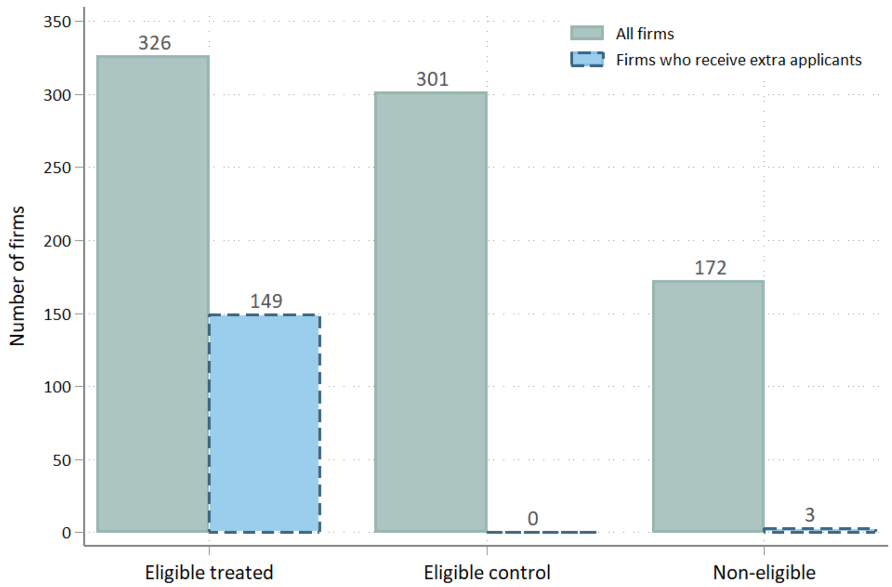
Panel B. Distribution of the total number of college educated applicants



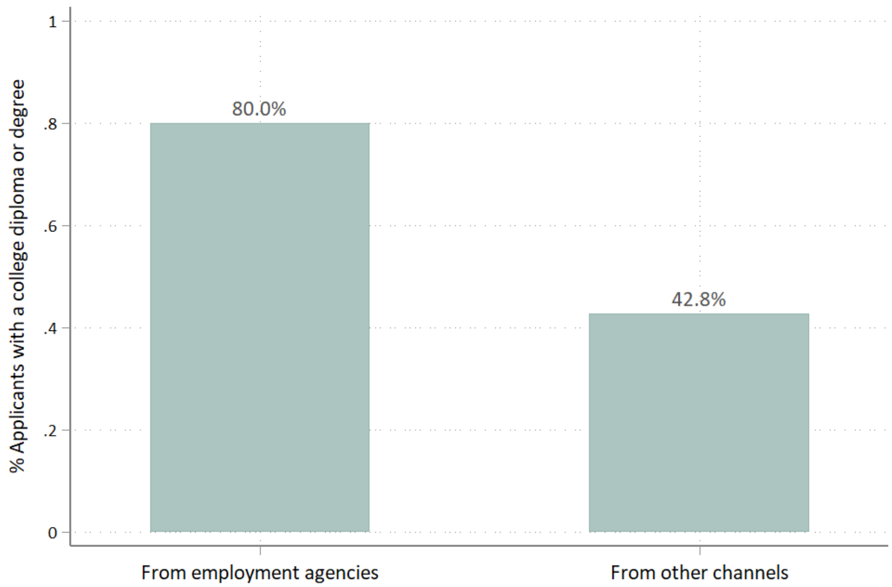
Notes: This figure shows the extent of search frictions by presenting the distribution of the total number of applicants for the posted vacancies by endline, not including applicants from the employment agencies introduced in the intervention. Panel A: Total number of applicants. Panel B: Total number of college educated applicants.

Figure 5: Treatment Implementation

Panel A. Treatment status



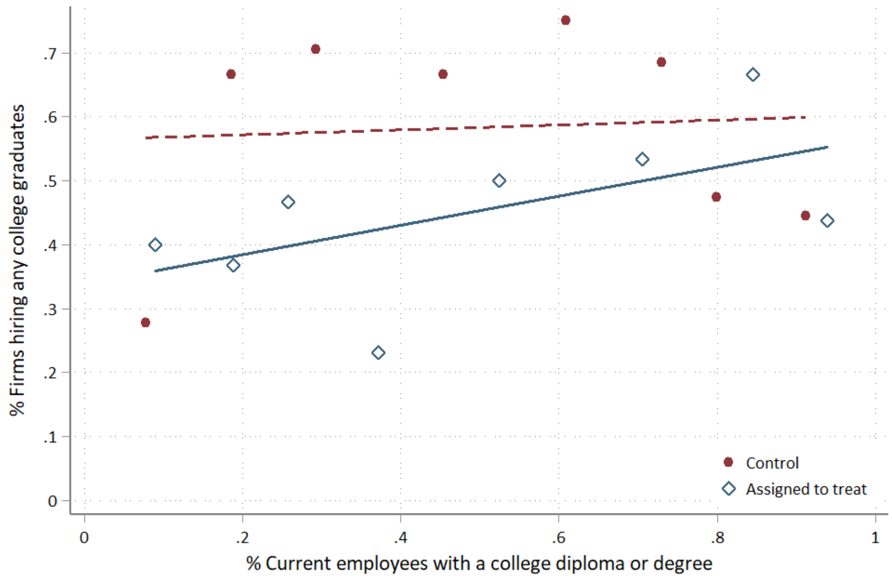
Panel B. Percentage of college educated applicants



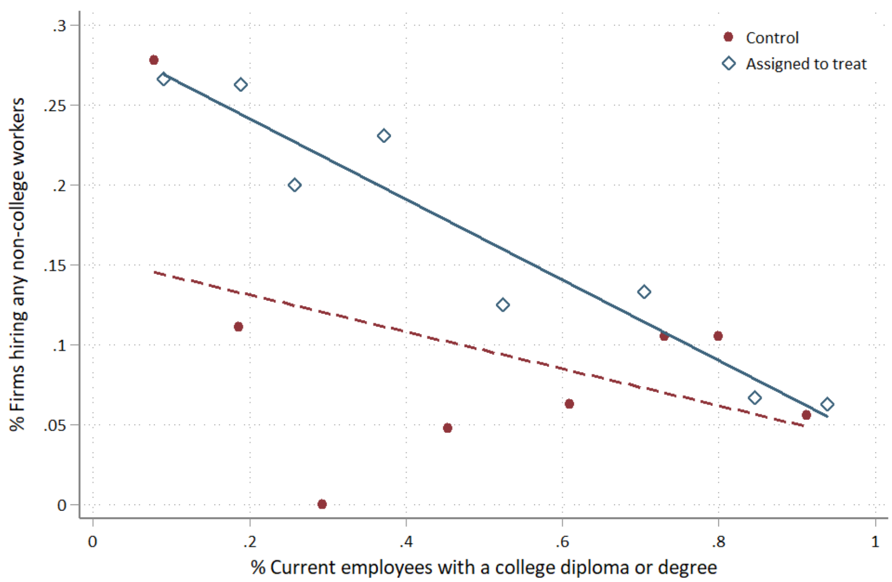
Notes: This figure shows the implementation of the treatment. Panel A shows the number of three groups of firms: (1) Eligible firms (reservation wage at least 2,000 ETB) selected into treatment group, (2) eligible firms selected into control group, (3) non-eligible firms. Panel B shows the percentages of college graduates among the applicants provided by the employment agencies and among the applicants from other hiring channels.

Figure 6: Hiring of College Graduates and Non-College Workers By College Share

Panel A. Hiring of college graduates



Panel B. Hiring of non-college educated workers



Notes: This figure presents the bin-scatter plots of the hiring of college graduates and non-college educated workers. The horizontal axis is the percentage of current employees with a college diploma or degree, a proxy for the exposure to college graduates. The vertical axis in Panel A is the percentage of firms hiring at least one college graduate; In Panel B, the percentage of firms hiring at least one non-college educated worker. Blue diamonds are firms initially assigned to treatment. Red dots are firms initially assigned to control group.

TABLES

Table 1: Balance Table

	(1)	(2)	(3)	(4)	(5)	(6)
	Mean outcomes					P-value
	All	Eligible control		Eligible treated		T−C
Observations	627	335		292		
Sector						
Manufacturing and construction	0.42	0.41	(0.49)	0.43	(0.50)	0.71
Hospitality (hotels, restaurants)	0.27	0.28	(0.45)	0.26	(0.44)	0.58
Education	0.11	0.12	(0.32)	0.11	(0.32)	0.91
Health	0.05	0.07	(0.25)	0.03	(0.18)	0.10
Current employees						
Number of current employees	66.30	57.84	(87.18)	76.00	(152.09)	0.16
Pct of female employees	0.53	0.54	(0.27)	0.52	(0.26)	0.26
Pct of employees with college diploma/degree	0.37	0.38	(0.29)	0.37	(0.29)	0.62
Pct of employees with zero exp	0.20	0.19	(0.23)	0.20	(0.24)	0.70
Pct of temporary employees	0.16	0.15	(0.27)	0.17	(0.28)	0.70
Pct of employees hired through rec	0.15	0.16	(0.22)	0.14	(0.22)	0.38
Hiring practices						
The firm has a HR department	0.51	0.50	(0.50)	0.51	(0.50)	0.77
Posting jobs on notice board	0.54	0.55	(0.50)	0.53	(0.50)	0.70
Posting jobs on newspaper	0.14	0.15	(0.35)	0.14	(0.34)	0.79
Posting jobs on online platforms	0.16	0.14	(0.35)	0.17	(0.38)	0.30
Hiring from formal employment agencies	0.08	0.07	(0.25)	0.10	(0.30)	0.19
Hiring from informal brokers	0.25	0.28	(0.45)	0.22	(0.42)	0.17
Hiring through recommendation	0.50	0.50	(0.50)	0.49	(0.50)	0.83
Posted vacancy						
Reservation wage (USD)	91.49	87.83	(61.29)	95.78	(91.71)	0.26
Requiring college diploma or degree	0.44	0.45	(0.50)	0.44	(0.50)	0.92
Requiring vocational certificate	0.08	0.07	(0.25)	0.09	(0.28)	0.32
Requiring high school degree	0.14	0.15	(0.35)	0.14	(0.34)	0.70
Requiring no experience	0.20	0.21	(0.41)	0.19	(0.39)	0.45
Requiring more than 2y experience	0.19	0.16	(0.37)	0.21	(0.41)	0.23
Skilled task	0.55	0.55	(0.50)	0.55	(0.50)	0.99
Manual task	0.64	0.65	(0.48)	0.63	(0.48)	0.55
Routine task	0.69	0.70	(0.46)	0.69	(0.46)	0.76

Notes: This table shows the balance between 292 eligible firms initially assigned to treatment and 335 eligible firms initially assigned to control group. Standard deviations are shown in parentheses. Column (6) shows the p-value of a simple comparison of each characteristics between eligible treated and eligible control firms, clustered at the level of business area.

Table 2: Effects on Interviewing and Hiring Any Applicant by Midline

Panel A. Interviewing any applicant

VARIABLES	(1) Interview	(2) Interview	(3) Interview	(4) Interview
Assigned to treat	0.142***	0.118***		0.179***
	(0.0503)	(0.0434)		(0.0506)
	[0.00590]	[0.00816]		[0.000769]
Actual treatment status			0.191***	
			(0.0651)	
			[0.00435]	
Observations	582	753	582	467
R-squared	0.293	0.241	0.127	0.332
Specification	OLS	Full sample	IV	No pilot
Control baseline char.	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes
Control mean	0.603	0.623	0.608	0.603
F-statistic			124.8	

Panel B. Hiring any applicant

VARIABLES	(1) Hire	(2) Hire	(3) Hire	(4) Hire
Assigned to treat	0.101*	0.0842*		0.135**
	(0.0517)	(0.0447)		(0.0535)
	[0.0547]	[0.0629]		[0.0139]
Actual treatment status			0.136**	
			(0.0674)	
			[0.0476]	
Observations	582	753	582	467
R-squared	0.274	0.232	0.120	0.310
Specification	OLS	Full sample	IV	No pilot
Control baseline char.	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes
Control mean	0.576	0.602	0.591	0.576
F-statistic			124.8	

Notes: This table presents the main firm-level results. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Column (1) only includes firms eligible for treatment with reservation wage at least 2,000 ETB. Column (2) includes the non-eligible firms into control group. Column (3) instruments the actual treatment status with the initial random assignment. Column (4) excludes pilot sample. Dependent variables in Panel A are whether firms interview at least one applicant by midline. Dependent variables in Panel B are whether firms hire at least one applicant by midline. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table 3: Effects on Interviewing and Hiring Agency Applicants

VARIABLES	(1) Interview Agency	(2) Interview Non-agency	(3) Diff: (2)−(1)	(4) Hire Agency	(5) Hire Non-agency	(6) Diff: (5)−(4)
Assigned to treat	0.103***	0.0976*	-0.00553	0.0307**	0.0907*	0.0600
	(0.0328)	(0.0527)	(0.0608)	(0.0134)	(0.0509)	(0.0525)
	[0.00238]	[0.0682]	[0.928]	[0.0248]	[0.0785]	[0.257]
Observations	582	582		582	582	
R-squared	0.226	0.286		0.173	0.281	
Control baseline char.	Yes	Yes		Yes	Yes	
Business area FE	Yes	Yes		Yes	Yes	
Cluster at business area	Yes	Yes		Yes	Yes	
Control mean	0.0242	0.592		0.00303	0.573	

Notes: This table presents the treatment effects on interviewing or hiring (non-)agency applicants. The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Dependent variables in Column (1) and (4) are whether firms interview or hire at least one agency applicant by midline. Dependent variables in Column (2) and (5) are whether firms interview or hire at least one non-agency applicant by midline. Column (3) and (6) compute the differences between the two estimates. The control means in Column (1) and (4) are not exactly zero because of the imperfect compliance when using initial treatment assignment to obtain causal inference. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table 4: Effects on the Perceptions of College Graduates' Productivity

	(1)	(2)	(3)	(4)
VARIABLES	Endline: Whether firm agrees that college graduates have better prod		Midline: % College applicants perceived with good prod	
Assigned to treat	-0.0867*		-0.260*	
	(0.0437)		(0.135)	
	[0.0505]		[0.0632]	
Assigned to treat X Requesting college		-0.0932*		-0.302**
		(0.0535)		(0.145)
		[0.0852]		[0.0450]
Assigned to treat X Not requesting college		-0.0823		-0.162
		(0.0566)		(0.202)
		[0.150]		[0.430]
Observations	568	568	106	106
R-squared	0.329	0.329	0.595	0.599
Control baseline char.	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes
Control mean	0.782		0.770	
Control mean: Requesting college		0.897		0.766
Control mean: Not requesting college		0.720		0.746

Notes: This table presents the treatment effects on the perceptions of college graduates' productivity. Only firms eligible for treatment with reservation wage at least 2,000 ETB are included in the regressions. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. We break down the treatment effects in Column (2) and (4) by whether firms request a college graduate at baseline. Dependent variables in Column (1) and (2) are whether firms believe that college graduates have better productivity than non-college educated workers at endline. Dependent variables in Column (3) and (4) are the percentages of non-agency college educated applicants perceived with good productivity (only in Round 2). Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table 5: Effects on Interviewing and Hiring College Educated Applicants by Endline

Panel A. Intention-to-treat effects

VARIABLES	(1) Interview College	(2) Interview Non-college	(3) Diff: (2)−(1)	(4) Hire College	(5) Hire Non-college	(6) Diff: (5)−(4)
Assigned to treat	-0.0405	0.0437	0.0842	-0.0613	0.0459	0.107
	(0.0509)	(0.0395)	(0.0653)	(0.0542)	(0.0382)	(0.0700)
	[0.428]	[0.272]	[0.201]	[0.261]	[0.233]	[0.130]
Observations	581	581		581	581	
R-squared	0.309	0.486		0.294	0.485	
Control baseline char.	Yes	Yes		Yes	Yes	
Business area FE	Yes	Yes		Yes	Yes	
Cluster at business area	Yes	Yes		Yes	Yes	
Control mean	0.399	0.427		0.375	0.412	

Panel B. Heterogeneity by baseline request

VARIABLES	(1) Interview College	(2) Interview Non-college	(3) Diff: (2)−(1)	(4) Hire College	(5) Hire Non-college	(6) Diff: (5)−(4)
Assigned to treat X Requesting college	-0.164**	0.0970*	0.261**	-0.195***	0.105**	0.300***
	(0.0714)	(0.0528)	(0.100)	(0.0710)	(0.0527)	(0.101)
	[0.0245]	[0.0701]	[0.0113]	[0.00753]	[0.0493]	[0.00401]
Assigned to treat X Not requesting college	0.0408	0.00851	-0.0323	0.0268	0.00670	-0.0201
	(0.0627)	(0.0516)	(0.0820)	(0.0644)	(0.0511)	(0.0892)
	[0.517]	[0.869]	[0.695]	[0.678]	[0.896]	[0.822]
Observations	581	581		581	581	
R-squared	0.317	0.487		0.304	0.487	
Control baseline char.	Yes	Yes		Yes	Yes	
Business area FE	Yes	Yes		Yes	Yes	
Cluster at business area	Yes	Yes		Yes	Yes	
Control mean: Requesting college	0.600	0.117		0.579	0.0966	
Control mean: Not requesting college	0.236	0.676		0.209	0.665	

Notes: This table presents the treatment effects on interviewing or hiring (non-)college educated applicants. Only firms eligible for treatment with reservation wage at least 2,000 ETB are included in the regressions. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Panel B presents the heterogeneous treatment effects by whether firms request a college graduate at baseline. Dependent variables in Column (1) and (4) are whether firms interview or hire at least one college educated applicant by endline. Dependent variables in Column (2) and (5) are whether firms interview or hire at least one non-college educated applicant by endline. Column (3) and (6) compute the differences between the two estimates. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table 6: Heterogeneous Effects by College Share

VARIABLES	(1) Interview College	(2) Interview Non-college	(3) Diff: (2)−(1)	(4) Hire College	(5) Hire Non-college	(6) Diff: (5)−(4)
Assigned to treat X Above-median college share	-0.0452	-0.0133	0.0320	-0.0441	-0.00786	0.0362
	(0.115)	(0.0724)	(0.151)	(0.110)	(0.0682)	(0.142)
	[0.696]	[0.855]	[0.833]	[0.690]	[0.909]	[0.800]
Assigned to treat X Below-median college share	-0.236*	0.130	0.366**	-0.246**	0.124	0.370**
	(0.128)	(0.0887)	(0.173)	(0.118)	(0.0888)	(0.167)
	[0.0702]	[0.147]	[0.0385]	[0.0407]	[0.167]	[0.0298]
Observations	244	244		244	244	
R-squared	0.451	0.449		0.466	0.481	
Control baseline char.	Yes	Yes		Yes	Yes	
Business area FE	Yes	Yes		Yes	Yes	
Cluster at business area	Yes	Yes		Yes	Yes	
Control mean: Above-median college share	0.611	0.111		0.583	0.0833	
Control mean: Below-median college share	0.589	0.123		0.575	0.110	

Notes: This table presents the treatment effects on interviewing or hiring (non-)college educated applicants by college share, defined as the percentage of current employees with a college diploma or degree, a proxy for exposure to college graduates. Only firms requesting a college graduate at baseline and eligible for treatment with reservation wage at least 2,000 ETB are included in the regressions. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Dependent variables in Column (1) and (4) are whether firms interview or hire at least one college educated applicant by endline. Dependent variables in Column (2) and (5) are whether firms interview or hire at least one non-college educated applicant by endline. Column (3) and (6) compute the differences between two estimates. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table 7: Effects on Match Quality

VARIABLES	(1) Voluntary quit	(2) Fired by firm	(3) Above-avg prod (surveyed)	(4) Above-avg prod (measured)	(5) Zero absent day	(6) Overtime work
Assigned to treat	-0.154	0.0814	0.0139	0.108	-0.00328	0.0498
	(0.148)	(0.0730)	(0.191)	(0.261)	(0.161)	(0.209)
	[0.304]	[0.271]	[0.942]	[0.683]	[0.984]	[0.812]
Observations	146	146	146	82	146	146
R-squared	0.485	0.426	0.575	0.787	0.513	0.476
Control baseline char.	Yes	Yes	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.110	0.0200	0.530	0.476	0.630	0.340

Notes: This table presents the treatment effects of employment agencies on match quality at endline. Only firms requesting a college graduate at baseline and eligible for treatment with reservation wage at least 2,000 ETB are included in the regressions. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Dependent variables: Column (1)—whether the hired worker voluntarily quits. Column (2)—whether the hired worker is fired by firms. Column (3)—whether the hired worker is considered to be more productive than average workers on the similar positions. Column (4)—whether the efficiency measure of the hired worker is above that of similar workers (only in Round 2). Column (5)—whether the hired worker has zero absent day in the last 30 days. Column (6)—whether the hired worker works overtime in the last 7 days. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

A Main Variable Descriptions

A.1 Firm-level variables

Module	Survey questions	Variables	Use in paper
Baseline sector	What is the main business of this company?	Manufacturing and construction	Baseline control
		Hospitality (Hotels, restaurants)	Baseline control
		Education	Baseline control
		Health	Baseline control
Baseline workforce	How many employees are currently in your company? (including both permanent and temporary)	Number of current employees	Baseline control
	What's the percentage/number of female workers currently hired in the company?	Pct of female employees	Baseline control
	What's the percentage/number of well-educated workers (at least diploma) currently hired in the company?	Pct of employees with college degree	Baseline control, mechanism test
	What's the percentage/number of workers with zero year of experience currently hired in the company?	Pct of employees with zero experience	Baseline control
	What's the percentage/number of temporary workers currently hired in the company?	Pct of temporary employees	Baseline control
	What's the percentage/number of workers currently hired through referrals or recommendations?	Pct of employees hired through recommendation	Baseline control
Baseline hiring	What's the respondent's position in the firm?	The firm has a HR department (the respondent is a human resource manager or expert)	Baseline control
		The respondent is less engaging (the respondent is the owner)	Robustness
	Have you tried to hire labor from notice boards, newspaper, or online platforms before?	Hiring only from formal channels	Baseline control
	Have you tried to hire labor from agencies or informal brokers before?	Hiring from agencies or brokers	Baseline control
	Which agency did you go to most often before?	Experience with emp agencies	Footnote
	Have you tried to hire labor through personal recommendation?	Hiring through recommendation	Baseline control
Baseline vacancy	What will be the highest salary you would pay for this position?	Reservation wage	Eligibility, baseline control, robustness
	How many vacancies are you posting?	Posting more than one vacancy (only in Round 2)	Robustness
	What is the minimal requirement on education?	Required college-level diploma or degree (incl. TVET Level 3–4)	Baseline control, mechanism test
		Required vocational certificate (excl. TVET Level 3–4)	Baseline control
		Required high school degree	Baseline control
	What is the minimal requirement on experience?	Required no experience	Baseline control
		Required $\geq$2y experience	Baseline control
	What will be the brief job description for this new position?	Skilled task, manual task, routine task	Baseline control, mechanism test
Endline outcome	What is the agreed monthly salary when you first hire this person?	Monthly salary	Cost-benefit
	Did the hired worker quit voluntary?	Voluntary quit	Cost-benefit
	Did you fire this hired worker?	Fired by firm	Cost-benefit
	Compare this worker to the average 1-3 workers in the similar positions. How productive do you think this worker is on the job?	Above-average prod. (surveyed)	Cost-benefit
	What's the performance measure of this worker in the last month?	Above-average prod. (estimated)	Cost-benefit
	How many days is this worker absent in the last 30 days?	Zero absent days	Cost-benefit
	How many overtime hours does this worker work in the last week?	Overtime work	Cost-benefit
	What channels are you planning to use to post vacancies?	Plan to hire from agencies, other formal channels, or informal recommendation	Alt mechanism
	Do you think it is easier for a college graduate to get a job in Addis Ababa, compared to someone who didn't go to college?	Perception: College graduates have more job opportunities	Descriptives
	Imagine two workers. They came from the same subcity, went to the same secondary school, and have the same work experience. The only difference is that one went to college and the other one didn't. For the vacancy you posted, which one do you think will be more productive?	Perception: College graduates are more productive	Mechanism test

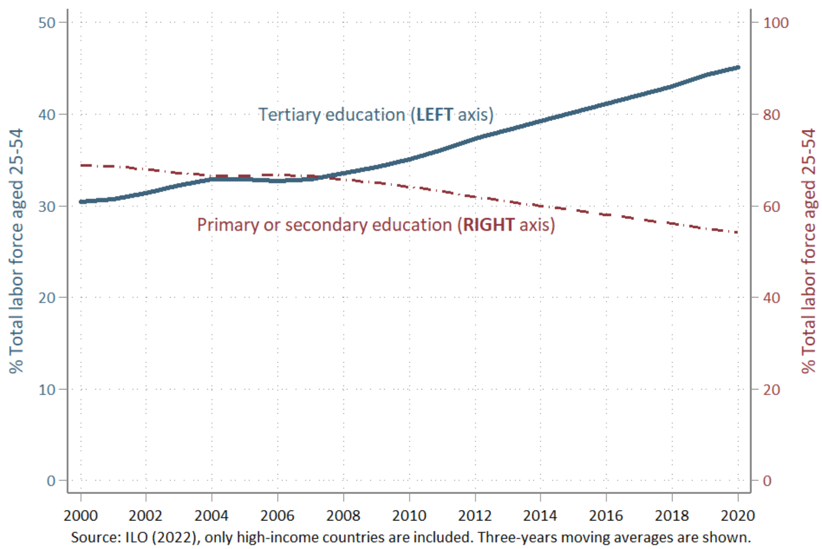
A.2 Applicant-level variables

Module	Survey questions	Variables	Use in paper
Firm applicant form	What's the education level of the applicant?	Educ: College-level diploma or degree (incl. TVET Level 3–4)	Main outcome
		Educ: Vocational (non-diploma, excl. TVET Level 3–4)	Figure B5
		Educ: At most high school	Figure B5
	Years of work experience	Experience: $\geq$2y	Section 5.5, Figure B5
		Experience: Some but <2y	Figure B5
		Experience: None	Section 5.5, Figure B5
	Was this worker sent by one of our employment agencies?	Agency/non-agency applicants	Mechanism test
	Did you invite this applicant to interview?	Invited to interview	Main outcome
	Did the applicant reject the interview invite?	Reject interview	Alt mechanism
	Did you offer a job to this applicant?	Hired	Main outcome
	Did the applicant reject the offer?	Reject offer	Alt mechanism
	If this worker is to be hired on the job, how productive would this worker be?	Perceived to be productive (only Round 2)	Mechanism test
Worker	Gender	Gender	Section 5.5, Figure B5
survey	What is your age?	Age: Above median	Section 5.5, Figure B5
	Are you currently employed?	Currently employed	Section 5.5, data validation
	What is your current job?		Data validation
	What is your monthly salary?	Current salary	Section 5.5, data validation

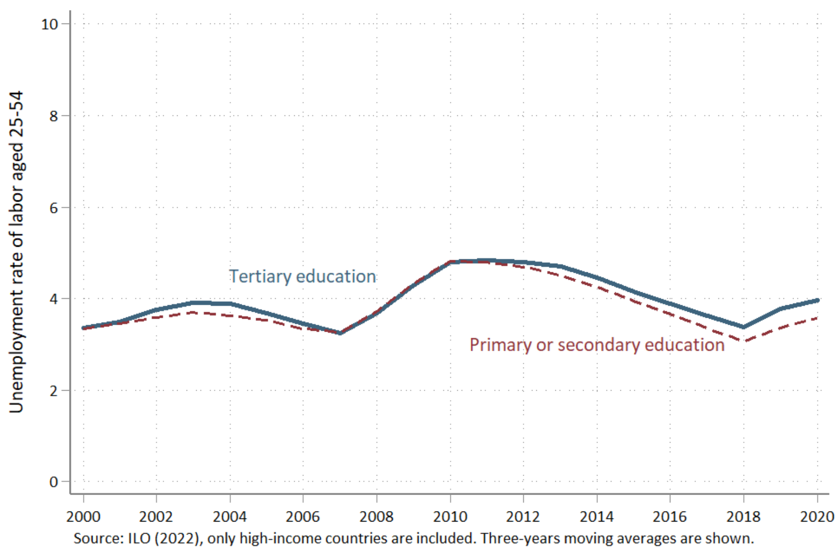
B Appendix Figure

Figure B1: Tertiary Education in High-Income Countries, 2000–20

Panel A. Percentage of tertiary educated workers, aged 25–54



Panel B. Unemployment of tertiary educated workers, aged 25–54



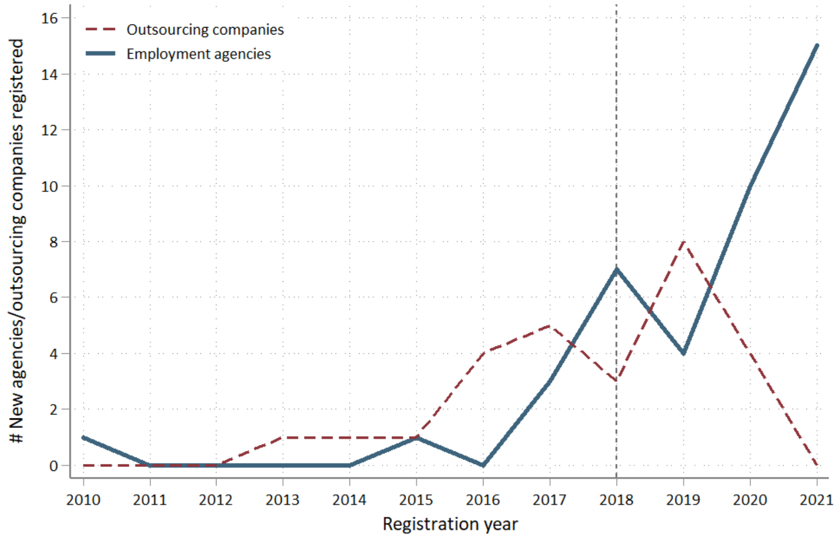
Notes: This figure shows the time series of percentages of labor aged 25–54 with tertiary education and unemployment rates in high-income countries, following the definition of World Bank. The labor force and unemployment data are from International Labor Organization database. We compute the three-year moving averages of yearly unemployment rates weighted by the total labor force aged 15–54 in the same year. Blue solid line shows the time series of labor with tertiary education. Red dashed line shows the time series of labor with non-tertiary education.

Figure B2: A Typical Employment Agency

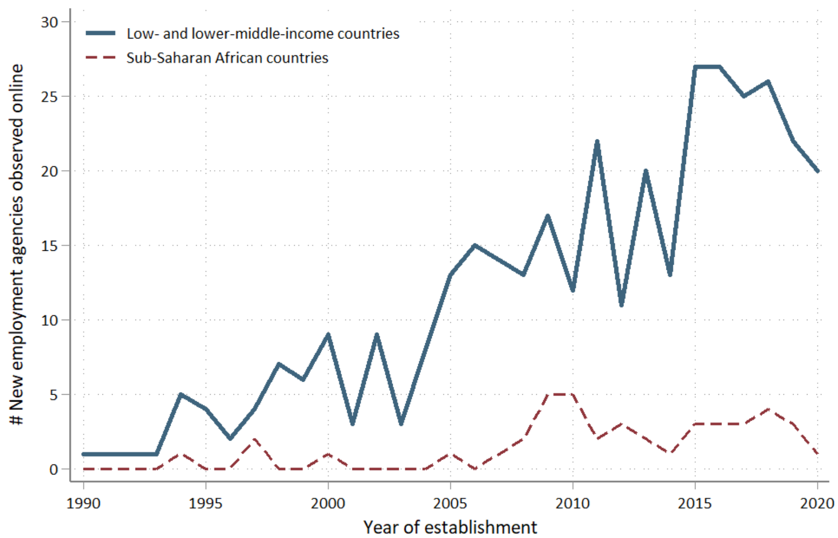
Notes: This figure shows a typical employment agency in our sample located in Bole sub-city, Addis Ababa, Ethiopia.

Figure B3: Trends of Employment Agencies

Panel A. Number of employment agencies in Bole sub-city, 2010–21

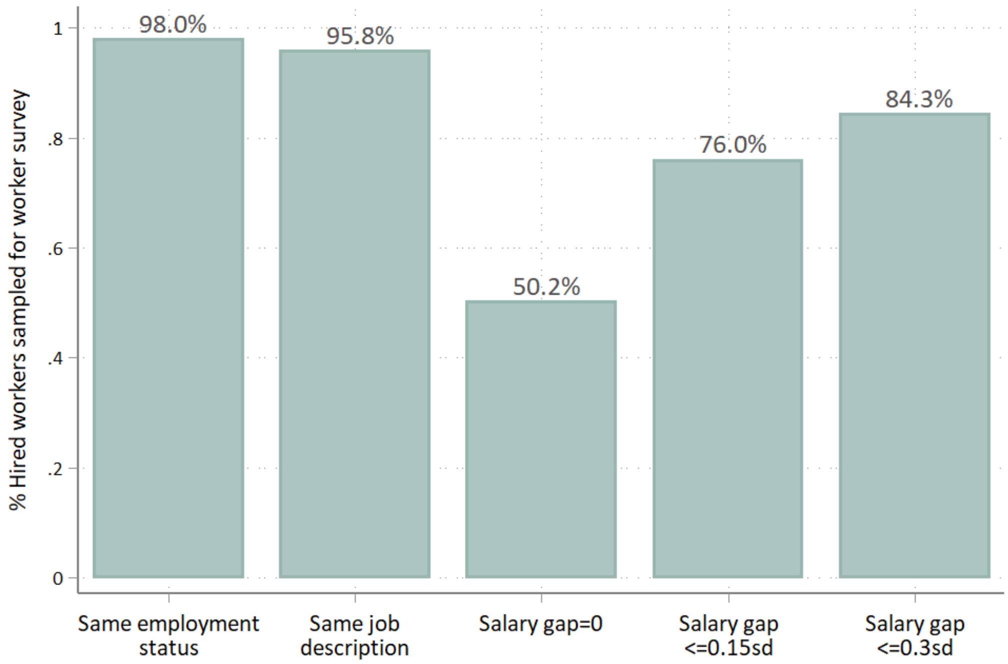


Panel B. Number of employment agencies in low- and middle-income countries, 1990–2020



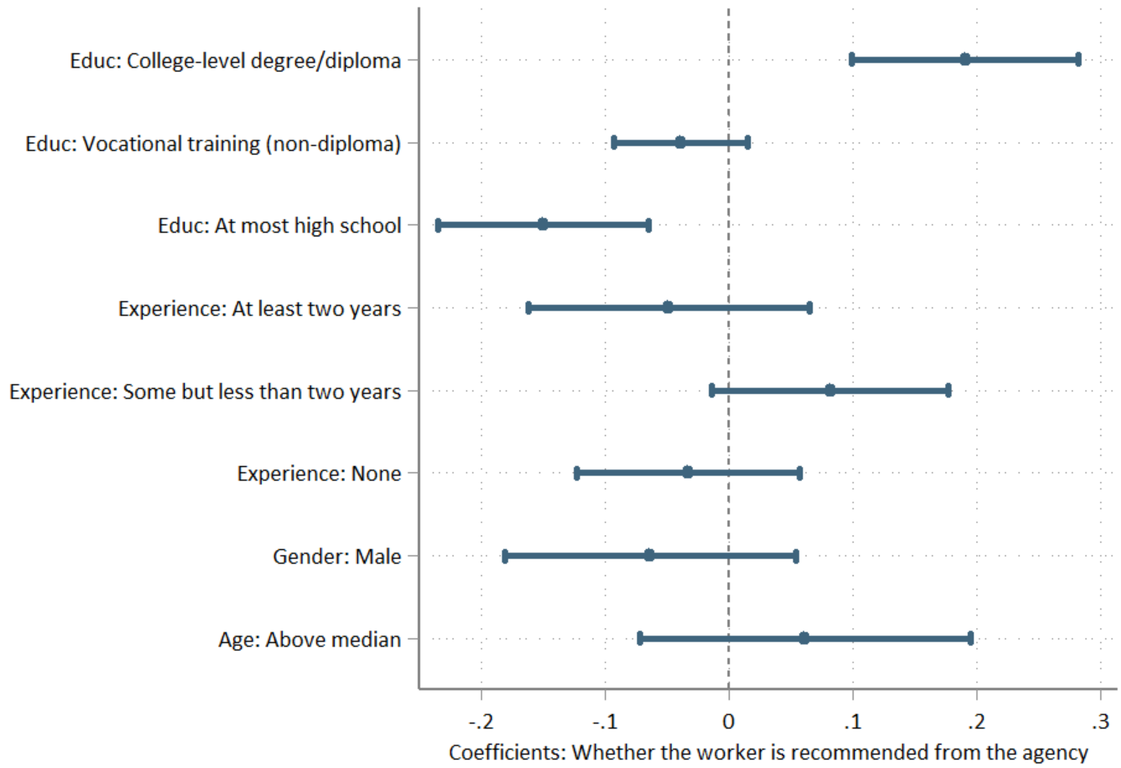
Notes: This figure shows the trend of employment agencies in the recent decades. Panel A shows the number of registered labor market intermediaries in Bole sub-city during 2010–21. The data come from the registry of employment agencies from Bole sub-city. Blue solid line shows the trend of employment agencies. Red dashed line shows the trend of outsourcing companies, another form of labor market intermediaries that focus exclusively on low-skill occupations such as construction, security guards, and janitors. Panel B shows the number of new employment agencies observed online from 1990–2020. The data come from one of the largest business-to-business service platforms where we search for all existing records of employment agencies of each country. Blue solid line shows the time series for low- and lower-middle-income countries according to World Bank definition. Red dashed line shows the time series only for sub-Saharan African countries.

Figure B4: Data Validation



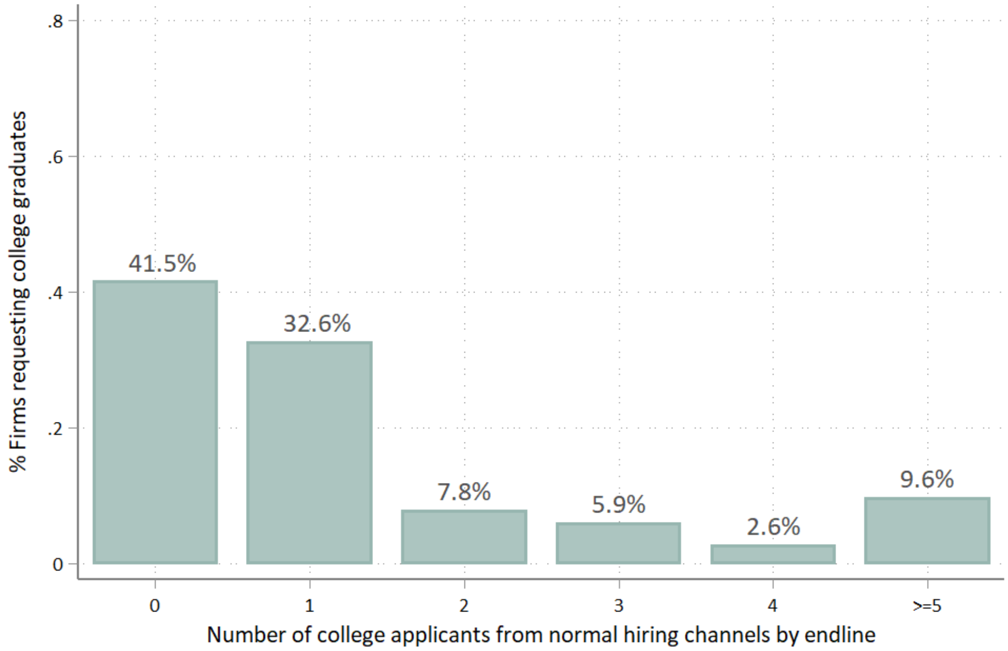
Notes: This figure shows the results from a data validation exercise. We focus on 683 workers who are sampled in the worker survey and hired by firms for the sampled vacancies according to firms' reports. We compare workers' self-reported data on whether they are employed, job description if employed, and salary if employed, to the records from the firms' records, and calculate the percentage of records with the same employment status, same job description, exactly same reported salary, and whether the gap between the reported salaries is no more than 0.15 standard deviation (10 USD) or 0.30 standard deviation (20 USD).

Figure B5: Selection of Applicants from Employment Agencies



Notes: This figure shows the selection of applicants from the employment agencies in terms of observable characteristics. For each characteristics, we compare agency applicants to non-agency applicants, controlling for firm fixed effects and cluster at the firm level. 95% confidence intervals are shown for each estimate.

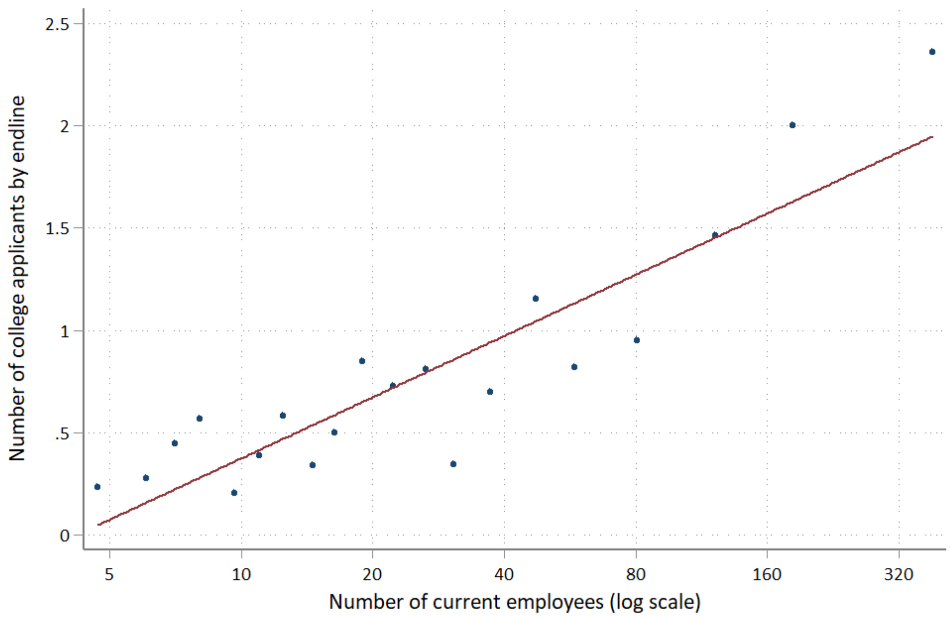
Figure B6: Distribution of College Applicants Among Firms Requesting College Graduates



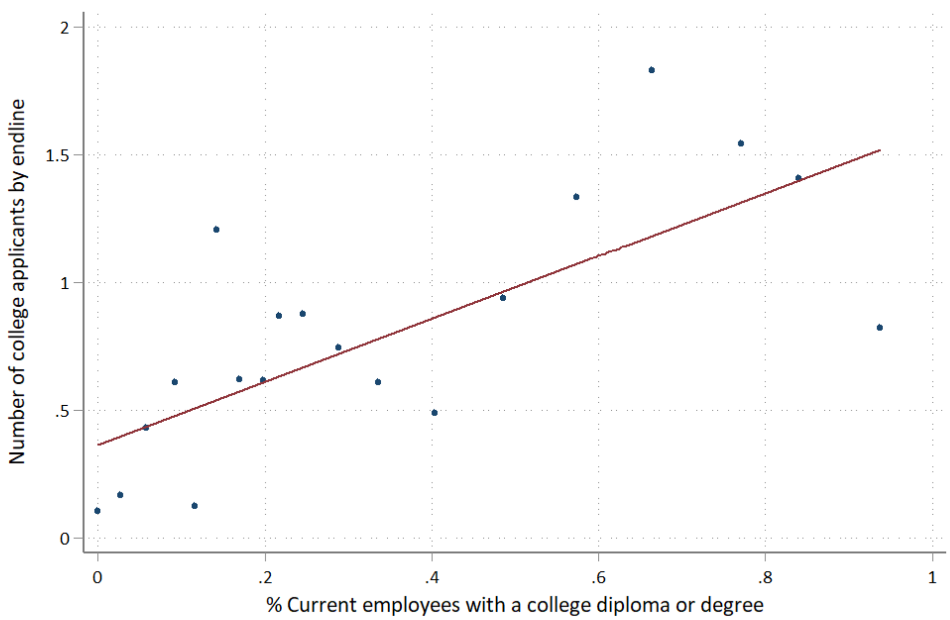
Notes: This figure shows the distribution of the total number of college applicants by endline for firms requesting college graduates, not including applicants from the employment agencies in the intervention.

Figure B7: Correlations Between the Number of College Applicants and Firm Characteristics

Panel A. Correlation with firm size



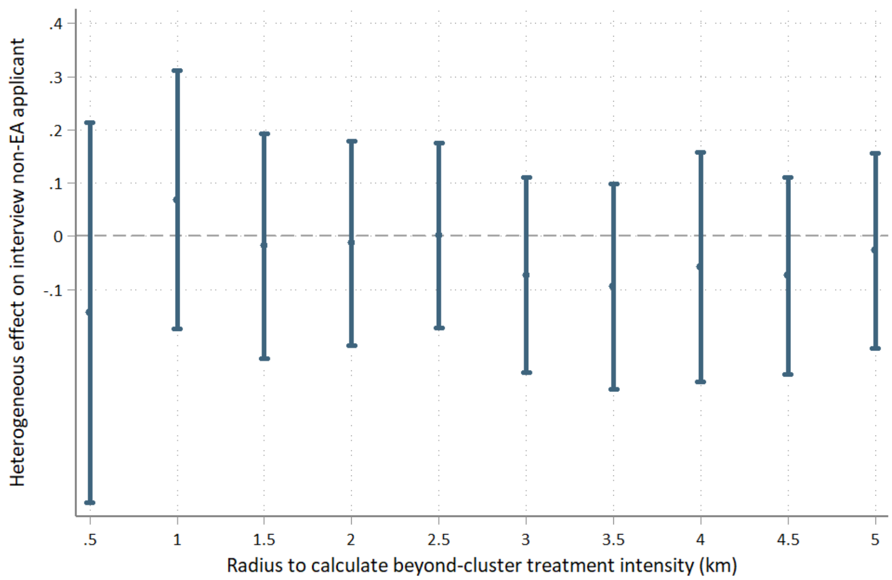
Panel B. Correlation with college share



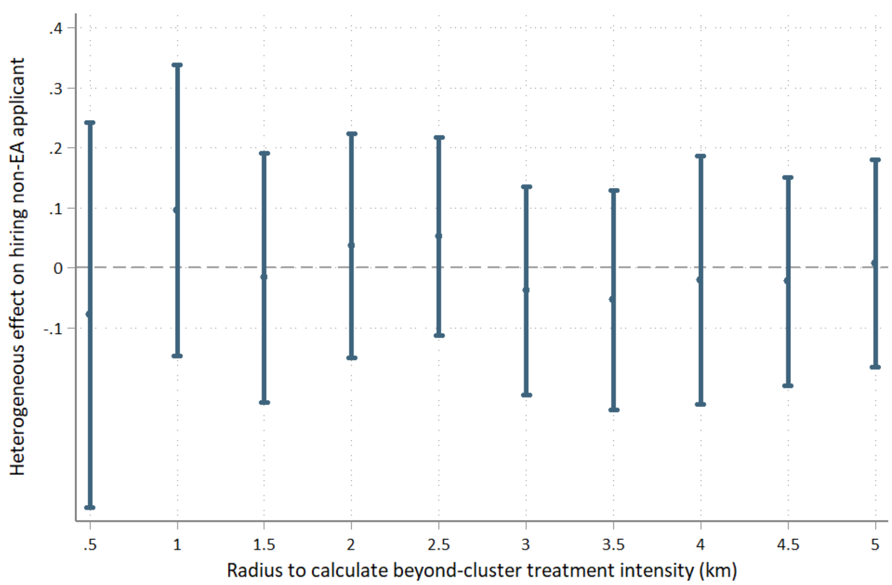
Notes: This figure shows the correlations between the number of college applicants received by each firm (excluding those from the employment agencies) and two firm characteristics: the number of current employees, and the percentage of current employees with a college diploma or degree.

Figure B8: Heterogeneous Effects on Interviewing and Hiring Non-Agency Applicants by Treatment Intensity

Panel A. Interviewing any non-agency applicant



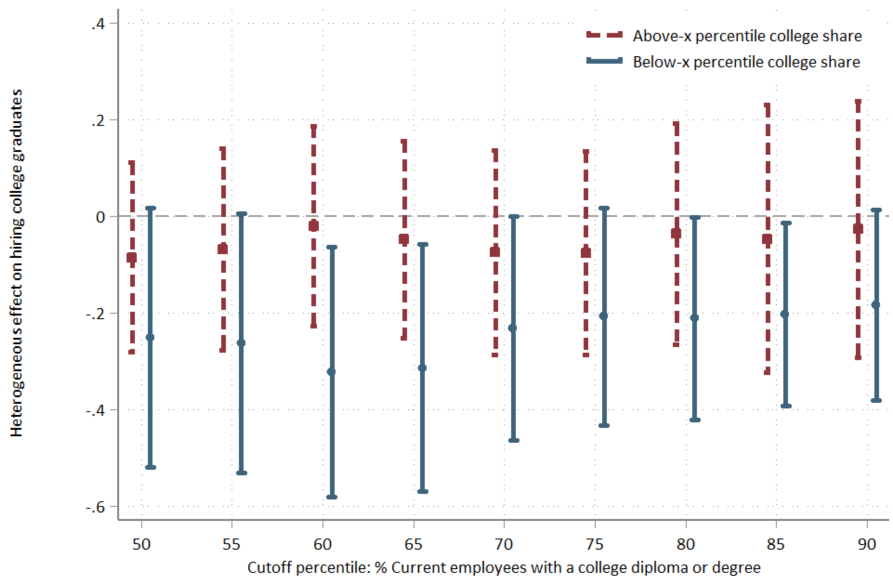
Panel B. Hiring any non-agency applicant



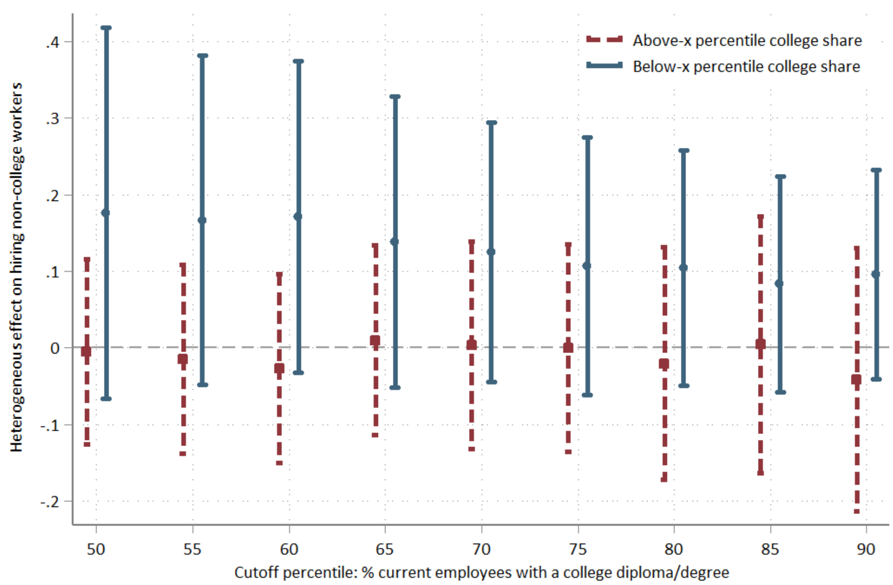
Notes: This figure shows the heterogeneous treatment effects by beyond-cluster treatment intensity in the nearby regions. Only firms with reservation wage at least 2,000 ETB (eligible firms) are included. In each regression, we regress whether firm interviews or hires any non-agency applicants on (1) initial treatment assignment and (2) interaction of treatment and whether the treatment intensity is above average. Treatment intensity is calculated by the percentage of firms in nearby x kilometers (excluding own business area) selected for treatment. We only report coefficients of the interaction terms. 95% confidence intervals are shown.

Figure B9: Heterogeneous Treatment Effects on Hiring College Graduates and Non-college Workers by College Share

Panel A. Hiring college graduates

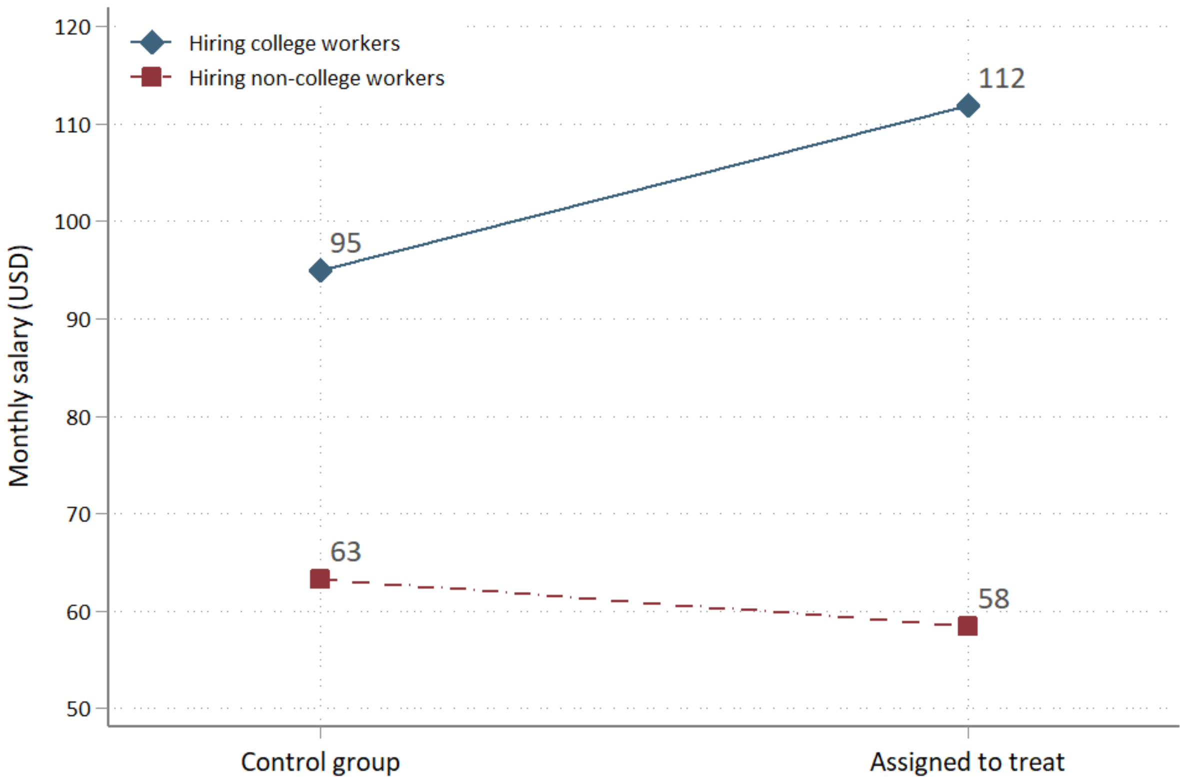


Panel B. Hiring non-college workers



Notes: This figure shows the heterogeneous treatment effects on hiring college and non-college workers by college share, defined as the percentage of current employees with a college-level diploma or degree. We select cutoffs from 50 percentile to 90 percentile, break down the treatment effects by above-x percentile and below-x percentile college share, and plot the heterogeneous treatment effects separately. Only firms requesting for college graduates at baseline and with reservation wage at least 2,000 ETB (eligible firms) are included. 95% confidence intervals are shown.

Figure B10: Monthly Salary for College Graduates and Non-college Workers



Notes: This figure shows the monthly salary separately for firms initially assigned to treatment and control groups. Dark blue squares show the monthly salary paid to college graduates. Red squares show the monthly salary paid to non-college workers.

C Appendix Tables

Table C1: Sample Selection Across Different Data

Panel A. Sampling of Firms

	This paper	Hensel et al. 2022	LMMIS 2014
Sector: Manufacturing	0.36	0.51	1.00
Sector: Hospitality	0.39	0.27	0.00
Sector: Others	0.25	0.22	0.00
Number of employees: Average	58	14	99
Number of employees: Median	20	10	32

Panel B. Sampling of Vacancies

Salary (birr)	This paper	Notice board pilot	Major online platform
25 percentile	2,000	3,500	4,609
50 percentile	3,000	4,020	8,017
75 percentile	4,800	5,208	13,926
Average	3,878	4,737	12,429

Notes: This table compares sampling of firms of vacancies between this paper and other data sources. Panel A compares the sampling of firms between this paper, Hensel et al. (2021), and Large and Medium Manufacturing and Electricity Industries Survey (LMMIS, the latest available year is 2014). Panel B compares the sampling of vacancies between this paper, vacancies collected from three major notice boards of Addis Ababa during our pilot in November 2020, and job posts from a major online job search platform in Ethiopia.

Table C2: Qualitative Survey: Functions of Employment Agencies

Panel A. Self report from 25 agencies

Functions of employment agencies	% all agencies
Check applicants' ID	91.3
Check applicants' education certificates	82.6
Recommend vocational training to workers	52.2
Check previous employers' recommendation	39.1
Provide additional training	13.0
Conduct additional grading test	4.3

Panel B. Report from 539 job seekers

Functions of employment agencies	% of 539 workers
Offer advice on job search or which job to apply to	51.9
Provide connections with employers/workers	12.1
Coach me on job interviews	5.8
Help me revise my CV	1.7

Notes: This table presents qualitative reports of the functions of employment agencies. Panel A shows the percentage of the 25 employment agencies during pilot survey who agree with each statement. Panel B shows the percentage of the 539 job seekers during worker survey who agree with with each statement.

Table C3: Balance Table with Actual Treatment Status

	(1)	(2)	(3)	(4)	(5)	(6)
	Mean outcomes					P-value
	All	Eligible control		Eligible treated		T-C
Observations	627	301		326		
Sector						
Manufacturing and construction	0.42	0.45	(0.50)	0.39	(0.49)	0.22
Hospitality (hotels, restaurants)	0.27	0.26	(0.44)	0.29	(0.45)	0.50
Education	0.11	0.11	(0.32)	0.12	(0.32)	0.90
Health	0.05	0.07	(0.26)	0.03	(0.18)	0.09
Current employees						
Number of current employees	66.30	57.50	(93.61)	74.43	(143.01)	0.17
Pct of female employees	0.53	0.53	(0.27)	0.53	(0.26)	0.93
Pct of employees with college degree	0.37	0.36	(0.28)	0.38	(0.29)	0.46
Pct of employees with zero exp	0.20	0.19	(0.23)	0.20	(0.24)	0.61
Pct of temporary employees	0.16	0.15	(0.27)	0.16	(0.28)	0.75
Pct of employees hired through rec	0.15	0.16	(0.22)	0.14	(0.22)	0.53
Hiring practices						
The firm has a HR department	0.51	0.49	(0.50)	0.53	(0.50)	0.29
Posting jobs on notice board	0.54	0.54	(0.50)	0.53	(0.50)	0.87
Posting jobs on newspaper	0.14	0.13	(0.34)	0.15	(0.36)	0.50
Posting jobs on online platforms	0.16	0.13	(0.33)	0.18	(0.39)	0.05
Hiring from formal employment agencies	0.08	0.07	(0.26)	0.10	(0.30)	0.26
Hiring from informal brokers	0.25	0.27	(0.44)	0.24	(0.43)	0.60
Hiring through recommendation	0.50	0.50	(0.50)	0.50	(0.50)	0.93
Posted vacancy						
Reservation wage (USD)	91.49	90.04	(82.09)	92.87	(71.61)	0.62
Required college degree	0.44	0.41	(0.49)	0.48	(0.50)	0.18
Required vocational certificate	0.08	0.05	(0.22)	0.10	(0.30)	0.04
Required high school degree	0.14	0.14	(0.35)	0.14	(0.35)	0.98
Required no experience	0.20	0.22	(0.42)	0.18	(0.38)	0.19
Required more than 2y experience	0.19	0.16	(0.37)	0.21	(0.41)	0.20
Skilled task	0.55	0.51	(0.50)	0.59	(0.49)	0.07
Manual task	0.64	0.69	(0.46)	0.60	(0.49)	0.07
Routine task	0.69	0.72	(0.45)	0.67	(0.47)	0.16

Notes: This table shows the balance between 326 eligible firms that are actually treated and 301 eligible control firms. Standard deviations are shown in parentheses. The last column shows the p-value of a simple comparison of each characteristics between eligible treated and eligible control firms, clustered at the level of business area.

Table C4: Effects on the Number of Applicants

VARIABLES	(1) # Agency	(2) # Non-agency	(3) # All	(4) # App ≥1	(5) # App ≥2	(6) # App ≥3
Assigned to treat	0.373***	-0.0114	0.361**	0.0675***	0.165***	0.0491
	(0.0783)	(0.170)	(0.179)	(0.0237)	(0.0527)	(0.0422)
	[8.56e-06]	[0.946]	[0.0470]	[0.00560]	[0.00236]	[0.248]
Observations	583	583	583	583	583	589
R-squared	0.420	0.309	0.311	0.267	0.280	0.309
Control baseline char.	Yes	Yes	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.137	1.961	2.099	0.875	0.331	0.230

Notes: This table examines the treatment effects on the number of applicants. The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. Observation with above 99.5 percentile are truncated (number of applicants above 13). All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Dependent variables: Column (1)—Number of extra agency applicants. Column (2)—Number of non-agency applicants. Column (3)—Total number of applicants. Column (4)–(6): Whether the number of applicants is at least 1–3 applicants. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C5: Effects on Additional Hiring Decisions

VARIABLES	(1) Postpone vacancy	(2) Cancel vacancy	(3) No qualified workers	(4) Relocate to current workers
Assigned to treat	-0.0796**	-0.0512*	-0.0198	-0.00479
	(0.0374)	(0.0298)	(0.0307)	(0.0188)
	[0.0366]	[0.0893]	[0.521]	[0.800]
Observations	589	589	589	589
R-squared	0.261	0.196	0.319	0.202
Control baseline char.	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes
Control mean	0.254	0.0537	0.101	0.0328

Notes: This table presents the treatment effects on additional hiring decisions. Only firms eligible for treatment with reservation wage at least 2,000 ETB are included in the regressions. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Dependent variables: Column (1)—Whether firms postpone the vacancies. Column (2)—Whether firms cancel the vacancies. Column (3)—Whether firms complain about not finding qualified workers. Column (4)—Whether firms relocate the tasks to current workers. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C6: Selection of Outcome Variables

Panel A. Number of interviewees

VARIABLES	(1) # Interviewees	(2) #≥1	(3) #≥2	(4) #≥3	(5) #≥4
Assigned to treat	0.234	0.142***	0.0712	0.0168	0.0233
	(0.163)	(0.0503)	(0.0431)	(0.0350)	(0.0300)
	[0.154]	[0.00590]	[0.102]	[0.632]	[0.440]
Observations	582	582	582	582	582
R-squared	0.331	0.293	0.300	0.302	0.279
Control baseline char.	Yes	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes	Yes
Control mean	1.342	0.603	0.267	0.173	0.103

Panel B. Number of new hires

VARIABLES	(1) # New hires	(2) #≥1	(3) #≥2	(4) #≥3	(5) #≥4
Assigned to treat	0.122	0.101**	0.0363	0.00360	-0.00355
	(0.110)	(0.0502)	(0.0314)	(0.0315)	(0.0276)
	[0.270]	[0.0485]	[0.251]	[0.909]	[0.898]
Observations	582	582	582	582	582
R-squared	0.342	0.282	0.355	0.307	0.269
Control baseline char.	Yes	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes	Yes
Control mean	0.897	0.567	0.173	0.0818	0.0424

Notes: This table examines the treatment effects on different hiring outcomes. The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. The dependent variables are the number of interviewees or new hires, whether the number of interviewees or new hires is greater than 1, 2, 3, or 4. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C7: Effects on Interviewing and Hiring Any Applicant by Endline

VARIABLES	(1) Interview Midline	(2) Interview Endline	(3) Hire Midline	(4) Hire Endline
Assigned to treat	0.142***	0.00989	0.101*	0.00659
	(0.0503)	(0.0502)	(0.0517)	(0.0514)
	[0.00590]	[0.844]	[0.0547]	[0.898]
Observations	582	581	582	581
R-squared	0.293	0.262	0.274	0.264
Control baseline char.	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes
Control mean	0.603	0.750	0.576	0.750

Notes: This table presents the treatment effects on interviewing or hiring any applicant by midline and endline. Only firms eligible for treatment with reservation wage at least 2,000 ETB are included in the regressions. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Dependent variables: Column (1) and (2)—Whether firms interview at least one applicant. Column (3) and (4)—Whether firms hire at least one applicant. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C8: Robustness: Statistical Inference

Panel A. Interview any non-agency applicant

VARIABLES	(1) Interview Non-EA	(2) Interview Non-EA	(3) Interview Non-EA	(4) Interview Non-EA	(5) Interview Non-EA	(6) Interview Non-EA	(7) Interview Non-EA
Assigned to treat	0.0976*	0.0976*	0.0976*	0.0976*	0.0886	0.142***	0.528***
	(0.0527)	(0.0531)	(0.0521)	(0.0542)	(0.0549)	(0.0538)	(0.166)
	[0.0682]	[0.0671]	[0.0611]	[0.0758]	[0.110]	[0.00989]	[0.00149]
Observations	582	582	582	582	527	470	475
R-squared	0.286	0.286	0.286	0.286	0.411	0.460	
Control baseline char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.592	0.592	0.592	0.592	0.685	0.716	
Specification	Main	Robust sd	Bootstrap	Permutation test	Weight by # app	Weight by # non-agency app	Binomial logit

Panel B. Hiring any non-agency applicant

VARIABLES	(1) Hire Non-EA	(2) Hire Non-EA	(3) Hire Non-EA	(4) Hire Non-EA	(5) Hire Non-EA	(6) Hire Non-EA	(7) Hire Non-EA
Assigned to treat	0.0907*	0.0907*	0.0907*	0.0907*	0.0870	0.134**	0.509***
	(0.0509)	(0.0536)	(0.0480)	(0.0538)	(0.0523)	(0.0534)	(0.166)
	[0.0785]	[0.0913]	[0.0586]	[0.0959]	[0.100]	[0.0141]	[0.00216]
Observations	582	582	582	582	527	470	475
R-squared	0.281	0.281	0.281	0.281	0.399	0.428	
Control baseline char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.573	0.573	0.573	0.573	0.667	0.697	
Specification	Main	Robust sd	Bootstrap	Permutation test	Weight by # app	Weight by # non-agency app	Binomial logit

Notes: This table examines the robustness of the standard errors of the effects on interviewing and hiring any non-agency applicant. The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Specifications: Column (1), main; Column (2), only robust standard errors; Column (3), bootstrapping standard errors; Column (4), permutation test; Column (5), observations weighted by the total number of applicants; Column (6), observations weighted by the total number of non-agency applicants; Column (7), using binomial logit regression. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C9: Robustness: Attrition

VARIABLES	(1) Attrition	(2) Interview Non-EA	(3) Interview Non-EA	(4) Interview Non-EA	(5) Hire Non-EA	(6) Hire Non-EA	(7) Hire Non-EA
Assigned to treat	0.0241	0.141**	0.0805	0.105*	0.120**	0.0739	0.0980*
	(0.0157)	(0.0570)	(0.0535)	(0.0526)	(0.0560)	(0.0526)	(0.0508)
	[0.128]	[0.0158]	[0.137]	[0.0503]	[0.0358]	[0.164]	[0.0574]
Treated X Attrit likelihood		-0.120			-0.0774		
		(0.0895)			(0.0856)		
		[0.184]			[0.368]		
Observations	589	582	589	589	582	589	589
R-squared	0.224	0.289	0.278	0.286	0.283	0.275	0.281
Control baseline char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.0149	0.592	0.585	0.600	0.573	0.564	0.579
Specification	Main	Interaction	All attrited firms hired	No attrited firms hired	Interaction	All attrited firms hired	No attrited firms hired

Notes: This table examines the robustness of the effects on interviewing and hiring any non-agency applicant regarding attrition. The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Specifications: Column (1), regressing treatment status on attrition; Column (2) and (5), including an interaction of treatment status and whether the predicted attrition likelihood is above average. The predicted attrition likelihood is constructed by regressing attrition on the entire set of baseline characteristics. Column (3) and (6), assuming all attrited firms interviewed or hired within one month; Column (4) and (7), assuming no attrited firms interviewed or hired within one month. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C10: Robustness: Matching Strategy of Employment Agencies

VARIABLES	(1) Interview Non-EA	(2) Interview Non-EA	(3) Interview Non-EA	(4) Interview Non-EA	(5) Hire Non-EA	(6) Hire Non-EA	(7) Hire Non-EA	(8) Hire Non-EA
Receive extra applicants	-0.0672	0.336*			-0.0788	0.313*		
	(0.0516)	(0.190)			(0.0525)	(0.184)		
	[0.197]	[0.0803]			[0.137]	[0.0933]		
Assigned to treat			0.113*	0.140*			0.117**	0.116
			(0.0576)	(0.0754)			(0.0552)	(0.0721)
			[0.0530]	[0.0675]			[0.0368]	[0.112]
Treated X High reservation wage			-0.0608				-0.0977	
			(0.0890)				(0.0846)	
			[0.497]				[0.251]	
Treated X Unlikely delivered				-0.0740				-0.0444
				(0.0795)				(0.0794)
				[0.355]				[0.577]
Observations	582	582	582	582	582	582	582	582
R-squared	0.283	0.030	0.288	0.287	0.279	0.036	0.284	0.281
Specification	OLS	IV	OLS	OLS	OLS	IV	OLS	OLS
Control baseline char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.592	0.592	0.592	0.592	0.573	0.573	0.576	0.576
F-statistic		29.73				29.73		
Hausman test	0.0226				0.0223			

Notes: This table examines the robustness of the effects on interviewing and hiring any non-agency applicant regarding strategic matching of employment agencies. The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. The independent variable for Column (1), (2), (5), and (6) is whether the firm receives extra applicants. Specifications: Column (1) and (5), OLS regression; Column (2) and (6), using initial random assignment as an instrument; Column (3) and (7), OLS regression with initial treatment assignment as the main independent variable and interacting with whether the reservation wage is above average; Column (4) and (8), OLS regression with initial treatment assignment as the main independent variable and interacting with whether the predicted likelihood of receiving extra applicants is below average. The predicted likelihood is constructed by regressing whether the firms receive any extra applicant on the entire set of baseline characteristics. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C11: Robustness: Demand Effect

VARIABLES	(1) Interview Non-EA	(2) Interview Non-EA	(3) Hire Non-EA	(4) Hire Non-EA
Assigned to treat	0.195**	0.136**	0.156*	0.129**
	(0.0940)	(0.0622)	(0.0857)	(0.0605)
	[0.0466]	[0.0313]	[0.0792]	[0.0359]
Treated X Many vacancies	0.00913		-0.00814	
	(0.166)		(0.148)	
	[0.957]		[0.957]	
Treated X Less engaging		-0.154*		-0.155*
		(0.0922)		(0.0915)
		[0.0981]		[0.0937]
Observations	208	582	208	582
R-squared	0.350	0.291	0.366	0.287
Control baseline char.	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes
Control mean	0.696	0.592	0.679	0.573

Notes: This table examines the robustness of the effects on interviewing and hiring any non-agency applicant regarding demand effects. The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Specifications: Column (1) and (3), interacting treatment assignment and whether there is more than one vacancy during baseline; we only collect the number of vacancies in Round 2. Column (2) and (4), interacting treatment status and whether the respondents are the owners themselves, a proxy for less engagement. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C12: Robustness: Spillover

VARIABLES	(1) Interview Non-EA	(2) Interview Non-EA	(3) Interview Non-EA	(4) Hire Non-EA	(5) Hire Non-EA	(6) Hire Non-EA
Intensely treated area	-0.124			-0.0882		
	(0.0927)			(0.0890)		
	[0.184]			[0.325]		
Assigned to treat		0.0897	0.106		0.0938	0.0783
		(0.0695)	(0.0806)		(0.0679)	(0.0763)
		[0.201]	[0.191]		[0.171]	[0.308]
Treated X Intensely treated area		0.0246			-0.00985	
		(0.0966)			(0.0908)	
		[0.799]			[0.914]	
Treated X High intensity w/n 2km			-0.0120			0.0171
			(0.0918)			(0.0888)
			[0.897]			[0.847]
Observations	317	582	582	317	582	582
R-squared	0.235	0.286	0.286	0.229	0.281	0.281
Only non-treated firms	Yes			Yes		
Local district FE	Yes			Yes		
Business area FE		Yes	Yes		Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.607	0.592	0.592	0.591	0.573	0.573

Notes: This table examines the robustness of the effects on interviewing and hiring any non-agency applicant regarding spillover on control firms. The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. All regressions include a full set of baseline characteristics from Table 1 and cluster at business area level. The independent variable in Column (1) and (4) is whether the business area is selected for the intense treatment arm. Specification: Column (1) and (4), only control firms are included, controlling for local district fixed effects. Column (2) and (5), interacting the treatment assignment and whether the business area is selected for the intense treatment arm, controlling for business area fixed effects. Column (3) and (6), interacting the treatment assignment and whether the treatment intensity within 2km radius is above average, controlling for business area fixed effects. Treatment intensity is calculated by the percentage of firms in nearby x kilometers (excluding own business area) selected for treatment. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C13: Effects on the Number of College Applicants by Endline

VARIABLES	(1) # College	(2) # College	(3) # College Non-EA	(4) # Non-college
Assigned to treat	0.329**		0.0173	-0.115
	(0.160)		(0.151)	(0.143)
	[0.0425]		[0.909]	[0.424]
Treated X Requesting college		0.602**		
		(0.285)		
		[0.0376]		
Treated X Not requesting college		0.148		
		(0.166)		
		[0.374]		
Observations	577	577	577	577
R-squared	0.385	0.388	0.341	0.434
Control baseline char.	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes
Control mean	1.125	1.125	1.030	1.238

Notes: This table examines the treatment effects on the number of college applicants observed by endline. The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. Observation with above 99.5 percentile are truncated (number of college applicants above 10). All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. We include the interaction of initial treatment assignment and baseline request for college graduates in Column 2. Dependent variables: Column (1) and (2), total number of college applicants; Column (3), total number of college applicants not recommended from employment agencies; Column (4), total number of non-college applicants. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C14: Heterogeneous Effects on Hiring College Graduates by Baseline Request and Task Types

VARIABLES	(1) Hire College	(2) Hire Non-college	(3) Hire College	(4) Hire Non-college	(5) Hire College	(6) Hire Non-college
Treated X Requesting college X (A=0)	-0.398***	0.390***	-0.142	0.0582	-0.183**	0.0380
	(0.147)	(0.143)	(0.0915)	(0.0611)	(0.0843)	(0.0639)
	[0.00836]	[0.00780]	[0.124]	[0.344]	[0.0329]	[0.554]
Treated X Requesting college X (A=1)	-0.176**	0.0801	-0.269***	0.182**	-0.230**	0.307***
	(0.0732)	(0.0529)	(0.0887)	(0.0844)	(0.109)	(0.0996)
	[0.0183]	[0.134]	[0.00331]	[0.0343]	[0.0373]	[0.00286]
Treated X Not requesting college X (A=0)	-0.0228	0.0396	0.0452	0.120	0.105	-0.0795
	(0.0669)	(0.0537)	(0.198)	(0.153)	(0.199)	(0.190)
	[0.734]	[0.463]	[0.820]	[0.435]	[0.598]	[0.676]
Treated X Not requesting college X (A=1)	0.174	-0.0983	0.0289	-0.00116	0.0205	0.00828
	(0.109)	(0.117)	(0.0652)	(0.0508)	(0.0664)	(0.0522)
	[0.115]	[0.404]	[0.659]	[0.982]	[0.758]	[0.874]
Task type A	Skilled	Skilled	Routine	Routine	Manual	Manual
Control baseline char.	Yes	Yes	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.375	0.412	0.375	0.412	0.375	0.412

Notes: This table presents the treatment effects on hiring (non-)college applicants. The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Task type in Column (1) and (2): Whether the vacancy involves skilled tasks. Task type in Column (3) and (4): Whether the vacancy involves routine tasks. Task type in Column (5) and (6): Whether the vacancy involves manual tasks. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C15: Effects on the Perceptions of College Applicants By College Share

	(1)	(2)	(3)	(4)
	Endline: Whether firm agrees that		Midline: % College applicants	
VARIABLES	College graduates have better prod		Perceived with good prod	
Assigned to treat	-0.0867*		-0.260*	
	(0.0437)		(0.135)	
	[0.0505]		[0.0632]	
Assigned to treat X Above-median college share		-0.0791		-0.224
		(0.0546)		(0.224)
		[0.151]		[0.324]
Assigned to treat X Below-median college share		-0.0917*		-0.288**
		(0.0527)		(0.137)
		[0.0859]		[0.0442]
Observations	568	568	106	106
R-squared	0.329	0.333	0.595	0.596
Control firm/vacancy char.	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes
Control mean	0.782	0.782	0.770	0.770

Notes: This table presents the treatment effects on the perceptions of college applicants by college share, defined as the percentage of current employees with a college diploma or degree, a proxy for exposure to college graduates. Only firms eligible for treatment with reservation wage at least 2,000 ETB are included in the regressions. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. We break down the treatment effects in Column (2) and (4) by whether the college share is above or below median. Dependent variables in Column (1) and (2) are whether firms believe that college graduates have better productivity than non-college workers at endline. Dependent variables in Column (3) and (4) are the percentages of non-agency college applicants perceived with good productivity (only in Round 2). Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C16: Heterogeneous Effects By Exposure to College Graduates, Different Proxies

Panel A. Proxy: Number of college employees

VARIABLES	(1) Interview College	(2) Interview Non-college	(3) Diff: (2)-(1)	(4) Hire College	(5) Hire Non-college	(6) Diff: (5)-(4)
Assigned to treat X	-0.119	0.00795	0.127	-0.0934	-0.00758	0.0858
Above-median # college employees	(0.149)	(0.0716)	(0.179)	(0.136)	(0.0698)	(0.165)
	[0.428]	[0.912]	[0.481]	[0.495]	[0.914]	[0.605]
Assigned to treat X	-0.158	0.0802	0.238	-0.183*	0.0890	0.272*
Below-median # college employees	(0.105)	(0.0863)	(0.156)	(0.0989)	(0.0861)	(0.150)
	[0.138]	[0.356]	[0.131]	[0.0692]	[0.306]	[0.0752]
Observations	244	244		244	244	
R-squared	0.444	0.446		0.459	0.484	
Control baseline char.	Yes	Yes		Yes	Yes	
Business area FE	Yes	Yes		Yes	Yes	
Cluster at business area	Yes	Yes		Yes	Yes	
Control mean	0.399	0.427		0.375	0.412	

Panel B. Proxy: Whether firms receive any non-agency college applicant

VARIABLES	(1) Interview College	(2) Interview Non-college	(3) Diff: (2)-(1)	(4) Hire College	(5) Hire Non-college	(6) Diff: (5)-(4)
Assigned to treat X	-0.227	0.00948	0.236	-0.214	0.00254	0.217
$\geq$ 1 non-agency college applicant	(0.158)	(0.0807)	(0.181)	(0.159)	(0.0812)	(0.185)
	[0.157]	[0.907]	[0.196]	[0.184]	[0.975]	[0.245]
Assigned to treat X	-0.250**	0.0807	0.330**	-0.204*	0.113*	0.317**
Zero non-agency college applicant	(0.0995)	(0.0630)	(0.132)	(0.103)	(0.0628)	(0.136)
	[0.0146]	[0.205]	[0.0148]	[0.0520]	[0.0765]	[0.0227]
Observations	244	244		244	244	
R-squared	0.549	0.445		0.535	0.478	
Control baseline char.	Yes	Yes		Yes	Yes	
Business area FE	Yes	Yes		Yes	Yes	
Cluster at business area	Yes	Yes		Yes	Yes	
Control mean	0.399	0.427		0.375	0.412	

Notes: This table presents the treatment effects on interviewing or hiring (non-)college applicants by whether the firm has more exposure to college graduates. In Panel A, we use the number of current employees with college degree ("college employees") as a proxy; in Panel B, we use whether firms receive any non-agency college applicant as a proxy. Only firms requesting college graduates at baseline and eligible for treatment with reservation wage at least 2,000 ETB are included in the regressions. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Dependent variables in Column (1) and (4) are whether firms interview or hire at least one college applicant within one month. Dependent variables in Column (2) and (5) are whether firms interview or hire at least one non-college applicant within one month. Column (3) and (6) compute the differences between the two estimates. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C17: Comparison Between College and Non-College Educated Applicants

Panel A. Applicants' characteristics

VARIABLES	(1) Years exp	(2) Zero exp	(3) $\geq$ 2y exp	(4) Matched exp	(5) Father educated	(6) # Other offers	(7) Better offer
College graduates	2.506***	0.0325	0.146*	0.00199	0.00279	0.0577	-0.0199
	(0.595)	(0.0614)	(0.0860)	(0.0838)	(0.114)	(0.0810)	(0.0457)
	[5.12e-05]	[0.598]	[0.0925]	[0.981]	[0.981]	[0.479]	[0.664]
College graduates X From agency	0.385	0.0481	0.0577	-0.0825	0.127	0.707	-0.0823
	(0.509)	(0.0768)	(0.0754)	(0.0862)	(0.211)	(0.449)	(0.0734)
	[0.450]	[0.532]	[0.446]	[0.341]	[0.547]	[0.119]	[0.265]
Observations	384	384	384	384	255	255	255
R-squared	0.718	0.579	0.604	0.562	0.490	0.553	0.397
Only interviewees	No	No	No	No	Yes	Yes	Yes
Control worker char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster at firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	2.702	0.264	0.509	0.354	0.108	0.0655	0.0114

Panel B. Firms' perceptions of productivity

VARIABLES	(1) Considered productive	(2) Considered very productive	(3) Productivity score
College graduates	-0.00304	0.0940	0.0153
	(0.0803)	(0.0759)	(0.180)
	[0.970]	[0.218]	[0.933]
College graduates X From agency	-0.0506	-0.0955	0.304
	(0.105)	(0.0853)	(0.210)
	[0.631]	[0.265]	[0.150]
Observations	381	381	384
R-squared	0.544	0.483	0.782
Control worker char.	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Cluster at firm	Yes	Yes	Yes
Control mean	0.779	0.270	0.0660

Notes: This table compares characteristics of college educated and non-college educated applicants applying to the same position. The sample is restricted to Round 2 firms eligible for treatment with reservation wage at least 2,000 ETB, for which we observe all listed characteristics. All regressions include years after graduation and gender, control for firm fixed effects, and cluster at firm level. Dependent variables in Panel A: Column (1)—years of experience. Column (2), (3), (4)—whether applicant has zero experience, at least two years of experience, or matched experience with the position. Column (5)—whether the worker's father has at least 8 years of education. Column (6)—number of outside offers. Column (7)—whether any outside offer pays higher salary. Dependent variables in Panel B: Column (1)— whether the applicant is considered productive. Column (2)—whether the applicant is considered very productive. Column (3)—normalized productivity score generated. For applicants not attending interviews, we regress the perceived productivity on experience variables. For applicants attending interviews, we regress the perceived productivity on all measures. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C18: Applicants' Rejection of Interview Invites or Offers

VARIABLES	(1) Reject interview	(2) Reject interview	(3) Reject offer	(4) Reject offer
College graduate	0.0339	0.0457	-0.0539	-0.0557
	(0.0597)	(0.0823)	(0.0696)	(0.0764)
	[0.570]	[0.578]	[0.438]	[0.466]
Observations	1,007	851	754	681
R-squared	0.470	0.458	0.714	0.748
Control worker char.	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Cluster at firm	Yes	Yes	Yes	Yes
Control mean	0.0198	0.0198	0.0225	0.0225

Notes: This table presents whether college graduates are more likely to reject interview invites or offers compared to non-college workers. All regressions control for firm fixed effects and cluster at firm level. Column (1) and (2) only include applicants who receive the interview invite. Column (3) and (4) only include applicants who receive an offer. Column (2) and (4) also control for workers' experience, gender, and age. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C19: Effects on Future Hiring Plan

VARIABLES	(1) Hire from agencies	(2) Hire from other formal channels	(3) Hire from informal channels
Assigned to treat	0.0278	-0.0596	0.0692
	(0.0372)	(0.0398)	(0.0455)
	[0.457]	[0.138]	[0.133]
Observations	568	568	568
R-squared	0.327	0.426	0.424
Control baseline char.	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes
Control mean	0.0935	0.480	0.480

Notes: This table presents the treatment effects on what hiring channels firms plan to use in the future. The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Dependent variables: Column (1)—whether firms plan to hire from employment agencies. Column (2)—whether firms plan to hire from other formal channels (notice boards, newspaper, online job search platforms). Column (3)—whether firms plan to hire from informal recommendations (including informal brokers). Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C20: Effects on Hiring College Applicants By Likelihood of Receiving Extra Applicants

VARIABLES	(1) Interview College	(2) Interview Non-college	(3) Hire College	(4) Hire Non-college
Treated X Requesting college	-0.136*	0.102*	-0.191**	0.108**
	(0.0775)	(0.0573)	(0.0833)	(0.0539)
	[0.0829]	[0.0796]	[0.0245]	[0.0477]
Treated X Requesting college X Unlikely to receive extra	-0.0960	-0.0123	-0.0136	-0.0117
	(0.115)	(0.106)	(0.114)	(0.100)
	[0.409]	[0.908]	[0.905]	[0.907]
Treated X Not requesting college	0.113	0.0426	0.0284	0.00233
	(0.125)	(0.109)	(0.116)	(0.0951)
	[0.367]	[0.697]	[0.808]	[0.981]
Treated X Not requesting college X Unlikely to receive extra	-0.0940	-0.0453	-0.00206	0.00563
	(0.125)	(0.110)	(0.117)	(0.0948)
	[0.456]	[0.681]	[0.986]	[0.953]
Observations	581	581	581	581
R-squared	0.318	0.488	0.304	0.487
Control baseline char.	Yes	Yes	Yes	Yes
Business area FE	Yes	Yes	Yes	Yes
Cluster at business area	Yes	Yes	Yes	Yes
Control mean	0.399	0.427	0.375	0.412

Notes: This table presents the treatment effects on interviewing or hiring (non-)college applicants by the likelihood of receiving extra applicants. Only firms eligible for treatment with reservation wage at least 2,000 ETB are included in the regressions. We predict the likelihood of receiving extra applicants using all baseline characteristics, and interact the initial treatment assignment with whether the predicted likelihood is below average. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Dependent variables in Column (1) and (3) are whether firms interview or hire at least one college applicant by endline. Dependent variables in Column (2) and (4) are whether firms interview or hire at least one non-college applicant by endline. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C21: Effects on Monthly Salary

VARIABLES	(1) Salary All	(2) Salary All	(3) Salary College	(4) Salary College	(5) Salary College	(6) Salary Non-college	(7) Salary Non-college	(8) Salary Non-college
Assigned to treat	15.85	14.70	16.94	-4.037		-4.813	-6.520	
	(14.77)	(23.92)	(11.42)	(30.39)		(4.746)	(5.066)	
	[0.285]	[0.542]	[0.139]	[0.895]		[0.312]	[0.203]	
Lee bounds: Lower					4.761			-1.625
					(7.829)			(4.304)
					[0.543]			[0.706]
Lee bounds: Upper					11.62			2.221
					(11.38)			(7.576)
					[0.307]			[0.769]
Observations	170	137	214	180	627	245	221	627
R-squared	0.007	0.647	0.010	0.570		0.004	0.698	
Control baseline char.	No	Yes	No	Yes	No	No	Yes	No
Business area FE	No	Yes	No	Yes	No	No	Yes	No
Cluster at business area	No	Yes	No	Yes	No	No	Yes	No
Control mean	112.2	112.2	94.92	94.92	94.92	63.31	63.31	63.31
Sample	Request College	Request College	Eligible	Eligible	Eligible	Eligible	Eligible	Eligible

Notes: This table describes the treatment effects of employment agencies on monthly salary of the hired workers (in US dollars). The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. Dependent variables: Column (1) and (2), monthly salary in USD, including both college graduates and non-college workers; Column (3)–(5), monthly salary if hiring at least one college graduate; Column (6)–(8), monthly salary if hiring at least one non-college worker. Column (1), (3), and (6) do not include any controls and only compute robust standard errors. Column (2), (4), and (7) include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Column (5) and (8) compute Lee bounds of the treatment effects following Lee (2009). Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C22: Complier Analysis

VARIABLES	(1) Salary	(2) Voluntary quit	(3) Fired by firm	(4) Above-avg prod (surveyed)	(5) Above-avg prod (measured)	(6) Zero absent days	(7) Overtime work
$E[Y_c \mid H_c(1) < H_c(0)]$	55.4	.322	.0646	.525	.277	.534	.541
	(7.71)	(.108)	(.0461)	(.125)	(.167)	(.125)	(.126)
	[0.000]	[0.003]	[0.161]	[0.000]	[0.098]	[0.000]	[0.000]
$E[Y_n \mid H_n(1) > H_n(0)]$	124	.137	.0242	.629	.675	.599	.275
	(16.5)	(.114)	(.0643)	(.163)	(.275)	(.161)	(.158)
	[0.000]	[0.230]	[0.707]	[0.000]	[0.014]	[0.000]	[0.081]
Diff	-68.7	.185	.0404	-.104	-.398	-.0647	.266
	(17.6)	(.155)	(.0765)	(.211)	(.341)	(.212)	(.208)
	[0.000]	[0.231]	[0.597]	[0.622]	[0.244]	[0.760]	[0.200]

Notes: This table presents the complier analysis following Abadie (2003). The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. Endogeneous variables: Whether firms hire any college graduates (H_c), and whether firms hire any non-college workers (H_n). Instrument: Interaction of initial treatment assignment and baseline request for college graduates. Potential outcomes: Column (1)—Monthly salary (USD). Column (2)—Whether the hired workers voluntarily quit within 5 months. Column (3)—Whether the hired workers are fired by firms within 5 months. Column (4)—Whether the hired workers are considered to be more productive than average workers on the similar positions. Column (5)—Whether the efficiency measures of hired workers are above those of similar workers (only in Round 2). Column (6)—whether the hired workers have zero absent day in the last 30 days. Column (7)—whether the hired workers work overtime in the last 7 days. Standard errors are shown in parentheses; p-values are shown in brackets. Standard errors are shown in parentheses; p-values are shown in brackets. Significance level: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

D Model

Belief $\widetilde{a}_j$ as a function of arrival rate q. Suppose firm j's prior of the college premium follows a distribution $F_j(\cdot|I_j^0)$, where I_j^0 is a set of college graduates that firm j observes in the past, and the mean of the distribution is $\widetilde{a}_j^0$. In each period, with probability q, firm j matches with a college graduate i and observes a signal of worker i's productivity $\mu + a_i$, where a_i draws from a given distribution of college premium with mean a_0. Firm j's information set thus becomes $I_j^i = I_j^0 \cup \{a_i\}$ if matched with worker i. The expected belief $\widetilde{a}_j$ can thus be expressed in the following way:

$$\widetilde{a}_j = (1-q)\widetilde{a}_j^0 + q\,\mathbb{E}\left[\mathbb{E}_j[a|I_j^i]\right] = \widetilde{a}_j^0 + q(\mathbb{E}\left[\mathbb{E}_j[a|I_j^i]\right] - \widetilde{a}_j^0)$$

Suppose firm j is initially over-optimistic about average college premium. Any learning model that generates $\widetilde{a}_j^0 > \mathbb{E}\left[\mathbb{E}_j[a|I_j^i]\right]$ would lead to a negative correlation between $\widetilde{a}_j$ and q. One can use a Bayesian learning model and derive $\mathbb{E}\left[\mathbb{E}_j[a|I_j^i]\right] \in (a_0, \widetilde{a}_j^0)$, hence more accurate beliefs with higher arrival rate q. Other non-Bayesian learning models can also generate the same predictions. For example, firm j may over-interpret one signal and drop the belief lower than the reality, *i.e.*, $\mathbb{E}\left[\mathbb{E}_j[a|I_j^i]\right] < a_0$. Similarly, with the same assumptions on learning models, belief $\widetilde{a}_j$ becomes a positive function of arrival rate q if firm j is initially over-pessimistic about the average college premium.

Proof of Proposition 5.1. Without loss of generality, we look at firms at the threshold θ^* where they are indifferent between hiring a college graduate or a non-college educated worker and whose belief is $\widetilde{a}_j$:

$$\theta^* = \frac{c}{(1-\beta)\widetilde{a}_j}$$

With the new search technology, firms at the threshold would switch to hiring a non-college educated worker if θ^* increases:

$$\theta^{*\prime} = \frac{c - \Delta c}{(1-\beta)(\widetilde{a}_j - \Delta\widetilde{a}_j)} > \frac{c}{(1-\beta)\widetilde{a}_j}$$

Hence the sufficient condition $|\Delta\widetilde{a}_j/\widetilde{a}_j| > |\Delta c/c|$.

Heterogeneity by post exposure. Suppose firm j's initial information set I_j^0 can be characterized by the number of college graduates in the past, n_j^0, and the initial mean $\widetilde{a}_j^0$. We impose the following assumption on firm j's learning of the college premium:

$$\frac{\partial |\,\mathbb{E}\left[\,\mathbb{E}_j[a|I_j^i]\right] - \widetilde{a}_j^0|}{\partial n_j^0} < 0 \tag{3}$$

Intuitively, this assumption imposes a decreasing return to learning. If firm j observes many college graduates in the past, having one more college graduate would not contribute to large update. This assumption encompasses a wide range of possible structures on $\widetilde{a}_j^0$ and $F_j(\cdot|I_j^0)$. Now we can derive the following proposition:

Proposition D.1. *Suppose firm j is initially over-optimistic of the college premium and condition 3 holds. $|\Delta\widetilde{a}_j/\widetilde{a}_j| - |\Delta c/c|$ decreases in the past exposure to college graduates n_j^0.*

Proof. One can rewrite the percentage changes in $\widetilde{a}_j$ and c in the following way:

$$\Delta\widetilde{a}_j/\widetilde{a}_j = \epsilon_{\widetilde{a}_j,q} \cdot \Delta q/q$$
$$\Delta c/c = \epsilon_{c,q} \cdot \Delta q/q$$

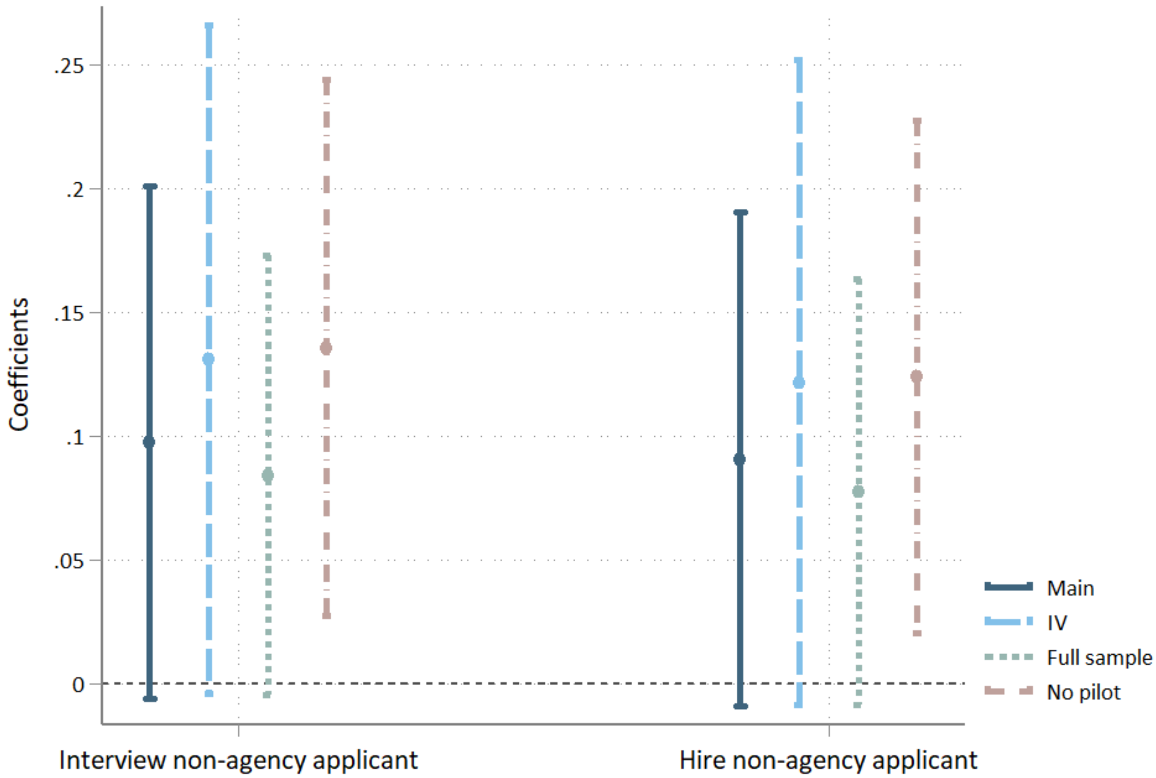
$\epsilon_{\widetilde{a}_j,q} = \partial\widetilde{a}_j/\partial q \cdot q/\widetilde{a}_j$ is the elasticity of belief $\widetilde{a}_j$ with regard to q, and $\epsilon_{c,q} = \partial c/\partial q \cdot q/c$ is the elasticity of search cost c with regard to q. With the standard DMP model, the elasticity $\epsilon_{c,q}$ always equals -1. One only needs to examine whether $|\epsilon_{\widetilde{a}_j,q}|$ decreases in n_j^0. From $\widetilde{a}_j(q_j) = \widetilde{a}_j^0 + q_j(\mathbb{E}\left[\,\mathbb{E}_j[a|I_j^i]\right] - \widetilde{a}_j^0)$, we have:

$$\epsilon_{a_j,q} = \frac{1}{1 + \frac{a_j^0}{q_j(\mathbb{E}\left[\,\mathbb{E}_j[a|I_j^i]\right] - \widetilde{a}_j^0)}}$$

Therefore, $|\epsilon_{\widetilde{a}_j,q}|$ increases in $|\,\mathbb{E}\left[\,\mathbb{E}_j[a|I_j^i]\right] - \widetilde{a}_j^0|$. Given the additional assumption in Condition 3, we have $|\epsilon_{\widetilde{a}_j,q}|$ decreasing in n_j^0, hence the proposition. Together with Proposition 5.1, we can derive the prediction on hiring behavior regarding past exposure to college graduates.

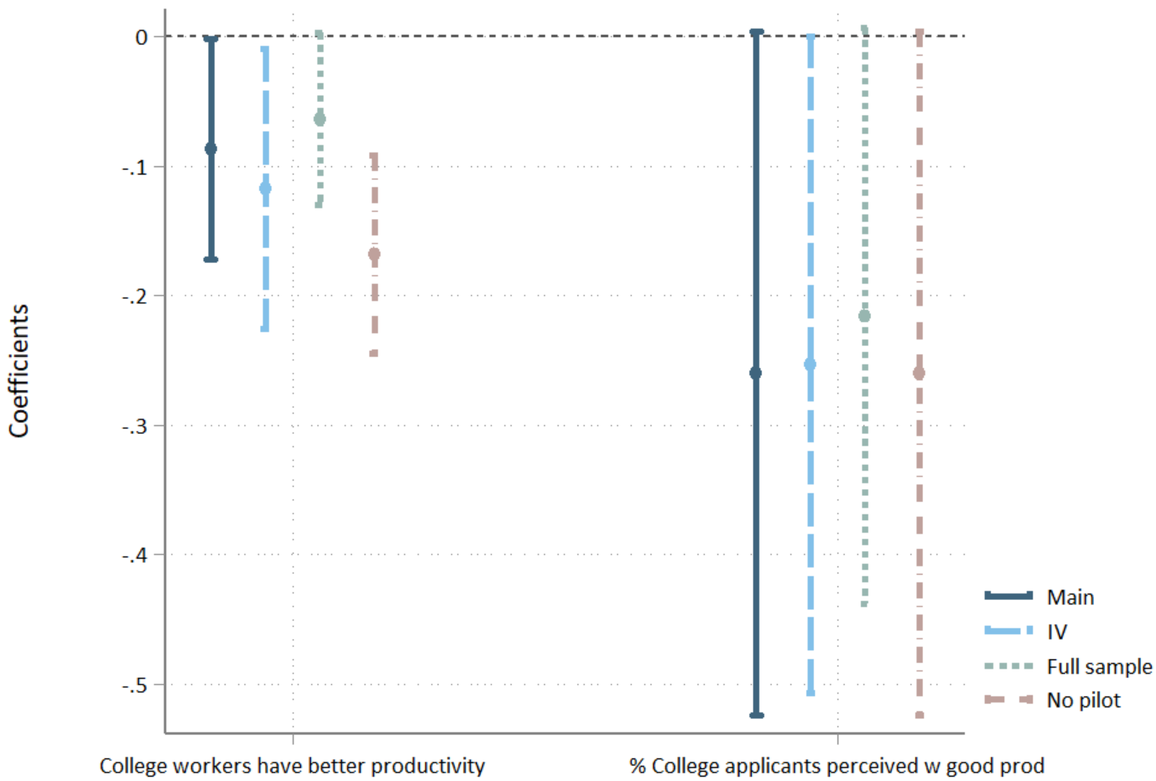
E Replications

Figure E1: Replication of the Effects on Hiring Non-Agency Applicants



Notes: This figure replicates the main results in Column (2) and (5) in Table 3. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. For each dependent variable, we show (1) reduced-form estimate from the main specification, (2) IV estimate on the actual treatment status, (3) reduced-form estimate using full sample, and (4) reduced-form estimate excluding pilot sample. 95% confidence intervals are shown.

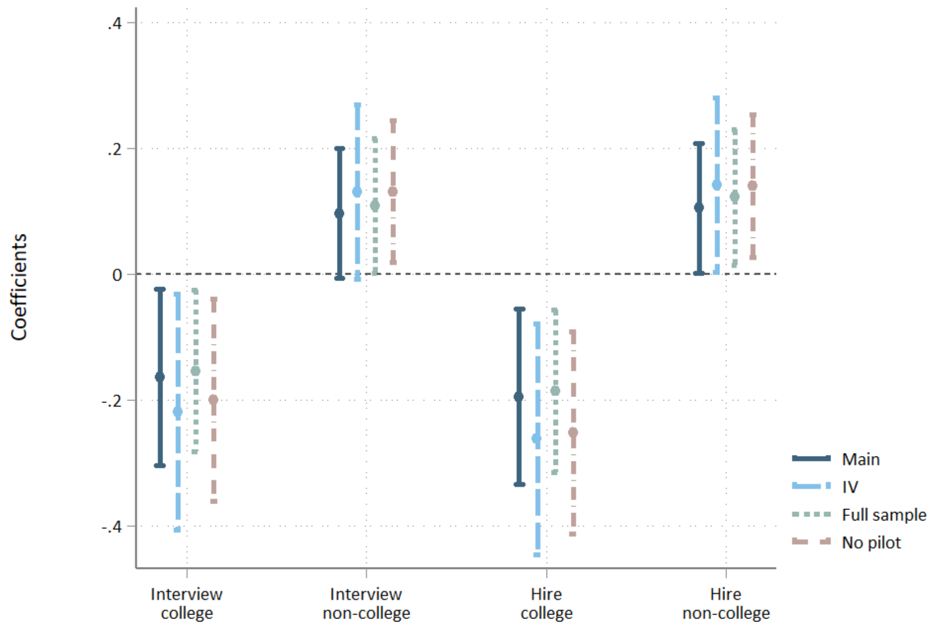
Figure E2: Replication of the Effects on Perceptions



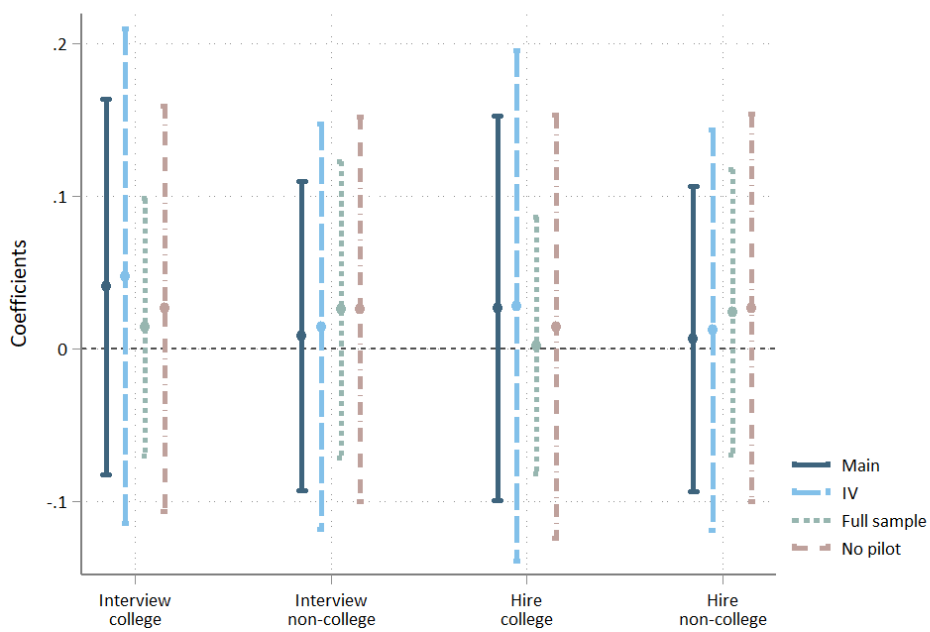
Notes: This figure replicates the main results in Table 4, Column (1) and (3). All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. For each dependent variable, we show (1) reduced-form estimate from the main specification, (2) IV estimate on the actual treatment status, (3) reduced-form estimate using full sample, and (4) reduced-form estimate excluding pilot sample. 95% confidence intervals are shown.

Figure E3: Replication of the Effects on Hiring by Baseline Request

Panel A. Heterogeneous effect on firms requesting college graduates



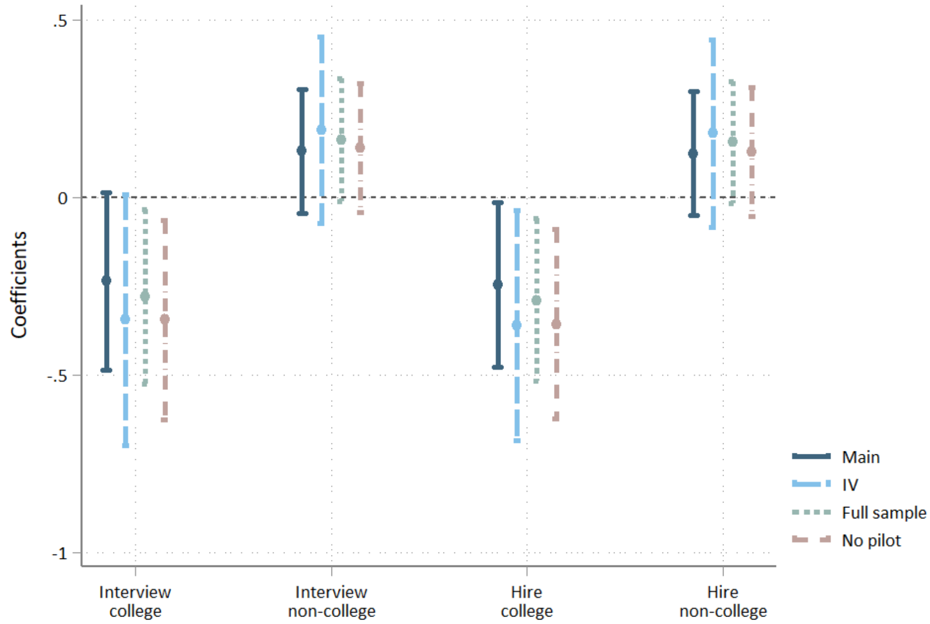
Panel B. Heterogeneous effect on firms not requesting college graduates



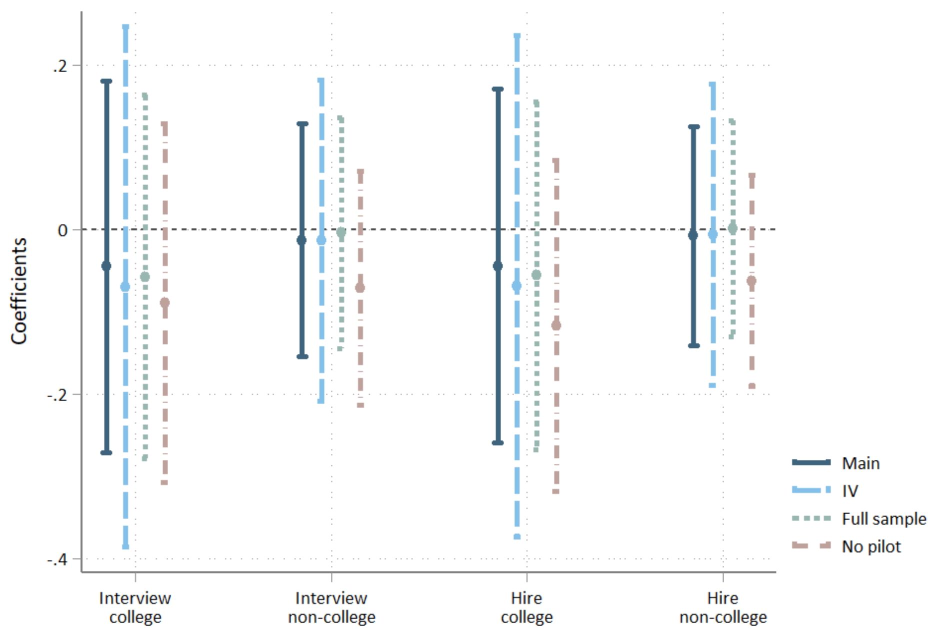
Notes: This figure replicates the main results in Table 5, Panel B. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. For each dependent variable, we show (1) reduced-form estimate from the main specification, (2) IV estimate on the actual treatment status, (3) reduced-form estimate using full sample, and (4) reduced-form estimate excluding pilot sample. 95% confidence intervals are shown.

Figure E4: Replication of the Effects on Hiring by College Share

Panel A. Heterogeneous effect on firms with below-median college share

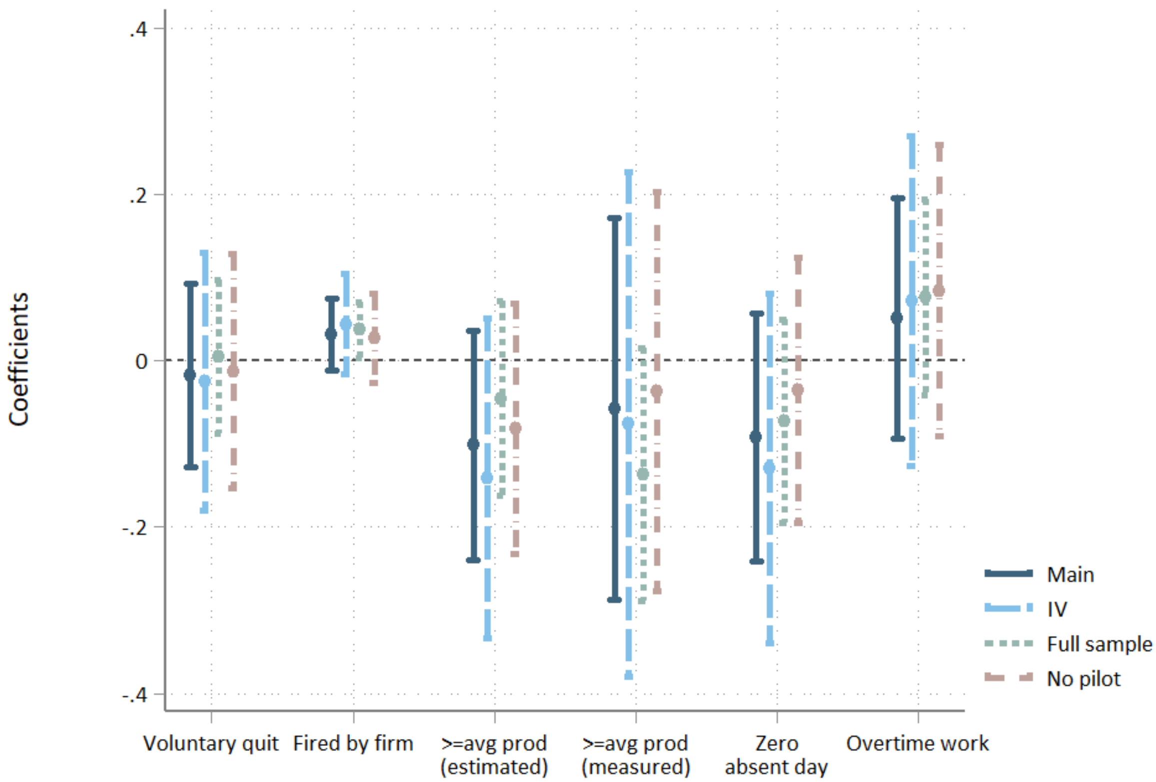


Panel B. Heterogeneous effect on firms with above-median college share



Notes: This figure replicates the main results in Table 6. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Only firms requesting college graduates at baseline are included. For each dependent variable, we show (1) reduced-form estimate from the main specification, (2) IV estimate on the actual treatment status, (3) reduced-form estimate using full sample, and (4) reduced-form estimate excluding pilot sample. 95% confidence intervals are shown.

Figure E5: Replication of the Effects on Match Quality



Notes: This figure replicates the main results in Table 7. All regressions include a full set of baseline characteristics from Table 1, control for business area fixed effects, and cluster at business area level. Only firms requesting college graduates at baseline are included. For each dependent variable, we show (1) reduced-form estimate from the main specification, (2) IV estimate on the actual treatment status, (3) reduced-form estimate using full sample, and (4) reduced-form estimate excluding pilot sample. 95% confidence intervals are shown.