Title
Essays in Development Economics

Author
Wu, David Qihang

Publication Date
2024

Peer reviewed|Thesis/dissertation

Essays in Development Economics

By

David Qihang Wu

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Edward Miguel, Chair
Professor Supreet Kaur
Professor Benjamin Schoefer
Professor Christopher Walters

Spring 2024

Essays in Development Economics

Abstract

Essays in Development Economics

by

David Qihang Wu

Doctor of Philosophy in Economics

University of California, Berkeley

Professor Edward Miguel, Chair

This dissertation is comprised of three essays in Development Economics. Chapter 1 and 3 jointly explore the underlying causes of the labor market frictions faced by firms in the developing countries. Chapter 2 provides an interdisciplinary perspective to understanding the origins of militias in conflict-prone contexts of developing countries.

My first chapter, titled *Search Frictions, Belief Formation, and Firm Hiring in Ethiopia* (coauthored with Sam Wang), examines how 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.

My second chapter, titled *Social Origins of Militias: The Extraordinary Rise of "Outraged Citizens"* (coauthored with Gauthier Marchais, Christian Mastaki Mugaruka, and Raúl Sánchez de la Sierra), uses a sharp withdrawal of the state that precipitated the emer-

gence of a prominent militia in the Democratic Republic of the Congo to analyze the role of community in the rise of militias. First, the state withdrawal drastically increased membership into the militia, predominantly driven by various *social* motivations and, to a much lesser extent, *private* economic motivations. Second, its extraordinary nature is explained by the response to the drastic rise in insecurity it created, and driven mostly by individuals' *intrinsic* social motivation to protect their community, but also *extrinsic* social motivations such as status concerns and social pressure. Third, the response to insecurity is in part explained by elite-driven informal community institutions' response, which engineer extrinsic social motivations and amplify pre-existing intrinsic ones. Our findings suggest that social motivations towards the community play a central role in the rise of militias, and nuance the distinction between economic and noneconomic incentives, showing that a range of social motivations, extrinsic, are engineered by community institutions to promote militia rise; given the later predatory turn of the militia, our findings emphasize how state weakness and social motivations can trigger communities to create security capacity that persists and can be later used opportunistically.

Finally, my third chapter (coauthored with Maximiliano Lauletta) examines the underlying causes of turnover in the manufacturing sector. Many developing countries are undergoing a rapid process of industrialization, yet many workers tend to quit early from large-scale manufacturing firms, which constitutes a major challenge for firms to sustain their operation. We study three potential causes of high turnover rates in the context of a flagship industrial park in Ethiopia: misperceptions of the job aspects in the manufacturing firms, temporary income shock, and sorting based on workers' productivity types. To understand the effect of misperceptions, we collect detailed measures of misperceptions from 1,203 new workers regarding 14 quantifiable job aspects, combined with the administrative records of turnover. We further conduct an intervention where we provide accurate information on the key job aspects of career progression and examine how misperceptions causally affects workers' turnover decisions. Correlational and causal evidence suggest that misperceptions can only explain a small proportion of early turnover rates (0.3–5% of total variation). We further examine the heterogeneous treatment effect to provide suggestive evidence for the other two causes. Among treated workers with high-level of misperceptions, workers more subject to temporary income shock do not quit more, suggesting temporary income shock may not be able to explain the high turnover rates. However, workers with high productivity type, proxied by high educational attainment and high dexterity level, are less likely to quit. Our results suggest that turnover may reflect an equilibrium outcome where workers with low productivity choose to quit when they realize their productivity type, which potentially benefits firms if the productivity premium of the workers who stay may compensate the productivity loss from those who quit.

To Qiye, who just had his first drink legally two months ago

Contents

List of Tables

List of Figures

Acknowledgments

I would like to express my deepest gratitude to the members of my dissertation committee who have been extraordinary mentors to me over the last seven years. First my advisor, Ted Miguel, who is my role model in academia with his commitment to policy-relevant research and his utmost kindness. Ted has been encouraging me and brainstorming so many brilliant ideas with me since my first year of Ph.D., without whose support I will not have been able to conduct my fieldwork in Ethiopia during the difficulty years since COVID-19. I am truly grateful for Supreet Kaur for her deep insights and sharp economic intuition. Her advice on my projects has shaped my taste for economic research and my research approach in development economics. I would also like to thank Chris Walters, who has been providing crucial advice from the perspective of labor economics from the very beginning of my projects. I have learned so much from Chris about how to rigorously implement causal inference techniques. Finally, I am greatly indebted to Benjamin Schoefer, who is very generous with his time and provides frequent check-ins before and around the job market. I have benefited immensely from Benjamin's knowledge of macroeconomic labor literature, his generous time for meeting and extremely helpful advice on the multiple versions of my job market paper, and, last but not least, my first business-class ticket after one of my flyouts.

I would also like to thank the many other professors who have provided helpful advice and encouragement over the last seven years. My journey at UC Berkeley started with being an undergraduate exchange student in the Political Economy class by Fred Finan, who has been providing his longstanding support and mentorship ever since then. I have learned so much from Stefano DellaVigna, Jeremy Magruder, and Ricardo Perez-Truglia for their guidance on developing fieldwork and survey design. I have benefited greatly from the conversations with Sydnee Caldwell and Pat Kline for their deep insights in labor economics. I would like to give my special thank you to Raúl Sánchez de la Sierra, my first research mentor, a long-time coauthor, and a very good friend, whose passion for research, detail-focused rigor, and curiosity for a wide variety of topics have significantly influenced me to become the researcher I am today.

This Ph.D. journey would not have been possible without the friendship and support from my wonderful classmates and colleagues. I am forever grateful to Luisa Cefala, Maddie Duhon, and Nick Swanson, the best classmates and support group one can hope for when trying to conduct any fieldwork during the difficult time of COVID-19 and going through one of the most difficult job markets for development economics. I want to thank my coauthors Maximiliano Lauletta, Miguel Ortiz, and Sam Wang, who are as much of a dear friend to me as an excellent scholar to work with. I want to thank Lukas Leucht, Dominik Jurek, Joan Martínez, Jacob Weber, and Vita Yaremko, for the numerous nice restaurants we explore in the Bay Area and the dearest friendship we have developed. So many happy hours and happy moments with the Berkeley development community, Stephanie Bonds, Christina Brown, Wei Lin, Jimmy Narang, and Ao Wang. I also want to thank my long-time friends from college, Cuimin Ba, Tony Qiaofeng Fan, Can Huang, Qingyang Huang, Anran Li, Sizhu Liu, and Yunbo Liu, with whom we started our academic journey at Peking University and all the way to United States. I also want to express my gratitude to my Ethiopian colleagues, Esayas Ayele, Endale Gebremedehen, Belay Mulat, Fekadu Nigussie, Eyoual Tamrat, whose support and companion have created many wonderful memories of Ethiopia. Special thank you to Esayas Ayele, one of the best colleagues I have ever worked with, who I owe so much to his instrumental role in the data collection for my job market paper as well as his fine taste for Ethiopian cuisine.

Special thank you to Center for Global Action at UC Berkeley, who has been extremely generous in financially supporting the data collection for my projects at the early stage and offering many opportunities to disseminate my work. I am also deeply grateful for the generous support from the Strandberg Fund at the Department of Economics and the Center for African Studies at UC Berkeley, International Growth Center, and Private Enterprises for Low and Middle Income Countries.

I am beyond thankful to my parents, Zhiwei Wu and Xuxia Wan, who never went to college but have unconditionally supported my decision of pursuing a Ph.D. from the very beginning. My father's business experience in Guangdong, China, and my mother's insights in human psychology, have always been one of the major inspirations for my research. I also miss my time in Berkeley with my brother, Qiye, who spent four Christmases with me in a small apartment in Berkeley playing Nintendo together, and is on his way of becoming a bright mechanical engineer. Last, I am extremely proud of my dear Steven, a world-class artist and an even better human being, with whom I share the happiest moments over the last four years, won his family Alphagetti Scramble contest with the word "heteroskedasticity".

Chapter 1

Search Frictions, Belief Formation, and Firm Hiring in Ethiopia

1.1 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., 2024; 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, 2023; 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., 2024; 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, 2023), 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, 2024; Méndez and Van Patten, 2022; Li et al., 2023) or administrative data (Caldwell and Danieli, 2024; 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 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]

---

[1] In particular, there are two papers that discuss the interplay of search cost and learning cost. Banerjee and Sequeira (2023) 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.

[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., 2022; 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.

1.2 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 1.1, 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.

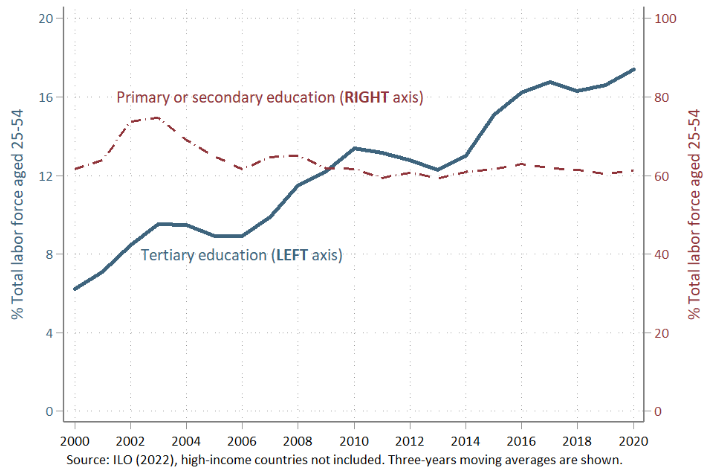
Less is certain, however, about the quality of education. For non-tertiary education, researchers find mixed effects of investment in schools on education quality (Evans and Mendez Acosta, 2021; Kremer et al., 2013; Kremer and Holla, 2009).[4] For tertiary education, Martellini et al. (2022) investigate 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 1.1, 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 A.1. Evidence depicts an ambiguous, if not deteriorating, return to college in low- and middle-income countries.[5]

[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.

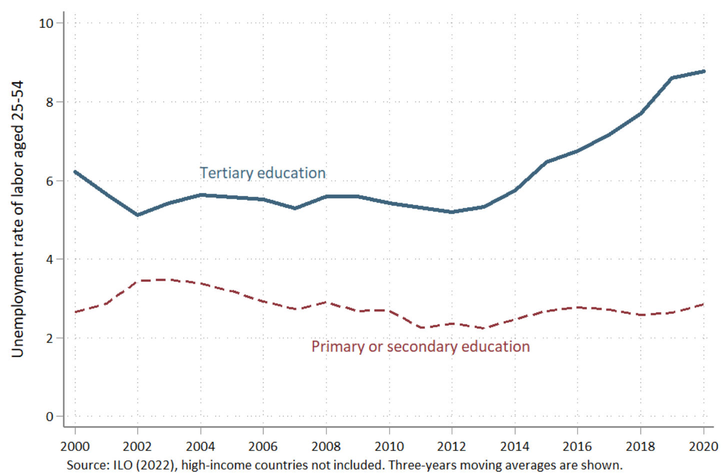
[5]The ILO database harmonizes the unemployment statistics across countries and time according with one standard of unemployment: not in employment, seeking employment, and currently available to take

Figure 1.1: 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.

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 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, 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.

1.2.1 Labor Market Frictions 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 1.2, 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.

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 (International Labor Organization, 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.

[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`.

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 1.2, 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 productivity, either through the value-added to human capital (Becker, 1964) or through the selective procedure of tertiary education (Spence, 1973).

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 1.3 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 A.2 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]

1.2.2 Employment Agencies and Labor Market Frictions

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

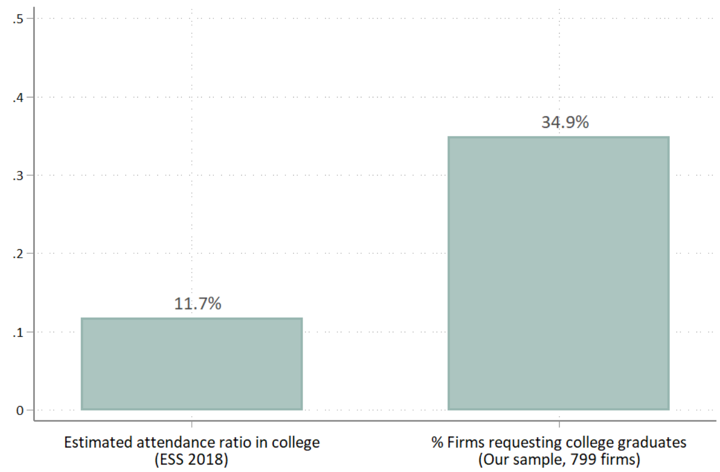
[8] In the baseline, we ask firms how they usually post jobs. 46% firms post jobs on notice boards, 45% ask for recommendations through personal networks, and 35% find workers through informal brokers. Only less than 13% post jobs on any online job platforms, and 8% seek help from employment agencies.

[9] Furthermore, Figure A.3 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.

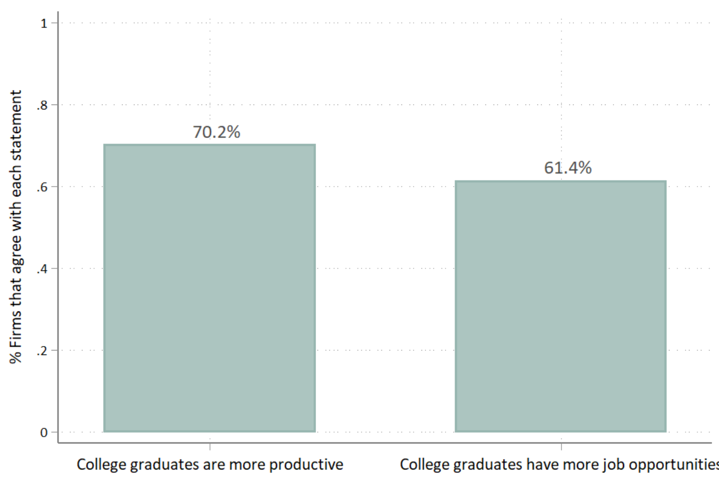
[10] In 2018, the new Ethiopian government issued an initiative to encourage qualified brokers to register in the government in hope for boosting private and formal employment. To qualify for registration, an

Figure 1.2: Demand for College Graduates

Panel A. Percentage of firms requesting a college graduate



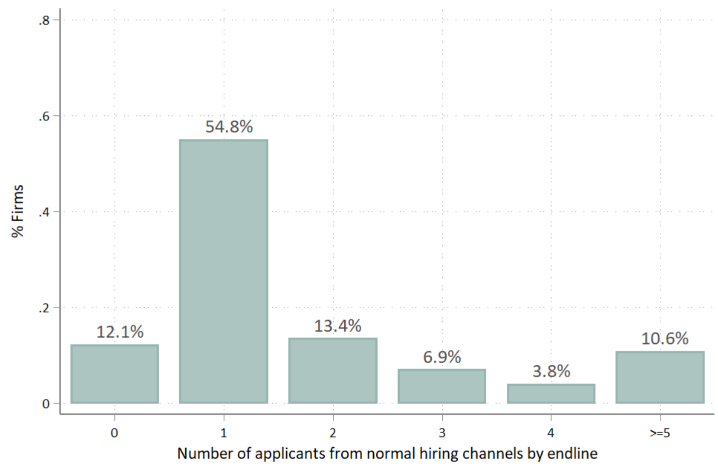
Panel B. Perceptions of college graduates



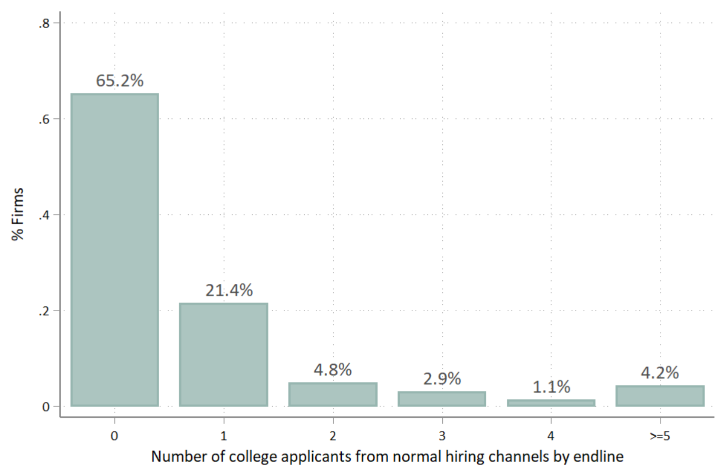
Notes: This figure presents firms' demand for college graduates. Panel A shows the estimated attendance ratio of tertiary education from Ethiopian Socioeconomic Survey in 2018, as a proxy for the percentage of labor force with a college degree, and the percentage of firms that request a college graduate 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 1.3: 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.

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 A.4 shows a representative employment agency. Figure A.5, 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 A.3, 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 A.3, 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 with networking, interview preparation or CV writing. This corroborates our observation

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 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 A.5, 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.

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.

1.3 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.

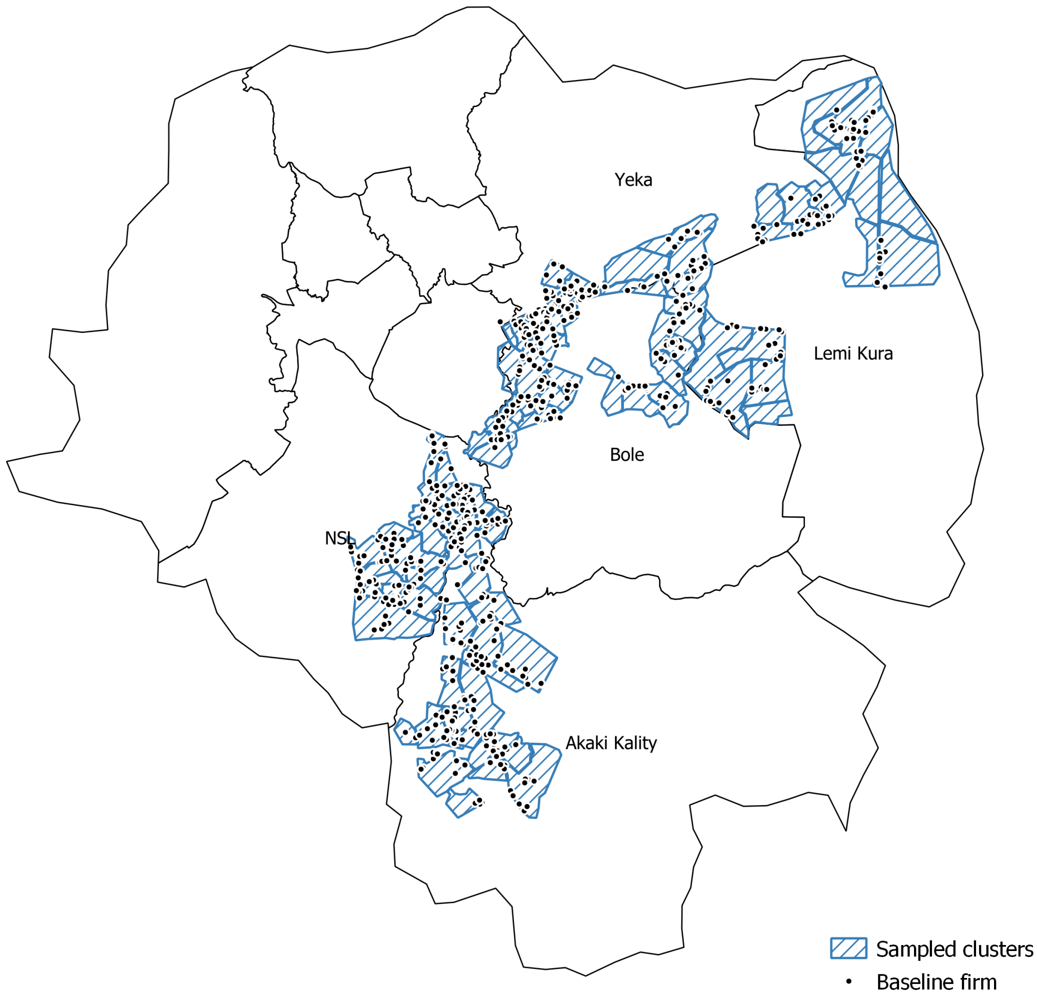
1.3.1 Sampling of Firms

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 1.4 shows the geographic distribution of 88 sampled business areas and 799 firms selected for the baseline survey.[13]

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 A.4, 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.

[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.

Figure 1.4: 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.

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 A.4, 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.

1.3.2 Intervention Leveraging Employment Agencies

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 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

[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.

[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

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 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.

1.3.3 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

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.

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 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. Appendix A.1 describes the construction of key variables.

We predominantly use firm-reported data in the main analysis. To validate the accuracy of the data especially on applicants, in Figure A.6, 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 1.5, 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]

---

[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.

[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 1.4.3 and alternative mechanisms in Section 1.5.6.

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 1.5, 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 A.7 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.

1.4 Effect of Employment Agencies on Hiring

1.4.1 Specification

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

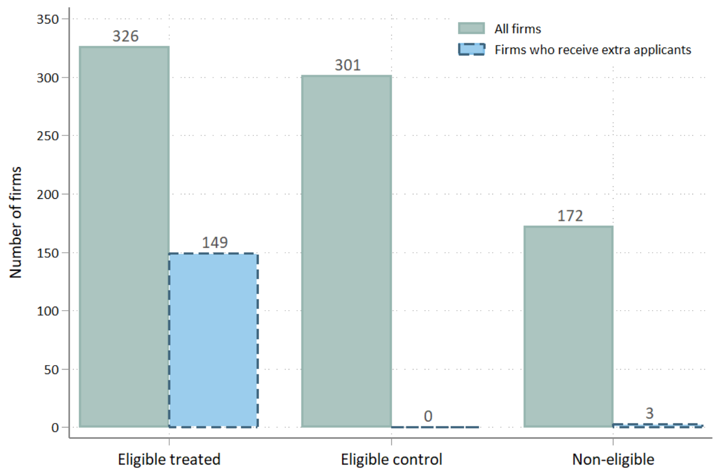
$$Y_{jc} = \alpha_c + \beta T_{jc} + \delta X_{jc} + \epsilon_{jc} \tag{1.1}$$

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. We replicate all main results by including non-eligible firms in the control group in Appendix A.3. Table 1.1 shows the balance between eligible firms initially assigned to treatment and control groups across all baseline characteristics.

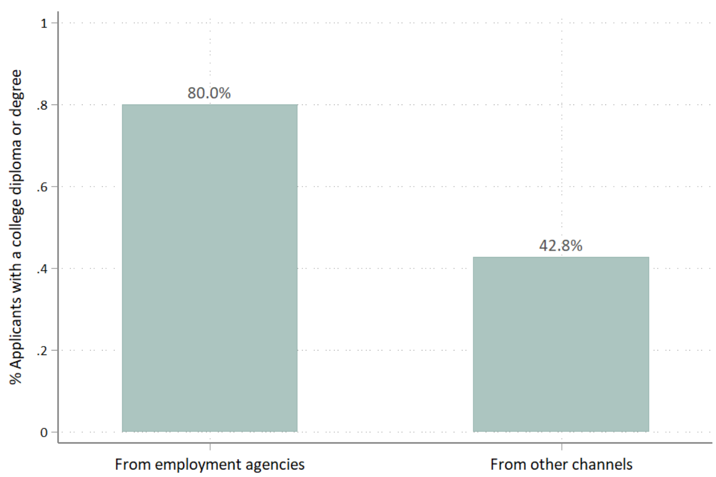
Given that not all firms assigned to treatment receive extra applicants, Specification 1.1 obtains an intention-to-treat (ITT) estimate of the effect of receiving extra applicants from the employment agencies. In addition, the actual treatment status is not exactly equal to the initial treatment assignment during the first two weeks of piloting due to logistical

Figure 1.5: 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.

Table 1.1: 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.

constraints.[22] To address the potential bias caused by the non-compliance, we conduct two additional replication exercises in Appendix A.3: 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 1.1.

1.4.2 Effect on Successful Matches

We first confirm the treatment effect on receiving extra applicants from the employment agencies in Table A.6, a replication of Figure 1.5. 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 1.2 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

[22]Table A.5 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 1.4.3.

[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 A.7 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.

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 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 A.3.[24]

However, we find that the treatment effect is mainly driven by applicants from non-agency hiring channels. Table 1.3 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 A.8 replicates the results on non-agency applicants using different samples and treatment status and finds robust estimates. In addition, in Table A.9, 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.

1.4.3 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 A.10. Column (2) does not cluster the standard errors at the level of business area. The standard errors are slightly higher than

[24]In Table A.8, 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.

Table 1.2: Effect 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 1.1, 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 1.3: Effect 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 1.1, 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: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

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 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

[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

remain significantly positive.

Second, we examine whether attrition of firms affects the main results systematically. Table A.11, 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 A.12 conducts this exercise. Column (1) and (5) present the reduced-form estimates and show that firms receiving agency 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

serves a robustness check of the estimation efficiency. We do not use this assumption in any other analysis.

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 A.5. In Table A.13, 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 A.14, 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 A.14, 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 A.9 further varies the length of radius and replicates this exercise. We do not find differential treatment effects in any specification.

1.5 Learning Mechanism

From Section 1.4, 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 A.15 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% increase compared to control firms. Such an increase is more salient among firms requesting a college graduate at baseline, by 0.602 more college educated applicant; for firms not requesting a college graduate, treated firms receive 0.148 more college educated applicant although not significantly. We do not observe that treated firms receive more non-agency college educated applicants, suggesting our intervention does not alter firms' effort of searching for college educated applicants. We also do not find evidence that employment agencies provide more non-college educated workers. Thus, treated firms may observe more college educated applicants and obtain information of their productivity, especially for firms that request a college graduate at baseline.

1.5.1 Updates 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 1.4, 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 1.4, Column (3) shows that among treated firms, college graduates are 32.1

[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.

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 A.10 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 employment agencies.

1.5.2 Conceptual Framework: Search Frictions Hinder Learning

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]

Given that Table A.15 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.

[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

Table 1.4: Effect on the Perceptions of College Graduates' Productivity

VARIABLES	(1)	(2)	(3)	(4)
	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 1.1, 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: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

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 A.2 shows that from a large class of learning 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:

$$(1-\beta)\widetilde{a}_j(q)\theta_j \geq c(q) \tag{1.2}$$

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 1.5.1. *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 1.5.1, 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.

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.

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 A.2, 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.

1.5.3 Effect on the 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 1.5, 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 1.5.2, 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 1.5, 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 A.11 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 A.16, 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.

1.5.4 Heterogeneity by the Exposure to College Graduates

We now examine the third prediction from Section 1.5.2. 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 A.17 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 1.6, Panel A between the college share and the percentage of firms hiring at least one college

Table 1.5: Effect 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 1.1, 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: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

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 1.6 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 A.12 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 A.18 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.

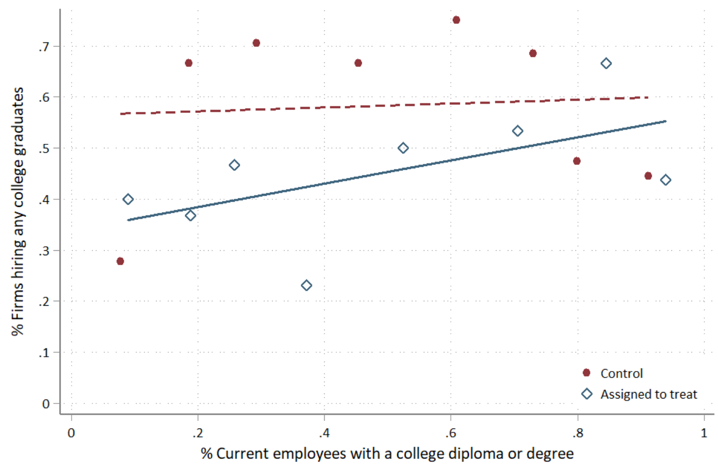
1.5.5 Signals from College Educated Applicants

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,

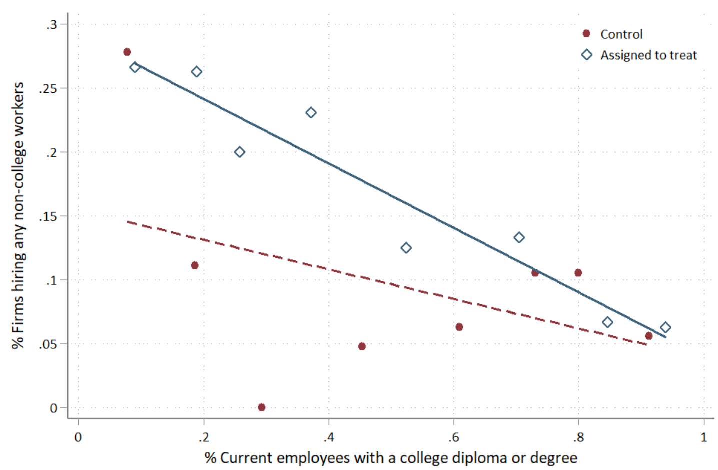
[30] In Figure A.13, we replicate the results on hiring a college graduate or a non-college educated worker among firms that request a college graduate at baseline using different cutoffs of college share (50–90 percentile). The patterns remain largely similar regardless of which percentile is selected as cutoff.

Figure 1.6: 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.

Table 1.6: Heterogeneous Effect 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 1.1, 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: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

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 A.19, 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.

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 A.19, 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.

1.5.6 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 A.20 shows 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

[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.

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 A.15. 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 A.12 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 A.21. 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 1.5.5 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 A.22 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.

1.6 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.

1.6.1 Salary

We first apply the same specification in Equation 1.1 to estimate the treatment effect on monthly salary among firms requesting a college graduate at baseline. Table A.23, 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 A.9. 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 A.14 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 A.24, 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 worker.[33]

1.6.2 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

[32]Table A.23, 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.

[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.

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 1.7 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 A.15 replicates the results using different samples and treatment status and finds robust estimates. We further conduct complier analysis on all the six measures of match quality in Table A.24, 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.

Table 1.7: Effect 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 1.1, 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: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

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 A.16 suggest that complier firms may not enjoy as much productivity gain from hiring a college graduate on non-skilled, manual, or routine tasks. To summarize, our evidence suggests a net increase in firm profit for the complier firms.

1.7 Conclusion

We leverage a new type of employment agency in Addis Ababa, Ethiopia, that mainly increases the arrival rate of college graduates, and conduct a randomized control trial on 799 firms, to understand how lower search frictions affect hiring decisions. We first find that firms initially assigned to treatment are 17.5% more likely to hire at least one worker to the position by midline, but the majority of the new hires do not come from the employment agencies, which cannot be explained by a simple decrease in search frictions. Instead, we find that treated firms are less likely to believe college graduates are more productive, suggesting that firms may obtain more information from the college educated applicants, but what they learn makes them less optimistic of the productivity of college graduates. Consistent with the conceptual framework where lower search frictions induce learning, we find that treated firms requesting a college graduate at baseline are less likely to hire a college graduate and more likely to hire a non-college educated worker. Such treatment effects are stronger among firms with less exposure to college graduates. The comparison between college educated and non-college educated applicants for the same vacancy suggests that firms may not observe signals of high college premium from the college educated applicants. Last, we do not find 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.

Chapter 2

Social Origins of Militias: The Extraordinary Rise of "Outraged Citizens"

2.1 Introduction

While nonstate militias have been important across history (Tilly, 1985), we know very little about the factors leading to their emergence because, as nonstates, they leave little evidence. A growing literature has analyzed the economic trade-offs they face as a unitary actor while taking their existence as given (Sánchez de la Sierra, 2020). Yet, that they are nonstate does not mean that they emerge from individuals acting in a vacuum. Indeed, a rich literature has documented the role of society, and of community in particular, in governing human life beyond states, through social institutions and social emotions (Greif, 1993; Ostrom, 1999; Bowles and Gintis, 2011).

The extraordinary emergence of the Raia Mutomboki ("Outraged Citizens," in French) in 2011 in the Democratic Republic of the Congo (henceforth, *DRC*) offers a "critical juncture" (Callen et al., 2023) illustrating the role of social emotions and of communities in the rise of militias. In 2011, Marcellin Chishambo, the governor of South Kivu province, travelled to Shabunda district to address mounting concerns that the recent sudden withdrawal of state forces from the area led to insecurity created by the Front de Libération du Rwanda (FDLR), a Rwandan armed group that has prayed upon civilians in DRC since the nineties. Instead of offering security guarantees, the governor told the population to take their security into their own hands. Shortly after, a large militia, the Raia Mutomboki (henceforth, *Raia*), emerged through popular mobilization and achieved what the state security forces had been unable to achieve for decades: chasing the FDLR.

In this paper, we analyze the rise of the Outraged Citizens in 2011 to interrogate the role of society and community in their success. After empirically examining whether they are comparable to other militias in the conflict, especially with regards to their relationship to the community, we answer the following research questions, leveraging their rise to establish a "proof of concept." Alongside the now well-understood individual economic motivations, can the rise of militias reflect *social* motivations? What are those social motivations, and what are their origins? What role do informal community institutions play in social motivations and, ultimately, in militias' rise?

The key input into our analysis, which allows us to tackle this challenge, is a unique panel dataset on armed groups, institutions, and households dating back to 1990, reconstituting historical events in 239 villages in North and South Kivu, two of the most conflict-affected provinces of the DRC. The dataset contains 707 episodes of armed groups' governance in 239 villages and details on each of 874 violent events and recruitment campaigns. It also contains histories of 7,454 individuals, obtained through interviews carried out with 2,964 households, with details on armed group participation by 640 combatants. In addition, we also gathered *anonymized* village-year-level number of participants into armed groups, that we reconstructed with local history experts across the villages and towns of South Kivu. The data about the Raia were gathered at the time of its creation, allowing to reduce survivor bias. To obtain this information, we obtained the approval of the relevant authorities to ask questions in surveys across their district. Individual reports of participation almost exactly match village participation numbers. This is consistent with our qualitative data, which suggested that participating in the Raia is a local recent phenomenon and is not taboo. This corroboration is the starting point of our analysis.

We begin our analysis by presenting three descriptive facts about the militias in the conflict and the Raia in particular. These facts underscore that, rather than being an anomaly in the conflict, the Raia are a paradigmatic case of a militia with regards to the intimate ties between community and militias. This motivates our focus on the shock that led to the rise of the Raia in the rest of the paper. *Fact 1.* Militias predominate the conflict and the Raia is a major militia. Of 76 armed groups for which we have data, 63% were a militia (and one of these was the Raia). While militias account for 31% of estimated attacks against villages in the sample, they account for 96% of the combatants in our sample. Similarly, while the Raia only account for 15% of the militias' attacks (a significant number given they are only one of 48 militias), half of the militiamen in the sample were Raia. *Fact 2.* The militias' stated objectives were to protect their communities against violence by foreign-led armed groups, and their chapters were supported by the communities they emerged from. The villagers tended to perceive the security they provided as effective; they tended to support them and, in a significant share of cases, they even encouraged fellow community members to join the militia village chapter. They reported to have joined them for various distinct social motivations: in order to protect the community against violence by foreign-led armed groups, for revenge, but also as a result of social pressure in the community as well as status concerns. These facts are also true for the Raia. *Fact 3.* Rather than being a source of income for the economically deprived, the militias were not filled by poorer community members, nor joining was associated with any increase in asset growth. Instead, community members who joined the militia chapters were only distinguishable from the rest in their village and region in that that they originated from households that had previously been victimized by foreign-led armed actors. These facts are also true for the Raia.

We then turn to analyzing the extraordinary rise of the Raia in 2011. We do so in three steps: we first document that the state withdrawal is the *proximate* cause for the rise and analyze the motivations of those who joined it; we then zoom in on the role that responding to insecurity played in its rise; finally, we analyze the role of community institutions in this response to insecurity.

First, we document that the removal of the state army is the *proximate* cause for the rise and growth of the Raia, fueled by a range of social motivations to join it. We establish this result by exploiting the sharp shock induced by the Congolese Army's Regimentation policy of 2011, which created a state vacuum in some areas, but not in others. The policy, described by qualitative researchers (Stearns, 2013; Vogel, 2014), consisted in relocating the Congolese army battalions from one district, Shabunda, into urban centers—where they waited to be streamlined into regiments for more than a year. In our sample, there are 46 villages in Shabunda affected by the policy, across a wide and diverse area. Our analysis reveals that in response, Raia militia village chapters emerged, filled by participants who joined for intrinsic social motivations such as revenge and community protection, but also for extrinsic social motivations such as status and social pressure. Even compared to a previous state vacuum in the same district in 2004, we find that the rise of 2011 is extraordinary. This contrast motivates the next step in our analysis.

Second, we provide evidence for two *ultimate* causes for the Raia's rise: the state vacuum provided an opportunity for populations to act upon revenge motivations arising from past foreign-led victimization; it *also* created a sharp rise in insecurity in certain areas and therefore in the value of providing security in those areas. Starting with the role of victimization-related revenge, we find that, for both the state vacuum we analyze and its predecessor, the rise in participation is larger among former victims of violence by foreign-led armed groups, and that those are motivated by revenge, consistent with victimization seeding social emotions of revenge that were expressed during the vacuum. To quantify the economic significance of this social motivation, we provide suggestive evidence on the associated willingness to pay to join caused by past victimization. Exploiting US mineral price changes that affect the income of community members in the period outside of a militia, we find that past victimization is associated with an increase in the willingness to pay to join a militia equal in magnitude to the effect of an 8-fold increase in the yearly p.c. income. Yet, despite its significance, victimization-related revenge only plays a minor role in the rise of the Raia in 2011. Turning to the role of community insecurity rather than victimization, we find that the state vacuum of 2011 led to a drastic rise in insecurity driven by foreign-led armed actors' presence in the affected communities; its predecessor in 2004 did not. Consistent with this rise in insecurity being central in the rise of the Raia, we find that the Raia's emergence is *entirely* concentrated in the villages in which the state vacuum caused a rise in insecurity, but not at all in the rest. Accounting for the rise in insecurity caused by the state vacuum explains entirely the difference in the magnitude between the rise of 2011 and of its predecessor; furthermore, this relationship is not explained by having previously participated in the predecessor, nor by prior victimization. More than 57% of the differential rise in Raia participation in towns affected by insecurity as a result of the 2011 state vacuum is driven by intrinsic social motivations to protect the community, yet interestingly the rest also includes extrinsic motivations such as social status and social pressure. Why? This motivates the last step in our analysis.

Third, we provide suggestive evidence that this response to insecurity is in part due to the activation of community informal institutions in response to the rise in insecurity in 2011. Using time and spatial variation in the presence of public and chief-initiated recruitment campaigns into village militia chapters, we find that, in response to the insecurity created by the vacuum of 2011, traditional village chiefs organize more militia recruitment campaigns. Furthermore, the rise in participation into the Raia is concentrated in the villages where the village leaders organized public militia recruitment campaigns during that period, providing suggestive evidence that elite-driven community responses were partly accountable for the rise. Most notably, the differential rise in these communities is entirely driven by individuals who join motivated by social status or social pressure—which are virtually absent in the rest of communities—or to protect their community—which are three times more present than in the rest. This suggests that community institutions increase militia participation by upholding community norms that create extrinsic social motivations and by amplifying intrinsic social motivations among community members.

These findings suggest militias are a central actor of the conflict and can be thought of as successful violent collective action sparked by community elite-driven responses and bottom-up intrinsic social emotions. The results paint a picture of external threats to the community and of community mechanisms to override individual self-interest as central in the emergence and growth of armed actors. Their success resembles revolutionary movements. Yet, the role played by revenge and violence, and their xenophobic discourses towards the populations they violently target, liken them to ethno-nationalist far-right movements (e.g., Fryer and Levitt (2012)). Their success at solving a major collective action problem taunts prevailing ideas that violent conflict weakens capacity for collective action (Humphreys et al., 2013; Gáfaro et al., 2022).

These findings contribute to the literature on protests in economics. Much of that literature has focused on solutions to the free rider problem based on selfish private motivations. Our study complements this literature by introducing the notion that mobilization can be achieved by the by-product of bottom-up extrinsic and intrinsic social emotions and communities' responses using community institutions to override self-interest, including upholding norms and status but also amplifying intrinsic social emotions. Our results do not contradict the relevance of strategic considerations in individuals' decision to protest (González, 2020; Cantoni et al., 2019), but they extend the set of existing explanations introducing the possibility that community rationality and not just individual rationality might be an important driver, by amplifying and creating a variety of social sentiments. Such rationality might reflect noneconomic community common interest public goods, such as avenging the community, but it also reflects economic common interest public goods, such as the protection of the assets of its members. Including the broad range of social motivations and providing an explanation for how they come about and how community influences them, this study extends a seminal study of political protests (Cantoni et al., 2022), which showed that *pro-social* motives are important. This also nuances a literature that has generally opposed economic and non-economic motivations for collective action, by showing that informal institutions engineer individual extrinsic incentives to solve the collective action problem of providing security. The idea that non-private motivations are important for violence is not new to a vast literature outside of economics (see Section 2.2. for an overview of that literature). Our contribution to the latter is to provide evidence based on large-scale-disaggregated data, and to document the top-down elite-driven and bottom-up moral sentiments channels of militia rise.

The findings also complement a growing literature in economics on the performing of state functions. Indeed, reflecting a former militia member's say that they "took the state into their own hands" (Marchais, 2016), the militias we document collect taxes, provide protection, and hold a monopoly of violence, i.e., they perform "essential functions of the state." This literature has tended to focus on the idea that state functions emerge when an armed elite aims to extract resources from the population (Mayshar et al., 2011; Sánchez de la Sierra, 2020; Carneiro, 1970). Interestingly, a vast literature in other disciplines (Wittfogel, 1953), and one notable exception in economics Heldring (2020), has documented that collective demands for public goods might also explain the emergence of such functions (Wittfogel, 1953).

2.2 Bringing Community Back into Economics of Violence

2.2.1 What We Know about Communities and Violence

While the role of communities in the rise of militias is largely unexplored in economics, a vast literature in other disciplines provides us with rich knowledge about some aspects of this relationship, even as the existing evidence is predominantly qualitative.

The prominence of community militias in many African countries is a manifestation of the persistence of longstanding modes of decentralized security provision (Heald, 2006; Pratten and Sen, 2007). "Bottom up" forms of collective defense, organized around villages and communities, have long existed alongside—or been incorporated into—defense and security organised at a larger scale. In pre-colonial equatorial Africa, collective defence was one of the key tenets of social and political organization, which revolved around households, villages and clans, who could be called upon by kingdoms for defense or war-related purposes (Vansina, 1990; Lwigulira, 1993). During the colonial era, local chiefs were incorporated into the state apparatus in order to mobilise labour among their communities, including for security and war-related purposes (Northrup, 1988).[1] In contemporary Africa as in many parts of the world, the lines between community level security provision and the subcontracting of state security functions to non state actors are often blurred.[2] Communities can mediate both "bottom-up" and "top down" mobilisations for security or violence, and play an important role in the "armed orders" that emerge (Staniland, 2012b, 2021).[3]

We also know that social and communal mechanisms exist alongside private factors to drive participation in violence. Considerable attention has been paid to the role of economic factors in explaining participation in violence: Poverty, inequality and relative deprivation on one hand (Gurr, 1970), and economic opportunism of leaders and recruits on the other (Collier and Hoeffler, 2004; Weinstein, 2007).[4] Empirical studies have found mixed evidence

[1]The Congolese state has a long history of subcontracting security and other functions to nonstate actors, from private concessionary companies during the Congo Free State (Lowes and Montero, 2021), to mercenaries, private armies, and militias in the post-Independence era (Kisangani, 2012).

[2]In Burkina Faso, local self-defense groups which have emerged to protect civilian populations from armed insurgents have been incorporated into the state counterinsurgency apparatus (Frowd, 2022).

[3]Recently, particular attention has been paid to governance by armed actors and rebel groups (Mampilly, 2011; Arjona et al., 2014; Arjona, 2017), and the range of actors and organisations involved in "multi-layered governance" (Kasfir et al., 2017) and hybrid security provision (Bagayoko et al., 2016): from religious and customary authorities, to private companies, to grassroots organisations and youth leaders.

[4]Joining rebel groups can constitute an exit strategy for youth stifled by constraining economic prospects, as has been shown in Sierra Leone (Richards, 1996), and DRC (Jourdan, 2011; Vlassenroot and Raeymaekers,

for these (Cerina et al., 2023), and a consensus has emerged that no single factor explains participation in violence (Humphreys and Weinstein, 2008; Viterna, 2006; Scacco, 2024). A vast literature has documented the central role of social and community networks in revolutionary and insurgent mobilisation (Gould, 1991, 1993, 1995; Petersen, 2001; Parkinson, 2013; Viterna, 2006, 2013; Staniland, 2012a, 2014; McDoom, 2013), including in the DRC (Stys et al., 2020). Communities mediate mechanisms of collective pressure and coercion as well as moral beliefs on justice (Gurr, 1970), ideology (Sanín and Wood, 2014), the "pleasure of agency" to enact history and express a shared identity (Wood, 2003), collective grievances and desires for revenge (Balcells, 2017, 2012) and collective identities (Gould, 1995; Østby, 2013; Shesterinina, 2021). Community defense groups can also evolve into predatory organisations as they are absorbed into violent political economies, often eroding their communal logic in the process (Stearns and Botiveau, 2013; Marchais, 2016).[5]

This overview presents two opportunities. First, it underscores the value of organizing these motivations and mechanisms in a simple decision-theoretic framework that reconciles individual self-interest and group interest. Second, it underscores the value of answering these questions using dis-aggregated quantitative data. We now articulate the organizing question and then describe the strategy to collect data.

2.2.2 Organizing Question

Historically, human groups were regularly threatened by external (or internal) actors aiming to expropriate or violate their physical safety, stifling incentives to invest. The most obvious way to mitigate such threats is for the groups to produce credible threats of violence against those actors—deterrence—or even destroying their capacity for nuisance. A problem, however, is that producing credible threats of violence is privately costly (it is often risky), while the benefits to an individual are dispersed and sometimes zero, as producing large enough threats is labor intensive. Thus, individuals' contributions are often not pivotal, and the collective benefit is dispersed among its members, i.e., it creates a group collective action problem (Olson, 1971).

When hiring a third-party to provide security is too costly, groups have often organized it themselves, which requires *community mechanisms*: mechanisms that allow the group's interest to override self-interest to provide (risky and a priori individually irrational) effort-

2004).

[5]In the 1990s in Medellin, the paramilitary movement emerged as a result of a strong demand for protection by communities and garnered substantial popular support by enacting swift justice and punishment against criminals, and by articulating community desires for a restoration of the social order; yet, over time, the paramilitaries started preying upon the population and attracting opportunist members (Gutiérrez-Sanín et al., 2015).

based contributions to security, such as joining the group's self-defense organization; we, somewhat arbitrarily, refer to a community as a group with such mechanisms.[6] Consider first the motivations of their members. They might contribute effort if they derive private economic gain, and sometimes they do (henceforth, private motivations); but as members of a social group, they may also hold a range of social motivations leading the group's interest to override private motivations. Some social motivations, such as status concerns or social pressure could be denoted as *extrinsic* social motivations insofar as a significant share of the value to the individual of taking the action is instrumental, as a significant share of the benefits to the individual (not necessarily all) accrue as a consequence of taking the action. Another type of social motivation, such as *social emotions* (Bowles, 2006) can be denoted *intrinsic* social motivations insofar as a significant share of the value to the individual (not necessarily all) accrues irrespective of the material consequence of taking the action and resides in the pleasure derived from taking the action itself. Social emotions such as the desire to avenge group members who were previously victimized are frequent in personal accounts of this life decision, but so is the desire to *take part* in a contribution to a group's goal, such as ensuring its survival through its safety (which is intrinsic if it is not pivotal).

The group's members might autonomously develop some of those motivations, such as the desire to protect their group, if the group possesses a group membership identity; similarly, victimization of one member by a third-party might autonomously induce the social emotion of revenge as a reciprocal preference between emotionally connected members. It is easier to imagine intrinsic social emotions autonomously generated to benefit the group than extrinsic ones. But, beyond the autonomous generation of social emotions, a second type of community mechanisms are informal community institutions that can *align* extrinsic social motivations such as status and social pressures. In the political traditions of eastern DRC, chiefs have played a important role at upholding social norms, notably community members' rights and responsibilities, social status, and the implied threats of social and economic losses, through which they are able to generate "social pressure," but also potentially persuade members to internalize intrinsic social motivations.

We now describe how the Outraged Citizens' rise offers an opportunity to empirically empirically examine the role of social motivations, and of community institutions, in their emergence.

[6] One common solution to this problem is for the group to impose contributions by their members in order to hire an organization to perform such threats such as a state or mercenaries (such as in the example portrayed in Seventh Samurai (1954), by Akira Kurosawa). However, this solution is often not feasible.

2.2.3 The Critical Juncture of the Rise of the "Outraged Citizens"

The Raia provide a paradigmatic example of the role of communities in the rise and military success of a militia. At the end of the Second Congo War (1998–2003), the rebel groups who had been fighting during the war were incorporated into the newly formed national army, the *Forces Armées de la République Démocratique du Congo* (FARDC), causing a state vacuum in rural eastern DRC. Over the same period, the FARDC launched military operations against the *Front de Liberation du Rwanda* (FDLR), a Rwandan rebel group who has its origins in the Rwandan Civil War. Several armed factions re-mobilised in 2003–04, including some Mayi-Mayi armed groups.[7] As the FDLR engaged in retaliatory violence against the predominantly Rega populations in the territory of Shabunda, South Kivu, a religious (Kimbanguist) minister, Jean Musumba, created a new armed movement, the Raia Mutomboki (standing for "outraged citizens," in Kirega), who successfully drove the FDLR out of Southern Shabunda in 2005–07 (Stearns, 2013; Vogel, 2014). The Raia re-emerged in 2011 at a time of heightened insecurity caused by the Congolese Army's policy of "regimentation", whereby troops positioned in rural areas were moved to larger cities in order to create regiments (Stearns, 2013). The FDLR took advantage of the resulting state vacuum to expand their presence in Shabunda, but they remained under pressure from military operations by the Congolese and Rwanda military (operations Umoja Wetu, Kimia II, and Amani Leo). Increased insecurity in Shabunda sparked a considerably larger mobilization of the Raia than in 2005–07, which spread to Northern Shabunda and neighbouring provinces, expelling the FDLR from a vast region, an exceptional military feat which successive national military operations had been unable to achieve. Figure B.1 shows a photo of some of its members.

Studies have shown that community mechanisms played a key role in the movement's initial popularity and success (Stearns, 2013; Vogel, 2014). The mobilisation was largely decentralized, allowing 'franchise like' local chapters to emerge in villages and towns, often with the support of local chiefs (Stearns, 2013, p.29).[8,9] Interviews carried out with

[7] The Mayi-Mayi emerged during the First Congo War (1997–98) as a popular armed resistance movement against the perceived invasion of the country by foreign forces.

[8] Consistent with the literature, we refer to the group of militiamen stationed in a village as a chapter. In some cases, they emerge from a village as the village's militia. In others, they arrive by expansion of another militia.

[9] The movement's control over its members was also less coercive than other groups, with participants free to enter and leave (Stearns, 2013, p.29). Another factor was the movement's simple message, which articulated longstanding grievances of the Rega populations against historical neglect by the Congolese state with a clear call to action to rid the area of the predatory presence of the FDLR, often tainted by xenophobic rhetoric.

Raia members repeatedly stressed that community protection and the commitment to bring justice following attacks on their entourage motivated their decision to join the movement. They also pointed to the movement's use of traditional religious beliefs and myths and its use of protective amulets known as *Dawa,* a strategy also deployed by the Mayi-Mayi militia (Hoffmann, 2015). Collective values and sentiments, rather than individual economic motivations, were said to be key drivers of mobilisation, as argued by (Vogel, 2014). Later, as the movement expanded into mineral rich areas, Raia factions started taxing mineral resources, a key source of financing for armed groups in Eastern DRC (Sánchez de la Sierra, 2020). The Raia eventually splintered into various competing factions, who displayed a more predatory behaviour.

The *rise* of the Raia is thus a critical juncture in the history of security and state capacity in Eastern DRC, which provides a unique opportunity to explore the role of social emotions and of community in response to threats against the community in the emergence of militias.

2.3 Measuring Community and Violent Collective Action

We developed a comprehensive database of rural membership in, and relationship with, all types of armed groups in South and North Kivu, two of the most conflict-affected provinces of DRC. The core sample comprises interviews conduced in 1,041 households, in 133 villages, in South Kivu.[10]

Research teams spent approximately one week in each village, during which they reconstructed village histories with village history experts,[11] and implemented eight household surveys. In each household, we randomly sampled one available male adult to work with a researcher during one full day, with appropriate breaks, compensation and food. The survey reconstructed the household's and respondent's history back to at least 1995,[12] in particular

[10]These data were collected between June 2012 and September 2013. Before, the research team spent weeks in the districts' (Chiefdoms) capitals and in the lower-level districts (Groupements) to draw lists of all villages by consulting state and customary authorities. In the lists, we identified villages with a natural resource as well as a matched sample of villages with no resources, matched using the Mahalanobis metric using the vector of all available geographic characteristics. Then, we randomly sampled 133 villages.

[11]Village history experts have in depth knowledge of a particular village or entity's history. These could be local chiefs, notables, teachers, or any person who was recommended by local populations.

[12]To identify the households to be interviewed, in each village, researchers first drew a village list with the the help of the village chief, and implemented random selection using pre-selected random numbers. We randomly sampled eight households in each village of South Kivu, and six households per village in North Kivu.

yearly participation in armed groups,[13] the dates of all violent attacks experienced by the household, occupational choices, migration, and the households' economic history. Appendix B.1 provides additional details. At the end of the week, the researchers held a day-long meeting with the history experts and triangulated their data with that of these experts. This allowed researchers to detect and correct reporting errors about village outcomes. Table B.1 explains the classification of armed groups in our sample. Complementing this procedure, the researchers conducted qualitative interviews aimed at gaining a deeper understanding of the militias and armed groups, their relationship with the population and their recruitment practices. The qualitative interviews were carried out with the village experts, local authorities, ex or current militia members or leaders, and security forces. The qualitative reports were then used to cross-validate participation data from the household survey. The authors also conducted dozens of in-depth interviews with combatants and ex-combatants. We refer to this as our "qualitative data."[14] Our design aims to address measurement error in the events' dates. Building on established methods in recall studies, we developed a set of time cues—a first set related to regional history, and a second set related to individuals history (date of birth, marriage, migration)—which were used to date events.

We define participation as the active involvement in the security-related activities of an armed group.[15] Our design tackled two concerns with the use of self-reported data to measure participation in armed groups. First, respondents may be averse to revealing their participation. Second, we interviewed respondents in the year of the survey, which means that survivor bias could affect whether our sample is representative. Anticipating these concerns, we gathered, in the separate village data gathering exercise led by the village experts, the anonymized aggregate number of villagers who joined each armed group, each year. Since villages rarely disappear, these are not subject to survivor bias. And since these data are collected without revealing participants' identity, they are less vulnerable to respondent social desirability bias. The anonymized data and the individual reports are

[13]Our design reflects our care to mitigate risks arising from the fact that the information we collect is sensitive. Participation in armed groups, and especially in militias, is commonplace in eastern DRC, and discussing participation is feasible with appropriate measures to minimise risks to participants and researchers. Moreover, several members of the research team were from the regions where the data collection took place, which meant that they spoke the languages and helped to build trust with participants.

[14]To gather this data, we obtained authorizations from provincial, territory, and village authorities. Ethical guidelines were followed to ensure that respondents did not feel obliged to participate.

[15]Our measure does *not* include involvement as informants, covert supporters, tax collectors, business partners, or any other role (see Petersen (2001)'s classification). We henceforth refer to this involvement as participation, and its start as enrollment or joining. To build this measure, we asked each household survey respondent to list all the armed groups that have been present in the survey village, whether they had participated in these, and the start and end dates. Respondents were also asked to describe participation episodes in any other group.

almost identical, providing confidence in the individual reports data.[16]

In a household attack history module, each respondent was asked to report up to nine attacks by armed actors that happened in the village where they live. Each respondent on average reports 2.08 attack events; the 99^{th} percentile is seven events. Thus, reporting limit did not lead to loss of data. For each event, we observe the perpetrators' group, the perceived intention, whether the household was targeted, the number of fatalities in the village, the number of persons who suffered sexual violence in the village. In addition, for each household member, each respondent reported up to three events in which armed actors targeted the household member being discussed. For each of those events, we identify the year in which they took place. In the analysis that follows, we focus on whether any household member other than the respondent was victimized (henceforth, household victimization), to exclude confounds arising from the direct effect of violence. Specifically, we use the attack information on each household member from both attack modules to construct an indicator for whether a nonrespondent household member was attacked. The information on victimization was gathered prior to that about participation. This helped prevent against motivated recounting of attacks. Appendix B.1 provides additional details.

Our design tackled possible measurement error arising from relying on individual self-

[16]Figure B.2 compares the household reports to the aggregate data. For comparison, for this figure, we exclude enrollment outside village armed group episodes. To construct the village level estimates based on the household reports, we first obtain the share of respondents who report to have participated in a group during a group governance episode. Then, we use the village size we recorded in the village survey, and the number of surveyed villages in South Kivu (n=133) to construct a village-level estimate of the number of participants. The mean village size in South Kivu in our sample is 203 households. The estimated number of militia participants based on the anonymized aggregate reports is 55.7 (dark blue bar with solid outline), against 50.2 from the household reports (light blue bar with dashed outline). The third, gray blue bar with dotted contour, excludes individual-year observations for which the respondent was living outside the village. The number remains almost identical. The data reported by Figure B.2 provide support to our data collection and reassures that participation in village militia chapters, the focus of this study, is not subject to social desirability or survivor biases that may confound our conclusions. Since combating is a hazardous occupation, with truthful reporting and in the absence of recall bias one would expect that the representative household level survey should be missing those killed in action, while the village level survey would not. Our quantitative and qualitative data provide reassurance by providing context to why the household reports are so similar to the village aggregate reports. First, the largest share of participation is driven by the Raia, which started one year before our data collection. This reduces the scope for survivor bias. Second, village militia chapter participation is a local phenomenon, and most of the time participants are working part-time as a militia members. This fact, which we gathered from our qualitative interviews, is also supported by the data: Table B.4 shows that while participating in a village militia chapter, 71% of the participants are also employed in other occupations, including agriculture, mining, and civil service. Providing additional support to the irrelevance of migration of village militia chapter participation, Section B.3 analyzes the role played by migration in this context and its relation to state vacuum, victimization, and participation in village militia chapters.

reported data to measure attacks against the household and the village, by cross-validating the attacks reported by participants in a same entity, and by triangulated these with the data compiled by the village experts. Figure B.3 conducts this triangulation exercise for attacks. We code one attack as verified by other households if at least one other household reports an attack by the same perpetrator taking place in the village in the same year. The same criterion is used for whether an attack is verified by the village chief survey. The vast majority of attacks reported by the household can be verified this way, ruling out that households are under-reporting attacks.[17]

The sample collected through this procedure constitutes the core sample for which we designed this study. In addition to this dedicated data collection in South Kivu, we implemented four additional data collections, which we used to assess the robustness of our result. First, we interviewed an additional random sample of 32 households in each sampled village of Shabunda, restricting these to the participation module. Second, we took advantage of a study conducted in North Kivu in 2015 (Sánchez de la Sierra, 2020) to implement the participation module with additional 591 households in 106 villages, increasing the sample to 239 villages. This yields participation histories of 4,336 household members. Finally, in 2016, we gathered minimal details on the respondent's participation history in 10 additional households in each of the 106 villages.

Taken together, our samples constitute participation information of 7,454 individuals, collected in 2,964 households of 239 villages, covering the period 1995–2013 and including data on 640 individuals who, at some point, participated in armed groups. In the analysis that follows, we use the core data to provide a picture of armed groups in the region and to analyze individual motivations. Figure B.4 presents the samples.

2.4 The Relevance of Community in the Raia, and in Militias

In this section, we present three descriptive facts about the role of community in the militias and, in particular, in the Raia. We define *militia* as Congolese armed groups other than the national army, in contrast to those that clearly represent foreign interests (henceforth, *foreign-led*).

The qualitative literature about the Raia suggests that, like many militias, they enjoyed large popular support and succeeded in pushing the FDLR out, thus that the Raia represent

[17]Since the data construction also includes victimization of household members, some of those can have taken place outside the village, hence some of those attacks that are not reported in the village data. When the data excludes those, both sources produce comparable means.

a paradigmatic case of a militia (Stearns, 2013; Vogel, 2014). In the sections that follow, we use the data we have collected to examine whether the anecdotal evidence about the role of communities in the rise and spread of militia, and the Raia in particular, is supported by empirical evidence.

Fact 1: Militias Predominate the Conflict, and the Raia is a Major Militia Recruiter

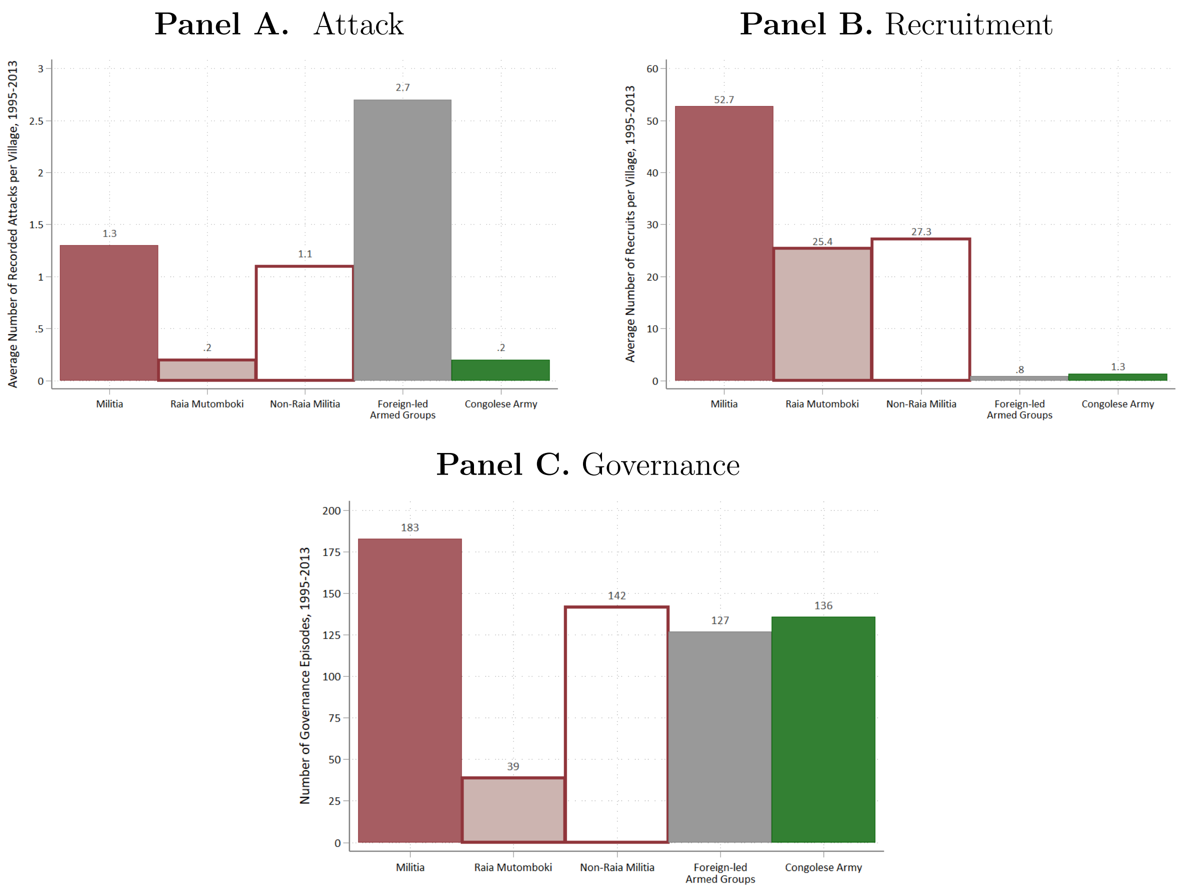
The militias are one of the main actors in the Congolese conflict. Of 76 armed groups on which we gathered information, 48 (63%) correspond to the definition of militias (see Table B.1).

Figure 2.1, Panel A, shows that, over the period of the Congolese conflicts up to the data collection, the average village in the core sample received 2.7 attacks by foreign armed groups, against 1.3 attacks by a militia and 0.2 by the Congolese army. This means that militias represent 31% of the violence recorded against the villages in the sample for the period, thus a significant share of the violence as measured by the number of violent attacks on the villages. The Raia, which are only one among hundreds of militias, naturally only represents 15% of militia attacks.

However, violent events are a poor indicator of their relevance in the conflict: our measure of attacks is about attacks against the Congolese rural communities, which are their own people. We thus turn to other metrics than violent events. First, Panel B shows recruitment of combatants from those communities. Over the twenty years of the conflict leading up to our data collection, the average village in the core sample had 53 village members who joined a militia. This far exceeds the number of those who joined a foreign-led armed group (0.8) or even the Congolese army (1.3). Overall, militias recruit 96% of the fighters that come from the rural villages and towns in our sample. Underscoring the significance of the Raia in the world of militias, around half of the fighters who ever joined a militia in the period actually joined the Raia, despite the fact that its rise was concentrated in the last few years of the sample. Second, Panel C shows that, of 444 armed group village governance episodes recorded in our data in the core sample, 308 were run by non-state armed groups, the rest were episodes of the Congolese army stationing in a village. Of the non-state armed groups' governance episodes, 183 (59%) were by militias. Of the 183 militia village governance episodes by militia chapters from the village, 21% were by the Raia. Thus, the Raia also represent a large share of the militia governance episodes.

In sum, far from being a marginal anecdote in the conflict, the militias predominate the conflict by various metrics including violent attacks against the villages, recruitment, and governance. Of particular significance, the Raia represent almost half of the recorded recruitment into militias in the entire period and 21% of the militia village governance

Figure 2.1: Militias Predominate in the Conflict, and the Raia is a Militia Recruiter



Notes. We use the core sample from South Kivu for the descriptives. Panel A presents the average number of attacks per village by each type of armed group. Panel B presents the number of participants in each type of armed group for a village in the core sample from South Kivu. We first obtain the share of respondents who report to have participated in a group during an episode where an armed group controlled the village. Then, we use the village size we recorded in the village survey, and the number of surveyed villages (n=133) to construct a village-level estimate of the number of participants. The mean village size in the core sample is 203 households. Figure B.2 shows that the patterns cannot be explained by measurement bias from household survey. Panel C presents the number of village governance episodes. Figure B.5 replicates the figure by including extra village sample from North Kivu.

episodes. Thus, the Raia represent a large share of the militias in the conflict as measured by those metrics and are thus well-suited as a case-study to isolate key drivers of militia rise and growth.

Fact 2: Like for Militias, Communities Supported the Protection Goals of the Raia

The militias' stated and realized goals in large part reflect the desire to protect the community, an objective which enjoys high support from the community. This is especially true for the Raia. First, most of the militias' stated goals that are publicly available (documented in Verweijen (2016)) relate to public goods for the community, in particular security. For example, the stated political objectives of the militia Mai Mai Charles and those of the NDC-R are about protecting against foreign armed groups such as the FDLR (United Nations Group of Experts, 2016), and those of the militia Mayi-Mayi Kapopo ("cahier des charges" in French) stated, in January 2011:

> "defend the territorial integrity and inviolability of the DR Congo against foreign forces; protect the Congolese peoples and their goods." Source: Verweijen (2016).

As described in Section 2.1, the Raia emerged to chase the FDLR out of Shabunda in order to restore security in the district. Many of their factions claimed to defend local communities against the abuses of FDLR and such claims appeared to have traction among the local population (Stearns, 2013). The Raia's stated objectives are thus aligned with the typical objectives of militia.

Second, these goals appear to be perceived as genuine by the population, or at least, to have been realized. Panel A shows that, in 73% of village chapter militia governance episodes, the chapter was perceived by the average villager to be effective at providing security. This rises to 95% in the case of Raia chapters. In contrast, militias from oustide the village and foreign-led armed groups were perceived to be effective only in 20% and 38% of episodes, respectively. The fact that the Congolese army was perceived to be effective only in 84% of episodes provides evidence that the Raia's success at chasing the FDLR was exceptional in the history of the Congo conflicts and that, in that regard, they were more effective than the state itself.[18]

[18]Table B.5 presents the estimated coefficients of a regression of various indicators of violence by any armed group against the household on various forms of militia chapter presence. We separately analyze exposure to violent events, and to sexual violence perpetrated by armed actors. This suggests that the presence of militia chapters formed in the village and, in addition, participating in them, drastically reduces the propensity that a household member is the victim of sexual violence.

Table 2.1: Like for Militias, the Communities Supported the Goals of the Raia

	Militia from Village			Militia from		
	All	Raia	Non Raia	Outside	Foreign	Army
A. Protection of the Community: # Episodes	129	39	90	50	127	136
Population Perceived Chapter's Security as Effective	0.73	0.95	0.63***	0.20***	0.38***	0.84*
A Chapter Member Attacked Villagers	0.29	0.13	0.36**	0.73***	0.70***	0.11
B. Support from the Community: # Episodes	129	39	90	50	127	136
Some Villagers Opposed the Chapter	0.16	0.05	0.20**	0.41***	0.45***	0.06
Parents Encouraged Their Children to Join the Chapter	0.42	0.63	0.37**	0.16***	0.18***	0.20***
Chief Encouraged the Youth to Join the Chapter	0.47	0.64	0.43**	0.14***	0.19***	0.23***
Chief or Relative Was the Chapter's Leader	0.41	0.62	0.32***	0.00***	0.01***	0.01***
Chief was Forced to Support the Chapter	0.26	0.08	0.33***	0.55***	0.72***	0.10
C. Members' Motivations: # Participants	245	134	112	30	4	7
Social Motivations, Intrinsic (Social Emotions)	*0.71*	*0.71*	*0.72*	*0.43****	*0.33*	*0.00*
For Revenge	0.11	0.12	0.10	0.07	0.00	0.00
For Community Protection	0.60	0.58	0.63	0.36**	0.33	0.00
Social Motivations, Extrinsic (Social Incentives)	*0.15*	*0.16*	*0.15*	*0.21*	*0.67***	*0.00*
For Status	0.04	0.06	0.01**	0.00	0.33*	0.00
Social Pressure	0.09	0.09	0.08	0.07	0.33	0.00
Social Coercion	0.03	0.00	0.07***	0.14***	0.00	0.00
Private Motivations	*0.13*	*0.14*	*0.12*	*0.36****	*0.00*	*1.00***
For Money	0.05	0.07	0.03	0.29***	0.00	1.00***
For Private Protection	0.08	0.07	0.10	0.07	0.00	0.00

Notes: We use the core sample from South Kivu for the descriptives. *Militia from Village* reports the sample of individuals who joined a militia chapter formed in the village of survey. *Raia* and *Non-Raia* report the sample of individuals who joined in Raia and other militia chapter formed in the village of survey that is not Raia, respectively. *Militia from Outside* reports the sample of individuals who joined a militia chapter formed outside of the survey village. *Foreign* reports the sample of individuals who joined a foreign armed group. *Army* reports the sample of individuals who joined Congolese national army. For motives, we classify all the answers into the seven groups: Revenge (to avenge; following an incident with family or community), to protect the community, status (to become a military; to be feared), social pressure (social pressure; convinced by family, villager, or other civilian; everybody participated), social coercion, for money (for financial advantage; there is no other opportunities), private protection (private protection; to find refuge; to protect own goods). Units for the number of observations are reported in the panel headers. We indicate the significance of differences compared to Column 2 with stars at 1, 5, or 10% significance levels (*, **, *** respectively). Table B.9 replicates the descriptives by including extra village sample from North Kivu.

Third, the stated goals and outcomes of the militias, and especially for the Raia, had high level of villagers' support. Table 2.1, Panel B, shows that the population tended to show high levels of support for the militias and especially the Raia. In 16% of the village militia governance episodes, there was opposition from the community; this compares to 45% for foreign-led armed groups. This number was even down to 5% for the Raia village chapter governance episodes. In 42% of village chapter militia episodes, the parents encouraged their children to join; this compares to 18% in the case of foreign-led armed groups. For the Raia, parental encouragement was present in 63% of governance episodes. The village chief also tended to encourage the youth to join: he did so in 47% of village chapter militia governance episodes, up to 64% for the case of the Raia; this compares to 19% in the case of foreign-led armed groups and 23% in the case of the Congolese army. The village chief or a relative was the leader of the chapter in 41% of village militia chapter episodes, up to 62% in the case of the Raia; this contrasts to 1% for all other types of armed groups. Similarly, while chiefs were rarely forced to support the chapter in the case of militia village chapters or for the Raia (26% and 8%, respectively), they were forced to do so most of the time for foreign-led armed groups (72% of episodes).

Finally, the people who became fighters in the militias were predominantly motivated by the protection of the community, in line with the militias' stated goals; this was especially true for the Raia. Panel C shows that intrinsic social motivations (social emotions, including revenge and the desire to contribute to protect the community) predominate the purported motives of combatants who joined militia village chapters (71%) and played a decreasing role in militias from outside the village, foreign-led groups, or the army (43%, 33% and 0%, respectively reported to have such motivations in these cases). Of those, the largest share is to protect the community, reported by 60% of combatants who join militia village chapters; the rest are for revenge (11%). In contrast, while extrinsic social motivations (social incentives, such as to achieve social status, experiencing various forms of community pressures amounting to social pressure, or social coercion) as well as private motivations (such as money or private protection) motivated only a minor share of combatants who joined militia village chapters, they motivated predominated the motivations of those who joined foreign-led armed groups.[19]

In sum, the militias' goals are about community protection, they have high popular support, and those who join them are motivated by the stated goals of the militia, predominantly

[19]Social pressure includes various forms of pressures by community members, the fear of being ostracized by the community, or even direct instructions from village powerholders, including the parents. 37% of those who were pressured said they were convinced by another villager, 23% mentioned social pressure, 14% said they were convinced by family or someone else, and 3% said they joined because everyone else participated. The remaining 23% provided no further details. Social coercion is a more severe form of social pressure where the individual said they had no choice.

to protect the community and for revenge; the Raia stood out as a paradigmatic case of a successful militia.

Fact 3: Among other Motives, Victimization and Revenge Fuel Militias and the Raia

The militias, and especially the Raias, disproportionately attracted joiners whose household members had previously been victimized (henceforth, *victims*), and who reported to have joined for revenge. First, we have seen that 11% of those who join militia village chapters join for revenge. Revenge for what? Table 2.2, Panel A, shows that, among the militia participants, those with family members who were previously victimized by foreign-led armed groups (henceforth, *victims*) are much more likely to have joined a militia village chapter for revenge (24% against 7%). This difference does not reflect spatially correlated victimization: Figures B.6, B.7, show participation episodes and attacks in a map; the difference in victimization by foreign-led groups is driven by attacks by the FDLR, whose attacks tend to be gruesome: Figure B.8 shows the perpetrators and targeted persons. Overall, this paints a picture of participants having been exposed to particularly gruesome violence against their family members, perpetrated predominantly by the FDLR, which is precisely the group that many militias, and in particular the Raia, have as mission to fight against, suggesting revenge against past FDLR violence may play a role.

Second, Table 2.2, Panel B, shows that, irrespective of the motives for joining, the militias are disproportionately staffed by victims. This is also true for the Raia. Does past victimization capture other variables that explain participation? Table B.6, Panels B, C, and D show that, contrary to what would be predicted by economic incentives, the members of militias are not the disadvantaged. On the contrary, they are just as likely to be unemployed, and in fact have more wealth, and own more plots, than non-members. Furthermore, joining the militia is associated with a smaller increas in assets compared to those who do not join. The significance levels reported in Table 2.2, Panel B, are from regressions that include controls for these other demographic variables, suggesting that other observable characteristics cannot account for why participants are disproportionately victimized. This suggests that it is unlikely that economic motivations are the central force explaining why victims converge to militias, and to the Raia. Rather, the evidence is consistent with revenge for violence against family being relevant.[20]

In sum, this section allows us to characterize the significance of the Raia among militias with regards to their community-oriented goals. First, the Raia village chapters stood out among militia village chapters as among the most successful in providing security. Second, they stood out as enjoying particularly high levels of community popular support. Third, those who joined the Raia had the same distribution of intrinsic motivations to protect the community or for revenge as those who joined any other militia village chapter. The Raia add a slightly larger share of combatants who reported to have joined out of status motivations (6% against 1% for other militia village chapters); consistent with our qualitative evidence, this suggests that communities may be able to engineer status motivations to induce more members to join the chapter when the chapter aims to solve a community need such as security, creating a collective action problem, a fact to which we return when analyzing the community responses to insecurity. Interestingly, a distinctive feature of the Raia is that 24% of the participants are also individuals who had previously participated in the 2004 militia in the same district, amounting to a predecessor of the Raia. We take advantage of this predecessor in the analysis that follows to separately identify factors that explain the extraordinary rise of the Raia in 2011 and not in its predecessor.

In what follows, we leverage a sharp shock to the absence of the state in 2011 to examine the causes of the Raia's emergence and its mechanisms, interrogating the role of victimization and revenge, as well as of individuals and communities desire to contribute to security.

[20] To dig deeper into the dynamic of past victimization and present participation, we estimate:

$$Part_{ijt} = \sum_{h=-10}^{h=10} (\gamma_h \mathbf{1}[K_{it} = h]) + \gamma_{h+}\mathbf{1}[K_{it} > 10] + \alpha_i + \alpha_j + \alpha_t + \alpha_a + \epsilon_{ijt} \tag{2.1}$$

where i, j, t index, individuals, villages, years, respectively. $\mathbf{1}[K_{it} = h]$ is an indicator variable that equals 1 if other members in the household of individual i are attacked at period $t' = t + h$, and zero otherwise. Parameters α_i, α_j, α_t, α_a are fixed effects for individual, village, year, and age, respectively. All villages contain individuals who are observed in another village in some year. $Part_{ijt}$ is an indicator variable taking value 1 if respondent i in village j participates in a militia chapter formed in the village in year t. To account for serial correlation and village-year shocks, standard errors are two-way clustered at the individual and at the village*year (respectively, 1,041 and 4,963 clusters). Since exposure to household victimization is staggered, in what follows, we implement Borusyak et al. (2024) estimator. Figure B.9 reports the coefficients for the leads and lags of the attack indicator. To gauge the significance of this difference, we use variations in the world price of gold to quantify the economic significance of victimization. Table B.7 and Table B.8 present the results from this quantification exercise. It shows that it would take a *permanent* increase in 8 times the yearly per capita income to undo the magnitude of the effect of *one* foreign-led armed group attack.

Table 2.2: Along other Motives, Victimization and Revenge Fuel Militias and the Raia

Panel A. Victimized Militia Combatants are More Motivated by Revenge

	Victimized Participants				Non-Victimized Participants			
	Militia from Village			Militia from	Militia from Village			Militia from
	All	Raia	Non-Raia	Outside	All	Raia	Non-Raia	Outside
Members' Motivations: # Participants	59	33	26	2	186	101	85	28
Social Motivations, Intrinsic (Social Emotions)	*0.64*	*0.70*	*0.58*	*0.00***	*0.74*	*0.71*	*0.77*	*0.46**
For Revenge	0.24	0.24	0.23	0.00	0.07***	0.08**	0.05***	0.08*
For Community Protection	0.41	0.45	0.35	0.00	0.67**	0.63*	0.72***	0.38
Social Motivations, Extrinsic (Social Incentives)	*0.22*	*0.18*	*0.27*	*0.00*	*0.13*	*0.15*	*0.11*	*0.23*
For Status	0.02	0.00	0.04	0.00	0.05	0.08*	0.00	0.00
Social Pressure	0.14	0.18	0.08	0.00	0.07**	0.06**	0.08	0.08
Social Coercion	0.07	0.00	0.15**	0.00	0.02	0.00	0.04	0.15**
Private Motivations	*0.14*	*0.12*	*0.15*	*1.00****	*0.13*	*0.15*	*0.11*	*0.31**
For Money	0.02	0.03	0.00	1.00***	0.06	0.08	0.04	0.23**
For Private Protection	0.12	0.09	0.15	0.00	0.07	0.06	0.08	0.08

2.5 Proximate Cause for the Raia's Rise: State Withdrawal

In this section, we examine the effect of a sharp withdrawal of state forces on the emergence and growth of the Raia, and document the type of motivations that it unleashed among the affected villagers that led them to join the Raia at that critical juncture.

We take advantage of a historical event—the state vacuum created by the Regimentation Policy in 2011, henceforth *Vacuum 2*—to explore the role of state withdrawal on the rise of the Raia. Studies of the Raia have argued that the Regimentation Policy was a key trigger in its emergence (Stearns, 2013; Vogel, 2014). Engineered by the central government to streamline the structures of command inside the army and break parallel structures of command, the policy caused the departure of all Congolese army units based in Shabunda territory in May 2011. The battalions that were withdrawn from Shabunda to be merged as regiments were not simultaneously redeployed to other areas. Instead, they were taken into training centers in urban areas (Stearns, 2013). This ensures that the policy did not increase

Panel B. The Victims are More Likely to Join Militia Village Chapters

	Participants				Non-participants		
	Militia from Village			Militia from	Living in the Same:		
	All	Raia	Non-Raia	Outside	Village	Chiefdom	Territory
# Participants/Indiv-Year Obs	245	134	111	30	899	13824	14947
Past Victimization	*0.25*	*0.26*	*0.23*	*0.07***	*0.11**	*0.09*	*0.09*
By Foreign Armed Group	0.24	0.25	0.23	0.07**	0.09**	0.07***	0.07***
By Congolese Militia	0.03	0.03	0.03*	0.03	0.03	0.02**	0.02*
Past Participation	*0.20*	*0.30*	*0.07****	*0.10****	*0.11****	*0.06****	*0.06****
In Militia Village Chapter	0.14	0.26	0.00***	0.03***	0.07***	0.04***	0.04***
In Raia Mutomboki or Mayi-Mayi	0.13	0.24	0.00***	0.00***	0.04***	0.01***	0.01***
In Militia Formed Outside Village	0.02	0.03	0.01	0.03	0.01	0.01	0.01

Notes: We use the core sample from South Kivu for the descriptives. *Militia from Village* reports the sample of individuals who joined a militia chapter formed in the village of survey. *Raia* and *Non-Raia* report the sample of individuals who joined in Raia and other militia chapter formed in the village of survey that is not Raia, respectively. *Militia from Outside* reports the sample of individuals who joined a militia chapter formed outside of the survey village. Panel A reports separately for respondents who experienced an attack by an external group in the past ("victimized participants", Column 1–4) and those who did not (Column 5–8). Panel B compares participants in militia chapters versus those where respondents do not participate in any militia chapter contemporarily (living in the same village, same chiefdom, or the same territory), regarding conflict background and participation history. We indicate the significance of differences compared to Column 2 with stars at 1, 5, or 10% significance levels (*, **, *** respectively). For Panel B, when calculating differences, we include year fixed effects, control for all variables in Table B.6, Panels B–D, and cluster at two-way at the individual respondent and the village*year level. We are not able to replicate Panel A with extra sample because only the core sample has both the motive information and the attack history for each participant. Table B.10 replicates Panel B by including extra village sample from North Kivu.

military presence outside Shabunda, and creates a sharp vacuum in Shabunda. Studies of the Raia have documented that its rise was preceded by a smaller precursor in 2004, after the Sun City peace agreement of 2003 left various areas of eastern Congo without state force presence, in particular Shabunda, henceforth *Vacuum 1* (Stearns and Botiveau, 2013). The main cause for this state vacuum was that, at the end of the second Congo War, the main rebel groups were incorporated into the newly formed national army, the FARDC. This policy, known as Brassage, entailed that the large armed groups (the RCD, which had taken the eastern half of the country, and the Mayi Mayi Padiri in particular), withdrew their forces from the regions they occupied to incorporate them in the national army. The predecessor of the Raia emerged in response to this state vacuum, although it acquired a smaller scale.

Before analyzing the effect of the Regimentation, we first verify that it consisted in a sharp state vacuum in Shabunda. The map in Figure 2.2 shows that the Regimentation was associated to a state vacuum concentrated in Shabunda, not elsewhere. In our study, 46 villages of Shabunda are directly affected by the Regimentation policy. While the Regimentation left only one battalion of the Congolese army in charge for all of Shabunda, the state vacuum it created affected most of the villages of Shabunda, and most of the villages in our sample.[21]

What do villagers do in response? Figure 2.3, Panel A, shows that, right as the state forces withdraw in 2011 from Shabunda, militia village chapters (all Raia) begin to emerge and literally skyrocket, with its share across villages jumping from 0% of villages in 2010 to 70% by 2012. The figure also shows the inflow indicator for village militia participation, showing that as the chapters emerge, so does the presence of village respondents who reported to join the Raia in that year. The large spike in the flow of new members is in 2012, where 60% of the sample villages in Shabunda observe at least a new member (based on the sample of 8 households alone). Panel B shows that these effects are entirely a Shabunda phenomenon, which is also the only district in the sample in which the Regimentation created a state vacuum. Comparing the evolution of militia chapters during Vacuum 1, Panels A and B show that the vacuum induced by the Sun City peace agreement was associated with a (smaller) rise in the emergence of new militia village chapters and the enrollment in militia. This smaller rise was a predecessor of the later rise of the Raia. In what follows, we exploit the difference in the rises to examine the factors underpinning the extraordinary rise in 2011.

To formally analyze the relationship between the Regimentation and the rise of the Raia, we estimate the following Equation. Let i, j, t index the individual, village, and year, respectively:

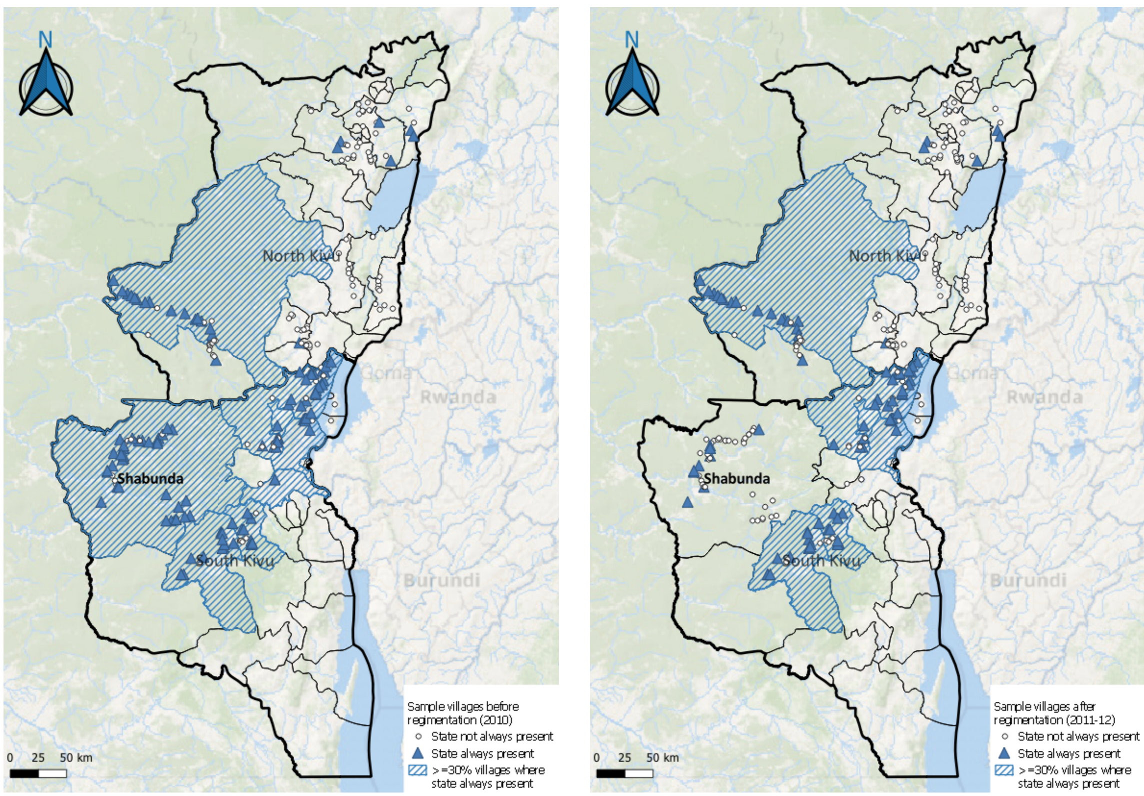
$$y_{ijt} = \theta_1 \mathbf{1}[V1_{jt} = 1] + \theta_2 \mathbf{1}[V2_{jt} = 1] + \alpha_i + \alpha_j + \alpha_t + \alpha_a + \epsilon_{ijt} \tag{2.2}$$

where y_{ijt} is an indicator for whether the individual joins a village militia chapter, $\mathbf{1}[V2_{it} = 1]$ is an indicator for whether village j in year t belonged to Vacuum 2 (it is the product of an indicator for the year window 2011, 2012, and an indicator for the district being Shabunda), and $\mathbf{1}[V1_{jt} = 1]$ is similarly an indicator for whether the village j in year t belonged to Vacuum 1 (it is the product between an indicator for the year window 2003, 2004, 2005, and an indicator for the district being Shabunda). The baseline standard errors are presented clustering at the level of the village but we present in all regressions the p-values on each coefficient clustering two-way at the village and chiefdom-post vacuum years level, as well as village and chiefdom-year levels.

Table 2.3, Panel A, presents the estimates of Equation 2.2, where the dependent variable is: an indicator for whether there is presence of the Congolese national army in the village, for

[21] Figure B.10, Panel A, presents the same set of maps for the predecessor.

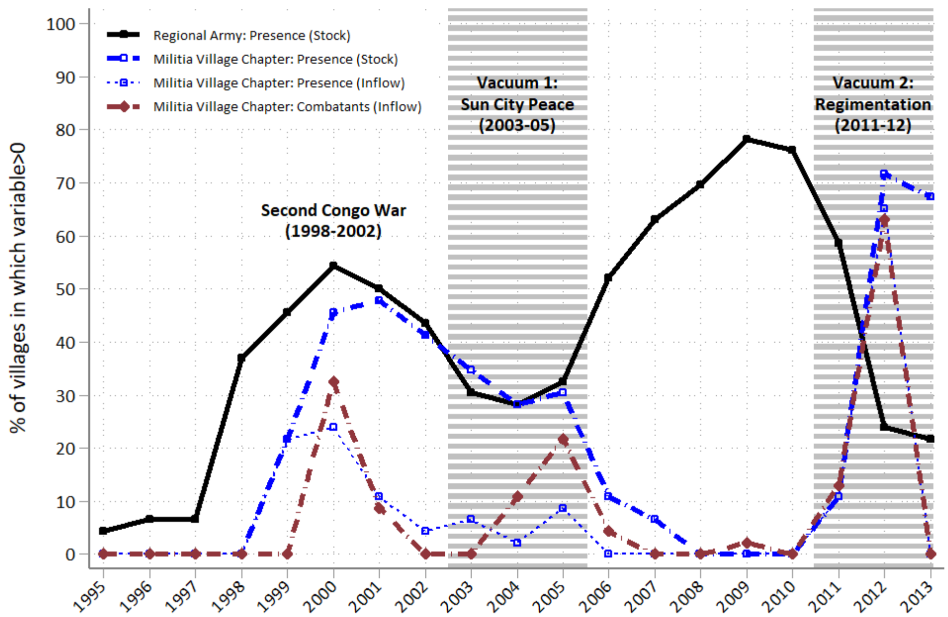
Figure 2.2: Presence of the Congolese Army Around the Regimentation



Notes: This figure shows the presence of the Congolese army around the time of the Regimentation (2011). Since the Regimentation took place in 2011, our indicator of Congolese army presence in 2011 captures the presence of the Congolese army in the months of 2011 leading up to their removal. Thus, in the post-regimentation map on the right, a blue triangle is a village where the Congolese army is present for both 2011 and 2012, and a white dot is a village where Congolese army is not always present in both years. The blue areas are chiefdoms where at least 30% are controlled by Congolese army; the cutoff 30% is selected because among cheifdoms where the Congolese army is present, on average, roughly 30% villages are controlled by the Congolese army.

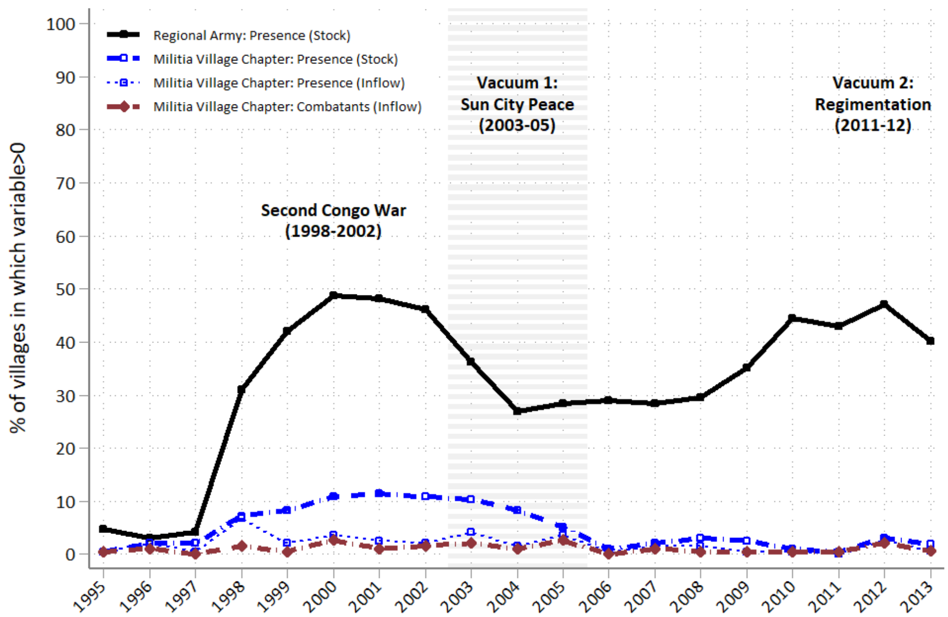
Figure 2.3: State Vacuum and the Birth of the Raia

Panel A. Villages in Shabunda



whether there is a militia village chapter (stock), for whether there is a new militia chapter (inflow), for whether there is new militia village chapter combatants in the village (inflow), and for whether the respondent of the household survey was participating in a militia village chapter at that year, in Columns 1–5 respectively. The latter is estimated at the level of the individual respondent * year (n=15,106), while the former are from the village * year dataset (and thus indexed by jt rather than ijt). Vacuum 2, the Regimentation, is associated to a decrease in national army presence from 76% to 45% that is statistically significant at the 1% level. Concomitantly, it is associated with an increase in the fraction of villages with the presence of a militia village chapter (Raia) from 0 to 31% and the emergence of a new militia village chapter from 0 to 35%, and in the fraction of villages with new militia village chapter combatants from 0 to 33%. Analyzing individual level participation data, it shows that Vacuum 2 is associated with a 17 pp. increase (from zero) in village militia participation. All coefficients are significant at the 1% level. Turning to Vacuum 1, the predecessor, we see that a similar rise ensues, albeit of a smaller magnitude: the state vacuum is less than half

Panel B. All Other Villages



Notes: The thick black solid line shows the fraction of villages where we observe regional army presence. A village is coded as having a regional army present if either the national army or Rassemblement Congolais pour la Democratie (RCD) is present. The inclusion of the RCD in the definition only affects 1998–2004, the years of its existence as it took over the state apparatus and is done for parsimony of presentation. The thick blue dashed line shows the fraction of villages where village militia chapters are present each year. The thin blue dashed line shows the fraction of villages where new village militia chapters emerge each year. The red thick dashed line shows the fraction of villages where the inflow of new village militia chapter combatants is larger than zero. Panel A restricts the sample to Shabunda, the district affected by the military policy-induced state vacuum of 2011. Panel B shows this for the remaining of the sample. We use both the core sample from South Kivu and the extra village sample from North Kivu to present the yearly trends. Left and right grayed areas indicate years in which documented policy-driven state vacuums were associated to the rise of the Raia and to its predecessor. Militia chapter and state presence data is taken from the village module. Number of individuals joining a militia village chapter is taken from the household surveys.

as intense as that of 2011, and the rise in village militia participation is 1.7 pp. in contrast to 17 pp. for Vacuum 2.

What motivations underpin the rise in the Raia? Panel B presents the estimates of Equation 2.2, where the dependent variable is: an indicator for whether the individual joins a militia village chapter (Column 1), whether they joined it motivated by intrinsic social emotions (Columns 2–3), extrinsic social incentives (Columns 4–6), or by private motivations (Columns 7–8). Vacuum 2's effect on village militia chapter participation comprises various types of social and private motivations, except for social coercion, underscoring the voluntary nature of the Raia. However, by far the largest share of this increase are joiners who joined *because they wanted to protect their community* (accounting for 9.75/16.96=57% of the rise in Vacuum 2). In contrast, the predominant motives underpinning the predecessor rise in Vacuum 1 are private protection (p-value 0.10) and revenge (p-value 0.16); unlike Vacuum 2, the protection of the community is markedly absent.[22]

This analysis poses a series of puzzles. Why was the response to the initial vacuum weaker than to the second vacuum? Why were the motives for participating also different and in particular, what was the role of community protection in the second vacuum? How does that square with the rise in joiners who joined for extrinsic incentives? In the remainder of the paper, we explore the role of two mechanisms in the rise of the Raia: past victimization and revenge, and public goods provision/security protection in stressing individual intrinsic social motivations as well as community pre-existing informal institutions to provide security. As we will see, while victimization and revenge are important in both rises, a sharp increase in insecurity is entirely accountable for why the rise in Vacuum 2 was so spectacular, explaining a range of intrinsic and extrinsic social motivations to join the Raia—some of which were engineered by community institutions.

[22]Using detailed data on migration histories of all respondents, Section B.4 extensively analyzes migration. It documents that migrants are generally comparable to non-migrants, that the coefficients on the state vacuums are entirely unaffected by excluding individuals who ever migrated, and presents a counterfactual exercise allowing to deduce bounds on the coefficients under extreme assumptions about the migrants. Overall, the section provides strong support to the view that migration cannot play a role in explaining these coefficients.

Table 2.3: State Vacuum and the Birth of The Raia—Statistical Analysis

Panel A. The State Vacuums Cause the Rise of the Raia

	(1)	(2)	(3)	(4)	(5)
	Presence in the Village			Active Combatants	
	National Army	Militia Village Chapter		Militia Village Chapter	
	Stock	Stock	Inflow	Inflow	Individual
Vacuum 1 [Sun City Peace]	-10.49**	7.48	-1.35	4.91*	1.68*
	(5.12)	(6.08)	(2.77)	(2.74)	(0.91)
Vacuum 2 [Regimentation]	-31.38***	30.84***	34.53***	33.45***	16.96***
	(6.42)	(4.63)	(3.60)	(4.30)	(2.55)
Observations	2,491	2,491	2,491	2,436	15,106
R-squared	0.51	0.29	0.17	0.22	0.19
Village FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Individual FE					Y
Clustered at Individual-level					Y
Pre-Vacuum 1 Shabunda mean	0.00	41.30	4.35	0.00	0.00
Pre-Vacuum 2 Shabunda mean	76.09	0.00	0.00	0.00	0.00
P-value: Vacuum 1, Clustered at:					
Village	0.04	0.22	0.63	0.08	0.07
Village & Chiefdom-post Vacuum	0.00	0.02	0.31	0.01	0.00
Village & Chiefdom-year	0.09	0.21	0.62	0.21	0.16
P-value: Vacuum 2, Clustered at:					
Village	0.00	0.00	0.00	0.00	0.00
Village & Chiefdom-post Vacuum	0.00	0.00	0.00	0.00	0.00
Village & Chiefdom-year	0.00	0.03	0.00	0.00	0.01

Notes: Panel A presents the estimates of Equation 2.2 using the core sample, where the dependent variables are (in decimal digits): an indicator for whether there is presence of the Congolese national army in the village, for whether there is a militia village chapter (stock), for whether there is a new militia chapter (inflow), for whether there is new militia village chapter combatants in the village (inflow), and for whether the respondent of the household survey was participating in a militia village chapter at that year, in Columns 1–5 respectively. Column 5 is estimated at the level of the individual respondent * year (n=15,106), while Column 1–4 are from the village * year dataset (and thus indexed by jt rather than ijt). Column 1–4 control for village fixed effects and year fixed effects; standard errors are clustered at the village-level. Column 5 controls for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the village-level and individual-level. Table notes below the regression coefficients report p-values calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level. Table B.11 replicates Panel A by including the extra village sample from North Kivu.

Panel B. The Rise of the Raia is Driven by Extrinsic and Intrinsic Social Motivations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Social Motivations					Private Motivations	
		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	1.68*	0.45	-0.14	0.17	0.25	0.38	0.11	0.48
	(0.91)	(0.32)	(0.50)	(0.12)	(0.16)	(0.28)	(0.12)	(0.30)
Vacuum 2 [Regimentation]	16.96***	2.26***	9.75***	0.83*	1.72***	0.01	1.36***	1.23***
	(2.55)	(0.62)	(1.80)	(0.45)	(0.58)	(0.02)	(0.47)	(0.45)
Observations	15,106	15,106	15,106	15,106	15,106	15,106	15,106	15,106
R-squared	0.19	0.08	0.14	0.14	0.09	0.06	0.07	0.07
Village FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y
Cluster at Individual-level	Y	Y	Y	Y	Y	Y	Y	Y
Pre-Vacuum 1 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 1, Clustered at:								
Resp. & Village	0.07	0.16	0.78	0.17	0.13	0.17	0.38	0.10
Resp. & Village & Chiefdom-post Vacuum	0.00	0.00	0.68	0.00	0.00	0.00	0.05	0.00
Resp. & Village & Chiefdom-year	0.16	0.18	0.80	0.04	0.00	0.06	0.12	0.03
P-value: Vacuum 2, Clustered at:								
Resp. & Village	0.00	0.00	0.00	0.06	0.00	0.78	0.00	0.01
Resp. & Village & Chiefdom-post Vacuum	0.00	0.00	0.00	0.00	0.00	0.70	0.00	0.00
Resp. & Village & Chiefdom-year	0.01	0.00	0.02	0.04	0.03	0.01	0.04	0.01

Notes: Panel B presents the estimates of Equation 2.2 using the core sample, where the dependent variables are (in decimal digits): an indicator for whether the individual joins a militia village chapter (Column 1), for whether they joined it motivated by intrinsic social emotions (Columns 2–3), by extrinsic social incentives (Columns 4–6), or by private motivations (Column 7–8). All regressions control for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the level of village-level and individual-level. Table notes below the regression coefficients report p-values calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level. Table B.12 replicates Panel B by including the extra household sample from South Kivu.

2.6 Ultimate Causes: Revenge, and Response to Insecurity

We have documented that the rise of the Raia and of its predecessor reflects a variety of social motives and, to a lesser extend, private economic motives. In this section, we zoom in on two central drivers of those motivations in driving the response to the state withdrawals: the opportunity to violently express revenge motivations seeded in prior victimization by foreign-led armed groups, and the creation of insecurity and the subsequent response to that insecurity.

2.6.1 The Opportunity to Act Upon Victimization-Related Revenge

We now examine the role of revenge and victimization in the rise of the Raia. Fact 3 established that individuals who participated in the militia and who exhibited revenge motivations tended to disproportionately be prior victims. Furthermore, in Section 5, we have found that the state vacuums fueled participation into militias that was, in part, driven by individuals who reported to be motivated by the desire for revenge. In this section, we explore this revenge motive and the type of victimization that seeded the corresponding revenge motives for joining the Raia.

Table 2.4 presents the estimates of Equation 2.2, in which we have also added as a control, the following two indicators: $\mathbf{1}[V1_{jt} = 1] \times F_{ijt}$ and $\mathbf{1}[V2_{jt} = 1] \times F_{ijt}$, where F_{ijt} is an indicator taking value one if the household members of individual i in village j in year t have previously been victimized by the FDLR.[23] The coefficients on these two indicators therefore can be interpreted as the differential effect of each vacuum among individuals whose household members were previously victimized by the FDLR (which we have denoted victims). We can therefore examine the differential effect of the vacuums on the propensity of individuals to join the Raia and why.

[23] The analysis that follows produces qualitatively identical results if the variable is the count of attacks instead of an indicator for strictly positive number of attacks.

Table 2.4: The Role of Victimization and Revenge in the Rise of the Raia

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Social Motivations					Private Motivations	
		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	0.50	0.11	-0.17	0.06	0.05	0.07	0.10	0.31
	(0.74)	(0.20)	(0.50)	(0.04)	(0.08)	(0.15)	(0.14)	(0.30)
Vacuum 1 X Victimization	15.26***	4.40*	0.41	1.54	2.55	4.08**	0.04	2.28
	(3.73)	(2.37)	(1.32)	(1.35)	(1.83)	(2.03)	(0.22)	(1.96)
Vacuum 2 [Regimentation]	15.05***	1.61***	9.20***	0.96*	1.06**	0.02	1.40***	0.95**
	(2.41)	(0.55)	(1.84)	(0.52)	(0.44)	(0.03)	(0.45)	(0.40)
Vacuum 2 X Victimization	14.08***	4.80	4.04	-0.97*	4.85**	-0.12	-0.28	2.12
	(4.02)	(2.94)	(3.11)	(0.52)	(2.32)	(0.17)	(1.11)	(1.68)
Observations	15,106	15,106	15,106	15,106	15,106	15,106	15,106	15,106
R-squared	0.20	0.09	0.14	0.15	0.10	0.08	0.07	0.08
Pre-Vacuum 1 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 2, Clustered at:								
Resp. & Village	0.00	0.00	0.00	0.07	0.02	0.40	0.00	0.02
Resp. & Village & Chiefdom-post Vacuum	0.00	0.00	0.00	0.00	0.00	0.24	0.00	0.00
Resp. & Village & Chiefdom-year	0.01	0.00	0.02	0.06	0.01	0.29	0.05	0.00
P-value: Vacuum 2 X Victimization, Clustered at:								
Resp. & Village	0.00	0.10	0.20	0.06	0.04	0.47	0.80	0.21
Resp. & Village & Chiefdom-post Vacuum	0.00	0.00	0.00	0.00	0.00	0.01	0.30	0.00
Resp. & Village & Chiefdom-year	0.02	0.01	0.00	0.16	0.11	0.00	0.57	0.19

Notes: This table presents the estimates of Equation 2.2 using the core sample, in which we have also added as a control, the following two indicators: $\mathbf{1}[V1_{jt} = 1] \times F_{ijt}$ and $\mathbf{1}[V2_{jt} = 1] \times F_{ijt}$, where F_{ijt} is an indicator taking value one if the household members of individual i in village j in year t have previously been victimized by the FDLR. The dependent variables are (in decimal digits): an indicator for whether the individual joins a militia village chapter (Column 1), for whether they joined it motivated by intrinsic social emotions (Columns 2–3), by extrinsic social incentives (Columns 4–6), or by private motivations (Column 7–8). All regressions include controls for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the individual-level and village-level. Table notes below the regression coefficients report the p-values for the key coefficients, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level. This estimation cannot be run with the extra samples because only the core sample from South Kivu has both information on the individual-level participation motives and attack history.

Column (1) shows that the effect of Vacuum 1 on participation in the Raia's predecessor was entirely driven by individuals from households who had previously been victimized by the FDLR. In contrast, while Vacuum 2's effect is almost twice as large among individuals from households that were previously victimized by the FDLR, a large share of its effect was also among non-victims. Therefore, while prior FDLR victimization is an important predictor of *who* joins the Raia in its rise, the effect of Vacuum 1 is *entirely* driven by the victims.[24]

What are the motives of victimized individuals who join in response to the vacuums? In Vacuum 1, where the entire effect is driven by the victims, victims join mostly for revenge (and also, because they were coerced by the village militia chapter). In Vacuum 2, while non victims join for a variety of private and social motivations (except being coerced by the chapter), victims who join in response to Vacuum 2 are predominantly motivated by social pressure and, while marginally significant, revenge and private protection.

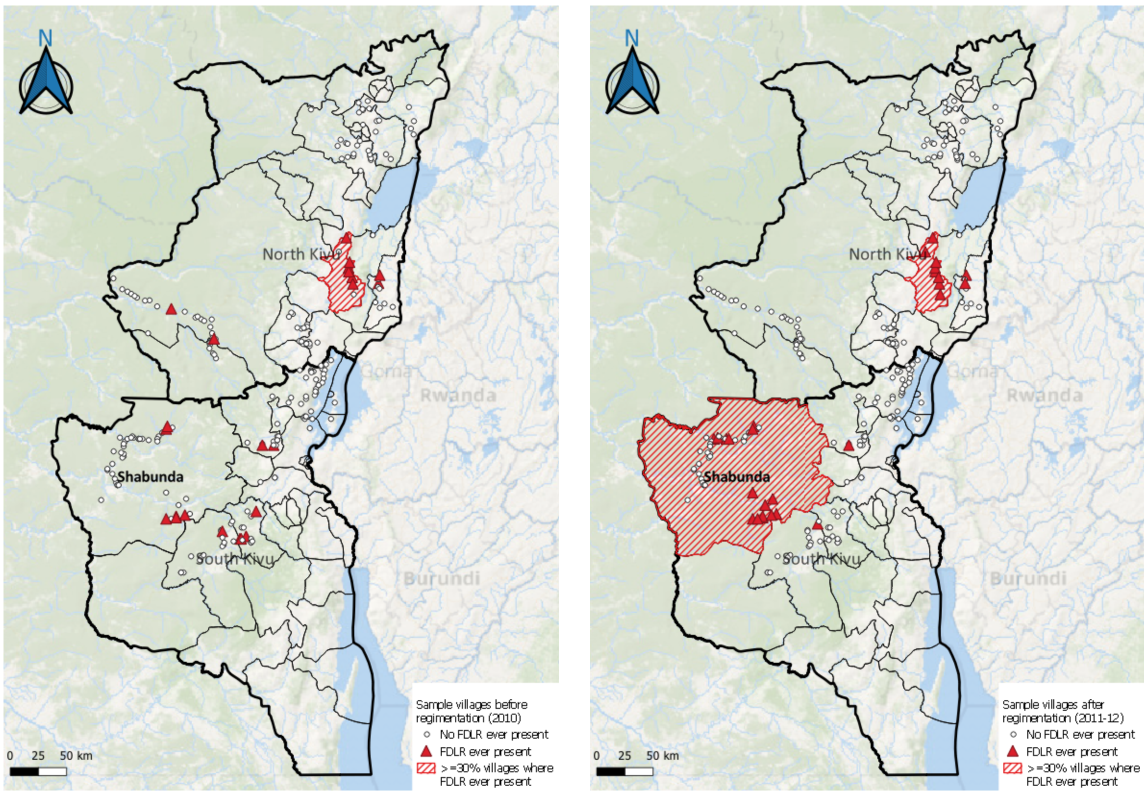
This analysis suggests that prior households victimization produces participation into the village militia chapters motivated, in part, for revenge, but also private protection and other types of social pressures indicating that communities exert pressure on victims to join (this is consistent, for example, with honor motives). To explore whether this reflects intra-village correlation of victimization histories or instead a between-households differentiation of victims and non-victims behavior, Table B.14 replicates Table 2.4, including $\mathbf{1}[V1_{jt}=1]\times F^H_{ijt}$ and $\mathbf{1}[V2_{jt}=1]\times F^H_{ijt}$, but we also include village-year fixed effects; thus the coefficients on $\mathbf{1}[V1_{jt}=1]\times F^H_{ijt}$ and $\mathbf{1}[V2_{jt}=1]\times F^H_{ijt}$ indicate the differential effect of the vacuums on previously victimized households compared to non-previously victimized households in the same community. Interestingly, the coefficient on $\mathbf{1}[V1_jt=1]\times F^H_{ijt}$ for participation as a dependent variable remains positive and significant (and is half in magnitude) but $\mathbf{1}[V2_{jt}=1]\times F^H_{ijt}$ loses positive sign and significance.

Taken together, this means that, for the predecessor of the Raia, which was of a smaller scale, it was, in large part, an FDLR victims' phenomenon; in contrast, this also means that, during the subsequent extraordinary rise of the Raia in 2011, while participation was twice as large in communities with more victimized households, the victimized households of those communities were just as likely to join the Raia in response to the vacuum of 2011 than their non-victimized neighbors in the village: it was a generalized mobilization, consistent with its extraordinary nature. Therefore, if revenge was indeed central in the rise of the Raia and its predecessor, it was revenge motives held by community members for the victimization of their

[24]Table B.13 provides a suggestive explanation suggesting that victimization explains the entire rise due to Vacuum 1. We estimate Equation 2.2 and as a control the interaction between the post Sun City years and an indicator for whether the community was previously victimized. The table shows that the location of village militia chapter participation ensuing the state withdrawals of Sun City is *entirely* driven by prior victimization. That underscores that the predecessor of the Raia is explained by prior FDLR victimization.

community and peers, not necessarily their own household, which sparked the spectacular rise of the Raia in 2011.

Figure 2.4: Security Vacuum: Presence of FDLR Predatory Group

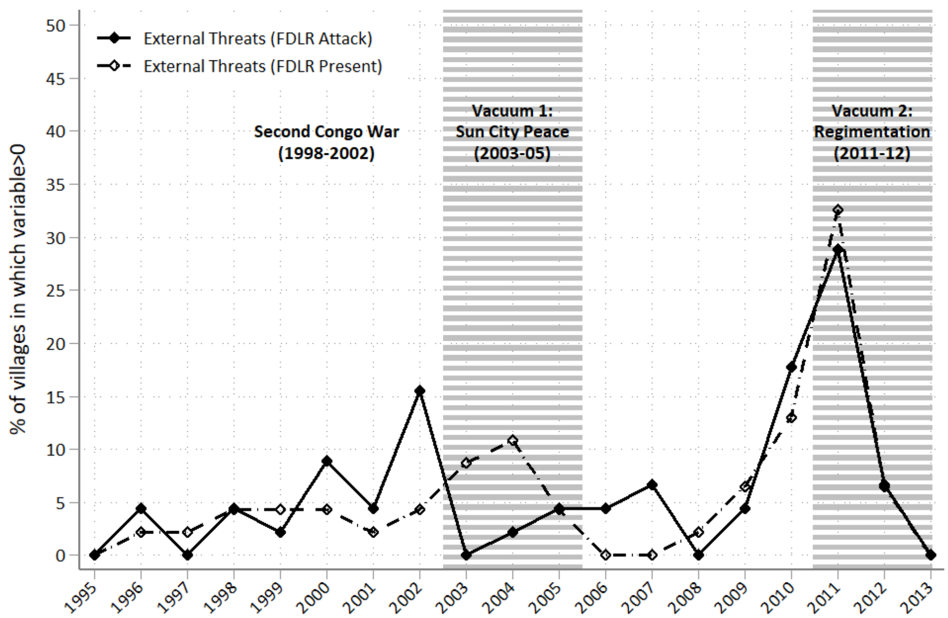


Notes: This figure shows the presence of the FDLR, a foreign-led predatory armed group known to be violent against civilians around the time of the Regimentation (2011). In the post-regimentation map on the right, a red triangle is a village where the FDLR is present for either 2011 or 2012, and a white dot is a village where the FDLR is not present in either years. The red areas are chiefdoms where at least 30% are controlled by the FDLR; the cutoff 30% is selected because among cheifdoms where the Congolese army is present, on average, roughly 30% villages are controlled by the Congolese army.

In sum, revenge is a proximate cause of the rise of the Raia. The vacuums unleashed pre-existing revenge motives towards the FDLR after prior victimization: for the spectacular rise of 2011, the vacuum unleashed revenge among victimized communities in victims and

Figure 2.5: Insecurity over Time: Shabunda and the Rest of Villages

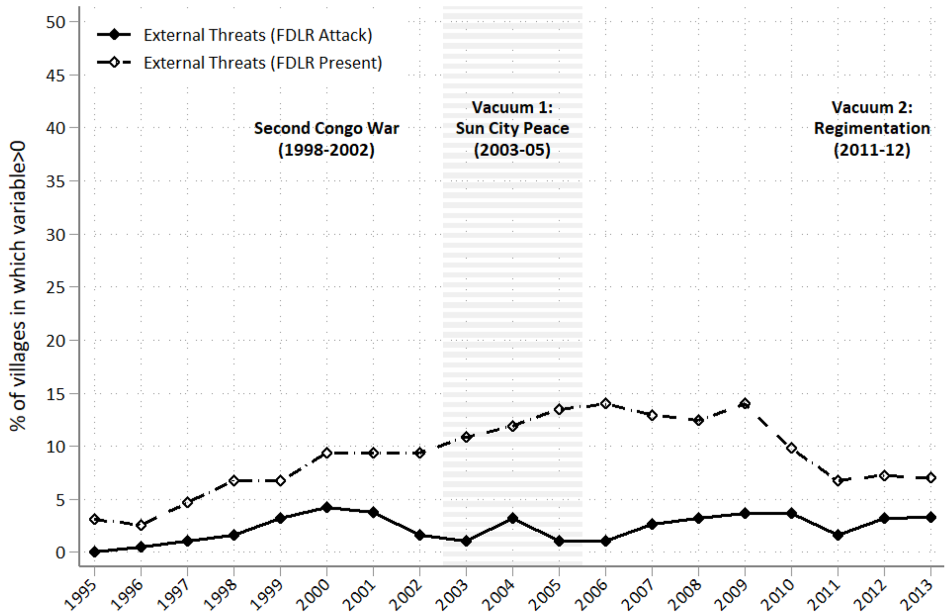
Panel A. Villages in Shabunda



non-victims.

This analysis also leaves a number of unanswered questions, to which we now turn. First, while the predecessor of the Raia was a victimization phenomenon, the rise of 2011 was largely composed of communities, and individuals, who had not been previously victimized by the FDLR. This rise was much larger than its predecessor and, while revenge appeared to be one of its proximate causes, the main motive for participating among previously victimized communities and the rest alike was the *protection of the community.* Can security provision explain the extraordinary scale of the Raia's emergence in 2011? Second, we have seen that social pressure motivations were central in the Raia's rise: how is social pressure related to the provision of community security? In the next sub-section, we examine the role of insecurity in the rise of the Raia and, in the last section, we explore the mechanisms through which communities' responses to insecurity created social pressure to join the Raia and amplified intrinsic motivations to protect the community.

Panel B. Other villages



Notes: This figure shows the presence of FDLR and attacks in a times-series format. Panel A restricts the sample to Shabunda, the district affected by the military policy-induced state vacuum of 2011. Panel B shows this for the remaining of the sample. Left and right grayed areas indicate years in which documented policy-driven state vacuums were associated to the rise of the Raia and to its predecessor. Both FDLR presence and FDLR attack information are taken from the village module. We use both the core sample from South Kivu and the extra village sample from North Kivu to present the yearly trends. The black solid line with solid dots shows the fraction of villages where FDLR conducts a violent attack. The black dashed line with hollow dots shows the fraction of villages where FDLR is seen present in the village.

2.6.2 A Sharp Increase in the Value of Providing Security

In the previous sections, we have seen that the rise in Raia participation in Shabunda in response to the second vacuum was in part driven by individuals with the motivation to protect the community. We now analyze whether genuine increases in the *value* of providing security, resulting from a rise in insecurity suggested in the qualitative studies (Stearns, 2013), can in part explain why the rise of the Raia acquired such an spectacular scale compared to its predecessor.

The Regimentation exposed the communities in Shabunda to an unprecedented rise in insecurity, but the earlier state vacuum of 2004 did not. The map in Figure 2.4, shows that just as the Regimentation withdrew the national army, the FDLR group, known to be predatory, flooded various areas of Shabunda, thus exposing various villages to extraordinary insecurity. Figure 2.5 shows that the fraction of Shabunda villages affected by FDLR neighboring presence or FDLR attacks spikes from 5% in 2009 to 32% for presence and 30% for attacks in 2011, but remains constant outside Shabunda. Importantly, and likely reflecting the effectiveness of the Raia in chasing out the FDLR, FDLR-related insecurity is drastically reduced by 2012 and essentially muted by 2013. This rise in insecurity is absent (and in fact, reversed) during the first vacuum, which marked the end of the Second Congo War, where large rebel groups (except the FDLR) vacated the region as part of a national peace agreement.

To examine whether the rise in insecurity due to Vacuum 2 is statistically significant, Table B.15 presents the estimates of Equation 2.2 whereby the dependent variables are the corresponding indicators of FDLR-related insecurity in the *initial* years of the vacuum, in order to examine insecurity prior to the countervailing effect of the Raia. The table shows that the increase in insecurity caused by the Regimentation in Shabunda is significant; in contrast, the first vacuum did not significantly increase FDLR-related insecurity: if anything, FDLR attacks only decreased concomitantly with the 2004 state vacuum, thus only the 2011 vacuum caused insecurity.[25]

Overall, this analysis shows that, unlike the state vacuum of 2004, the 2011 state vacuum induced by the Regimentation *caused* an increase in insecurity, and is therefore a well-suited case study to examine the how the value of providing security may explain the rise of the Raia. However, this analysis is not evidence that the *larger* rise of the Raia after the Regimentation is due to the *larger* rise in insecurity—it might just be coincidental. In what follows, we analyze the spatial patterns of the rise in insecurity of 2011, which, as we have seen, affected only 32% of the Shabunda villages, and ask whether the spatial pattern of insecurity coincides with the rise of the Raia chapters, and isolate the effect of insecurity from other factors such as past victimization in why the rise of the Raia was so much larger than its predecessor of 2004.

We now examine of the role of insecurity by breaking down the effect of the second vacuum on participation by whether the community experienced a rise in insecurity at Vacuum 2.

The role of insecurity in the extraordinary rise of the Raia. Table 2.5 conducts this analysis. Column (1) replicates the main effect of the vacuums as benchmark. The table also reports the p-value for rejecting the null hypothesis that the coefficient on Vacuum 2 is smaller than that on Vacuum 2. The p-value is 0, indicating that the effect of Vacuum 2 on participation is significantly larger than that of Vacuum 1. Column (2) turns to testing the main hypothesis in this section, namely, whether the differential rise of the Raia in Vacuum

[25]This pattern is consistent with the known facts about the end of the Second Congo War: while other armed groups, such as the RCD and many Mayi-Mayi militias vacated the area, the FDLR remained as they were not part of the agreement, reducing conflict beween present parties.

2 can be explained by the differential creation of insecurity. The column includes as a control the product between Vacuum 2 and an indicator for FDLR presence in nearby villages in the window 2010–11, *Vacuum 2 X Insecurity (2010-11)*. The coefficient on *Vacuum 2 X Insecurity (2010-11)* is large (20.5%) and significant at the 1% level, and including this coefficient completely destroys the coefficient on *Vacuum 2*. Importantly, the p-value for rejecting the null hypothesis that the coefficient on Vacuum 2 is smaller than that on Vacuum 1, jumps from 0.00 when *Vacuum 2 X Insecurity (2010-11)* is not included as a control to .86, indicating the entire difference is explained by the places that experience an initial rise in insecurity due to the Regimentation; the rise in the Raia in the other communities is not larger than the predecessor rise of 2004. This is therefore conclusive evidence that the effect of Vacuum 2 on the rise of the Raia was *entirely* driven by the communities that, as a result of the state vacuum, did experience an initial rise in insecurity.

However, the differential rise in the communities that experience a rise in insecurity is not conclusive evidence that the rise is concentrated in those communities *because* of their sudden exposure to insecurity. This strong relationship could mask that Vacuum 2 differs from Vacuum 1 on various dimensions, and those could potentially also be different in the places that see the drastic rise in insecurity due to the Regimentation. We now attempt to unbundle the comparison between the effect of Vacuum 1 and Vacuum 2. We consider the following two important alternative channels. First, the predecessor mobilization might have created a legacy in some places, enabling their later rise (dynamic spillovers from the first mobilization). Second, violence by the FDLR continued, and it is possible that the stock of victims might have been concentrated in the same places that experienced the sudden rise in insecurity.

Separately identifying the role of insecurity from past participation and past victimization. If dynamic spillovers from the first mobilization matter, such as the creation of networks and expertise, then individuals who have previously participated in the predecessor of the Raia might be more likely to participate than the rest in Vacuum 2. Column (3) includes, as a control, the product of Vacuum 2 with an indicator for whether individual i participated in a village militia chapter in Shabunda during the first vacuum *Vacuum 2 X Past participation (2003-05)*. The coefficient on *Vacuum 2 X Past participation (2003-05)* is positive and statistically significant, as one would expect, confirming that participation in the predecessor is positively associated with subsequent participation. However, including *Vacuum 2 X Past participation (2003-05)* as a control leaves the coefficient on Vacuum 2 unaffected and significant. The magnitude drops only from 16.96 in Column (1) to 15.80 and the p-value for whether the coefficient on Vacuum 2 is larger than Vacuum 1 remains 0.00, entirely unaffected at the second digit. This provides evidence that, while past participation is important in explaining the rise of the Raia in 2011, it alone cannot explain away the rise of the Raia. Column (4) includes as a control, the product of Vacuum 2 with the stock of victimization by the FDLR in individual *i Vacuum 2 X Stock of Victimization*. The coefficient on *Vacuum 2 X Stock of Victimization* is positive and statistically significant, as we know, confirming that past FDLR victimization is positively associated with subsequent participation. However, just as for past participation, its inclusion leaves the role of insecurity unaffected. Indeed, including *Vacuum 2 X Stock of Victimization* as a control leaves the coefficient on Vacuum 2 unaffected and significant. The magnitude drops only from 16.96 in Column (1) to 15.53 and the p-value for whether the coefficient on Vacuum 2 is larger than Vacuum 1 remains 0.00. This provides evidence that, while the stock of victimization is important in explaining the rise of the Raia in 2011, it alone cannot explain away the rise of the Raia. Unlike insecurity, neither past participation nor past victimization can explain away why the rise of the Raia was so spectacular. To unbundle these two proximate causes for the rise of the Raia, Column (5) includes all three *Vacuum 2 X Insecurity (2010-11)*, *Vacuum 2 X Past participation (2003-05)* and *Vacuum 2 X Stock of Victimization* as controls. The result is quite unambiguous: while the coefficients on *Vacuum 2 X Past participation (2003-05)* and *Vacuum 2 X Stock of Victimization* are small and marginally significant, the coefficient on *Vacuum 2 X Insecurity (2010-11)*, is unaffected by their inclusion. Indeed, the coefficient on *Vacuum 2 X Insecurity (2010-11)*, drops from 20.54 in Column (2) (where such additional two controls were not included) to 19.01 in Column (5) (where they are), and remains statistically significant at the 1% level. What is more, the coefficient on Vacuum 2 remains indistinguishable from zero, and the p-value for whether the coefficient on Vacuum 2 is larger than that on Vacuum 1 is .92.

In sum, this analysis shows that, together, dynamic spillovers, cumulative victimization, and insecurity can explain away the extraordinary rise of the Raia in the second vacuum compared to the first, yet, as Column (2) has shown, this difference is *entirely* driven by the location of the rise in insecurity caused by the sharp departure of state forces in Vacuum 2, absent in Vacuum 1.

Yet, what remains to be interrogated is whether the rise in participation motivated by the protection of the community in Vacuum 2 is entirely channeled through the communities that, as a result of Vacuum 2, *do* experience in rise in actual insecurity. To examine this question, Panel B, estimates Equation 2.2 on individual participation and participation by motives, but includes *Vacuum 2 X Insecurity (2010-11)* as a regressor. Column (1) replicates the result that Vacuum 2's effect on participation is entirely channeled through communities that did experience a rise in insecurity in 2010-2011 as benchmark. In Columns (2)–(8), the dependent variables are indicators for individual participation for each participation motive. The analysis in Panel B allows us to conclude that the effect of Vacuum 2 on participation motivated by protection of the community is also *entirely* driven by the communities that experience a rise in insecurity.

Table 2.5: Unbundling the Extraordinary Rise of the Raia: the Role of Community Insecurity

Panel A. Community Insecurity Explains Entirely the Extraordinary Rise of the Raia

	(1) Participate	(2) Participate	(3) Participate	(4) Participate	(5) Participate
Vacuum 1 [Sun City Peace]	1.68*	1.86**	1.70*	1.66*	1.86**
	(0.91)	(0.91)	(0.92)	(0.91)	(0.91)
Vacuum 2 [Regimentation]	16.96***	0.46	15.80***	15.53***	0.09
	(2.55)	(0.99)	(2.58)	(2.48)	(1.07)
Vacuum 2 X Insecurity (2010-11)		20.54***			19.01***
		(2.71)			(2.74)
Vacuum 2 X Past Participation (2003-05)			10.91**		6.63
			(4.48)		(4.64)
Vacuum 2 X Stock of Victimization				7.43***	4.82*
				(2.63)	(2.70)
Observations	15,106	13,982	15,106	15,106	13,982
R-squared	0.19	0.21	0.19	0.19	0.21
Pre-Vacuum 1 Shabunda	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 2, Clustered at:					
Resp. & Village	0.00	0.64	0.00	0.00	0.94
Resp. & Village & Chiefdom-post Vacuum	0.00	0.38	0.00	0.00	0.87
Resp. & Village & Chiefdom-year	0.01	0.92	0.01	0.01	0.99
P-value: Vacuum 2 < Vacuum 1	0.00	0.86	0.00	0.00	0.92

Notes: Panel A presents the estimates of Equation 2.2 using the core sample, in which we have added controls $\mathbf{1}[V1_{jt} = 1] \times F_{ijt}$, where F_{ijt} is one of the following: (i) an indicator whether FDLR is present in nearby villages in the same Groupement during 2010–11, but not in own village (Insecurity, indexed by j), (ii) an indicator whether the respondent participated a militia village chapter during the first state vacuum induced by Sun-city peace agreement (Past participation, indexed by ij), and (iii) number of household-level FDLR attacks in the past (indexed by ijt). Column 5 includes all three controls in the same regression. The dependent variable is (in decimal digits) an indicator for whether the respondent of the household survey was participating in a militia village chapter in that year. All regressions control for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the village-level and individual-level. Table notes below the regression coefficients report the p-values for the coefficients of Vacuum 2, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level. In the last row, we compute p-values of rejecting the null hypothesis that the coefficient of Vacuum 2 is smaller than that of Vacuum 1. Table B.16 replicates Panel A by including the extra village sample from North Kivu and the extra household sample from South Kivu.

Panel B. Community Insecurity Entirely Explains the Motivations to Protect the Community in the Rise of Raia

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Social Motivations					Private Motivations	
		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	1.86**	0.45	0.01	0.16	0.23	0.45*	0.11	0.48
	(0.91)	(0.32)	(0.49)	(0.12)	(0.17)	(0.27)	(0.12)	(0.30)
Vacuum 2 [Regimentation]	0.46	-0.06	-0.25	0.03	0.10**	0.09*	0.77	-0.01
	(0.99)	(0.14)	(0.35)	(0.02)	(0.04)	(0.05)	(0.82)	(0.12)
Vacuum 2 X Insecurity (2010-11)	20.54***	2.83***	12.39***	0.98*	1.96***	-0.10	0.72	1.51***
	(2.71)	(0.75)	(1.96)	(0.53)	(0.69)	(0.06)	(0.98)	(0.54)
Observations	13,982	13,982	13,982	13,982	13,982	13,982	13,982	13,982
R-squared	0.21	0.09	0.15	0.15	0.09	0.07	0.07	0.08
Pre-Vacuum 1 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 2, Clustered at:								
Resp. & Village	0.64	0.68	0.48	0.20	0.01	0.06	0.35	0.94
Resp. & Village & Chiefdom-post Vacuum	0.38	0.68	0.34	0.00	0.00	0.00	0.02	0.83
Resp. & Village & Chiefdom-year	0.92	0.90	0.93	0.93	0.85	0.13	0.07	0.98
P-value: Vacuum 2 X Insecurity, Clustered at:								
Resp. & Village	0.00	0.00	0.00	0.07	0.01	0.10	0.46	0.01
Resp. & Village & Chiefdom-post Vacuum	0.00	0.00	0.00	0.00	0.00	0.00	0.04	0.00
Resp. & Village & Chiefdom-year	0.10	0.02	0.12	0.19	0.17	0.09	0.19	0.09

Notes: Panel B presents the estimates of Equation 2.2 using the core sample, in which we have also added as a control, the following indicator: $\mathbf{1}[V2_{jt} = 1] \times F_j$, where F_j is an indicator taking value one if FDLR is present in nearby villages in the same Groupement during 2010–11, but not in own village j. The dependent variables are (in decimal digits): an indicator for whether the individual joins a militia village chapter (Column 1), for whether they joined it motivated by intrinsic social emotions (Columns 2–3), by extrinsic social incentives (Columns 4–6), or by private motivations (Column 7–8). All regressions include controls for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the individual-level and village-level. Table notes below the regression coefficients report the p-values for the key coefficients, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level. Table B.17 replicates Panel B by including extra household sample from South Kivu.

Panel B also presents the last puzzle to provide a coherent explanation for the social origins of the Raia. Intriguingly, the communities that, as a result of the Regimentation, experience a sudden rise in insecurity also see a disproportionate increase in Raia participation due to *status concerns* and *social pressure*, and the rise in participation due to these motives is almost entirely driven by communities that experience a rise in insecurity. How is social pressure and status, extrinsic social motivations, at all related to the desire to protect the community? As the following section suggest, this differential rise reflects the activation of community institutions in response to the collective action problem of providing security when the value of providing security has skyrocketed.[26]

2.7 Community Mechanisms for the Response to Insecurity

The previous section has presented evidence that the spectacular rise of the Raia in 2011 can be explained only by the spatial pattern of insecurity created by the Regimentation policy. At the same time, when examining the motivations of the villagers who joined its rise, alongside private motivations and intrinsic social motivations to protect the community, a significant share responded to the insecurity by joining because they were socially pressured or for status. In this section we examine the role of community institutions in inducing pressure and status concerns, but also other social motivations, to solve the collective action problem of providing security.

To empirically analyze the activation of community institutions in the rise of the Raia, we focus on community "sensitization" campaigns. Campaigns can be of two types. On the one hand, public sensitization campaigns are regular communal gatherings where the customary leaders communicate with the community, often to bring the attention to challenges, or to uphold community norms to navigate particular collective action problems, commonly referred to as mobilization sensitization campaigns.[27] Mobilization sensitization campaigns are sometimes initiated by militia themselves and are announced as such, and sometimes are the initiative of the chief and are also announced and known to be as such, but both generally rely on the community's existing mechanisms and leadership. On the other hand, private sensitization through networks is also a common way for armed groups, especially

[26]Columns (2) and (8) show that the communities experiencing a rise in insecurity as a result of the Regimentation also experience a deferentially larger rise in participation motivated by revenge and private protection. Yet, Table B.18 further shows that this rise cannot be explained by the higher concentration of victimized communities among those that experience a rise in insecurity.

[27]Figure B.12 presents an example of a public recruitment campaign by a militia in eastern DRC, taken in 2013.

foreign-led and thus who lack the legitimacy to organize public meetings to call for people to join them, to obtain recruits. Enrollment into militias in the rural communities is channeled through either public or private campaigns, where public campaigns are the activation of community institutions.

We gathered information, for each militia chapter episode and for each year, on whether there were recruitment related sensitization campaigns, whether those were public and/or private, and whether those were directly *initiated* by the chief. While the actual initiator might sometimes be hard to ascertain with certainty (it could be that in some cases the group asks the chief to pretend that the chief initiated the campaign), the data collection techniques we have developed, based on one week of building trust, allow us to be confident whether the chief really voluntarily initiated a recruitment related sensitization campaign. Table B.19 shows that militia chapters, especially those that are formed in the village, draw on recruitment campaigns during their governance episode. Militia chapters formed in the village are the only type of armed group chapter whose recruitment campaigns are directly initiated by the chief himself. They are also more likely to rely on public meetings. Militia chapters formed in the village rely on public or chief-initiated campaigns in 40% of their years. This contrasts with chapters formed outside, who do so only in 10%. Figures B.13 shows the distribution of public and chief-initiated campaigns in the sample, as well as over time across communities. There are 142 public and 25 chief-initiated campaigns. In what follows, we focus on public campaigns because, as shown in Figure B.13, there are only 25 chief-initiated campaigns, and public campaigns are a proxy, as just discussed, for community initiative (as opposed to recruitment by foreign-led groups which tends to be carried out secretly).

Using these data, we now examine the role of sensitization campaigns in the rise of the Raia and in the type of motivations that the vacuum unleashed. Our analysis proceeds in three steps: we first analyze whether the state vacuum and in particular the insecurity it created, caused a rise in sensitization campaigns, which would be implied by these campaigns being a response to the collective action problem of providing security. We then, examine whether the rise in the Raia participation is in part channeled through the communities that do have those campaigns. And, finally, we analyze what types of motivations are channeled through those campaigns in response to the Regimentation-induced vacuum.

Table 2.6: The Extraordinary Rise of the Raia is in Part Channeled by Communities Responses to Insecurity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
					Social Motivations					Private Motivations	
	Campaign		Participation		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	Public	Public	General	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	0.51	0.68	1.68*	1.64*	0.45	-0.16	0.17	0.25	0.38	0.11	0.48
	(3.77)	(3.84)	(0.91)	(0.91)	(0.32)	(0.50)	(0.12)	(0.16)	(0.28)	(0.12)	(0.30)
Vacuum 2 [Regimentation]	8.75**	2.07	16.96***	11.62***	1.95***	6.79***	0.05	0.74*	0.02	1.33***	0.90**
	(3.56)	(7.03)	(2.55)	(2.42)	(0.69)	(1.73)	(0.12)	(0.39)	(0.03)	(0.45)	(0.41)
Vacuum 2 X Insecurity		8.26									
		(7.83)									
Vacuum 2 X Public Campaign				31.62***	1.89	17.48**	4.65*	5.79**	-0.07	0.21	2.00
				(10.35)	(1.76)	(7.25)	(2.65)	(2.67)	(0.10)	(1.72)	(1.99)
Observations	2,454	2,284	15,106	14,855	14,855	14,855	14,855	14,855	14,855	14,855	14,855
R-squared	0.26	0.26	0.19	0.23	0.09	0.16	0.17	0.10	0.06	0.07	0.08
Pre-Vacuum 1 Shabunda	23.91	23.91	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda	2.17	2.17	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 2, Clustered at:											
Resp. & Village	0.02	0.77	0.00	0.00	0.01	0.00	0.70	0.06	0.55	0.00	0.03
Resp. & Village & Chiefdom-post Vacuum	0.00	0.63	0.00	0.00	0.00	0.00	0.02	0.00	0.14	0.00	0.00
Resp. & Village & Chiefdom-year	0.11	0.64	0.01	0.02	0.00	0.03	0.61	0.03	0.44	0.07	0.00
P-value: Vacuum 2 X Insecurity/Campaign, Clustered at:											
Resp. & Village		0.29		0.00	0.28	0.02	0.08	0.03	0.47	0.90	0.32
Resp. & Village & Chiefdom-post Vacuum		0.09		0.00	0.00	0.00	0.00	0.00	0.00	0.67	0.00
Resp. & Village & Chiefdom-year		0.16		0.00	0.01	0.00	0.00	0.00	0.29	0.78	0.02

Notes: This table presents the estimates of Equation 2.2 using the core sample. In Column 2 and Column 4–11, we have also added as a control, the following indicator: $\mathbf{1}[V2_{jt} = 1] \times F_{jt}$, where F_{jt} is one of the following indicators: (i) whether FDLR is present in nearby villages in the same Groupement during 2010–11, but not in own village j (Column 2, Insecurity, indexed by j), and (ii) whether village j in year t has a public campaign (Column 4–11, Campaign, indexed by jt). The dependent variables are (in decimal digits): an indicator for whether the village has a public campaign (Column 1–2), for whether the individual joins a militia village chapter (Column 3–4), for whether they joined it motivated by intrinsic social emotions (Columns 5–6), by extrinsic social incentives (Columns 7–9), or by private motivations (Column 10–11). Column 1–2 control for village fixed effects and year fixed effects; standard errors are clustered at the village-level. Column 3–11 control for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the individual-level and village-level. Table notes below the regression coefficients report the p-values for the key coefficients, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level. Table B.20 replicates the table by including extra village sample from North Kivu (Column 1–2) and extra household sample from South Kivu (Column 3–11). Table B.21 replicates the table by replacing public campaigns with 25 chief-initiated campaigns.

First, we analyze whether Vacuum 2 caused a rise in recruitment-related sensitization campaigns. Table 2.6 presents the analysis.[28] In Column (1), we estimate Equation 2.2 where the dependent variable is an indicator for whether there is a public campaign. The coefficient on Vacuum 2 is large (8.8%) and significant; for Vacuum 1 it is zero and not significant at any conventional level.[29] Column (2) shows that this rise is entirely concentrated in places that have insecurity, consistent with those campaigns being a response to community insecurity. Thus, consistent with Vacuum 2 creating a public goods problem, it led to a rise in sensitization campaigns; consistent with Vacuum 1 not creating a public goods problem, it did not cause that rise.

Second, we analyze whether the rise in individual participation in the Raia in response to Vacuum 2 is channeled through places that hold such campaigns. Column (3) replicates Equation 2.2 as benchmark at the level of the individual respondent, and Column (4) includes as a regressor the product of the Vacuum 2 indicator and an indicator for whether there was a recruitment-related public campaign in that year and village, *Vacuum 2 X Campaign*. The coefficient on Vacuum 2 itself in Column (4) is about 69% the magnitude in Column (3), and the coefficient on *Vacuum 2 X Campaign* is large and statistically significant at the 1% level. Therefore, a significant share of the effect of Vacuum 2 on militia village chapter participation (about 31%) is channeled through communities that hold a recruitment-related sensitization meeting, where the participation response is much larger, consistent with community institutions playing at least some role in the Raia's emergence.

Third, we analyze the type of motivations held by the participants who join the rise in the places that do have campaigns vs. those that do not in response to the vacuum. Columns (5)–(11) replicate the analysis in Column (3) but the dependent variables are now, respectively, an indicator for participation for each purported motivation. The coefficient on *Vacuum 2 X Campaign* is positive and statistically significant for status and social pressure, and is also positive and significant for community-protection motivated participation. Regarding community protection, the coefficient on Vacuum 2 remains large (it is about a third of the benchmark coefficient of Column (3) and statistically significant). In contrast, for status and social pressure, the coefficients on Vacuum 2 are either zero and insignificant, or marginally significant and quite small relative to the baseline. Taken together, this provides suggestive evidence that the community institutions are important in channeling the effect of Vacuum 2 by upholding community norms that: a. simply engineer extrinsic social incentives for participating (creating status considerations and inducing social pressure for

[28]We replicate the following analysis using the 25 chief-initiated campaigns in Table B.21 in the Appendix and the results are similar and somewhat weaker.

[29]Figure B.14 in the Appendix shows the corresponding time series. Both campaigns spike in Shabunda after Vacuum 2, but are unaffected in Vacuum 1, consistent with the paper's thesis that Vacuum 2 engendered community mechanisms for security provision.

participating); b. amplify intrinsic social motivations to participate for the protection of the community. Some villagers already feel the desire to protect the community, but the sensitization campaigns amplify/validate those intrinsic social emotions, and also uphold norms creating status and social pressure concerns to participate.

Campaigns are, of course, endogenous too. This is important, because the latter relationship could reflect leaders' priming and inducing social desirability bias in the survey responses, and public meetings are endogenous, hence this analysis could also simply indicate that the public campaigns occur precisely in places with the strongest extrinsic and intrinsic motivations to participate. While this is possible, it would however be more natural to expect these public meetings to be called precisely in the places that have a collective action problem to solve in the first place, that is, where individual motivations are the weakest to begin with, not the largest.[30]

Overall, this suggests that, by upholding norms and hierarchies of the community, community institutions were able to respond to the public goods problem of insecurity created by the Regimentation, and engineered the creation of extrinsic motives of status and social pressure to enhance participation into the village militia chapters, complementing the already-existing motivations. While previous sections had shown that the rise in insecurity caused by the Regimentation explains away entirely why the rise of the Raia was so extraordinarily large in 2011 compared to 2004, such extraordinary response to insecurity is only in part (31%) explained by community institutions while the rest are existing individual motivations that align individual incentives with the community public good.

2.8 Conclusion

Analyzing the critical juncture of the Outraged Citizens in 2011 in the Democratic Republic of the Congo in "real time" (Callen et al., 2023) as a proof of concept, we provide evidence for the role of community rationality in the emergence and growth of nonstate armed actors. Using both self-reported motivations and a revealed preference approach, we documented that community-oriented social motivations are the most prevalent explanation for the rise of militia chapters. Using data on the type of recruitment campaigns and self-reported motivations, we documented that village leaders play an important role in engineering ex-

[30]Complementing this analysis, Table B.22 breaks down the effect of the campaigns by whether the household was victimized. It shows that public campaigns channel the rise in participation among victims that is associated to social pressure, while campaigns channel the rise in participation among non victims associated with status, consistent with community institutions inducing social pressure on the victimized to increase participation.

trinsic social motivations such as status and social pressure and in amplifying pre-existing bottom-up intrinsic motivations such as the desire to protect the community.

Joining a militia constitutes an important life decision for individuals who experience the extreme violence of civil war. We show that, while individual gain to joining a militia is limited, membership in those organizations is motivated by community-oriented motivations similar to those that scholars typically attribute to social movements, protests, and political movements. This is consistent with the qualitative empirical literature on the eastern Congolese recent history, which has documented the importance of community for armed mobilisation (including for the case of the Raia (Vogel, 2014)), as well as accounts of the lives of Congolese ex-combatants (Stearns, 2011; Brabant, 2016; Dunia Butinda, 2021). It is also consistent with a large body of literature in the social sciences that has explored political, social and emotional motivations for armed mobilisation, as well as novels that describe the personal process that leads to taking up arms (Hemingway, 1940; Guevara and Ortiz, 1969; Barea, 1984; Malraux, 1938; Kourouma, 2000).

Community militias are not unique to this historical episode. This event mirrors a phenomenon observed across the contemporary world: The proliferation of vigilante groups and community militias who effectively replace the state and, often, resolve security issues more effectively than state security forces. In 2012–13, a self-defense movement led by Jose Manuel Mireles Valverde was similarly able to chase the Knights Templar Cartel from large parts of the state of *Michoacán*, in Mexico. In a documentary, the late Dr. José Manuel Mireles Valverde, one of the leaders of the Autodefensas movement in Michoacán, Mexico, explained that every single one of the members of his armed self-defence militia has lost a relative or close friend to the drug cartels. That experience, he explains, was the foundation of their commitment (Heineman, 2015). In a documentary on Afghanistan (Knappenberger, 2021), Hilaludin, the son of Malik Jalaludin, a tribal elder of North Waziristan in Pakistan, confesses *"I had lots of friends in the village. I have seen many of them getting amputated because of the [US drone] bombing. Their bodies would be covered in blood, they had no hands nor feet [...] I will not forget this suffering even if I live 100 years. We will take our revenge, God willing."* His father then explains: *"You see how their mind is full of hatred now. You create terrorists [...] They say that 'If death is our only fate, we would rather die fighting back.' So, they join the Taliban."* The unfolding violent conflicts around the world today involving militias make it urgent to make progress on this question.

Chapter 3

What Causes Turnover?

3.1 Introduction

Many developing countries are undergoing a rapid process of industrialization, with governments actively pushing to transition away from subsistence-agriculture in favor of manufacturing. A major part of this effort includes industrial park policies: governments provide amenities and subsidies to attract large-scale, often multinational, manufacturing firms to start production in the country. Despite the fact that these manufacturing firms generally offer comparatively good formal job opportunities, turnover rates are high. In a flagship industrial park in southern Ethiopia, we collect administrative turnover record of 35,288 workers between July 2018 and March 2020, and find that 25% workers quit before signing a permanent contract with the firm, and a total of 48% workers quit within one year. The high turnover rates potentially pose a major challenge for firms and policy makers to sustain the operation in the industrial park.

Scholars have speculated three major reasons underlying the high turnover rates (Blattman and Dercon, 2018). First, in a traditionally agrarian society, workers may not fully learn about the jobs in these large manufacturing firms and may develop certain misperceptions. Second, a common view among firms and policy makers is that most workers only work in these manufacturing jobs due to temporary income shock, and would eventually go back to their normal occupations. Third, new workers may learn on the job whether the manufacturing job is a good match for them, and thus turnover is an equilibrium outcome where low-productivity workers voluntarily quit, reflecting a sorting mechanism (Jovanovic, 1979). In this paper, we collect extensive measures of misperceptions of 1,203 new hires, and implement an information treatment to causally estimate the extent to which misperceptions may explain the turnover. Leveraging the exogenous variation induced to workers' quitting, we provide suggestive evidence on whether workers more subject to income shock are more likely to quit, and whether workers with low productivity proxies are more likely to quit.

We sample 1,203 new female hires between March and May 2023 before they start the job in the industrial park. We then elicit workers' perceptions on 14 job aspects in the following four categories: (i) *Amenities*, which capture workers' utility derived from the jobs regardless of positions or incentives. (ii) *Performance pay and bonus*, which capture the incentives if workers exert more effort conditional on the positions. (iii) *Entry-level career incentives*, including entry-level salary in the first month, percentage of new hires assigned to the entry-level positions, and percentage of new hires fired in the first month. (iv) *Career progression*, including the salary after being promoted to the upper-level positions, and percentage of entry-level workers promoted after one year. These 14 job aspects cover the majority of what workers care about during job search. We then observe whether the worker left the company without officially signing a permanent contract from the administrative record provided by the industrial park.

We first observe that on average, new hires have relatively correct perceptions over most job aspects. This is consistent with the qualitative evidence that the government and firms endeavor to deliver information on some important job aspects, mostly regarding amenities, performance pay and bonus, and entry-level incentives. One exception is that none but one of the firms would inform workers of the details of career progression. Second, we find relatively small explanatory power of baseline misperceptions on early turnover. Notably, if a worker is overoptimistic of entry-level salary in the first month, top performance salary premium, or percentage of firms providing attendance bonus, she is more likely to quit before signing a permanent contract. This is consistent with the type of information firms provide to workers on the first day of work, and that it is fairly easy for new hires to learn about these job aspects in the first month of work. Combining with the relatively correct baseline perceptions, we calculate the explanatory power from all the baseline misperceptions to be around 5% of the total variation in turnover. One concern, however, is that baseline misperceptions are correlated with unobserved workers' characteristics, such as latent productivity, and thus the correlational exercise presented above does not necessarily reflect the causal impact of misperceptions on turnover.

To address this concern, we leverage the existing informational gap about career progression, partially due to the lack of information provided from the firms. We first observe a wide variation in the baseline perceptions of salary after being promoted, and that workers with baseline overoptimistic perception of salary after promotion are significantly less likely to quit before signing a permanent contract, suggesting it is relatively difficult for workers to learn about career progression on their job within a short amount of time. We then implement an information treatment, in which we randomly select a subset of respondents and provide them with accurate information on after-promotion salary and the likelihood of being promoted to an upper-level position, both of which are calculated from a confidential survey conducted by the government and cross-validated by the major firms in the industrial park. We first find significant updates in respondents' beliefs about the salary after promotion and the likelihood of being promoted, with the posterior beliefs being concentrated around the true values for the treated group and remaining relatively unchanged for the control group. Our identification strategy follows Cullen and Perez-Truglia (2022), where we leverage the exogenous shock to workers' perceptions conditional on their baseline perception level.

We find that beliefs about salary after being promoted significantly affect turnover rates: optimistic updates about after-promotion salaries increase the likelihood of remaining employed within the industrial park, while pessimistic updates reduce it. Specifically, one standard deviation increase in the perceived after-promotion salary reduces the probability of the worker quitting before signing a contract by about 9.7 percentage points, a 23.7% change relative to the average rate of early turnover. Interestingly, we find little effect of beliefs about the probability of being promoted to an upper-level position. The results are not subject to different functional forms of measuring misperceptions, potential imbalance regarding baseline workers' characteristics, firm-specific characteristics, potential update on workers' own type, workers' ability of retaining information, potential update on other perceptions, or spillover to workers in the control group. We calculate the explanatory power of the belief update about salary after promotion to be around 0.3% of total variation in the early turnover.

How should we make sense of the large proportion of unexplained turnover? We explore two other potential mechanisms with the following heterogeneity exercise. Leveraging the exogenous information shock introduced to workers conditional on their baseline perception level, we examine whether workers more subject to income shock or workers of low productivity are more likely to quit after the treatment, from which we can infer whether income shock or sorting mechanism is a more realistic interpretation of turnover.

We generate two proxies for whether a worker is more subject to income shock: whether the total income from sources is below median, and whether the total expenditure for food and rent is above median, both potentially correlated with a higher likelihood of hitting liquidity constraint. We do not find any significant treatment effect regarding the two proxies, suggesting workers more subject to income shock do not react more to the exogenous information shock. We further examine whether treatment effect is stronger among workers with higher perceptions of outside options, who arguably are more tempted to quit the current job and explore other opportunities. If anything, we find the treatment effect to be weaker among this subgroup of workers. The results provide suggestive evidence against the hypothesis that workers only work in the industrial park because of temporary income shock. This is also aligned with our descriptive statistics that although only 48% workers self-report to apply for the jobs in the industrial park because of better salary prospects, 89%

workers self-report to apply because they want to learn more skills in the garment sector. On average, a worker self-reports to plan to stay in the industrial park for 3.8 years, against the common view held by the firms and the policy makers that entry-level workers are more short-run focused.

Second, we examine heterogeneous treatment effects regarding four proxies of productivity: whether the worker has vocational training or college education, whether the worker has previous work experience in garment factories, cognitive skill measured from a series of cognitive questions, and dexterity skill measured from two common exercises in garment factories. We find that the exogenous information shock may induce low productivity type workers to quit earlier, but it does not induce more early turnover among workers with higher educational attainment as well as workers with higher dexterity measure. The results are consistent with the hypothesis that turnover may reflect an equilibrium outcome of a sorting mechanism: workers with low productivity may realize their productivity type on the job and choose to quit if their comparative advantages lie in other sectors. Under this hypothesis, high turnover rates may not necessarily be a "bad" equilibrium outcome, provided that firms can afford to lose many low-productivity workers on a frequent basis, and that firms may benefit more from retaining high-productivity workers.

Our paper contributes to two major branches of the literature. The main contribution is to the literature that studies high turnover rates in manufacturing industries. The early literature focused on rich countries (Montgomery, 1989; Beckert, 2015; Farber, 1994, 1999), while more recent work has found high turnover rates in developing countries (Groh et al., 2016; Blattman and Dercon, 2018). These papers provide speculative evidence of potential causes of high worker turnover rates. We contribute to this literature by providing empirical evidence combining detailed survey data on misperceptions and administrative data on turnover from a large industrial compound, and leverage an exogenous information shock to provide causal evidence on the underlying mechanisms of high turnover rates.

Our paper also contributes to the literature on labor market frictions in workers' job search. Recent research documents that search frictions (Franklin, 2018; Abebe et al., 2021), matching frictions (Banerjee and Chiplunkar, 2023), and over-optimism (Spinnewijn, 2015; Banerjee and Sequeira, 2023) can significantly prevent job seekers from finding stable matches with employers. Our paper also speaks to the literature on behavioral job search (DellaVigna and Paserman, 2005; DellaVigna et al., 2017) where behavioral factors may hinder optimal job search outcomes. Although our findings suggest that misinformation may not explain the majority of the early turnover in the manufacturing firms, our causal evidence suggests that workers with high-level of baseline misperceptions may not conduct their quitting decisions efficiently, which has important welfare implications on job seekers, especially those who have disadvantaged information access.

3.2 Context: Hawassa Industrial Park

Ethiopia is a low-income country in East Africa, with a GDP per capita of 944 US dollars in 2021. Starting from early 2000s, the government endeavored to attract foreign investment opportunities, predominantly in light manufacturing sector, in hope to transition the economy from agriculture-based to industrialization. In particular, many East Asian companies, state-owned or private, have invested in many infrastructure projects and light manufacturing sector, primarily because of relatively low labor cost in Ethiopia and the large subsidies promised by the government.

One of the major government-led project is Hawassa Industrial Park, a major project of industrialization in Ethiopia and one of the largest industrial parks in sub-Saharan Africa. It is located in Sidama region, traditionally an agrarian society featuring cash crops such as coffee beans. A total of 20 active firms are currently operating in the industrial park, all but one in the garment sector.[1] Since its start of operation in 2016, the industrial park has been employing 20,000 to 30,000 workers every year, actively making itself the major recruiter of the garment sector in southern Ethiopia (Hardy et al., 2022). There is no other competitor of similar size in the region; all other industrial parks are located at least 225km north to Sidama region.

The 20 firms in the industrial park function close to a cartel. They reached an agreement in 2018 to set the base salary for entry-level jobs at 1,000 ETB, or about 20 US dollars per month, to prevent firms from poaching entry-level workers.[2] About 90% new hires are assigned to entry-level positions, such as sewing, cutting, and helpers. New hires first go through a probational phase. Roughly 45 days after, workers will sign a formal contract with firms and can enjoy more incentives such as performance pay and attendance bonus, but also have to submit a 30-day notice if they decide to leave. All 20 firms have a similar career ladder. Some entry-level operators with top performance will be promoted to quality

[1]Before November 2021, there used to be 22 active firms. Since then, the civil war and the termination of African Growth and Opportunity Act (AGOA) agreement in Ethiopia heavily affected the exporting industries, especially firms who predominantly exported to the United States and Europe. One major US company exited the park in December 2021; another one exited a few months later in 2022. Nevertheless, the majority of the companies are from East and South Asia whose major exporting markets are not in US or Europe, therefore less affected. Most of the remaining companies are operating at the normal capacity currently.

[2]The starting salary is around the lower 25 percentile of the salary distribution in Southern Nations, Nationalities, and Peoples Region, calculated from Living Standards Measurement Study in Ethiopia during 2015–16. One possibility is that workers accept a lower salary to get a chance of working in the industrial park and gain higher salary later, similar to the model in Terviö (2009). Indeed, workers are promised a salary raise usually six months after the job, from 1,000 ETB to 1,600 ETB, about the lower 40 percentile of the salary distribution.

control team where better skills are required. Other entry-level operators with exceptional performance will be promoted to line supervisors if the vacancy is open and the former line supervisors leave a good recommendation. On average, 15% of the entry-level operators will be promoted to either quality control team or line supervisors within one year. Workers from both upper-level positions enjoy a similar monthly salary 2,413 ETB, or about 48 US dollars.

Another feature of the cartel is the centralized hiring system. To support the hiring process of entry-level workers, the Ethiopian government established a grading center next to the industrial park and hired own staff to centralize the hiring procedure of entry-level workers. Job seekers can either directly walk in and register for a job, or they can sign up in one of the 10 local recruiting centers within 60km around industrial park and be sent to the grading center for registration.[3] After entering the labor pool, job seekers would be randomly assigned to one of the firms requesting for workers.[4] Importantly, because of the non-poaching agreement, an entry-level worker cannot choose their employer because of better job aspects; they have to re-enter the labor pool and wait for a new draw. Therefore, if a new worker quits their job, it is very likely they would simply leave the industrial park and employ in a different sector, given the lack of competition in the garment sector locally.

Despite that, many new workers in the industrial park do not stay on the job for long. We obtain administrative records of turnover from the grading center between July 2018 and March 2020, right before the Covid shutdown, to have a first glimpse at the workers' turnover. Figure 3.1, Panel (a) shows the percentage of new hires who quit within a certain period. 11.3% workers quit within the first day, of which most are fired by the company (7.7%) possibly because of lack of basic qualifications.[5] Before the workers sign a permanent contract by 45 days, 25.4% workers quit, of which 11.0% voluntarily quit, which potentially suggests that a large share of new workers realize this job is not a good match. Within one year, another 22.6% workers quit (totalling 48.0%), of which 12.1% are voluntary quit

[3]Firms used to conduct their own hiring before the establishment of the grading center. Around the same time when firms reached a non-poaching agreement, to ensure each firm has an equal access to the labor pool, firms agreed to use the centralized grading center as the sole hiring platform and not to conduct their own hiring or make any public announcement of their hiring requests. On average, each firm submits one hiring request every two weeks during the survey period.

[4]In principle, firms can reject the assigned workers and ask for new ones. In reality, firms accept 95% of the first-assigned workers. The ones rejected by firms are usually the following two scenarios: (i) The workers used to work in the firm and are already fired by the firm once before. (ii) The firm requests for workers for specific tasks for which they have more stringent criteria.

[5]This may be a "disguised" voluntary quit because when a worker is absent for more than three days in a month, firms would start the firing process. In the main analysis, we use total turnover as the main outcome. We plan to collect further personnel data from employers to measure voluntary quitting and firing more precisely.

(totalling 23.1%).[6] The high turnover rate after signing the permanent contract thus reflects at least two layers of matching frictions: that firms are not able to screen out unproductive workers sooner, and that workers may not fully realize whether the industrial park is a good job before they sign the permanent contract, potentially due to lack of information, or the nature of novices in a new sector.[7]

The Ethiopian government and firms in the industrial park have tried at least two methods to address these two matching frictions. On the screening, new job seekers have to first go through a basic screening process before starting a new job. Only applicants at least 18 years old who graduate from eighth grade are qualified for a job in the industrial park. After that, the grading center used to conduct a series of grading tests to measure workers' cognitive skills and dexterity skills, some of which we borrow in our data collection. This, however, discontinued in 2022, shortly after many firms complained that the grading results were not as helpful and that firms had to conduct their own grading tests anyway. As a result, the government shifted their focus to addressing the second matching friction by providing accurate information about some of the job aspects. Job seekers will be given a handout about the entry-level salary in the industrial park, the expected work schedule and required tasks, and basic amenities. Once workers are hired, firms also provide an orientation on the first day to walk through the basics of the job. If a worker has any misperception of some of these job aspects, they can quickly correct their misperceptions through government's information handout, firm's orientation, or by their own learning experience on the job if the information is not difficult to obtain, and thus the effect of misperceptions will be mostly confined to early turnover, on which we will focus throughout the main analysis.

3.3 Measuring Misperceptions and Turnover

We combine two sources of data for the analysis. The first source of data is our own survey, which we conducted among 1,203 newly hired female workers in Hawassa Industrial Park between March and May of 2022.[8] Our second source of data comes from administrative

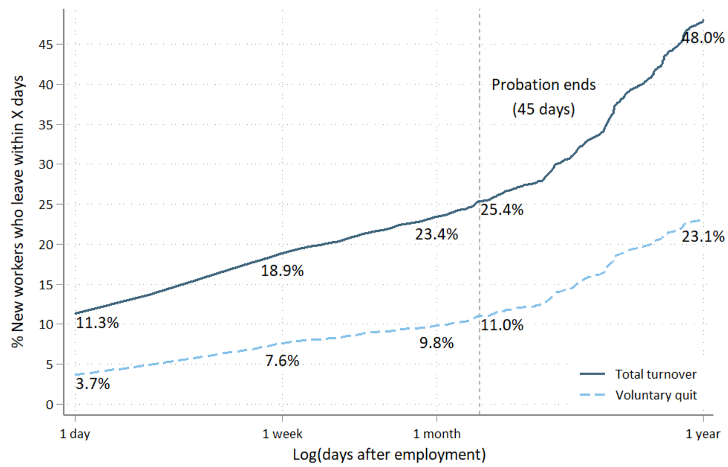
[6]High turnover rates are commonly documented in the early stage of industrialization. Montgomery (1989) documented as high as almost 100% turnover within a year in the US factories in the early 20th century. Blattman and Dercon (2018) documented a 31% turnover in the first month among workers from five major manufacturing companies in Ethiopia, a very similar statistics as in our context.

[7]Figure 3.1, Panel (b) shows that right before the 45-day probation period ends, there is a small bunching of turnover, yet there is still a sizeable share of workers who quit after 45 days in the industrial park. Although one potential reason is that firms and workers are not strictly obligated to sign the permanent contract exactly at Day 45, this cannot explain why the one-year turnover rate after 45 days is still so high.

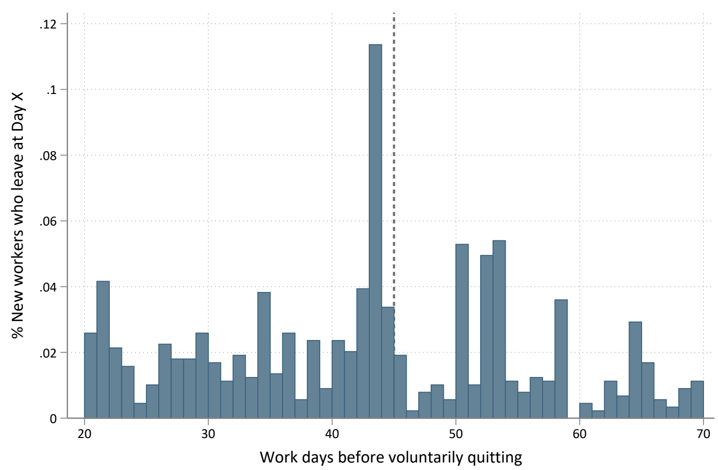
[8]96% of all workers registered in the industrial park are female.

Figure 3.1: Turnover in the Industrial Park

(a) Cumulative distribution of turnover



(b) Bunching around 45-day probation cutoff



Notes: This figure shows the distribution of the turnover in the industrial park, using the entire administrative turnover record from the grading center between July 2018 and March 2020. Panel (a) shows the key statistics of turnover within one day, one week, one month, 45 days when the probation period ends, and one year. Panel (b) shows the percentage of workers who voluntarily quits per day around the 45-day probation period.

records from the government authorities of the industrial park, which track the workers' entry and exit within the industrial park.

Worker survey. Workers are sampled after they registered in the industrial park and were waiting for job assignment. We collected a series of demographic characteristics (age, marital status, origin, languages, religion), educational attainment, prior work experience, social network information, career plans, and reasons of joining the industrial park. We also conducted a series of cognitive tests and dexterity tests to generate an objective measure of skills. Appendix C.1 describes the measurement of each variable in detail. Table 3.2, Column 1 shows that on average, new hires are young (21.5 years old), few married (12%), with little past experience (17%) especially in garment sector (11%). Yet, many of them have a plan to stay in the industrial park for long (the average is 3.8 years); many apply for the jobs in the industrial park to learn skills (89%), or because the job is interesting (80%); only 48% apply for the jobs because the future salary prospect is good. In general, these descriptive statistics seem to suggest that a sizeable portion of new workers come to the industrial park to seek for more than temporary employment.

We then collect a comprehensive set of respondents' impressions on 14 job aspects of the industrial jobs. We first conceptualize four groups of job aspects in Appendix C.1. (1) Amenities, which capture workers' utility terms regardless of positions. (2) Performance pay and bonus, which capture the incentives if workers exert more effort conditional on the positions. (3) Entry-level career incentives, which do not depend on workers' effort level. (4) Career progression, which do not depend on workers' effort level provided workers are already in the upper-level position (supervisor). Table C.1 verifies that these job aspects cover most of what job seekers care about during job search. In particular, a sizeable share of workers emphasize the importance of career progression: 34.7% listed "upper-level" salary as one of the top 3 job aspects, and 35.9% listed the likelihood of promotion as one of them. These statistics again seem to reject what some firms and policy makers assume that low-skill workers who apply for the jobs in the industrial park tend to seek of short-term employment, instead of long-run career development.

Based on this classification, we conduct a quantitative interview with 10 major employers in the industrial park, and quantify 14 job aspects in these four categories. (1) Amenities, including the number of days per week required to work, hours per day, average overtime hours per week, average minutes per day allowed during work day, percentage of firms providing free transportation, and percentage of firms providing free lunch. (2) Performance pay and bonus, including the salary premium between a top-10% entry-level workers compared to average entry-level workers, tenure premium if an entry-level worker stays for at least one year, and the percentage of firms providing attendance bonus. (3) Entry-level career incentives, including entry-level salary in the first month, percentage of new hires assigned to entry-level positions, and percentage of new hires fired in the first month. (4) Career progression, including salary level when an entry-level worker gets promoted to the next level (supervisor), and the percentage of entry-level workers promoted after one year. For each job aspect, we collect the benchmark from the qualitative interview with the 10 major firms. We further cross-validate some of the information with a confidential worker survey conducted by the Ethiopian Investment Commission from October 2021 to February 2022. Both sources provide very similar benchmarks.

We elicit workers' baseline perceptions of the 14 job aspects with the following incentivized scheme. We first inform workers to provide their best guesses for each of the 14 job aspects, and that they will get a reward based on the correctness of their answers. After they answer all questions, the system automatically calculates the distance of their answers of four questions to the benchmark with a quadratic loss function, and the survey team pays between 5–25 Ethiopian birr as a reward.[9] As such, workers are incentivized to provide their most accurate guesses for all job aspects. We also design the survey in a way such that respondents are unlikely to discuss their answers with others during the survey, but we further tackle the potential spillover with our clustered treatment design in Section 3.5.5.

Administrative turnover records. We merge our survey with administrative records from the grading center of the industrial park using anonymized identification numbers. The administrative record first shows the date of workers entering the general labor pool. Then, more importantly, if a worker signs a permanent contract with a firm, the firm is required to enter the information through the grading center system. Thus, if we observe a worker enters the labor pool but is never assigned to any firm, we infer that this worker quits the job without signing a contract.[10] In addition, if the worker shows up in a firm record but quits within 45 days of entering the general labor pool, we also consider this worker to have left the job without signing a permanent contract. The remaining quitting events are considered as quitting after signing a permanent contract. Currently, the administrative records of sampled workers in 2022 do not include the turnover reasons (voluntarily quitting or being fired) or longer-spanned observations, which will be collected soon in the future.

[9]The four questions are: Entry-level salary in the first month, percentage of new hires assigned to the entry-level positions, upper-level salary after being promoted, percentage of entry-level workers promoted to upper-level positions after one year. We pay minimum 5 Ethiopian birr to workers even if their answers are far away from the benchmark because some workers may raise concerns of fairness if they are paid nothing. Workers and the survey team do not know how the system calculates the reward, so it is unlikely that the survey team may prime the workers to get higher reward. Respondents were awarded 16 ETB on average.

[10]Many workers are hired on the same day and most workers will be assigned a job within 3 days. It is likely that applicants may leave on the first day without being assigned any job. We will collect follow-up worker survey in the future to cross check the turnover data with workers' retrospective employment records.

3.4 Correlational Evidence of the Effect of Misperceptions

Table 3.1 presents the descriptive statistics of workers' perceptions on the 14 job aspects. For each job aspect, we compute the distance of average perception to the benchmark, divided by the standard deviation of the perception. In general, we do not see a large gap between average perceptions and the benchmarks. On most job aspects, the average perceptions do not exceed more than one standard deviation from the benchmarks, with two exceptions: percentage of firms providing attendance bonus, and the percentage of new hires assigned to entry-level positions. One possible explanation is that neither of the two job aspects is the priority of workers' job search decisions, and that workers may not actively search for information on these two aspects before they start the job. However, most firms would provide details on attendance bonus on the first day and allocate the tasks within the first few days, and thus it is not difficult for workers to correct their misperceptions on these two job aspects shortly after they start the job.

To what extent do misperceptions explain the early turnover? We first attempt to provide some correlational evidence by simply regressing early turnover decision on the distance between workers' perceptions and the benchmarks of all 14 job aspects. In the last column of Table 3.1, we report the explanatory power of each measured misperception on the total variation of early turnover, granted each misperception measure is the sole explanatory variable. Notably, none of the misperception measure explains more than 1% of the total variation in the early turnover. The three misperceptions with the highest explanatory power are: hours per day required to work, percentage of firms providing free transportation, and salary after promotion. Together, accounting for correlations among all the 14 measured misperceptions, all measures of the misperceptions can explain 5% of the total variation in the early turnover.

Figure 3.2 further presents the coefficients of each measured misperception on the early turnover. Most coefficients, if anything, are positive, potentially suggesting that workers quickly correct most of their misperceptions, either on the job or through the information provision from the grading center of from the firms' orientation, and leave before signing a permanent contract for a better job opportunity outside. One notable exception, however, is salary after promotion: workers with higher misperceptions of salary after promotion are less likely to quit before signing a permanent contract. This is consistent with our observation during the qualitative interviews that firms usually do not provide details of the promotion scheme.[11] In addition, Figure C.1 suggests that new workers who have friends and family working in the industrial park previously do not have more precise misperceptions along

[11]Out of 10 major firms we conducted qualitative interviews with, precisely one firm would inform new

Table 3.1: Baseline Misperceptions

	Benchmark	Mean	SD	$\frac{\text{Mean - Bench}}{\text{SD}}$	Expl. Power (%)
A. Amenities					
Days per week required to work	6	5.68	0.50	-0.64	0.14
Hours per day required to work	8	8.73	1.26	0.58	1.02
Overtime hours per week	7	5.02	3.46	-0.57	0.16
Minutes of break per day allowed	30	30.51	13.74	0.04	0.13
# of 10 major firms providing free transport	4	5.70	2.14	0.79	1.02
# of 10 major firms providing free lunch	6	6.24	2.14	0.11	0.60
B. Performance pay and bonus					
Top performance salary premium (USD)	8	8.18	6.61	0.03	0.20
Tenure bonus, entry-level, one year (USD)	6	6.79	4.35	0.18	0.01
# of 10 major firms providing attendance bonus	10	6.47	2.74	-1.28	0.64
C. Entry-level career incentive					
Entry-level salary first month (USD)	20	21.24	5.27	0.23	0.40
% new hires assigned to entry-level	90	67.58	20.19	-1.11	0.00
% new hires fired first month	10	11.05	8.97	0.12	0.65
D. Career progression					
Salary after promotion (USD)	48	51.09	11.79	0.24	0.89
% entry-level workers promoted in one year	15	16.51	8.51	0.18	0.33

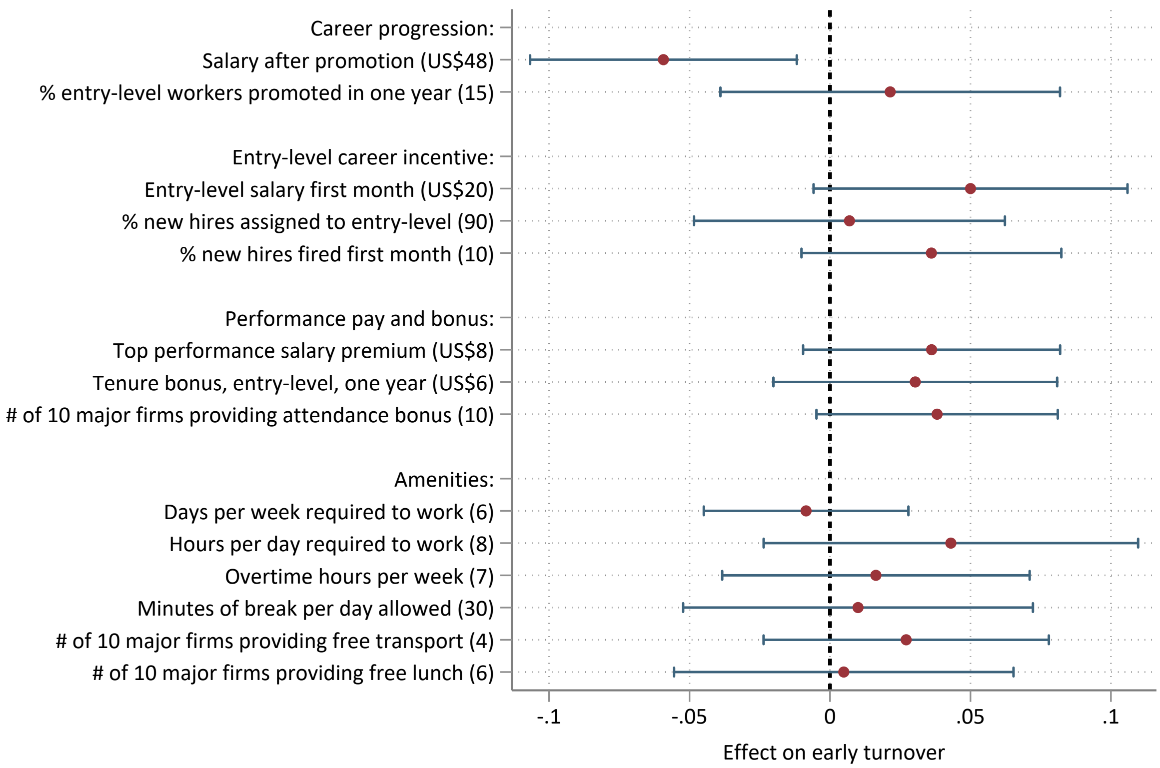
Notes: This table presents the baseline perceptions of all 1,203 workers on the 14 job aspects. We compute the difference between the average and the benchmark divided by the standard deviation in the second last column. In the last column, we regress whether worker quits before signing a permanent contract on her relative perception of each job aspect compared to benchmark, using only workers in the control group and clustered at the survey day level, and report the value of R^2 (x100) of each regression.

most job aspects, implying the limited extent of self-learning through informal network. In the next section, we take advantage of this existing information gap of career progression and conduct an information treatment to introduce an exogenous shock to workers' perceptions regardless of their baseline misperceptions, which allows us to causally estimate the effect of

hires of the promotion likelihood and salary of upper-level positions; interestingly, this firm happens to be the only Ethiopian-owned firm in the industrial park. One may wonder why firms do not provide information about upper-level salary to entry-level workers on the first day of job. The negative correlation between misperception of salary after promotion and early turnover may suggest that firms intentionally withhold information to "lure" workers to stay for longer. It is, however, difficult to explain why they only withhold information of upper-level salary, not other types of information. From our qualitative discussion with firms, many firms tend to think workers do not care about long-run career progression because many of them quit within the first month.

misperceptions regarding career progression.

Figure 3.2: Predictions of Early Turnover Using Baseline Misperceptions



Notes: This figure shows the prediction of workers quitting before signing a permanent contract, using the baseline perceptions on the 14 job aspects. We compute the differences of baseline perceptions and the benchmark information collected from firms' qualitative surveys and the confidential surveys conducted by the government (shown in the brackets on the vertical axis), divided by the standard deviation of the baseline perceptions. We only include workers in control cohorts in the regression and cluster at the cohort (day of hire) level. The coefficient and 95% confidence interval are shown.

3.5 Causal Evidence of the Effect of Misperceptions

3.5.1 Information Treatment

We conduct a clustered information treatment designed as below. We sample 1,203 new workers across 42 days in total. Among the 42 survey days, we randomly select 26 survey days (63%) for the information treatment. For each of the treated days, 82% of the sampled workers receive the benchmark information on career progression collected by us from the confidential survey of Ethiopian Investment Commission. In total, 53% of the sampled workers received benchmark information of career ladder at the end of the baseline survey.

Specifically, we first collect salary and position information from a representative worker survey during October 2021 – February 2022, conducted by the Ethiopian Investment Commission. We generate two benchmark statistics: (i) The likelihood of being promoted from entry-level to an upper-level position within 1 year is 15%; (ii) the average salary of upper-level positions is 2,413 ETB (roughly 48 US dollars). We cross-check these two statistics with the qualitative interviews with 10 major recruiters and confirm the accuracy.[12] Then, at the end of the baseline, we inform treated workers of the two benchmark statistics. We also design a visual presentation of the two statistics to help workers understand the meanings, as shown in the infographic card in Appendix figure C.2. Immediately after providing the information, we elicit workers' perceptions one more time to observe whether they update the perceptions. Control workers will be asked again the perception questions but without any new information provided.[13]

Table 3.2 further compare treated workers and control workers regarding all baseline characteristics; the difference is calculated by clustering at the same survey day. Treated workers are not significantly different from control workers in most of the characteristics; they are more likely to come from Hawassa, more likely to be a high school graduate, less likely to have friends who apply for the for job together, and less likely to apply for the job because

[12]Ideally, one would calculate the promotion likelihood and salary of upper-level positions from the personnel records from each firm. This method is not feasible at the time we conducted the survey because international firms were protective of their human resources records. Ethiopian Investment Commission was the only institute at the time allowed to conduct surveys with current workers and obtain information about salary. Reassuringly, the benchmark information we generated from the survey tracks the qualitative records well: on average, firms report to pay 2,276 ETB for the upper-level positions including line supervisors and quality checkers, and about 14.6% entry-level workers would be promoted after one year.

[13]We use the same incentivized method to elicit post-treatment perceptions as described in Section 3.3. One concern is the demand effect: workers may simply answer the information we provide during the baseline survey without effectively changing their true perceptions. This, however, does not explain why we observe a significant treatment effect on workers' early turnover. Given the limited learning through informal network, we plan to collect a follow-up survey to test whether such misperceptions persist over time.

the job is interesting. We do not observe any systematic pattern of potential selection into treatment. In addition, our main results remain unchanged after controlling for all observable characteristics. Table C.2 further shows the balance between control and treated groups regarding baseline perceptions. Although treated workers have a slightly higher perception on salary after promotion and a slightly lower perception on the promotion likelihood, the difference is much smaller than the standard deviation, and all our main results presented are robust to controlling for baseline perceptions.

Figure 3.3, Panel (a) shows the distribution of baseline perceptions about the salary after promotion, and Panel (b) shows the distribution for the perceived likelihood of being promoted to an upper-level position. The dashed vertical line indicates the benchmark values. In both panels, although workers on average have roughly correct perceptions of career ladder, there is substantial variation with some workers being overly-optimistic and some overly-pessimistic. We will leverage the dispersion of baseline misperceptions in our main identification strategy presented in the next subsections.

3.5.2 Efficacy of the Information Treatment

We first estimate the first-stage effect of information treatment on updated misperceptions on career incentives using a Bayesian update specification. Let $P_i^{x,0}$ be worker i's prior belief of job aspect x, $P_i^{x,1}$ the posterior belief immediately after the information provision, $P_i^{x,2}$ the posterior belief in the follow-up survey, $P_i^{x,s}$ the signal provided by the survey team. Bayesian learning implies that, after the signal is provided (information treatment), the mean of the posterior belief should be a weighted average between the signal and the mean of the prior belief; the weight α, ranging from 0 and 1, is determined by the variance of the prior and the variance of the signal. This prediction can be summarized as follows:

$$\log(P_i^{x,1}) - \log(P_i^{x,0}) = \alpha_1 \big(\log(P_i^{x,s}) - \log(P_i^{x,0}) \big)$$

To empirically test the first-stage effect of information treatment on belief update, we use the following specification:

$$\begin{aligned} \log(P_i^{x,1}) - \log(P_i^{x,0}) = \tau + \alpha_1 T_{c(i)} \cdot \big(\log(P_i^{x,s}) - \log(P_i^{x,0}) \big) \\ + \beta_1 \big(\log(P_i^{x,s}) - \log(P_i^{x,0}) \big) + \epsilon_i, \text{ where} \end{aligned} \tag{3.1}$$

$T_{c(i)}$ is whether the entire cohort is treated. α_1 is the parameter of interest—the weight by which treated workers immediately update their perceptions when presented the benchmark information compared to control workers. We plan to collect long-run perceptions to test the persistence of information treatment. β_1 captures the spurious reversion towards the signal among control worker, which is not the focus of the analysis.

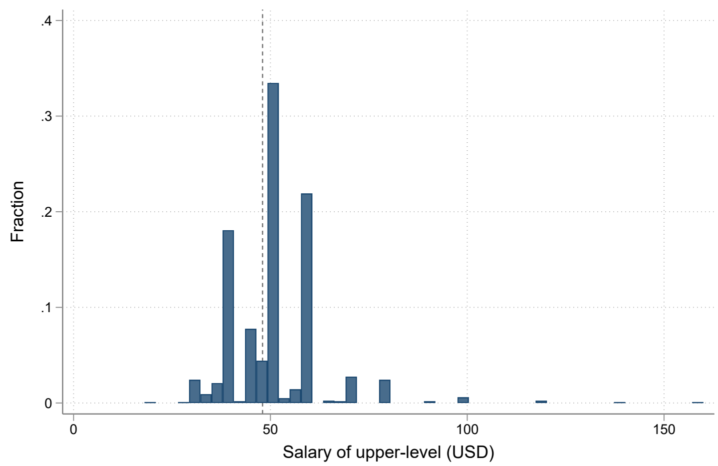
Table 3.2: Balance Table

	Mean outcomes					Diff
	All	Control		Treated		T-C
Observations	1203	566		637		
A. Demographics						
Age	21.53	21.62	(2.07)	21.44	(2.11)	-0.17
Married	0.12	0.11	(0.32)	0.12	(0.33)	0.01
From Hawassa	0.38	0.34	(0.47)	0.41	(0.49)	0.07**
Speaks Sidamagna at home	0.76	0.74	(0.44)	0.78	(0.42)	0.03
Speaks Amharic at home	0.24	0.25	(0.44)	0.23	(0.42)	-0.03
Protestant	0.91	0.90	(0.30)	0.91	(0.29)	0.01
B. Education and experience						
TVET or college educated	0.31	0.31	(0.46)	0.31	(0.46)	-0.00
High school graduate	0.31	0.28	(0.45)	0.34	(0.47)	0.06*
Has work experience	0.17	0.16	(0.37)	0.19	(0.39)	0.02
Has work experience in garment	0.11	0.09	(0.29)	0.13	(0.33)	0.04
C. Skill measures						
Memory score	5.32	5.32	(1.05)	5.32	(1.02)	-0.00
Raven score	3.90	3.91	(2.12)	3.90	(2.09)	-0.01
Game: When Abiy got Nobel Prize	0.46	0.48	(0.50)	0.44	(0.50)	-0.03
Game: How many regions in Ethiopia	0.39	0.37	(0.48)	0.40	(0.49)	0.02
Cognitive score (normalized)	0.00	0.01	(1.00)	-0.01	(1.00)	-0.01
Game: Finger coordination	34.80	34.83	(9.15)	34.76	(8.54)	-0.07
Game: Threading needles	11.78	11.56	(4.79)	11.97	(4.53)	0.41
Dexterity score (normalized)	0.00	-0.03	(1.02)	0.02	(0.98)	0.05
D. Social network						
Number of friends who worked in HIP before	2.30	2.35	(5.33)	2.24	(4.95)	-0.11
Number of friends who apply together	2.98	3.30	(4.98)	2.70	(4.08)	-0.60*
Number of the treated workers she knows	0.06	0.07	(0.35)	0.05	(0.28)	-0.01
Network score (normalized)	-0.00	0.05	(1.07)	-0.05	(0.94)	-0.10
E. Career plan and motivations						
Plans to start their own business	0.54	0.54	(0.50)	0.53	(0.50)	-0.01
Number of years planned to stay in HIP	3.75	3.77	(1.92)	3.73	(1.80)	-0.04
Cares about long-run salary	0.20	0.18	(0.38)	0.22	(0.41)	0.03
Applies for HIP b/c she wants to learn skills	0.89	0.90	(0.29)	0.88	(0.32)	-0.02
Applies for HIP b/c the future salary is good	0.48	0.47	(0.50)	0.49	(0.50)	0.02
Applies for HIP b/c the job is interesting	0.80	0.83	(0.38)	0.77	(0.42)	-0.06***
Intrinsic motivation score (normalized)	-0.00	0.03	(0.98)	-0.02	(1.01)	-0.05

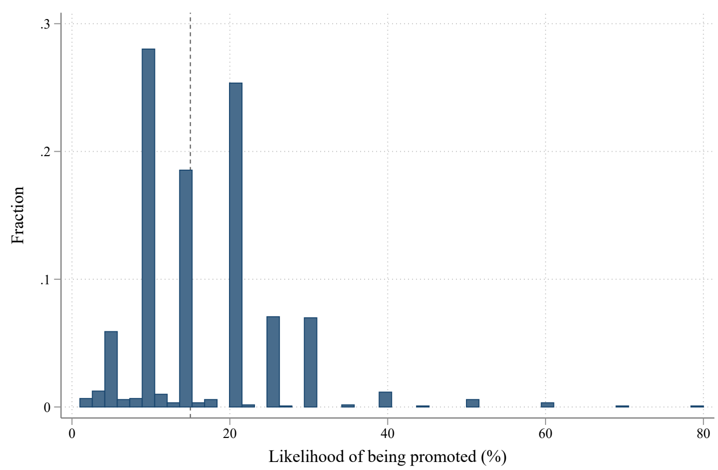
Notes: This table shows balance between the baseline characteristics of treated and control workers. Standard deviations in brackets. We compute the difference in the last column; standard errors are clustered at the cohort (day of hire) level. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Figure 3.3: Baseline Perceptions of Career Incentives

(a) Belief about salary after promotion



(b) Belief about likelihood of being promoted



Notes: This figure shows histograms of the prior beliefs reported in the survey. Panel (a) shows the histogram of prior beliefs about the after-promotion salary (measured in US dollars) and panel (b) shows the histogram for prior beliefs about the probability of being promoted to an upper-level position (measured as a percentage). The dashed vertical line indicates the true value in both plots.

Figure 3.4 shows the binned scatterplot plot of workers' updated perceptions immediately after the information treatment, with the Bayesian weight α_1 shown in the graph. Across both measures of long-run career incentives, the information treatment is impactful: posterior beliefs for the treatment group are closely concentrated around the true value we inform respondents of, while posteriors for the control group closely track baseline beliefs.

3.5.3 Effect of Misperceptions on Turnover

To causally identify the effect of misperceptions of career incentive on turnover, simply regressing early turnover on baseline perceptions would potentially suffer from classic omitted variable bias: workers with over-optimistic perceptions at baseline may have specific characteristics that affect turnover, as we discuss in Section 3.4.

What about simply estimating the average treatment effect? Figure 3.5 implies the potential issues of such a simple method by showing the reduced form effect of misperceptions on turnover, separately for treated cohorts and control cohorts. In Panel (a), among control cohorts, higher baseline perception of average salary after promotion is correlated with lower quitting rate before signing a contract, but such pattern is reversed among treatment cohorts. In Panel (b), the correlation between baseline perception of promotion likelihood and early quitting is much higher among treatment cohorts than control cohorts.

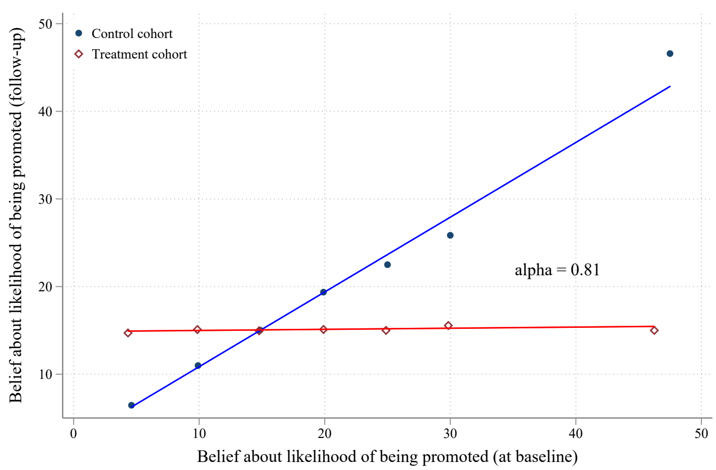
The comparison shows three important facts. First, without information treatment, workers who are over-optimistic of salary after promotion are associated with lower quitting rate before signing a contract, consistent with the correlational evidence in Figure 3.2. With information treatment at the end of baseline, workers present drastically different turnover behavior. Second, the treatment effect on turnover is positive if workers are over-optimistic at baseline, negative if workers are over-pessimistic at baseline, and insignificant if workers have roughly correct perceptions at baseline. Third, the magnitude of the treatment effect is larger when the baseline perceptions are further away from the benchmark. Therefore, a simple estimate of the average treatment effect is a weighted average of heterogeneous treatment effects regarding different baseline perceptions, potentially rendered less informative because the average baseline perceptions of career progression are not too far away from the benchmark. However, given the large dispersion of baseline misperceptions, and the treatment is random given each level of baseline perceptions, one can causally infer the effect of misperceptions on turnover at each level of baseline perceptions.

In the main analysis, we use the interaction of cluster treatment and baseline perceptions as the main instrumental variable for causal inference and to capture a larger first-stage correlation. Specifically, we follow Cullen and Perez-Truglia (2022) and adopt the following

Figure 3.4: Perception Update of Career Incentives

(a) Belief about salary after promotion

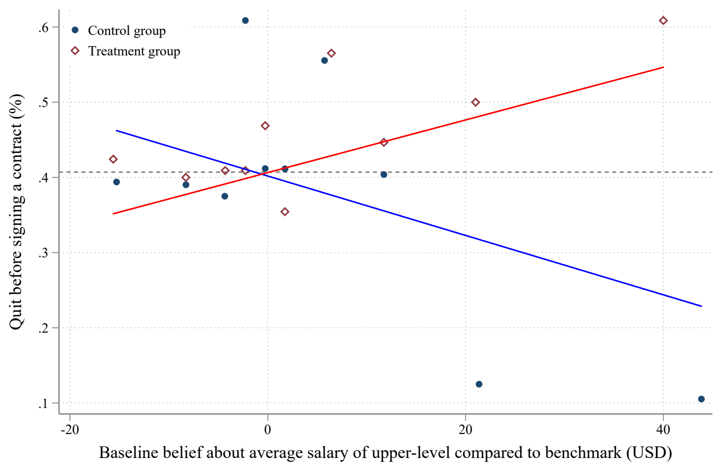
(b) Belief about likelihood of being promoted



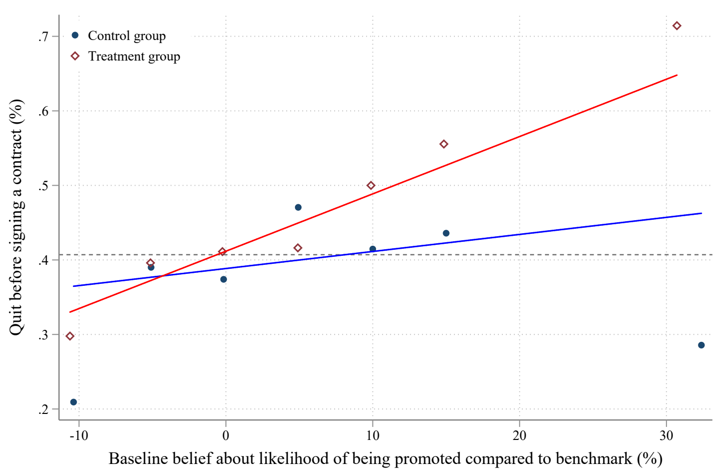
Notes: This figure shows binned scatterplots of the posterior beliefs relative to the prior beliefs reported in the survey. The control group is shown in blue and the treatment group is shown in red. Panel (a) reports beliefs about the after-promotion salary and panel (b) reports beliefs about the probability of being promoted to an upper-level position. The Bayesian update coefficient α from equation 3.1 is overlayed in both plots.

Figure 3.5: Reduced Form: Effect of Misperceptions on Early Turnover

(a) Belief about salary after promotion



(b) Belief about likelihood of being promoted



Notes: This figure shows reduced-form binned scatterplots of the probability of quitting before signing a permanent contract (before the 45-day trial period ends) in relation to baseline beliefs. Panel (a) shows the probability of early exit relative to baseline beliefs about the after-promotion salary and panel (b) shows the probability of early exit relative to baseline beliefs about the probability of being promoted to an upper-level position within a year.

instrumental variable approach:

$$Y_i^t = \pi + \delta \log(P_i^{x,t}) + \eta \log(P_i^{x,0}) + A_i\phi + u_i \tag{3.2}$$

$$\log(P_i^{x,t}) = \kappa + \gamma_t T_{c(i)} \cdot \left(\log(P_i^{x,0}) - \log(P_i^{x,s})\right) + \zeta \log(P_i^{x,0}) + A_i\psi + v_i \tag{3.3}$$

Equation 3.3 corresponds to the first stage of the IV regression, a variation of the Bayesian update model-derived equation 3.1. Equation 3.2 is the second stage of the IV regression. In particular, the main parameter of interest is δ, interpreted as the magnitude change in outcome Y_i^t caused by a 100 percentage change in perception $P_i^{x,t}$, *i.e.* worker i's updated perception on job aspect x. We use $P_i^{x,1}$ as the main independent variable, that is, the immediate updated perception of x (promotion likelihood or salary after promotion) at the end of baseline in Round 2. The reason we use $\log(P_i^{x,t})$ as the main independent variable instead of the bias measure $\log(P_i^{x,t}) - \log(P_i^{x,0})$ is to keep a flexible functional form in the estimation. As a robustness check, we will also use a linear term of $P_i^{x,t}$ as the main independent variable.

Table 3.3 presents the main results from this specification. The dependent variable is an indicator equal to 1 if the worker leaves the firm before signing a permanent contract after the 45-day probation period. Column (2) shows the reduced-form estimate. Column (3) shows the IV estimate of the causal effect: 100 percentage increase in the posterior belief of average salary after promotion leads to 41.8 fewer percentage points in early turnover (p-value 0.044). Given the average prior of salary after promotion is 51.1 USD and the standard deviation 11.8 USD, 1 standard deviation increase in the perception of average salary after promotion causes 9.7 percentage points decrease in early turnover, or a 23.7% decrease compared to the average early turnover rate. Column (1) shows the OLS estimate, almost half as large as the IV estimate, suggesting a downward bias in the OLS estimate.

Table 3.3: Causal Effect of Misperceptions on Early Turnover

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Quit early	Quit early	Quit early	Quit early	Quit early	Quit early	Quit early	Quit early
Updated belief of upper-level salary	-0.219*		-0.418**					-0.408*
	(0.130)		(0.208)					(0.211)
	[0.099]		[0.044]					[0.053]
Treated cohort * Baseline bias of upper-level salary		0.262**					0.257**	
		(0.121)					(0.122)	
		[0.035]					[0.041]	
Updated belief of promotion likelihood				-0.034		-0.001		0.013
				(0.058)		(0.107)		(0.096)
				[0.557]		[0.995]		[0.893]
Treated cohort * Baseline bias of promotion likelihood					0.000		0.003	
					(0.071)		(0.070)	
					[0.995]		[0.970]	
Observations	1,165	1,166	1,165	1,167	1,167	1,167	1,166	1,165
R-squared	0.003	0.003	0.000	0.010	0.009	0.009	0.012	0.011
Specification	OLS	RF	IV	OLS	RF	IV	RF	IV
Cluster	Cohort	Cohort	Cohort	Cohort	Cohort	Cohort	Cohort	Cohort
Dep var mean	0.407	0.407	0.407	0.407	0.407	0.407	0.407	0.407
F-stat			33.33			179.6		17.60

Notes: This table reports estimates of equation 3.2. In all specifications the dependent variable is a dummy variable equal to 1 if the worker left the industrial park prior to signing a permanent contract, which occurs after completing the 45-day trial period. Updated belief of upper-level salary and promotion likelihood is the natural logarithm of the posterior belief of the after-promotion salary and the likelihood of being promoted. Baseline bias is the distance between the natural logarithm of the prior belief and the natural logarithm of the benchmark. Columns 1 and 4 report OLS estimates; Column 2, 5, and 7 report reduced-form estimates; Columns 3, 6, and 8 report instrumental variables estimates. Dep var mean reports the mean for the dependent variable. F-stat reports the first-stage F-statistics for IV estimation. Standard errors are clustered at the cohort (day of hire) level. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

We do not find such a large effect on turnover when it comes to the posterior belief of promotion likelihood. Column (4), (5), and (6) show the OLS, reduced-form, and IV estimates. The IV specification suggests a precise zero effect of the perceived promotion likelihood. Column (7) and (8) applies IV estimation on both misperceptions in the same regression; results do not differ significantly from the previous four columns. Thus, the primary driver of early turnover among long-run career incentives is the misperceptions of upper-level salary, less so about the misperception of the promotion likelihood, consistent with the correlational evidence from Table 3.1.[14]

Table C.3 replicates the main table but replacing the natural logs of perceptions with the linear terms of perceptions. Results are very similar to Table 3.3. Specifically, in Column (2), Table C.3, one dollar increase in the posterior perception of average salary of upper-level positions leads to 1.0 percentage points decrease in early turnover. Given the standard deviation of baseline perception is 11.8 USD, one standard deviation increase in the posterior belief of average salary of upper-level positions causes 11.8 percentage points decrease in early turnover, not far off from the estimate from Table 3.3 (9.7 percentage points). Although the effect of promotion likelihood is still insignificant, the standard error of the estimate is smaller (p-value 0.399). Results suggest that the functional form of beliefs do not significantly affect the magnitudes of the effects or the inferences.

Taking all the main results together, we leverage an information treatment to causally estimate the effect of misperceptions regarding career progression, of which we observe an informational gap in the current labor market. We find a standard deviation increase in the perception of salary after promotion leads to 9.7 percentage points decrease in early turnover, or a 23.7% decrease compared to baseline. Although on average, workers tend to have correct perceptions of the upper-level salary, which renders a low explanatory power for misperceptions (using the estimate from Column (3), about 0.3% of the total variation in early turnover), we introduce exogenous shock to workers' early turnover especially for those with higher levels of misperceptions at the baseline, which we will take advantage in Section 3.6 to explore the implications of turnover.

3.5.4 Exclusion Restrictions and Robustness

The main exclusion restriction assumption to establish the causation is $E\big[\big(\log(P_i^{x,s}) - \log(P_i^{x,0})\big)\cdot T_{c(i)}\cdot u_i\big] = 0$. For all levels of prior belief of job aspect x, the clustered treatment is not correlated with unobserved factors captured in the error term ϵ_i'.

[14]The results on promotion likelihood are different from the graphic intuition from Figure 3.5 because the OLS or IV regression assigns more weights to lower values of baseline belief of promotion, where workers from the treated cohorts are less likely to quit compared to control cohorts.

We first examine whether the treatment effects can be explained by the baseline characteristics in Table 3.2 or baseline perceptions in Table 3.1. Table C.4, Column (2) and (3) provide the test for the main IV specification. The F-statistics for first-stage increase from 33.3 to more than 300 because of the improved precision in estimation. The main coefficient goes down from 41.8 percentage points to 29.2–29.7 percentage points, but the estimates remain significant at the 10% level.

Another potential violation of the exclusion assumption may happen when the treatment provides general information different than the career trajectory of the assigned firm. Treated workers may update the perceptions of the average career trajectory, but when they are assigned to a firm with higher (or lower) promotion likelihood or salary after promotion, they may be less (or more) likely to quit before signing the contract and rejoin the industrial park in the hope for a better draw.

To address this concern, we include firm fixed effects and cluster standard errors within firm (workers who never join any firm will be considered as one group). If the main results hold, the treatment effect is unlikely to be explained by the alternative mechanism that workers may quit early because the salary of the assigned firms fare below the provided information. Table C.4, Column (4) provides this test. The magnitude shrinks to 27.5 percentage points, but the estimate remains statistical significant (p-value 0.010), suggesting the results are difficult to be explained by firm-specific characteristics.

One may concern that workers update their true type after receiving the information. For example, when a worker learns the average salary after promotion is higher than what they expected at the baseline, she may lower her ranking compared to average workers and reassess how likely she can be promoted. This is the case when $E\big[\big(\log(P_i^{x,s})-\log(P_i^{x,0})\big)\cdot T_{c(i)}\cdot u_i\big] < 0$. To address this concern, we first compute the expected earnings of an average worker in one year using workers' answers of entry-level salary in one year, promotion likelihood in one year, and average salary after promotion. Then, we ask each worker how much they expect to earn in one year, and divide it by the computed average earnings in one year to calculate each worker's self-assessed relative type compared to average workers. We then add two interaction terms to control for potential update of workers' own types: treatment interacted with self-assessed relative type, and treatment interacted with the difference between expected own earnings in one year and benchmark earnings of average workers. Table C.4, Column (5) provides this test. Result shows that potential updating on own type does not absorb the main effect.

A fourth concern is that worker's characteristics correlated with prior belief may affect the retention of information. For example, suppose workers with higher cognitive ability are more likely to have overly high prior of promotion likelihood; meanwhile, they are also more likely to retain information when treated. This is the case when $E\big[\big(\log(P_i^{x,s}) -$

$\log(P_i^{x,0})\big) \cdot T_{c(i)} \cdot u_i\big] > 0$, leading to overestimation of parameters of interest. To deal with this concern, we first examine what observed characteristics predict higher retention of information in Equation 3.1. Then, we include these characteristics interacting treatment status in the control vector A_i of Equation 3.2 and 3.3. Table C.4, Column (4) includes the interaction of treatment status and four variables that affect treated workers' retention of information.[15] The effect remains unchanged in Column (6), suggesting that differential information retention cannot fully explain the main empirical patterns.

Last, one may be concerned that the treatment may also update workers' other perceptions and affect turnover. Table C.4, Column (7) includes the interaction of treatment status and all other perceptions at baseline that may be related to career incentives. The magnitude remains similar (27.3 percentage points), but the significance level worsens (p-value 0.197), potentially because too many interactions with treatment are included and decrease the statistical power. We further conduct a following placebo test, assuming that the information treatment affected workers' perceptions on one of the 14 job aspects listed in Table 3.1. Econometrically, we replace the perception measure in Equations 3.2 and 3.3, and report the reduced-form estimates in Figure C.3.[16] We do not find any other perceptions that may generate the similar pattern using perception of salary after promotion.[17]

Taken together, our main estimate of the effect of misperception from Equations 3.2 and 3.3 are robust to potentially unbalanced baseline demographics or misperceptions, firm-specific characteristics, potential update on workers' own type, workers' ability of retaining information, or potential updates on other perceptions.

3.5.5 Spillover

The information treatment may spread to other workers in the same cohorts or through social networks. For instance, if control workers discuss with their treated peers hired on

[15]We first run regression 3.1 including interactions of treatment status and a set of demographic characteristics, skills-relevant variables, social network proxies, and behavioral traits. We then select variables where the p-value of the coefficients is at least lower than 0.20. These four variables are: whether the worker has work experience before, standardized raven score, whether the worker has friends who will join in the industrial park after, whether the worker joins the industrial park because they want to develop skills.

[16]We cannot conduct our IV estimation on all 14 job aspects because we only elicit workers' updated perceptions on four of the 14 job aspects.

[17]In particular, most coefficients from the placebo test are indistinguishable from zero. For two job aspects, percentage of firms providing attendance bonus and days per week required to work, if anything, workers with over-optimistic baseline perceptions are less likely to quit early after the treatment, which do not make sense intuitively. In fact, if we only include the interactions between treatment and the misperceptions of these two job aspects in Table C.4, Column 7, the main coefficient remains of similar magnitude (42.2 percentage points) and significant (p-value 0.048).

the same day or with acquaintances who were treated on a previous day. In this section, we analyze these two types of spillovers: (i) within-cohort spillovers, where workers may observe the information treatment taking place and actively seek out for information; and (ii) across-cohort spillovers, where workers may absorb new information from co-workers hired on a previous day.

Table C.5, Column (1) first reports the reduced-form estimate of the treatment effect with all sample. Column (2) examines the within-cohort spillover by only including control workers; the estimate can be interpreted as the difference between control workers in treated cohorts compared to other control workers in control cohorts. Results suggest control workers in treated cohorts who are over-optimistic of salary after promotion are no less likely to quit, if not less likely, suggesting that the potential spillover to control workers may generate an underestimation of the real treatment effect, not overestimation.

Column (3) and (4) inspect across-cohort spillover with our measurement of social network in the baseline. Each worker is presented names of five treated workers in the last two weeks and asked if they know any of them. In Column (3), we control in the main specification for whether workers know any treated workers. The main coefficient remains unaffected; the well-connected workers are actually more likely to stay on the job, despite the fact that they might receive the benchmark information from previous treated workers. In Column (4), we construct a network index by extracting principal component from the following variables: Number of previous treated workers that the worker knows, number of friends who joined the industrial park before, number of friends who joined the industrial park today. Results are similar to Column (3).

These results suggest that not only spillover does not affect the main estimation, workers with better connections to treated workers are more likely to stay even with higher misperceptions at the baseline, although this conclusion is at best speculative. At the bottom line, the lack of evidence of spillover effects, combining with the correlational evidence from Figure C.1, suggest that informal network may not be capable to address the information gap regarding career progression, which possibly feeds into the current equilibrium of persisting information frictions.

3.6 Alternative Explanations of Turnover

We formally discuss in Sections 3.4 and 3.5 to what extent misperceptions may explain the high turnover rate we observe in the industrial park. Given that firms and the policy markers provide most of the information at the start of the job, we focus on early turnover before workers sign a permanent contract, and randomly provide information of career progression for a random subset of workers to causally estimate the effect of misperceptions on turnover.

Neither causal estimate or correlational evidence suggests that misperceptions can explain the majority of the early turnover. In this section, we discuss two other potential explanations of turnover conceptually, and leverage our information treatment design to provide some suggestive evidence.

3.6.1 Turnover Reflecting Income Shock

One may think of turnover as a reflection of individual income shock of workers prior to joining. Suppose workers suffer a temporary, negative income shock (for example, a bad harvesting season for coffee beans), and apply to a job in the industrial park to avoid hitting the liquidity constraint. Once the negative income shock passes, they would leave the industrial park and go back their previous occupations. This view is consistent with what many firms and policy makers think of entry-level workers as short-term focused, that most entry-level workers only apply for jobs in the industrial park as a temporary income insurance.

This view, however, is not consistent with some descriptive statistics we observe in the data. First, on average, new workers in our sample self-report to plan to stay for 3.8 years, with a sizeable share of workers wanting to stay for more than 4 years. Many workers also self-report to apply for the jobs because the job is interesting or they hope to learn some skills. Although it is likely that these self-reported answers are induced by demand effect, many new workers choose to migrate from rural area and stay around the industrial park, hoping to continue their employment in the garment sector, which makes it a very costly decision to quit early. Second, to explain the high early turnover rate with income shock, one needs to assume a temporary shock right before the worker applies to the job in the industrial park, and the span of such temporary shock only lasts till right before the worker quits, which seems difficult to explain such a high volume of early turnover in our data.

In addition, we provide a suggestive test leveraging the exogenous shock introduced to the treated cohorts. If a worker is introduced such exogenous shock (*i.e.*, workers in the treated cohorts with a high level of misperception of upper-level salary) and are more subject to temporary income shock, she is more likely to quit earlier. Table 3.4 conducts this test by examining the heterogeneity regarding workers' financial vulnerability. Column (1) defines a worker to be more subject to income shock if her total income from all sources is below median. Column (2) defines a worker to be more subject to income shock if her total expenditure on food and rent, the two major expenditure items, is below median. We use the reduced-form specification for higher precision, interact the key independent (treated cohort X workers' baseline misperception of upper-level salary) with the indicator of whether a worker is more subject to temporary income shock. We do not find that workers who are more financially vulnerable are more likely to quit in response to the information treatment. Another relevant test is that, if a worker perceives the outside options to be higher, when encountering a temporary income shock, she is more likely to quit and search for a better outside option. Column (3) and (4) examines the heterogeneity regarding whether a worker perceives outside options within the first month or after one year to be above-median. If anything, workers with higher expectations of outside options are less likely to quit, opposite to what one may expect if a worker only chooses to work in the industrial park because of temporary income shock.

Table 3.4: Sorting of Workers Regarding Income Vulnerability

	(1)	(2)	(3)	(4)
VARIABLES	Quit early	Quit early	Quit early	Quit early
Treated X Baseline salary bias	0.266**	0.245	0.423***	0.388**
	(0.128)	(0.153)	(0.147)	(0.149)
	[0.045]	[0.118]	[0.006]	[0.013]
Treated X Baseline salary bias * Vulnerability	0.010	0.037	-0.326**	-0.214
	(0.204)	(0.206)	(0.161)	(0.144)
	[0.961]	[0.858]	[0.049]	[0.146]
Vulnerability	-0.008	-0.068*	0.002	0.040
	(0.036)	(0.037)	(0.026)	(0.025)
	[0.815]	[0.075]	[0.948]	[0.116]
Observations	1,156	1,166	1,166	1,161
R-squared	0.003	0.008	0.006	0.005
Specification	RF	RF	RF	RF
Cluster	Cohort	Cohort	Cohort	Cohort
Dep var mean	0.407	0.407	0.407	0.407
Proxy for vulnerability	Low income	High expenditure	Expect high Outside options	Expect high future Outside options

Notes: This table reports heterogeneity analysis by workers' income vulnerability. In all specifications the dependent variable is a dummy variable equal to 1 if the worker left the industrial park prior to signing a permanent contract, which occurs after completing the 45-day trial period. Baseline bias is the distance between the natural logarithm of the prior belief and the natural logarithm of the benchmark. We break down the main reduced-form estimates by (1) whether the total income from all sources is below median, (2) whether the total expenditure on food and rent is above median, (3) whether worker's perception of the salary of entry-level jobs outside of the industrial park is above median, and (4) whether the worker's perception of the salary outside of the industrial park after one year is above median. Standard errors are clustered at the cohort (day of hire) level. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

3.6.2 Turnover as Sorting

Another view of turnover regards it as an equilibrium outcome of an endogenous, time-costly matching process. Suppose worker i and firm j has an idiosyncratic matching component μ_{ij}, unobserved to both workers and firms prior matching. To simplify the discussion, suppose μ_{ij} draws from a Bernoulli with only two discrete values, L for low-type workers, and $H > L$ for high-type workers. Workers apply for jobs in the industrial without knowing their types but quickly realize once they start the job. Similarly, firms do not know workers' types when hiring but quickly observe their types once on the job. In this equilibrium, only low-type workers would quit early because their comparative advantage lies outside of the industrial park, while high-type workers would stay because their comparative advantage remains higher within the industrial park.

This hypothesis produces a useful prediction: one can observe whether the workers who stay in the industrial park are high-type workers, which we may proxy by education, experience, and baseline skill measures. We plan to further collect personnel records from the employers to measure workers' productivity and effort. One difficulty is that workers who quit are endogenous to other factors that might also correlate with latent productivity types (*e.g.*, misperceptions and income shock). We leverage our information treatment again that induce a random subset of workers to quit earlier conditional on their baseline misperception level, to provide suggestive evidence of such sorting regarding productivity type.

We generate four proxies for latent productivity: (i) Education: whether worker at least receives vocational training or college education. (ii) Experience: whether worker has previous experience of working in garment factory. (iii) Cognitive ability: we first conduct a 12-question Raven test on each worker and compute a Raven score from the test. We then conduct a short memory test to measure the extent to which they remember a number sequence. In addition, we ask two simple questions to test their knowledge of current affairs (the year when Prime Minister Abiy Ahmed won Nobel Peace Prize; number of regions in Ethiopia). We extract the principal component from these measures as a cognitive index, and examine the heterogeneity on whether the cognitive index is above median.. (iv) Dexterity: We conduct two simple games to measure workers' dexterity relevant to sewing and coordination. The first game requires workers to thread three needles within a minute. The second game requires workers to take 10 pin balls from a box, put each pin ball through a tube and drop it in a different box. Both games were inspired from the grading center of the industrial park who used to conduct grading test on new workers. We extract the principal component from the two measures as a dexterity index, and examine the heterogeneity on whether the dexterity is above median.

Table 3.5 conducts the test by examining the heterogeneity of the main reduced-form results in Table 3.3 regarding the four high-type proxies. First, the coefficients before high-type proxies are significantly negative when using education and experience as proxies, providing some correlational evidence that high-type workers tend to stay longer in the industrial park. Second, for the workers in the treated cohorts with high-level of baseline misperceptions of upper-level salary, we observe significant, negative differential treatment effect among workers with higher educational attainment and higher dexterity scores. These results provide causal evidence that even when induced to leave the industrial park earlier, it is workers with less education and lower-level of dexterity who tend to quit earlier, not the high-type workers. Provided firms can afford to lose a large share of low-type workers at the early stage of their employment, the high turnover may not necessarily pose a challenge for hiring, but an advantage to employers to sort out workers with high-level dexterity and retain better-educated workers.

Table 3.5: Sorting of Workers Regarding Productivity Types

	(1)	(2)	(3)	(4)
VARIABLES	Quit early	Quit early	Quit early	Quit early
Treated X Baseline salary bias	0.390***	0.254*	0.242*	0.452**
	(0.130)	(0.140)	(0.143)	(0.169)
	[0.005]	[0.078]	[0.098]	[0.011]
Treated X Baseline salary bias * High type	-0.448***	0.128	0.042	-0.309*
	(0.150)	(0.264)	(0.169)	(0.180)
	[0.005]	[0.632]	[0.806]	[0.095]
High type	-0.117***	-0.302***	0.004	-0.015
	(0.037)	(0.048)	(0.037)	(0.034)
	[0.003]	[0.000]	[0.914]	[0.666]
Observations	1,166	1,153	1,166	1,135
R-squared	0.023	0.040	0.003	0.007
Specification	RF	RF	RF	RF
Cluster	Cohort	Cohort	Cohort	Cohort
Dep var mean	0.407	0.407	0.407	0.407
Proxy for high type	Educated	Experienced	High cognitive	High dexterity

Notes: This table reports heterogeneity analysis by proxies for productivity type. In all specifications the dependent variable is a dummy variable equal to 1 if the worker left the industrial park prior to signing a permanent contract, which occurs after completing the 45-day trial period. Baseline bias is the distance between the natural logarithm of the prior belief and the natural logarithm of the benchmark. We break down the main reduced-form estimates by (1) whether worker attended vocational training school or colleges, (2) whether the worker worked in a garment factory before, (3) whether the worker has an above-median cognitive score measured in our survey, and (4) whether the worker has an above-median dexterity score measured in our survey. Standard errors are clustered at the cohort (day of hire) level. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

3.7 Conclusion

We study the causes of high turnover rates in the context of a flagship industrial park in Ethiopia. We collect detailed measures of misperceptions from 1,203 new workers, combined with the administrative records of turnover and an information treatment to introduce exogenous shock to workers' perceptions of salary after promotion and the likelihood of being promoted. Correlational and causal evidence both suggest that misperceptions can only explain a small proportion of early turnover rates (0.3–5% of total variation). Instead, we further use our information treatment design to test whether turnover can reflect temporary income shock or serve as a sorting mechanism regarding latent productivity types. We find suggestive evidence that although temporary income shock is unlikely to explain our empirical patterns, educated workers and workers with higher-level of dexterity are more likely to stay in the industrial park even when randomly induced to quit earlier, the latter suggesting that, to the employers' benefit, turnover may be an equilibrium outcome where high-type workers stay.

We do not attempt to provide a final answer to this important puzzle of high turnover rates in many developing countries. Instead, it begs further research to provide more causal evidence to understand the extent to which temporal income shock and sorting may explain the high turnover rates in these industrial settings. For example, regarding temporary income shock, we plan to explore the origins of 35,288 workers registered in the industrial park between July 2018 and March 2020, using rainfall shocks and global price shocks in cash crop (*e.g.*, coffee beans) to causally estimate the effect of temporary income shock on turnover. Regarding sorting, we plan to collect detailed personnel data from employers regarding the workers from the administrative records as well as the workers from our survey sample, to provide better measurement of the effort and productivity for those who stay in the industrial park, and observe potential sorting during temporary income shock or induced information shock. We are also not able to provide a concrete answer to the cost of the turnover, partly because the government provides substantial support for the industrial park to source entry-level workers on a daily basis. We call for future research to provide a more precise estimate of the cost and benefit of turnover.

Bibliography

Abadie, A. (2003). Semiparametric Instrumental Variable Estimation of Treatment Response Models. *Journal of Econometrics*, 113(2):231–263.

Abebe, G., Caria, A. S., Fafchamps, M., Falco, P., Franklin, S., and Quinn, S. (2021). Anonymity or Distance? Job Search and Labour Market Exclusion in a Growing African City. *The Review of Economic Studies*, 88(3):1279–1310.

Abebe, G., Caria, S., Fafchamps, M., Falco, P., Franklin, S., Quinn, S., and Shilpi, F. (2023). Matching Frictions and Distorted Beliefs: Evidence from a Job Fair Experiment. *Working Paper*.

Abel, M., Burger, R., and Piraino, P. (2020). The Value of Reference Letters: Experimental Evidence from South Africa. *American Economic Journal: Applied Economics*, 12(3):40–71.

Alfonsi, L., Namubiru, M., and Spaziani, S. (2023). Meet Your Future: Experimental Evidence on the Labor Market Effects of Mentors. *Working Paper*.

Algan, Y., Crépon, B., and Glover, D. (2022). Are Active Labor Market Policies Directed at Firms Effective? Evidence from a Randomized Evaluation with Local Employment Agencies. *Working Paper*.

Angrist, J. and Imbens, G. (1995). Identification and Estimation of Local Average Treatment Effects. *Econometrica*, 62(2):467–475.

Arjona, A. (2017). *Rebelocracy: Social Order in the Colombian Civil War*. Cambridge University Press.

Arjona, A., Kasfir, N., and Mampilly, Z. C. (2014). *Rebel Governance in Civil War*. Cambridge University Press.

Autor, D. (2001). Why do Temporary Help Firms Provide Free General Skills Training? *The Quarterly Journal of Economics*, 116(4):1409–1448.

Autor, D. (2008). The Economics of Labor Market Tntermediation. *NBER Working Paper*, No. 14348.

Bagayoko, N., Hutchful, E., and Luckham, R. (2016). Hybrid Security Governance in Africa: Rethinking the Foundations of Security, Justice and Legitimate Public Authority. *Conflict, Security & Development*, 16(1):1–32.

Balcells, L. (2012). The Consequences of Victimization on Political Identities: Evidence from Spain. *Politics & Society*, 40(3):311–347.

Balcells, L. (2017). *Rivalry and Revenge The Politics of Violence during Civil War*. Cambridge University Press, Cambridge.

Banerjee, A. and Chiplunkar, G. (2023). How Important are Matching Frictions in the Labor Market? Evidence from a Large Indian Firm. *Working paper*.

Banerjee, A., Fischer, G., Karlan, D., Lowe, M., and Roth, B. N. (2023). Do Microenterprises Maximize Profits? A Vegetable Market Experiment in India. *mimeo*.

Banerjee, A. and Sequeira, S. (2023). Learning by Searching: Spatial Mismatches and Imperfect Information in Southern Labor Markets. *Journal of Development Economics*, 164:103111.

Barea, A. (1984). *The Forging of a Rebel*. London: Fontana.

Bassi, V. and Nansamba, A. (2022). Screening and Signalling Non-Cognitive Skills: Experimental Evidence from Uganda. *The Economic Journal*, 132(642):471–511.

Beaman, L. and Magruder, J. (2012). Who Gets the Job Referral? Evidence from a Social Networks Experiment. *American Economic Review*, 102(7):3574–3593.

Becker, G. S. (1964). *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education*. University of Chicago press.

Beckert, S. (2015). *Empire of Cotton: A Global History*. Vintage.

Blattman, C. and Dercon, S. (2018). The Impacts of Industrial and Entrepreneurial Work on Income and Health: Experimental Evidence from Ethiopia. *American Economic Journal: Applied Economics*, 10(3):1–38.

Bloom, N., Eifert, B., Mahajan, A., McKenzie, D., and Roberts, J. (2013). Does Management Matter? Evidence from India. *The Quarterly Journal of Economics*, 128(1):1–51.

Borusyak, K., Jaravel, X., and Spiess, J. (2024). Revisiting Event Study Designs: Robust and Efficient Estimation. *Review of Economic Studies*.

Bowles, S. (2006). Group Competition, Reproductive Leveling, and the Evolution of Human Altruism. *Science*, 314(5805):1569–1572.

Bowles, S. and Gintis, H. (2011). *A Cooperative Species: Human Reciprocity and Its Evolution*. Princeton University Press.

Brabant, J. (2016). *"Qu'on Nous Laisse Combattre, et la Guerre Finira"': Avec les Combattants du Kivu*. La Découverte, Paris.

Brown, C. and Andrabi, T. (2023). Inducing Positive Sorting through Performance Pay: Experimental Evidence from Pakistani Schools. *Working Paper*.

Caldwell, S. and Danieli, O. (2024). Outside Options in the Labor Market. *Review of Economic Studies*.

Callen, M., Weigel, J. L., and Yuchtman, N. (2023). Experiments about Institutions. *NBER Working Paper*.

Cantoni, D., Heizlsperger, L.-J., Yang, D. Y., Yuchtman, N., and Zhang, Y. J. (2022). The Fundamental Determinants of Protest Participation: Evidence from Hong Kong's Antiauthoritarian Movement. *Journal of Public Economics*, 211:104667.

Cantoni, D., Yang, D. Y., Yuchtman, N., and Zhang, Y. J. (2019). Protests as Strategic Games: Experimental Evidence from Hong Kong's Antiauthoritarian Movement. *The Quarterly Journal of Economics*, 134(2):1021–1077.

Card, D. (2001). Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems. *Econometrica*, 69(5):1127–1160.

Carneiro, P., Heckman, J. J., and Vytlacil, E. J. (2011). Estimating Marginal Returns to Education. *American Economic Review*, 101(6):2754–2781.

Carneiro, R. L. (1970). A Theory of the Origin of the State. *Science*, 169(3947):733–738.

Carranza, E., Garlick, R., Orkin, K., and Rankin, N. (2023). Job Search and Hiring with Limited Information about Workseekers' Skills. *American Economic Review*.

Cerina, R., Barrie, C., Ketchley, N., and Zelin, A. Y. (2023). Explaining Recruitment to Extremism: A Bayesian Hierarchical Case–Control Approach. *Political Analysis*, pages 1–19.

Collier, P. and Hoeffler, A. (2004). Greed and Grievance in Civil War. *Oxford Economic Papers*, 56(4):563–595.

Cowgill, B. and Perkowski, P. (2020). Delegation in Hiring: Evidence from a Two-Sided Audit. *Columbia Business School Research Paper*.

Cullen, Z., Dobbie, W., and Hoffman, M. (2023). Increasing the Demand for Workers with a Criminal Record. *The Quarterly Journal of Economics*, 138(1):103–150.

Cullen, Z. and Perez-Truglia, R. (2022). How Much Does Your Boss Make? The Effects of Salary Comparisons. *Journal of Political Economy*, 130(3):766–822.

Dale, S. B. and Krueger, A. B. (2002). Estimating the Payoff to Attending a More Selective College: An Application of Selection on Observables and Unobservables. *The Quarterly Journal of Economics*, 117(4):1491–1527.

DellaVigna, S., Lindner, A., Reizer, B., and Schmieder, J. F. (2017). Reference-Dependent Job Search: Evidence from Hungary. *The Quarterly Journal of Economics*, 132(4):1969–2018.

DellaVigna, S. and Paserman, M. D. (2005). Job Search and Impatience. *Journal of Labor Economics*, 23(3):527–588.

Dorn, D., Schmieder, J. F., and Spletzer, J. R. (2018). Domestic Outsourcing in the United States. *US Department of Labor Technical Report*.

Duflo, A., Kiessel, J., and Lucas, A. (2020). External Validity: Four Models of Improving Student Achievement. *NBER Working Paper*.

Dunia Butinda, L. (2021). *D'Enfant de Walikale a la Prise de l'Arme: Un Voyage Aller-Retour*. Unpublished Autobiography.

Evans, D. K. and Mendez Acosta, A. (2021). Education in Africa: What are We Learning? *Journal of African Economies*, 30(1):13–54.

Farber, H. S. (1994). The Analysis of Interfirm Worker Mobility. *Journal of Labor Economics*, 12(4):554–593.

Farber, H. S. (1999). Mobility and Stability: The Dynamics of Job Change in Labor Markets. *Handbook of labor economics*, 3:2439–2483.

Fernando, A. N., Singh, N., and Tourek, G. (2022). Hiring Frictions and the Promise of Online Job Portals: Evidence from India. *mimeo*.

Franklin, S. (2018). Location, Search Costs and Youth Unemployment: Experimental Evidence from Transport Subsidies. *The Economic Journal*, 128(614):2353–2379.

Frowd, P. M. (2022). The Politics of Non-State Security Provision in Burkina Faso: Koglweogo Self-Defence Groups' Ambiguous Pursuit of Recognition. *African Affairs*, 121(482):109–130.

Fryer, R. and Levitt, S. (2012). Hatred and Profits: Under the Hood of the Ku Klux Klan. *Quarterly Journal of Economics*, 127(4):1883–1925.

Gáfaro, M., Ibáñez, A. M., and Justino, P. (2022). Community Organization and Armed Group Behaviour: Evidence from Colombia. Technical report, WIDER Working Paper.

Geenen, S. (2013). Who Seeks, Finds: How Artisanal Miners and Traders Benefit from Gold in the Eastern Democratic Republic of Congo. *European Journal of Development Research*, 25:197 – 212.

Gigerenzer, G., Reb, J., and Luan, S. (2022). Smart Heuristics for Individuals, Teams, and Organizations. *Annual Review of Organizational Psychology and Organizational Behavior*, 9:171–198.

Goldschmidt, D. and Schmieder, J. F. (2017). The Rise of Domestic Outsourcing and the Evolution of the German Wage Structure. *The Quarterly Journal of Economics*, 132(3):1165–1217.

González, F. (2020). Collective Action in Networks: Evidence from the Chilean Student Movement. *Journal of Public Economics*, 188:104220.

Gould, R. V. (1991). Multiple Networks and Mobilization in the Paris Commune, 1871. *American Sociological Review*, 56(6):716–729.

Gould, R. V. (1993). Collective Action and Network Structure. *American Sociological Review*, 58(2):182–96.

Gould, R. V. (1995). *Insurgent Identities: Class, Community, and Protest in Paris from 1848 to the Commune*. University of Chicago Press.

Greif, A. (1993). Contract Enforceability and Economic Institutions in Early Trade: the Maghribi Traders' Coalition. *American Economic Review*, 83(3):525–48.

Groh, M., Krishnan, N., McKenzie, D., and Vishwanath, T. (2016). The Impact of Soft Skills Training on Female Youth Employment: Evidence from a Randomized Experiment in Jordan. *IZA Journal of Labor & Development*, 5(1):9.

Guevara, C. and Ortiz, V. (1969). *Reminiscences of the Cuban Revolutionary War: Translated by Victoria Ortiz*. Penguin.

Gurr, T. R. (1970). *Why Men Rebel*. Routledge.

Gutiérrez-Sanín, F., Arjona, A., Kasfir, N., and Mampilly, Z. (2015). Organization and Governance: The Evolution of Urban Militias in Medellín, Colombia. *Rebel Governance in Civil War*, pages 246–264.

Haegele, I. (2024). Talent Hoarding in Organizations. *American Economic Review*.

Hardy, M., Kagy, G., Meyer, C., Tamrat, E., and Witte, M. (2022). The Impact of Firm Downsizing on Workers: Evidence from Ethiopia's Ready-Made Garment Industry. *Working paper*.

Hardy, M. and McCasland, J. (2023). Are Small Firms Labor Constrained? Experimental Evidence from Ghana. *American Economic Journal: Applied Economics*.

Hausman, J. A. (1978). Specification Tests in Econometrics. *Econometrica: Journal of the Econometric Society*, pages 1251–1271.

Heald, S. (2006). State, Law, and Vigilantism in Northern Tanzania. *African Affairs*, 105(419):265–283.

Heath, R. (2018). Why do Firms Hire Using Referrals? Evidence from Bangladeshi Garment Factories. *Journal of Political Economy*, 126(4):1691–1746.

Heineman, M. (2015). Cartel Land, documentary.

Heldring, L. (2020). The Origins of Violence in Rwanda. *The Review of Economic Studies*, 88(2):730–763.

Hemingway, E. (1940). For Whom the Bell Tolls. *New York: Scribner's*.

Hensel, L., Tekleselassie, T., and Witte, M. (2021). Formalized Employee Search and Labor Demand. *Working Paper*.

Hoffmann, K. (2015). Myths Set in Motion: The Moral Economy of Mai Mai Governance. In Arjona, A., Kasfir, N., and Mampilly, Z., editors, *Rebel Governance in Civil War*. Cambridge University Press, Cambridge.

Humphreys, M., Sánchez de la Sierra, R., and Van der Windt, P. (2013). Fishing, Commitment, and Communication: A Proposal for Comprehensive Nonbinding Research Registration. *Political Analysis*, 21(1):1–20.

Humphreys, M. and Weinstein, J. M. (2008). Who Fights? The Determinants of Participation in Civil War. *American Journal of Political Science*, 52(2):436–455.

International Labor Organization (2013). Resolution I Concerning Statistics of Work, Employment and Labour Underutilization. *Conference doument*.

Jourdan, L. (2011). Mayi-Mayi: Young Rebels in Kivu, DRC. *Africa Development*, 36(3-4):89–112.

Jovanovic, B. (1979). Job Matching and the Theory of Turnover. *Journal of Political Economy*, 87(5, Part 1):972–990.

Jäger, S., Schoefer, B., Young, S., and Zweimüller, J. (2023). Wages and the Value of Nonemployment. *The Quarterly Journal of Economics*, 135(4):1905–1963.

Kasfir, N., Frerks, G., and Terpstra, N. (2017). Introduction: Armed Groups and Multi-layered Governance. *Civil Wars*, 19(3):257–278.

Kelley, E. M., Ksoll, C., and Magruder, J. (2024). How do Online Job Portals Affect Employment and Job Search? Evidence from India. *Journal of Development Economics*.

Kisangani, E. F. (2012). *Civil Wars in the Democratic Republic of Congo, 1960–2010*. Lynne Rienner Publishers, Boulder, Colorado.

Knappenberger, B. (2021). Turning Point: 9/11 and the War on Terror, documentary.

Kourouma, A. (2000). Allah N'est Pas Obligé. *Paris: Éditions du Seuil*.

Kremer, M., Brannen, C., and Glennerster, R. (2013). The Challenge of Education and Learning in the Developing World. *Science*, 340(6130):297–300.

Kremer, M. and Holla, A. (2009). Improving Education in the Developing World: What Have We Learned from Randomized Evaluations? *Annual Review of Economics*, 1(1):513–542.

Lee, D. S. (2009). Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects. *Review of Economic Studies*, 76(3):1071–1102.

Lee, D. S., McCrary, J., Moreira, M. J., and Porter, J. (2022). Valid t-ratio Inference for IV. *American Economic Review*, 112(10):3260–3290.

Li, D., Raymond, L. R., and Bergman, P. (2023). Hiring as Exploration. *National Bureau of Economic Research*.

Lowes, S. and Montero, E. (2021). Concessions, Violence, and Indirect Rule: Evidence from the Congo Free State. *The Quarterly Journal of Economics*, 136(4):2047–2091.

Lwigulira, B. L. (1993). *Histoire et Culture des Bashi au Zäire:" Six Derniers Règnes" Antérieurs à 1980*. Centre Protestant d'Editions et Diffusion.

Malraux, A. (1938). *Man's Hope*. Random Hous.

Mampilly, Z. C. (2011). *Rebel Rulers: Insurgent Governance and Civilian Life during War*. Cornell University Press.

Marchais, G. (2016). He Who Touches the Weapon Becomes Other: A Study of Participation in Armed Groups in South Kivu, Democratic Republic of the Congo. *PhD thesis*.

Martellini, P., Schoellman, T., and Sockin, J. (2022). The Global Distribution of College Graduate Quality. *Working Paper*.

Mayshar, J., Moav, O., and Neeman, Z. (2011). Transparency, Appropriability and the Early State. *CEPR Discussion Paper*, 8548.

McDoom, O. S. (2013). Who Killed in Rwanda's Genocide? Micro-Space, Social Influence and Individual Participation in Intergroup Violence. *Journal of Peace Research*, 50(4):453–467.

Méndez, E. and Van Patten, D. (2022). Multinationals, Monopsony, and Local Development: Evidence from the United Fruit Company. *Econometrica*, 90(6):2685–2721.

Montgomery, D. (1989). *The Fall of the House of Labor: The Workplace, the State, and American Labor Activism, 1865-1925*. Cambridge University Press.

Muralidharan, K., Singh, A., and Ganimian, A. J. (2019). Disrupting Education? Experimental Evidence on Technology-Aided Instruction in India. *American Economic Review*, 109(4):1426–1460.

Muralidharan, K. and Sundararaman, V. (2011). Teacher Performance Pay: Experimental Evidence from India. *Journal of Political Economy*, 119(1):39–77.

Northrup, D. (1988). *Beyond the Bend in the River: African Labor in Eastern Zaire, 1865-1940*. Ohio University Center for International Studies, Athens, Ohio, monographs edition.

Olson, M. (1971). *The Logic of Collective Action: Public Goods and the Theory of Groups, Second Printing with a New Preface and Appendix*. Harvard University Press.

Østby, G. (2013). Inequality and Political Violence: A Review of the Literature. *International Area Studies Review*, 16(2):206–231.

Ostrom, E. (1999). Coping with Tragedies of the Commons. *Annual Review of Political Science*, 2(1):493–535.

Parkinson, S. E. (2013). Organizing Rebellion: Rethinking High-Risk Mobilization and Social Networks in War. *American Political Science Review*, 107(3):418–432.

Petersen, R. D. (2001). *Resistance and Rebellion: Lessons from Eastern Europe*. Cambridge University Press, New York.

Piper, B., Destefano, J., Kinyanjui, E. M., and Ong'ele, S. (2018). Scaling Up Successfully: Lessons from Kenya's Tusome National Literacy Program. *Journal of Educational Change*, 19:293–321.

Pratten, D. and Sen, A. (2007). *Introduction: Global Vigilantes: Perspectives on Justice and Violence*. Hurst Publishers.

Richards, P. (1996). *Fighting for the Rain Forest: War, Youth & Resources in Sierra Leone*. Heinemann.

Sánchez de la Sierra, R. (2020). On the Origins of the State: Stationary Bandits and Taxation in Eastern Congo. *Journal of Political Economy*, 128(1):32–74.

Sanín, F. G. and Wood, E. J. (2014). Ideology in Civil War: Instrumental Adoption and Beyond. *Journal of Peace Research*, 51(2):213–226.

Scacco, A. (2024). *Anatomy of a Riot*. Cambridge University Press.

Shesterinina, A. (2021). *Mobilizing in Uncertainty: Collective Identities and War in Abkhazia*. Cornell University Press.

Smith, J., Goodman, J., and Hurwitz, M. (2020). The Economic Impact of Access to Public Four-Year Colleges. *National Bureau of Economic Research*.

Spence, M. (1973). Job Market Signaling. *The Quarterly Journal of Economics*, 87(3):355–374.

Spinnewijn, J. (2015). Unemployed but Optimistic: Optimal Insurance Design with Biased Beliefs. *Journal of the European Economic Association*, 13(1):130–167.

Staniland, P. (2012a). Organizing Insurgency: Networks, Resources, and Rebellion in South Asia. *International Security*, 37(1):142–177.

Staniland, P. (2012b). States, Insurgents, and Wartime Political Orders. *Perspectives on Politics*, 10(2):243–264.

Staniland, P. (2014). *Networks of Rebellion: Explaining Insurgent Cohesion and Collapse*. Cornell University Press, Ithaca and London.

Staniland, P. (2021). *Ordering Violence: Explaining Armed Group-State Relations from Conflict to Cooperation*. Cornell University Press.

Stanton, C. T. and Thomas, C. (2016). Landing the First Job: The Value of Intermediaries in Online Hiring. *The Review of Economic Studies*, 83(2):810–854.

Stearns, J. (2011). *Dancing in the Glory of Monsters*. Public Affairs, New York.

Stearns, J. and Botiveau, R. (2013). Repenser la Crise au Kivu: Mobilisation Armee et Logique du Gouvernement de Transition. *Politique Africaine*, 1(129):204.

Stearns, J. K. (2013). Raia Mutomboki: The Flawed Peace Process and the Birth of an Armed Franchise. Technical report, The Usalama Project, Understanding armed groups. The Rift Valley Institute, London.

Stys, P., Verweijen, J., Muzuri, P., Muhindo, S., Vogel, C., and Koskinen, J. H. (2020). Brokering between (Not so) Overt and (Not So) Covert Networks in Conflict Zones. *Global Crime*, 21(1):74–110.

Terviö, M. (2009). Superstars and Mediocrities: Market Failure in the Discovery of Talent. *The Review of Economic Studies*, 76(2):829–850.

Tilly, C. (1985). War Making and State Making as Organized Crime. In Peter Evans, Dietrich Rueschemeyer, T. S., editor, *Bringing the State Back In*, Cambridge. Cambridge University Press.

United Nations Group of Experts (2016). Final Report of the Group of Experts on the Democratic Republic of the Congo. Technical report, United Nations.

Vansina, J. (1990). *Paths in the Rainforests Toward a History of Political Tradition in Equatorial Africa*. University of Wisconsin Press, Madison, Wisconsin.

Verweijen, J. (2016). Stable Instability: Political Settlements and Armed Groups in the Congo. *Rift Valley Institute (RVI)*.

Vitali, A. (2023). Consumer Search and Firm Location: Theory and Evidence from the Garment Sector in Uganda. *mimeo, University College London*.

Viterna, J. (2013). *Women in War: The Micro-processes of Mobilization in El Salvador*. Oxford University Press.

Viterna, J. S. (2006). Pulled, Pushed, and Persuaded: Explaining Women's Mobilization into the Salvadoran Guerrilla Army. *American Journal of Sociology*, 112(1):1–45.

Vlassenroot, K. and Raeymaekers, T. (2004). *Conflict and Social Transformation in Eastern DR Congo*. Academia Press.

Vogel, C. (2014). Contested Statehood, Security Dilemmas and Militia Politics: The Rise and Transformation of Raïa Mutomboki in Eastern DRC. In *L'Afrique des Grands Lacs. Annuaire.* L'Harmattan, Paris.

Vogel, C., Salvaggio, G., Boisselet, P., and Stearns, J. K. (2021). The Landscape of Armed Groups in Eastern Congo: Missed Opportunities, P-rotracted Insecurity and Self-Fulfilling prophecies. Technical report, Kivu Security Tracker - Congo Research Group - NYU Center on International Cooperation, New York.

Vogel, C. and Stearns, J. K. (2018). Kivu's Intractable Security Conundrum, Revisited. *African Affairs*, 117(469):695–707.

Weinstein, J. (2007). *Inside Rebellion*. Cambridge University Press.

Wittfogel, K. A. (1953). Oriental Despotism. *Sociologus*, 3(2):96–108.

Wood, E. J. (2003). *Insurgent Collective Action and Civil War in El Salvador*. Cambridge University Press.

Zimmerman, S. D. (2014). The Returns to College Admission for Academically Marginal Students. *Journal of Labor Economics*, 32(4):711–754.

Appendix A

Additional Materials for Chapter 1

A.1 Data and Measurements

For firm-level variables, see Table A.1. For worker-level variables, see Table A.2.

Table A.1: Definitions of 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 ≥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 Additional Details on the 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

Table A.2: Definitions of Worker-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 A.7
		Educ: At most high school	Figure A.7
	Years of work experience	Experience: ≥2y	Section 1.5.5, Figure A.7
		Experience: Some but <2y	Figure A.7
		Experience: None	Section 1.5.5, Figure A.7
	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 survey	Gender	Gender	Section 1.5.5, Figure A.7
	What is your age?	Age: Above median	Section 1.5.5, Figure A.7
	Are you currently employed?	Currently employed	Section 1.5.5, data validation
	What is your current job?		Data validation
	What is your monthly salary?	Current salary	Section 1.5.5, data validation

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 1.5.1. 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:

$$\frac{\partial |\,\mathbb{E}\left[\mathbb{E}_j[a|I_j^i]\right] - \widetilde{a}_j^0|}{\partial n_j^0} < 0 \tag{A.1}$$

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 A.2.1. *Suppose firm j is initially over-optimistic of the college premium and condition A.1 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 A.1, we have $|\epsilon_{\widetilde{a}_j,q}|$ decreasing in n_j^0, hence the proposition. Together with Proposition 1.5.1, we can derive the prediction on hiring behavior regarding past exposure to college graduates.

A.3 Additional Tables and Figures

Table A.3: 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 A.4: 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 A.5: 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 A.6: Effect 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 1.1, 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 A.7: Effect 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 1.1, 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 A.8: 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 1.1, 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 A.9: Effect 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 1.1, 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 A.10: 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 1.1, 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 A.11: 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 1.1, 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 A.12: 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 1.1, 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 A.13: 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 1.1, 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 A.14: 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 1.1 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 A.15: Effect 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 1.1, 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 A.16: Heterogeneous Effect 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 1.1, 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 A.17: Effect on the Perceptions of College Applicants By College Share

VARIABLES	(1)	(2)	(3)	(4)
	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 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 1.1, 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 A.18: Heterogeneous Effect 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 1.1, 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 A.19: 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 A.20: 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 A.21: Effect 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 1.1, 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 A.22: Effect 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 1.1, 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 A.23: Effect 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 1.1, 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: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

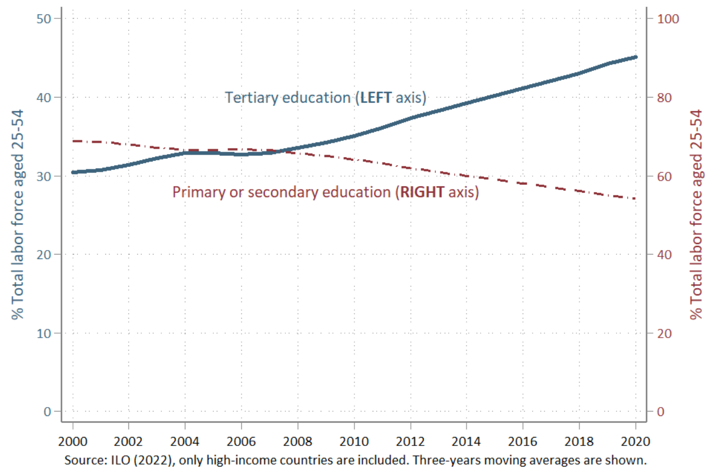
Table A.24: Complier Analysis

VARIABLES	(1) Salary	(2) Voluntary Quit	(3) Fired By firm	(4) Above-avg prod (Surveyed)	(5) Above-avg prod (Measured)	(6) No 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]

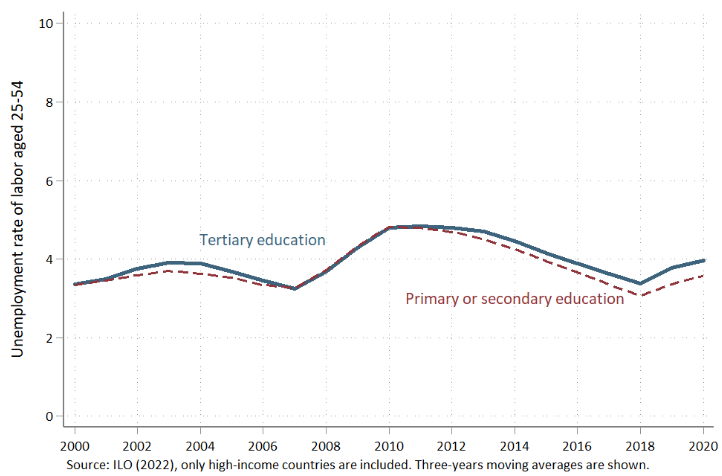
Notes: This table presents the complier analysis following Abadie (2003). The sample is restricted to firms eligible for treatment with reservation wage at least 2,000 ETB. Endogeneous variables: Whether firms hire any college graduates (H_c), and whether firms hire any non-college workers (H_n). Instrument: 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: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Figure A.1: Tertiary Education in High-Income Countries, 2000–20

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

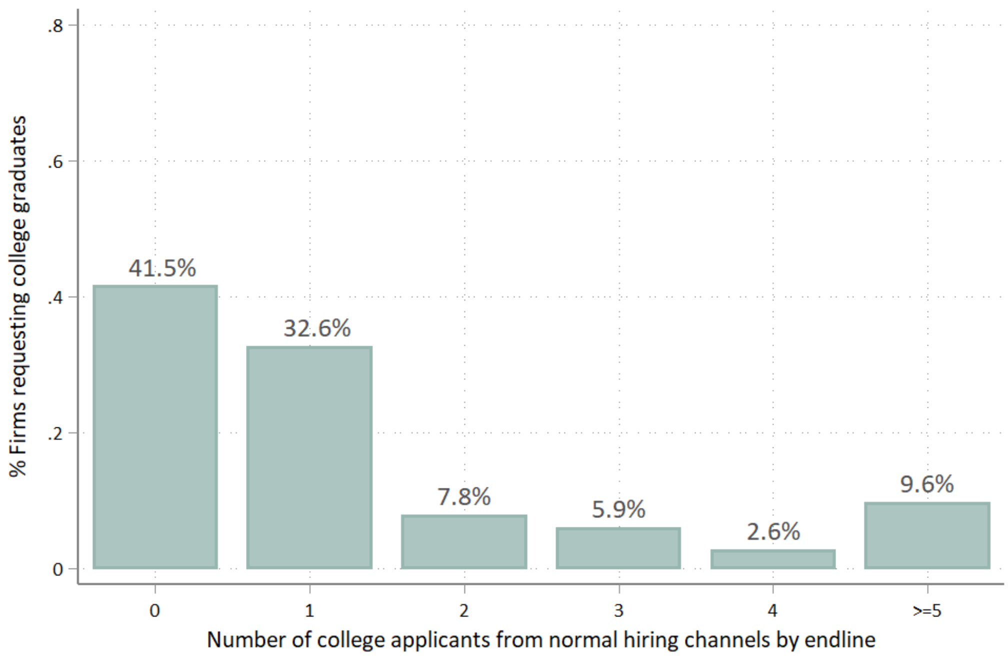


Panel B. Unemployment of tertiary educated workers, aged 25–54



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.

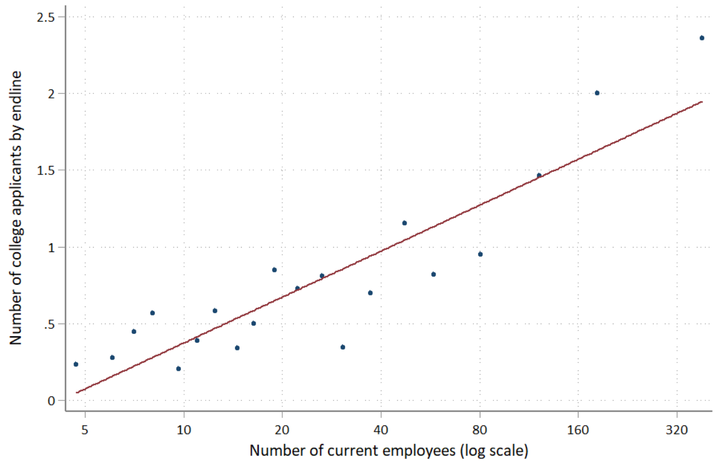
Figure A.2: Distribution of College Applicants Among Firms Requesting College Graduates



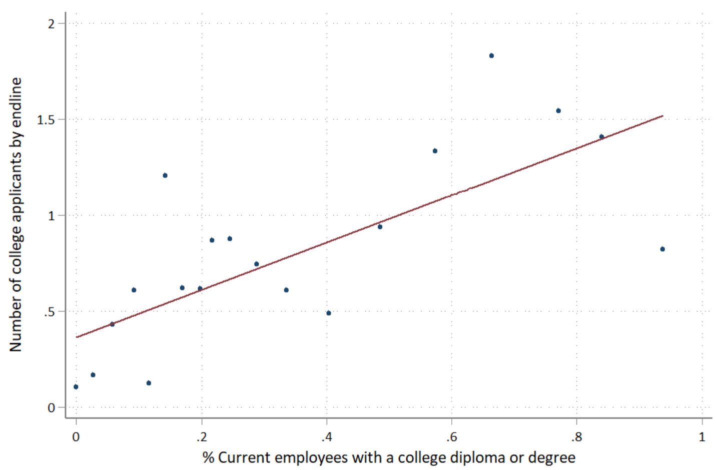
Notes: This figure shows the distribution of the total number of college applicants by endline for firms requesting college graduates, not including applicants from the employment agencies in the intervention.

Figure A.3: Correlations Between the Number of College Applicants and Firm Characteristics

Panel A. Correlation with firm size



Panel B. Correlation with college share



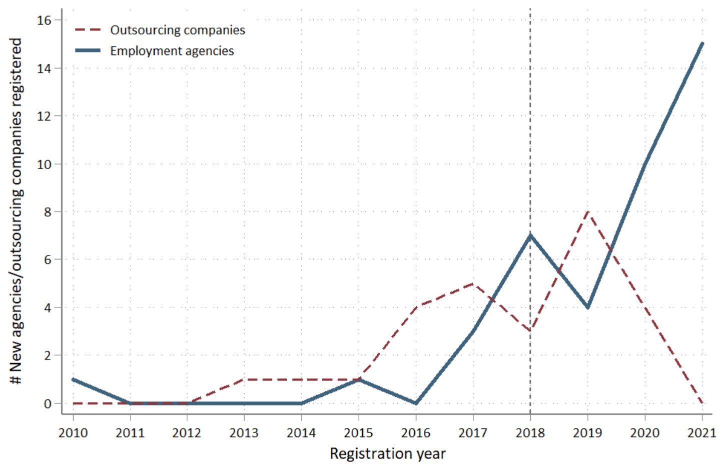
Notes: This figure shows the correlations between the number of college applicants received by each firm (excluding those from the employment agencies) and two firm characteristics: the number of current employees, and the percentage of current employees with a college diploma or degree.

Figure A.4: A Typical Employment Agency

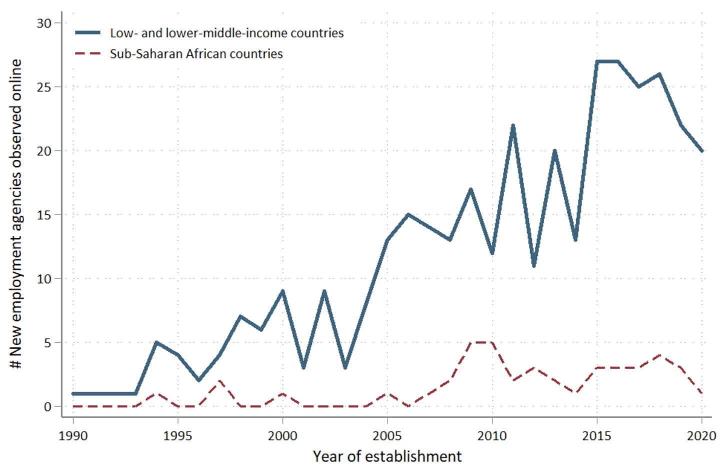
Notes: This figure shows a typical employment agency in our sample located in Bole sub-city, Addis Ababa, Ethiopia.

Figure A.5: Trends of Employment Agencies

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

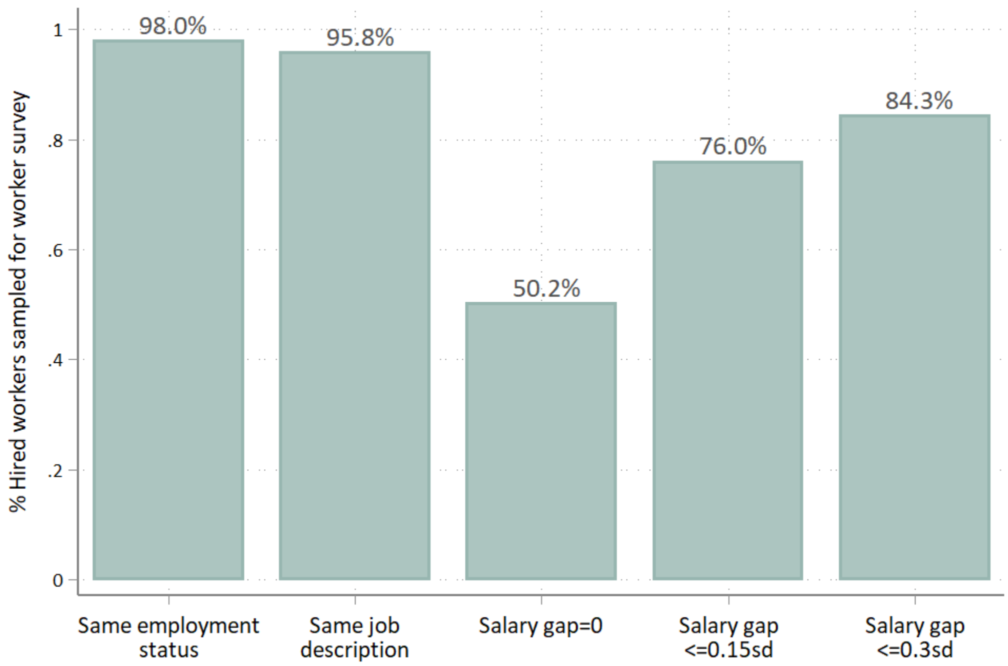


Panel B. Number of employment agencies in low- and middle-income countries, 1990–2020



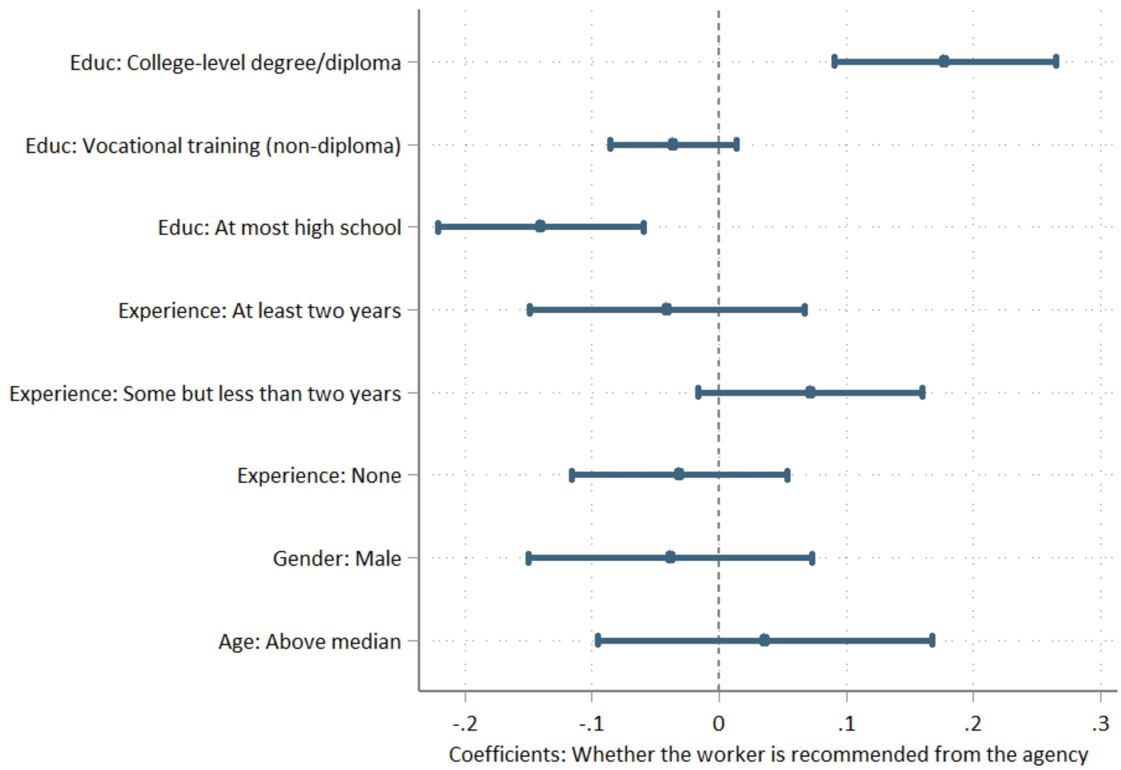
Notes: This figure shows the trend of employment agencies in the recent decades. Panel A shows the number of registered labor market intermediaries in Bole sub-city during 2010–21. The data come from the registry of employment agencies from Bole sub-city. Blue solid line shows the trend of employment 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 A.6: 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 A.7: Selection of Applicants from Employment Agencies



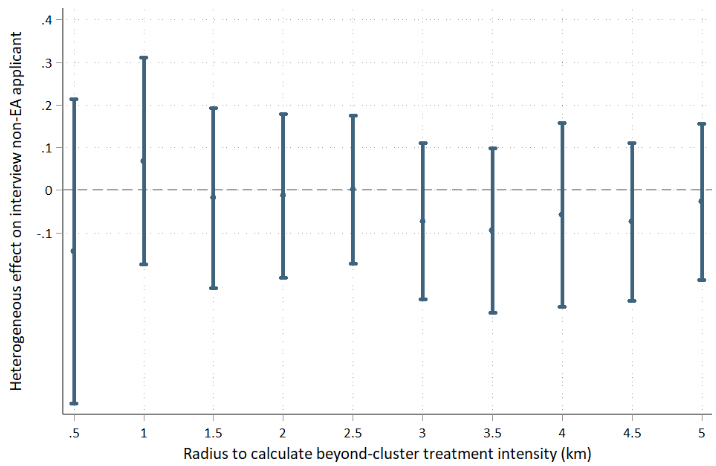
Notes: This figure shows the selection of applicants from the employment agencies in terms of observable characteristics. For each characteristics, we compare agency applicants to non-agency applicants, controlling for firm fixed effects and cluster at the firm level. 95% confidence intervals are shown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

Figure A.8: Replication: Effect on Hiring Non-Agency Applicants

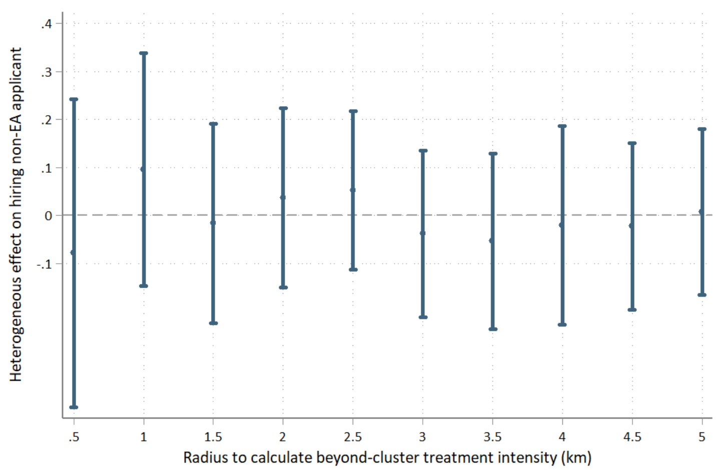
Notes: This figure replicates the main results in Column (2) and (5) in Table 1.3. All regressions include a full set of baseline characteristics from Table 1.1, control for business area fixed effects, and cluster at business area level. For each dependent variable, we show (1) reduced-form estimate 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 A.9: Heterogeneous Effect on Interviewing and Hiring Non-Agency Applicants by Treatment Intensity

Panel A. Interviewing any non-agency applicant

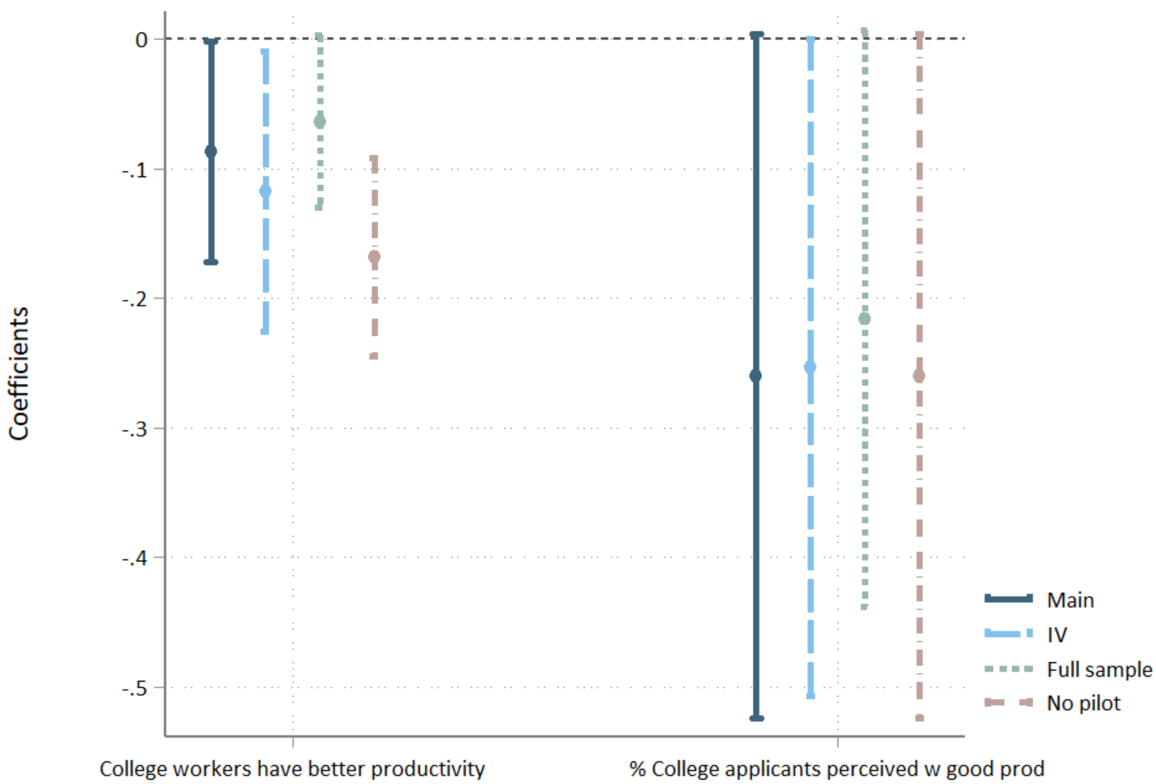


Panel B. Hiring any non-agency applicant



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.

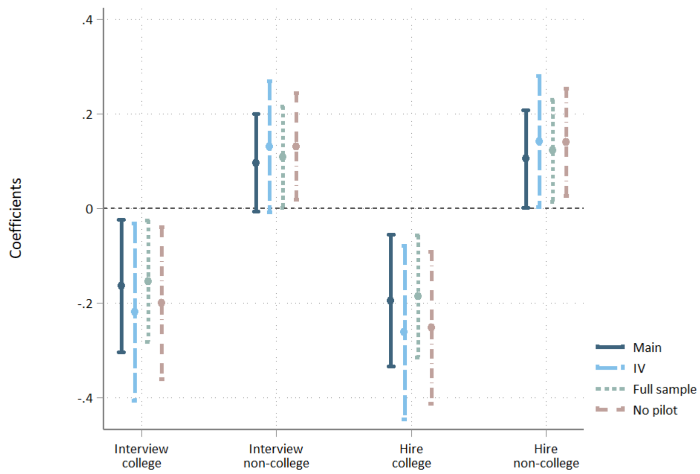
Figure A.10: Replication: Effect on Perceptions



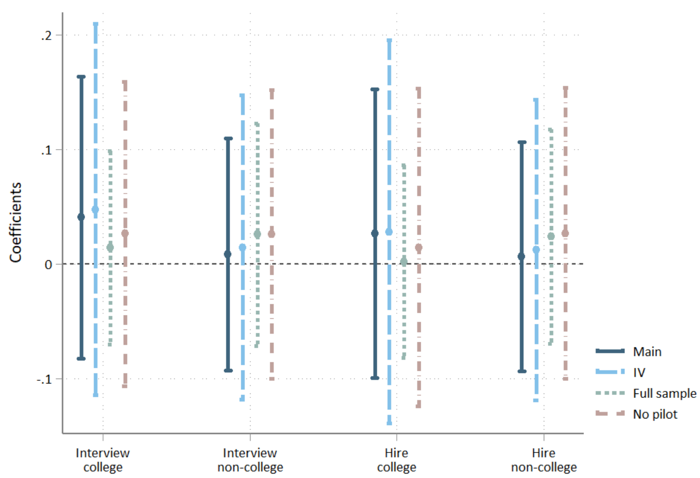
Notes: This figure replicates the main results in Table 1.4, Column (1) and (3). All regressions include a full set of baseline characteristics from Table 1.1, control for business area fixed effects, and cluster at business area level. For each dependent variable, we show (1) reduced-form estimate from 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 A.11: Replication: Effect on Hiring by Baseline Request

Panel A. Heterogeneous effect on firms requesting college graduates



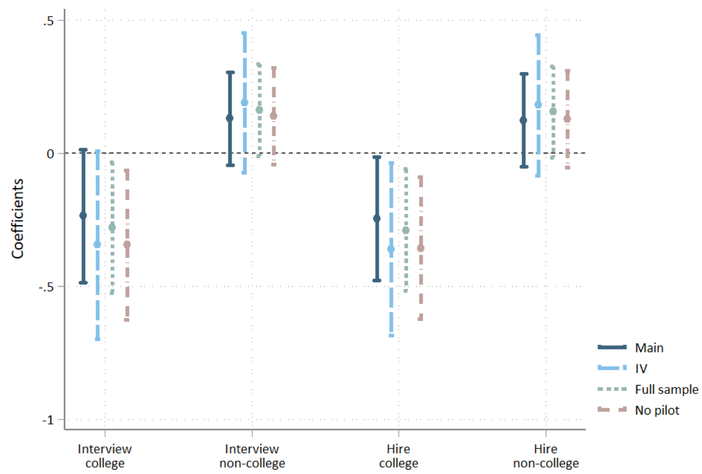
Panel B. Heterogeneous effect on firms not requesting college graduates



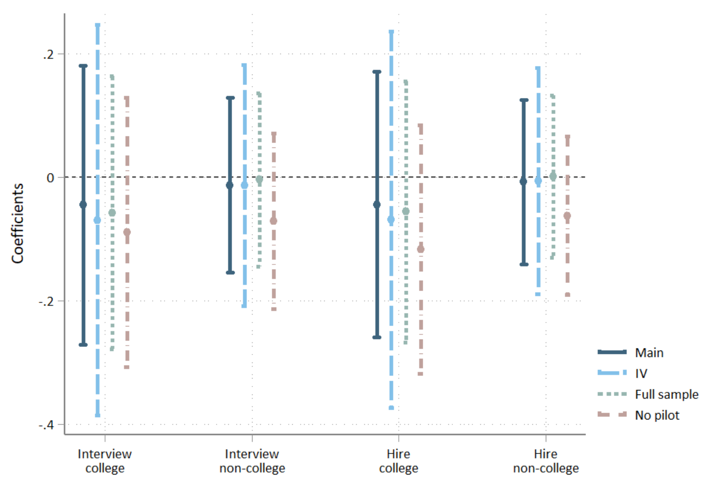
Notes: This figure replicates the main results in Table 1.5, Panel B. All regressions include a full set of baseline characteristics from Table 1.1, control for business area fixed effects, and cluster at business area level. For each dependent variable, we show (1) reduced-form estimate from the main 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 A.12: Replication: Effect on Hiring by College Share

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



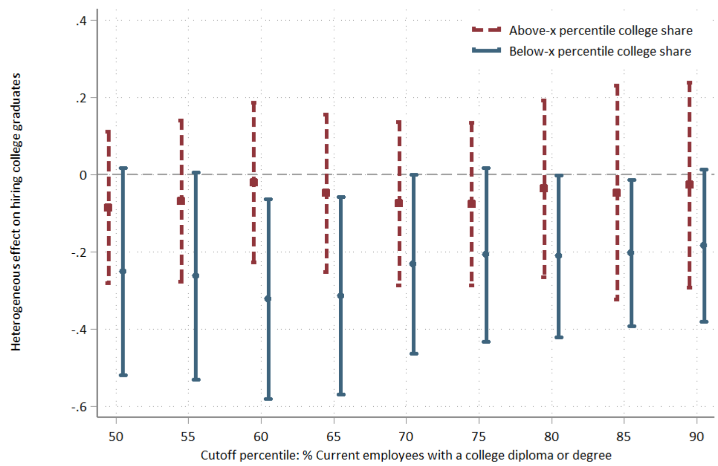
Panel B. Heterogeneous effect on firms with above-median college share



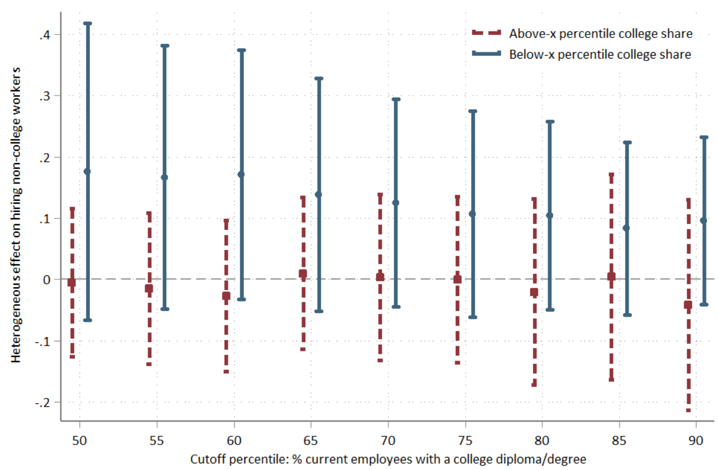
Notes: This figure replicates the main results in Table 1.6. All regressions include a full set of baseline characteristics from Table 1.1, control for business area fixed effects, and cluster at business area level. Only firms requesting college graduates at baseline are included. For each dependent 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 A.13: Heterogeneous Effect on Hiring College Graduates and Non-college Workers by College Share

Panel A. Hiring college graduates

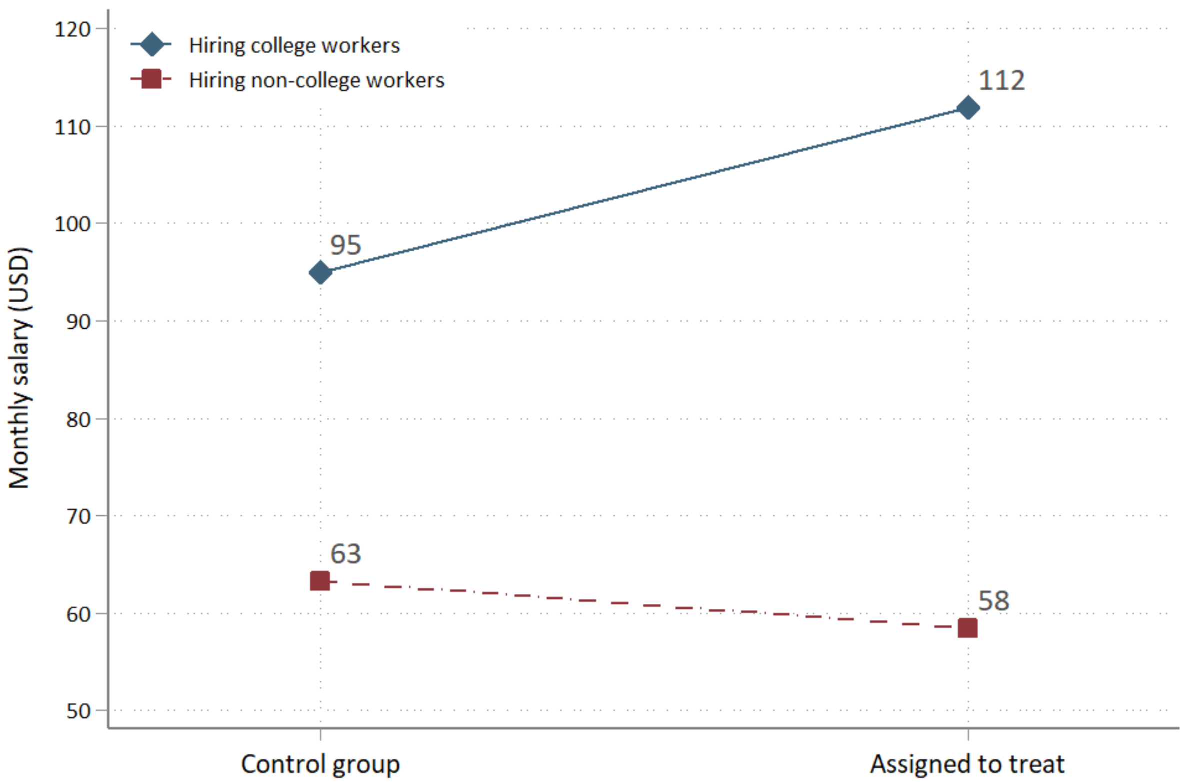


Panel B. Hiring non-college workers



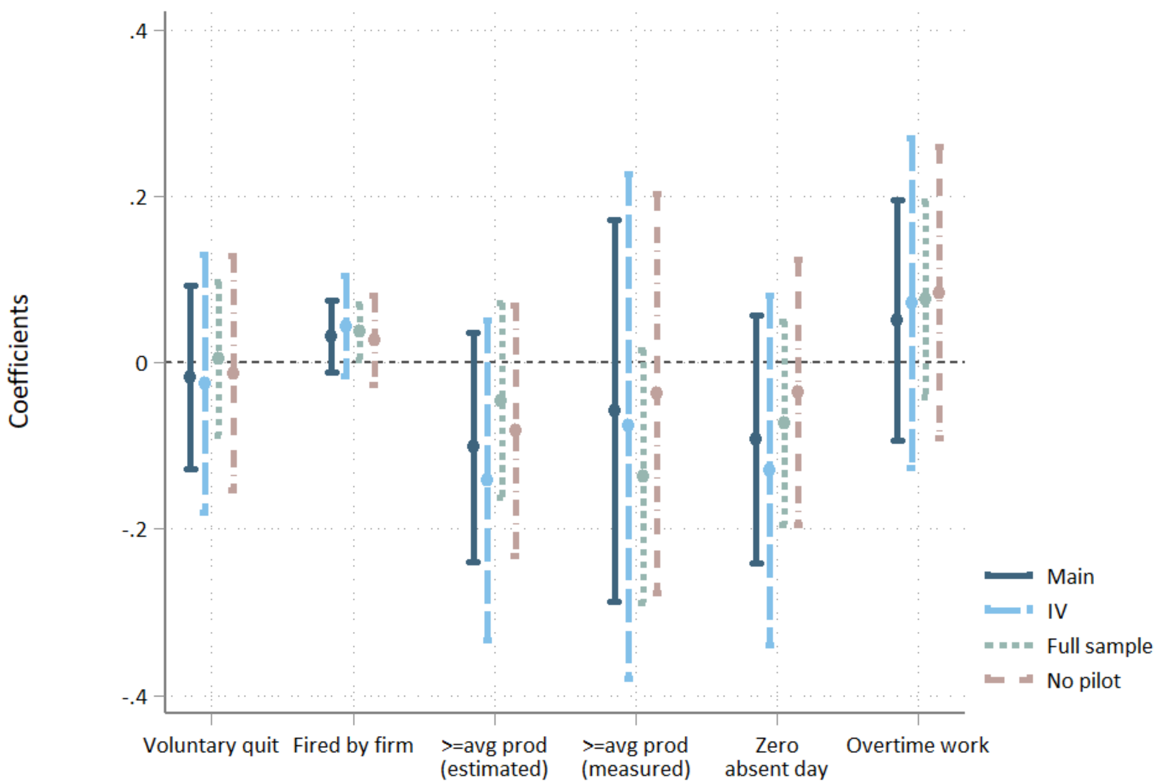
Notes: This figure shows the heterogeneous treatment effects on hiring college and non-college workers by college share, defined as the percentage of current employees with a college-level diploma or degree. We select cutoffs from 50 percentile to 90 percentile, break down the treatment effects by above-x 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 A.14: 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.

Figure A.15: Replication: Effect on Match Quality



Notes: This figure replicates the main results in Table 1.7. All regressions include a full set of baseline characteristics from Table 1.1, 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.

Appendix B

Additional Materials for Chapter 2

B.1 Data and Measurements

B.1.1 Militia Classification and Measurement

The sources used in consolidating the list of names in Table B.1 are the module of attacks experienced by the household, organizations to which the individual has participated, and organizations that ever controlled the village. The classification follows the existing qualitative research on the DRC (Marchais, 2016; Sánchez de la Sierra, 2020; Vogel, 2014; Vogel et al., 2021; Stearns, 2013; Vogel and Stearns, 2018). The two exceptions are: a. the Nyatura, a Congolese popular militia that merged in Masisi, a land predominantly inhabited by Congolese Hutu (which we classify as militia) and b. the "local defense," which are Congolese village militias that were nonetheless encouraged through the foreign-led armed group Rassemblements Congolais pour la Democratie (which we also classify as militia).

During the village chief survey, we ask the village chief and village history experts (i) what armed groups control the villages, and (ii) what armed groups control the nearby mining sites of the villages. When discussing the origin of militias, we emphasize the prevalence of militia governance in the village, less so about how militias extract revenues potentially from the mining sites. Thus, when constructing an indicator of the presence of militia village chapter, we mainly use the armed group information in the village, *i.e.*, whether a militia originated from the village is present in the village j in year t, not accounting for potential militia presence in the nearby mining sites. This variable is used in Figure 2.3, Table 2.3, Table B.11, and Table B.13. All main results remain unaffected if we account for the presence of armed groups in mining sites.

In Sánchez de la Sierra (2020), we used a similar indicator of "village militia" constructed in a similar way. Since then, while working on this paper, we further examined the information about whether the armed group originated from the village by consulting with the field team, adjusted a limited number of values which better aligned with the qualitative evidence collected during the village chief survey, and used this updated variable to construct a new indicator of the presence of militia village chapter as described above. Both measures produce very similar results in the data analysis.

In addition, in Section 2.6 where we discuss insecurity induced by the withdrawal of state forces, when constructing an indicator of the presence of the national army and FDLR, we account for the presence of armed groups in the mining sites, *i.e.*, whether national army or FDLR is present in the village j and its nearby mining sites in year t. The presence of the national army in mining sites may provide a certain level of security, while the presence of FDLR in mining sites may constitute a credible threat to the village security given its predatory nature. All main results remain unaffected if we do not account for the presence of armed groups in mining sites.

B.1.2 Household Attack Indicators

Table B.2 presents the survey questions used for reconstructing whether an individual household was previously victimized. Subscript j indicates that information comes from respondent attack module where respondents are asked about violent events in contemporary villages. The information can vary across different respondents who live in the same village in the same year, but for concise notation we do not add additional individual subscript. Subscript i indicates the action was imposed on respondent i. Subscript $f(i)$ indicates the action was imposed on respondent i's other household members, excluding respondent himself. In the next subsection, subscript o indicates the action was imposed on other households in the same village in year t.

Table B.3 presents the procedure we apply on the survey data to construct the household attacked indicator. The main definition of attack in this paper focuses on reported violent events with nonconquest motives on other household members, excluding attacks that affect the respondent only. Violent events with conquest motives mainly involve combatants during war, thus they do not capture the type of victimization that our qualitative data suggests is important. We focus on attacks that affect any member of the household.

The main explanatory variable in Equation 2.1, I_{it}^{Victim}, is constructed as an indicator for whether respondent i reported any attack on his household in the past. Subscript $j(it')$ indicates that reported attacks took place in villages where respondents lived in year t'.

Table B.1: Classification of Armed Organizations in the Sample

Name in Dataset	Classification	Comments
Alliance des Forces Démocratiques pour la Libération du Congo (AFDL)	Foreign	The AFDL was a a politico-military coalition supported by Rwanda, Uganda Burundi and Congolese dissidents, widely perceived as a foreign led
Alliance of Democratic Forces (ADF) - Nalu	Foreign	An armed group that originated in Uganda and operates in Congolese territory
Batiri	Popular Militia	Mayi-Mayi militia from Masisi driven by the Hunde
Bwende	Popular Militia	Mayi-Mayi militia
Combattants	Popular Militia	Militia from the Congolese Hutu communities
Congolese Army (Before 1996: Forces Armées Zairoises. After 2004: Forces Armées de la République Démocratique du Congo)	Congolese state	These are the Congolese Armed forces
Congolese State Agencies: Police, Intelligence Agency (Agence Nationale des Renseignement, ANR)	Congolese state	By definition
Congres National Pour la Defense du Peuple (CNDP)	Foreign	Armed group supported by Rwanda
Desertors	Ambiguous	Armed actors who deserted the Congolese army. This is recorded only in one episode of village control, in the district of Beni in 1998. We coded it as foreign-led, but this has no impact on any result.
Force vive	Popular Militia	Civil Society group
Foreigners	Foreign	This was only reported in one episode of village control, in one village of the district of Rutshuru between 2012 and 2013, and one attack in the same district in 2012. While the origin is ambiguous, given the historical context, this is likely to be the M23 (See M23).
Front de Libération du Rwanda	Foreign	The FDLR was created in 2000 bringing together multiple Rwandan Hutu militias, including the Interahamwe
Front de Libération du Rwanda - Tanganyika	Foreign	The FDLR split into various factions, Tanganyika is one of them
Hunde combattants	Popular Militia	Mayi-Mayi militia, recorded attacking two villages in 1993 in the district of Masisi. The Hunde is an "ethnic" group originating from North Kivu
Hutu combattants	Foreign	Hutu fighters, most likely FDLR otherwise would be Nyatura or Magrivi
Hutu or Magrivi	Popular Militia	Congolese Hutu militia
Hutus	Ambiguous	These are only recorded in three attack episodes, taking place in three villages of the district of Masisi in 1993 and 1996. It is likely that those in 1993 are a Congolese militia called Magrivi and that that in 1996 may be the Interahamwe (See Interahamwe)
Interahamwe	Foreign	The Interahamwe were Rwandan Hutu militia who took part in the Rwandan genocide
Katanguese	Popular Militia	Combatants from Katanga
Katanguese military	Congolese state	By definition
Katuku	Popular Militia	The Katuku are a local self-defense militia created in the 1990's in Walikale
Local defense	Popular Militia	These are decentralized, village-level militia during the Second Congo War, initiated by the RCD
M23	Foreign	Tutsi-led group armed group reportedly supported by Rwanda (March 23 mouvement)
Magrivi	Popular Militia	Congolese Hutu militia
Maimai sirimukoko d'isangi	Popular Militia	Mayi-Mayi militia
Mayi-Mayi	Popular Militia	Mayi-Mayi is a term broadly used to indicate community based popular militia
Mayi-Mayi Geremie	Popular Militia	Mayi-Mayi militia
Mayi-Mayi Janvier (Alliance patriotique pour un Congo libre et souverain, APCLS)	Popular Militia	Mayi-Mayi militia called APCLS led by Janvier Karairi
Mayi-Mayi kabuchibuchi	Popular Militia	Mayi-Mayi militia
Mayi-Mayi Kachigumka	Popular Militia	Mayi-Mayi militia
Mayi-Mayi Kaganga	Popular Militia	Mayi-Mayi militia
Mayi-Mayi Kasindiens	Popular Militia	Mayi-Mayi militia led by Vita Kambala
Mayi-Mayi Katuko	Popular Militia	Mayi-Mayi militia

Name in Dataset	Classification	Comments
Mayi-Mayi Kifuafua	Popular Militia	Mayi-Mayi militia from Masisi
Mayi-Mayi Kirikichwa	Popular Militia	Mayi-Mayi militia
Mayi-Mayi Lafontaine	Popular Militia	Mayi-Mayi militia from Lubero
Mayi-Mayi Lulwako	Popular Militia	Mayi-Mayi militia from Ituri
Mayi-Mayi Mudohu	Popular Militia	Mayi-Mayi militia
Mayi-Mayi Mze	Popular Militia	Mayi-Mayi militia
Mayi-Mayi Ngilima	Popular Militia	Mayi-Mayi militia
Mayi-Mayi Nyakiliba	Popular Militia	Mayi-Mayi militia
Mayi-Mayi Padiri	Popular Militia	Mayi-Mayi militia
Mayi-Mayi Sam	Popular Militia	Mayi-Mayi militia
Mayi-Mayi Simba	Popular Militia	Mayi-Mayi militia from ituri, under General Morgan
Mayi-Mayi Sirimukogo	Popular Militia	Mayi-Mayi militia from Isangi
Mayi-Mayi Surambaya	Popular Militia	Mayi-Mayi militia
Mayi-Mayi-KAG	Popular Militia	Mayi-Mayi militia
Mayi-Mayi-WEM	Popular Militia	Mayi-Mayi militia
Mbairwe	Popular Militia	Mayi-Mayi militia
Mbwaire	Popular Militia	Mayi-Mayi militia
Mercenaries of the AFDL which we call Banyamulenge	Foreign	See AFDL
Mongore	Popular Militia	Other name for Local Defense, encouraged by the Rassemblement Congolais Pour la Democratie but village initiated mobilization (RCD)
MONUC/MONUSCO	Congolese state	UN Mission in the DR Congo
Mouvement de Libération du Congo (MLC) - Jean Pierre Bemba	Foreign	Large armed group led by JP Bemba and supported by foreign powers
Mudundu 40	Popular Militia	Armed group formed in Bukavu and Walungu
Nduma Defense of Congo (NDC) - Sheka	Popular Militia	Armed group born in Walikale
Nyatura	Popular Militia	A local militia of Congolese Hutu
Patriotes Resistants Congolais (PARECO)	Popular Militia	Comprised of a mixture of Mai Mai and Hutu (Congolese and Rwandan)
Police d'intervention rapide	Congolese state	Rapid Intervention Police
Raia Mutomboki	Popular Militia	The Raia Mutomboki emerged in Shabunda among lega populations
Raia Mutomboki - Eyadema	Popular Militia	Largest faction of the Raia Mutomboki in 2013
Rassemblement Congolais Pour la Democratie (RCD)	Foreign	Large rebel group during the second Congo war, supported by Rwanda and Uganda
Rassemblement Congolais Pour la Democratie (RCD) - Goma	Foreign	The RCD split into a Ugandan-supported (Kisangani) and a Rwandan-supported faction (Goma)
Rassemblement Congolais Pour la Democratie (RCD) - Kisangani	Foreign	The RCD split into a Ugandan-led (Kisangani) and a Rwandan-led faction (Goma)
Rassemblement Congolais Pour la Democratie (RCD) - Mon	Foreign	The RCD split into a Ugandan-supported (Kisangani) and a Rwandan-supported faction (Goma)
Rassemblement congolais pour la Democratie-Kisangani-Mouvement de liberation (RCD-K-ML)	Foreign	Rebel movement backed by Uganda
Rondo	Popular Militia	Term used for neighborhood autodefense groups
Rwandan Army (Rwandan Patriotic Front)	Foreign	The Rwandan national army
Rwandese	Foreign	Unidentified Rwandese armed men
Thief	Other	By definition
Ugandan military	Foreign	By definition
Unidentified Congolese armed group	Popular Militia	By definition
Unidentified Rwandan armed group	Foreign	By definition
Unknown people	Unknown	By definition
Village autodefense group with no other name	Popular Militia	By definition
Villagers	Popular Militia	Term used for neighborhood autodefense groups

Table B.2: Description of Survey Questions on Individual Attacks

Variable	Survey question	Code
Module: Respondent attack history (Up to 9 attack events)		
A_{jt}	*Was there any violent event in village j where you lived in year t?*	= 1 if resp. reported a violent event in village j in year t
$Perp_{jt}$	*Who was the perpetrator?*	We classify perpetrators into militia, Raia Mutomboki, non-Raia militia, foreign-led armed group, or the Congolese national army
Mot_{jt}	*What was the attack motive?*	= whether resp. reported a violent event where the motive was pillage, sanction, or conquest
A_{ijt}	*Were you physically assaulted during the attack?*	= 1 if resp. reported a violent event where resp. was physically assaulted
$Theft_{f(i)jt}$	*Was any property of your household stolen during the attack?*	= 1 if resp. reported a violent event where any property of his household was stolen
$Chief_{jt}$	*Was the village chief assaulted during the attack?*	= 1 if resp. reported a violent event where the village chief was attacked
Module: Household information		
$A_{f(i)t}$	*For each of your household members, including yourself, list three episodes he/she was assaulted*	= 1 if any of the household members (excluding resp. himself) reported being assaulted in year t
$Viol_{f(i)t}$	*For each of your household members, including yourself, list three episodes he/she was sexually victimized*	= 1 if any of the household members (excluding resp. himself) reported being sexually victimized in year t

The fact that the main attack variable is constructed by combining information from different modules might complicate the interpretation in at least two scenarios:

- Suppose a respondent reports two violent events in the same year, both with pillage motive. The first event was perpetrated by a foreign-led armed group, the second event was perpetrated by a militia. The respondent also reports an attack on his spouse in the same year, and in reality his spouse was attacked in the second event. Our construction of main attack variable, however, would create a "false" attack on respondent's spouse by a foreign-led armed group. This scenario, however, is infrequent. In total, 873 respondents from South Kivu have reported 2,803 nonconquest violent events, and 70.2% are reported in the year when the respondent does not report any other nonconquest violent events.

- Other household members might live in a different locations than the respondent in year t. This is also infrequent: the majority of the households observed in the data are nuclear family households. Out of 1,038 households from South Kivu that have detailed rosters of current family members that live with the respondent, 71.7% of the households do not include family members other than spouse and children. If the respondent reported that his spouse or children were attacked in year t, we assume that his spouse or children were living with the respondent in the village.

Table B.3: Construction of Household Attacked Indicator

Variable	Construction	Interpretation
$AA_{f(i)jt}$	$= A_{jt} \times (Mot_{jt} \neq c) \times A_{f(i)t}$	Whether resp. i reported an attack in year t in village j with nonconquest motive, and in which year any of the household members, other than the respondent only, reported being assaulted
I_{it}^{Victim}	$= \mathbf{1}\big(\exists t' < t, AA_{f(i)j(it')t'} = 1\big)$	Whether resp. i reported an attack on household before year t (j depends on i's living history)

B.1.3 Recall Data on Wealth

Each respondent in South Kivu is asked to list yearly purchase and sales for farm animals (cows, goats, and pigs) and fields since 1990. For asset stock at birth, we ask how many cows, goats, pigs, and fields the respondent's father had when the respondent was born. We also ask about the farm animals owned at the survey year, but not fields. We adopt the following approach to construct the yearly household asset stock.

If the respondent is not married at year t, for farm animals and lands, we start from respondent's current asset stock and calculate respondent's asset stock in previous year by subtracting respondent's net purchase of asset this year from current asset stock. We calculate respondent's asset stock in each year backward up to year 1995.

If the respondent is married at year t, we calculate the asset stock backward up to the year when he was first married (89.9% of respondents who have hold marriages are only married once). Before the year respondent was first married, we start from respondent's asset stock at birth and calculate the asset stock in following years by adding net purchase of asset up to the year before respondent was first married. The reason is that a respondent that gets married may separate from his original household and start a new household.

For plots, we calculate respondent's stock of plots starting from his stock of plots at birth and adding net purchase of plots in the years that follow. We assume that when the respondent gets married, he acquires one extra plot of land.

The construction of wealth variables above does not take into account the potential effect of attack on asset stocks—they are based on asset acquisition and asset liquidation, but do not include direct measurement of houeholds' assets lost to theft. To impute the value of assets lost for a household during theft, and update our measure of the capital stock, we use the following method to account for the loss of properties during a violent event. We first calculate the average loss in farm animals across all recorded violent events, and assume that each household would lose the average amount of farm animals if their household suffers from theft. Then, during the years when respondent reports a violent event with theft on the household, we decrease the total asset by the assumed amount of loss of farm animals. We assume that violent events do not affect the stock of fields owned.

We then extract the principal component from the computed asset stock of cows, goats, and pigs, to construct our farm assets variable. The results are unchanged whether the calculation of the asset stocks account for loss of properties. For investment, we compute the principal component from the purchase of cows, goats, pigs.

For the wealth of birth, we compute the principal component from the amount of cows, goats, pigs, and fields the respondent's father had at the respondent's birth, and the number of wives of his father and whether the respondent is a relative of the village chief.

B.2 Additional Details on the Origins of the FDLR

The armed group known as the Front de Liberation du Rwanda (FDLR) is an ethnic Hutu group. In July 1994, a rebel movement took power in Rwanda, ending the genocide that had been perpetrated by government supported Hutu dominated militias, the Interahamwe, and the government forces, against the Tutsi. In response to the change of power, two million Rwandans, mostly Hutus, fled into eastern DRC, specifically North Kivu. Among them were the Interahamwe, but also former Rwandan state bureaucrats and armed forces. They formed the Armée de Libération du Rwanda (AliR), predecessor of the FDLR.

In 1996, the Rwandan government launched a military campaign that started the First Congo War (1996–97). One of the goals was to eliminate the insurgent threat coming from the Kivus. Rwandan rebel activity in eastern DRC was not defeated.

Failed negotiations between the new Congolese government and its Rwandan and Ugandan backers in 1998 plunged the DRC into the Second Congo War (1998–2004). During this war, Rwanda backed a rebel group, the Rassemblement Congolais pour la Democratie (RCD), that quickly controlled the eastern half of the country, where it took over the state apparatus and controlled the main cities, and sought to impose its authority over rural areas, where there was armed resistance. In the countryside, resistance militias had formed, which the RCD fought through counterinsurgency operations. The state had no control over the east during this period (Verweijen and Vlassenroot, 2015, Clark, 2002, Ngonzola-Ntalaja, 2002).

Instead, the Congolese government supported various armed groups and provided them with funds and ammunition to fight the RCD. Among them were the former Rwandan government forces and militia members, AliR, who in 2000 formed the FDLR. By 2004, all major armed groups, except the FDLR, vacated the east in exchange for benefits precluded in a peace agreement (Sun City peace agreement). The Congolese state struggled to regain control over the eastern provinces and the FDLR expanded their territory. The FDLR became notorious as one of the most violent groups. The Rwandan government continued to support armed groups who fought against the FDLR, while the Congolese state alternatively tolerated or actively supplied the FDLR.

B.3 Social Desirability Bias in Reporting Participation

The survey protocols were designed to minimise involuntary omissions, but voluntary omissions can occur on a sensitive topic like participation. Indeed, the measurement of participation through self-reports can be subject to bias arising from the respondent's perceived risks in disclosing past, or present, participation to an unknown researcher. Specifically, respondents can choose not to disclose participation in armed groups generally because they fear it might expose them to risks, such as retaliation or arrest. More concerning to our analysis is that respondents may omit participation in *specific* groups. This is a real concern, as our survey protocols encouraged respondents not to disclose participation if they felt it could expose them to such risks, to protect the safety of respondents and researchers and to ensure the study respected research ethics.

Qualitative fieldwork and existing literature suggest that respondents can be less likely to report participation in groups that have behaved violently or badly with the population, as well as less socially accepted group. In our sample, this is likely to be the case for foreign groups, which, as we show in this paper, behave more violently and are less supported.

Given the anonymity inherent to the aggregate measures, we collected information on participation in militias and foreign armed groups through an alternative channel for comparison. Specifically, we obtained the total number of individuals, for each village control episode, which participated in the corresponding group. Contrary to the household reports, this measure is anonymized, hence protects the reports against any sort of social desirability bias that may arise from respondents fearing about individual consequences of reporting participation. Its average can thus be expected to be a more unbiased estimator of participation numbers (even as it may have larger classical measurement error due to recall). We can then compare those to the subset of individual reports that arise from participation in armed groups governing the village.

To examine this possibility, Figure B.2 compares, back to back, the individual reports of participation into militia, and into foreign armed group, to the aggregate reports that we collected from the village chief survey. This analysis has three take-aways.

First, the estimated number of participants in any armed group, obtained through village chief survey aggregate reports is comparable to that estimated based on the individual reports in household surveys. Contrary to what individual under-reporting in the household survey would suggest, we find that the estimated numbers are even slightly higher than those estimated through village anonymized aggregates. This provides confidence that households do not under-report participation in armed groups on average.

Second, disaggregating this analysis by type of armed group, we find that the estimated numbers of foreign armed group participation through respondent reports is somewhat smaller than those estimated through village-level aggregates. This is consistent with respondents potentially under-reporting participation in foreign armed groups. It could also indicate, instead, that individuals who have participated in foreign armed groups may be less likely to have returned to their village.

A number of reasons could explain this conjecture: they may fear to be ostracized, they may be more likely to die in combat, or they may be more likely to be actively fighting in other areas — all of which are weaker concerns for militias. Whatever source of bias may explain this lower estimated numbers based on the household survey, it suggests that the estimates of average participation in foreign armed groups constructed based on individual reports collected through the household survey may be biased downwards.

Third, disaggregating the analysis by type of armed group, we find that the estimated number of participants into militia, as estimated using the household survey self-reports, is considerably larger than that estimated using the village aggregates. This provides confidence that respondents do not feel compelled to hide their participation history in the survey (and that survivor bias is unlikely to be a concern for this analysis).

This finding is also consistent with a wealth of qualitative evidence we have amassed, which shows. In many cases in the Congolese war, militia replaced the state, and participating in militia was tagged with the same patriotic connotations as those of participating in the army. In general, participating in militia is a normal occurrence in rural life in this region, and is socially accepted in our experience talking to hundreds of fighters, their friends, their families, their village authorities, many of which themselves proudly belonged to some of these militias.

Individuals spoke to us very openly about having participated in militia, and about who else had participated, and we obtained referrals to other militia members. Even if they at times fight the state, they often collaborated with the state, and were even armed and logistically supported by the government during the First and Second Congo wars.

B.4 Potential Selection from Migration

In what follows, we consider the biases that may ensue if individuals who were previously in the village have left the village, and those that may ensue if individuals who are today in the village come from other areas. These patterns of selection can arise from death, migration, or active involvement in armed groups in other areas. They can threaten the validity of our main coefficient if, for instance, individuals who are more likely to have migrated out of the sample are also more (or less) likely to have been attacked, and also to have participated in armed groups. We refer to all of these sample selection issues as migration in what follows. Migration can affect external validity of our results if the selection of households present today is not representative of those who were present in the past. In that case, we estimate the effect for a population subset.

We first analyze whether villagers who migrated are systematically different. Table B.23 compares individual-year observations where villagers moved to a new village in year t versus those where villagers stay in the same village. In total there are 1,389 migration episodes.

Notably, migrants are not more likely to have reported a violent attack on household members in the past, suggesting respondents in the core sample do not migrate because of past victimization experience. Migrants are also less likely to have participated in militia village chapter, against the hypothesis that respondents migrate to avoid being targeted as an ex-combatant. Regarding other demographics, migrants tend to be younger, less likely to have married, more likely to be unemployed before moving, and more educated. They do not differ in the father's wealth index, number of plot, or farm animal index, although they invest more in plots and farm animals after they migrate.

Table B.24, Panel A examines the past migration history for participants in militia chapters, compared to other contemporary non-participants living in the same village, the same chiefdom, and the same territory, respectively. On average, 58% participants in militia village chapters have any migration episodes in the past, slightly higher than that of non-participants living in the same village, albeit not significantly. Participants in militia village chapters are also more likely to have migrated to out-of-sample villages in the past. This potentially constitutes a selection bias when estimating the treatment effects of state vacuums because we are not able to observe the state vacuums in out-of-sample villages. In the following sections, we formalize this type of selection bias, and provide several tests to address this concern. Reassuringly, we do not find that participants in militia chapters are more likely to migrate in the same year compared to non-participants, suggesting the decision to participate would not induce selection bias due to migration.

We now introduce a formal model to discuss migration as a potential source of selection bias. Suppose at t_0 we have a representative sample from the villages we interview ("sample villages"), and we want to estimate the treatment effect of past attack on villagers from the sample villages. After a period Δt, however, some villagers emigrate to an out-of-sample village (In-Out migration), and some villagers migrate into a sample village (Out-In migration). A random draw from the sample villages in $t_0 + \Delta t$ will not be representative of villagers from the villages we interview at time t_0. Notice that some villagers migrate within sample villages, but this does not cause the selection bias because they do not alter the composition of villagers from the survey villages. We formalize the selection bias due to migration as follows, assuming villagers within or outside of sample villages have the same tendency to migrate :

1. Suppose sample villages (Group A) constitute proportion $a \in [0, 1]$ of the East Congo population;

2. Within sample villages, proportion $1 - \pi$ of the villagers will never migrate outside (stayers, A_s). Proportion π of the villagers will migrate to out-of-sample villages at least once throughout the period (Emigrants, A_m) with probability p;

3. Within out-of-sample villages (Group B), proportion $1-\pi$ of the villagers will never migrate outside (stayers, B_0). Proportion π of the villagers will migrate to sample villages at least once throughout the period (immigrants, B_m) with probability p.

Table B.25, Panel A replicates Table 2.3, Panel B with only respondents who never migrated outside of the sample villages. Results remain mostly unchanged. Among the stayers, the state vacuum induced by the Regimentation policy seems less likely to lead to forced participation, although the result is only borderline significant and not robust to other specifications. Table B.25, Panel B include the entire core sample, interacting the state vacuum indicators with whether the respondent is an immigrant from outside of the sample villages. Results remain largely unchanged. Notice that because we can only measure state vacuums for the sample villages, our main regressions leave out observations where respondents resided outside of the sample villages, and thus we are not able to include emigrants in the regressions.

We further provide a counterfactual analysis based on our previously detailed migration framework. Assume the real treatment effect of each group is $T(X)$, and state vacuums do not change the composition of different subgroups (*i.e.*, parameters a and π are unaffected). The average treatment effect on the villagers from the core sample (A) can thus be written as follows:

$$ATE(A) = (1-\pi)T(A_0) + \pi T(A_m),$$

where A_0 is the subset of stayers, A_m is the subset of emigrants outside of the sample villages. We are not able to observe $T(A_m)$ in the data because we do not observe whether there is a state vacuum induced by policies in villages outside of the sample.

The actual estimate of the treatment effect can be written in the following two ways:

$$\begin{aligned}\widehat{ATE(A)} &= \frac{a(1-\pi)}{a(1-\pi)+(1-a)p\pi}T(A_0) + \frac{(1-a)p\pi}{a(1-\pi)+(1-a)p\pi}T(B_m) \\ &= \frac{1}{1+(\frac{1-a}{a}p-1)\pi}ATE(A) + \frac{(1-a)p\pi T(B_m) - a\pi T(A_m)}{a(1-\pi)+(1-a)p\pi}, \end{aligned} \tag{B.1}$$

where B_m is the subset of immigrants from outside of the sample villages. Assuming that immigrants in our sample are representative of the entire set of immigrants from outside of the sample villages, we are able to observe B_m and provide an unbiased estimate of $T(B_m)$, *i.e.*, the coefficients for the interactions between state vacuum indicators and the immigrant status in Table B.25, Panel B.

We can now describe our counterfactual exercise. With proper assumptions of $T(A_m)$ and calibrations of parameters a, π, and p, we can calculate the counterfactual values $T^{ct}(B_m)$, such that we can generate the same estimate $\widehat{ATE(A)}$ even if the underlying $ATE(A)$ equals zero. Then, we can compare the counterfactual values $T^{ct}(B_m)$ to the actual estimates $\widehat{T(B_M)}$. If the calculated p-value to reject $T^{ct}(B_m) = \widehat{T(B_M)}$ is sufficiently small, it is unlikely for one to generate the same estimate $\widehat{ATE(A)}$ with only assumptions on the migrants and without a real effect on the general population in the sample villages.

We calibrate the key parameters as follows. (1) Migration likelihood for migrants (p): On average, each migrant is observed for 16 years in the sample, and moves on average twice. We calibrate $p = 1/8 = 0.125$. (2) Proportion of villagers in the sample villages (a): According to village chief survey, on average, there are 427 villagers in a sample village in South Kivu. Consider the total population in South Kivu in 2015 to be 5,772,000, and apply the average number of villagers to all 133 villages in South Kivu, we calibrate $a = 0.98\%$. (3) Proportion of villagers who migrate at least once throughout the observation period (π): Out of 1,041 respondents, 588 have never migrated outside of the sample once. We calibrate $\pi = 1 - 588/1041 = 44\%$.For the treatment effect on emigrants $T(A_m)$, we assume it to be within the range $[-10, 10]$, $[-20, 20]$, and $[-30, 30]$ percentage points, respectively. These are relatively extreme assumptions because the largest magnitude we have seen in the main analysis is no more than 31.6 percentage points (Table 2.6, Column 4). We report the maximum p-values amongst different assumptions of $T(A_m)$.

Table B.26 presents the counterfactual analysis. Even with extreme assumptions on emigrants, it is unlikely to generate the large effect of the state vacuum during Regimentation on general participation in militia village chapter. The results on participation to protect the community and for private motivations are also unaffected by the extreme assumptions on emigrants, although results on other motives might be somewhat subject to different assumptions of migration.

B.5 Additional Tables and Figures

Table B.4: Description of Participants' Concurrent Occupations

	Participants			
	Militia from Village			Militia from
	All	Raia	Non-Raia	Outside
# Participants	245	134	111	30
One year before joining ($t-1$)				
Employed	0.69	0.71	0.68	0.47
In Mining Sector	0.16	0.18	0.12	0.13
In Agricultural Sector	0.42	0.38	0.47	0.30***
As a Civil Servant	0.12	0.15	0.08	0.03***
The year when joining (t)				
Employed	0.71	0.70	0.72	0.38
In Mining Sector	0.17	0.22	0.10	0.10
In Agricultural Sector	0.41	0.34	0.50	0.24
As a Civil Servant	0.13	0.14	0.12	0.03
One year after joining ($t+1$)				
Employed	0.72	0.72	0.73***	0.32***
In Mining Sector	0.16	0.22	0.09	0.08
In Agricultural Sector	0.41	0.34	0.51	0.20
As a Civil Servant	0.14	0.15	0.13	0.04***
Two years after joining ($t+2$)				
Employed	0.75	0.89	0.74	0.43
In Mining Sector	0.11	0.56	0.07	0.07***
In Agricultural Sector	0.53	0.22	0.56***	0.32
As a Civil Servant	0.11	0.11	0.11***	0.04***

Notes: We report the occupations for participants around the time of participating in the militia village chapter, using the core sample from South Kivu. *Militia from Village* reports the sample of individuals who joined a militia chapter formed in the village of survey. *Raia* and *Non-Raia* report the sample of individuals who joined in Raia and other militia chapter formed in the village of survey that is not Raia, respectively. *Militia from Outside* reports the sample of individuals who joined a militia chapter formed outside of the survey village. We indicate the difference compared to Column 2 (P-value: *** 0.01, ** 0.05, * 0.10), computed after including year FE, and clustered two-way at the individual respondent and the village*year level.

Table B.5: Quantifying the Security Provided by Militias

	(1)	(2)	(3)	(4)	(5)	(6)
	Violent attack on				Sexual violence on	
	Village	Village	Household	Household	Household	Household
Presence of Militia Village Chapter	0.01		-0.07		-0.26**	
	(0.03)		(0.65)		(0.12)	
Participation in Militia Village Chapter		-0.00		-5.73**		-1.39**
		(0.00)		(2.26)		(0.64)
Observations	53,136	53,124	54,534	70,052	54,534	70,052
R-squared	0.16	1.00	0.12	0.28	0.10	0.19
Control mean	0.09	0.09	2.46	2.39	0.24	0.24

Notes: We estimate $V_{ijt} = \alpha + \gamma I_{ijt}^{Militia} + \alpha_i + \alpha_j + \alpha_t + +\mathbf{X}_{ijt}'\Gamma + \epsilon_{ijt}$, where $I_j^{Militia}$ is one of the following indicators: (i) whether there is a militia village chapter present in the village j in year t (Column 1, 3, 5), and (ii) whether individual i participates in militia village chapter in village j in year t (Column 2, 4, 6). We use both the core sample from South Kivu and extra village sample from North Kivu for estimation. The dependent variables are (in decimal digits): Column 1–2, whether the village experienced a violent attack; Column 3–4, whether a household member was violently attacked; Column 5–6, whether a household member experienced sexual violence. Columns 1, 3, and 5 control for individual fixed effects, village fixed effects, year fixed effects, and whether an armed group is stationed in the village. Columns 2, 4, and 6 control for individual fixed effects, village-year fixed effects, and whether the respondent participates in any armed group in general. All regressions include respondent, village, and year fixed effects (P-value: *** 0.01, ** 0.05, * 0.10).

Table B.6: Description of Participants Compared to Non-Participants

	Participants				Non-participants		
	Militia from Village			Militia from	Living in the Same:		
	All	Raia	Non-Raia	Outside	Village	Chiefdom	Territory
# Participants/Individual-Year Obs.	245	134	111	30	899	13824	14947
A: Conflict Background							
Past Victimization by Foreign Armed Group	0.24	0.25	0.23***	0.07	0.09***	0.07***	0.07***
By Congolese Militia	0.03	0.03	0.03*	0.03	0.03	0.02	0.02
Past participation in Militia Village Chapter	0.14	0.26	0.00***	0.03***	0.07***	0.04***	0.04***
In Raia Mutomboki or Mayi-Mayi	0.13	0.24	0.00***	0.00***	0.04***	0.01***	0.01***
In Militia Formed Outside village	0.02	0.03	0.01*	0.03	0.01	0.01	0.01
B: Demographic Characteristics							
In the Family of the Village Chief	0.11	0.10	0.11	0.23***	0.11	0.10	0.10
Age in year t	31.19	33.75	28.10	21.00***	27.04	26.49	26.48
Married in year t	0.14	0.02	0.29***	0.23	0.20**	0.34***	0.35***
C: Productive Capacity in Nonviolent Sector							
Employed in year t-1	0.69	0.71	0.68	0.47	0.56	0.55	0.55
In Mining Sector in year t-1	0.16	0.18	0.12	0.13	0.11	0.10	0.11
In Agricultural Sector in year t-1	0.42	0.38	0.47	0.30***	0.36	0.40*	0.39
As a Civil Servant in year t-1	0.12	0.15	0.08	0.03***	0.09	0.05**	0.06**
Father's Wealth Index	0.16	0.14	0.19	-0.34***	-0.11***	-0.19***	-0.20***
# Plots Owned in year t-1	0.89	0.94	0.82	0.40***	0.55	0.46	0.46*
Farm Animal Index in year t-1	0.27	0.32	0.22	-0.07***	0.02	0.04**	0.04*
Primary Education Complete	0.58	0.58	0.57***	0.63***	0.53	0.50	0.50
Secondary Education Complete	0.20	0.25	0.15	0.13***	0.16	0.14	0.14
D: Average Increase in Future Assets							
# Plots Owned	0.14	0.01	0.30	0.38	0.19*	0.19	0.19
Farm Animal Index	0.15	0.01	0.31	0.40	0.22	0.24	0.23

Notes: We report the descriptives of participants using the core sample from South Kivu. *Militia from Village* reports the sample of individuals who joined a militia chapter formed in the village of survey. *Raia* and *Non-Raia* report the sample of individuals who joined in Raia and other militia chapter formed in the village of survey that is not Raia, respectively. *Militia from Outside* reports the sample of individuals who joined a militia chapter formed outside of the survey village. We compare participants in militia chapters versus those where respondents do not participate in any militia chapter contemporarily (living in the same village, same chiefdom, or the same territory). We indicate the difference compared to Column 2 (P-value: *** 0.01, ** 0.05, * 0.10), computed after including year FE, and clustered two-way at the individual respondent and the village*year level. For father's wealth index, we use whether respondents reported their fathers are rich, stock of plots at birth, and number of father's wives. For the farm animal index, we use stock of cows, goats, and pigs. In Panel D, we calculate the mean of asset stock after year t and subtract from the asset stock in current year t.

Table B.7: Economic Incentives as Benchmark Using Price Shocks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Participation						Occupation			
	Militia Village	Militia Village	Militia Village	Militia Outside	Foreign g.	Army	Ag	Mining	Govt	Unemployed
Victimization by foreign g.	3.15**		6.20***	0.71	-0.76	-0.12	-1.86	0.27	4.96*	1.97
	(1.31)		(1.87)	(0.72)	(0.59)	(0.22)	(3.07)	(2.70)	(2.80)	(2.62)
Gold_j x Local Price_t		-0.30***	-0.30***	0.04*	-0.01	0.03	-0.17	0.40***	-0.07	0.06
		(0.10)	(0.10)	(0.03)	(0.01)	(0.03)	(0.13)	(0.13)	(0.07)	(0.13)
Observations	17,576	15,034	15,034	15,034	15,034	15,034	13,829	13,829	13,829	13,829
R-squared	0.59	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Village FE	N	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	N	Y	Y	Y	Y	Y	Y	Y	Y	Y
Village-Year FE	Y	N	N	N	N	N	N	N	N	N
Cluster at Individual	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cluster at Village-Year	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control mean	2.33	3.71	3.18	0.90	0.10	0.17	45.10	9.45	5.42	26.01

Notes: We estimate $Part_{ijt} = \alpha + \gamma I_{it}^{Victim} + \gamma^E I_j^m \times P_t^m + \alpha_i + \alpha_j + \alpha_t + \alpha_a + \mathbf{X}'_{it}\Gamma + \epsilon_{ijt}$, where I_j^m is an indicator taking value 1 for all years if village j has mineral m deposits and P_t^m is the local price of gold m in year t. We use the core sample for the estimation. I_{it}^{Victim} is an indicator taking value 1 if respondent i reports an attack on the household before year t. We instrument local gold price with world gold price. We include individual, village, year, and age fixed effects and standard errors are clustered two-ways at the individual and the village-year level. Column 1 includes village-year fixed effects. The dependent variables are (in decimal digits): (a) indicators for whether the respondent participates in any armed group, a militia formed in the village, militia formed outside the village, a foreign-led armed group, or Congolese army, respectively, in a given year, and (b) indicators for whether the respondent works in agriculture, mining sector, government office, or unemployed. P-value: *** 0.01, ** 0.05, * 0.10.

Table B.8: Benchmarking Exercise: The Price of Victimization Motives

	Participation in Militia Formed in the Village (%)
Control mean:	
Never experienced an attack on household by foreign group	2.33
Effect of experienced shock:	
An attack on household by foreign group before year t	3.15** (1.31)
Local gold price increased in year t by \$1 per g.	
(Use world gold price as an IV)	-0.30** (0.10)
⇒ One past attack on household by foreign group has	
an equivalent effect of an decrease in local gold price by:	\$10.5 per g.
Daily production of gold miner (Geenen 2013)	1 g
Total number of work days per year (Assumed)	300 d
Tax by local authority (Own data)	45%
⇒ Decrease in yearly income by:	\$1,733
GDP per capita in 2005 in DRC (World Bank)	\$218
⇒ Decrease in yearly income as in GDP per capita:	7.9 times

Notes: We compare the effect of past foreign-led armed group attack on household (Table, B.7, Column 1) to the effect of gold price shock on participation in a militia form in the village (Table B.7, Column 2). Control mean is computed among observations where respondents never experienced an attack by foreign armed groups on household before year t. P-value: *** 0.01, ** 0.05, * 0.10. One foreign-led armed group attack on the household requires an increase in US\$10.5 in the local price per gram of gold to be undone, in gold villages, equivalent to 1.27 standard deviations, and an increase in 45% of the local price of gold. Second, we estimate the equivalent rise in per capita income outside the armed groups that would be necessary to undo the effect of household victimization by foreign armed groups. We use information on the daily production of gold by a gold miner (Geenen, 2013), our data on gold taxation by local authorities, the GDP per capita of the DRC in that period (in year 2005), and assume miners work 300 days a year. We find that it would take a *permanent* increase in 8 times the yearly per capita income to undo the magnitude of the effect of *one* foreign-led armed group attack. This estimate is based on the assumption that a miner works 300 days a year, and is naturally sensitive to this assumption. To provide further confidence in the economic significance of the role of past victimization, we also calculate a lower bound of this effect, based on a miner working 50 days per year, a generally unlikely low number of years for a miner. In that case, the impact of one foreign-led violent attack on a household member induces an increase in the probability of participation that would require an increase in 1.3 the yearly GDP per capita to undo. The share of gold value that armed groups can tax is small (Sánchez de la Sierra, 2020), thus, in gold mining villages, the world price of gold passes through down to miners' net income, but has a weak effect on the revenues armed groups can hope to tax. There is also coltan. Coltan is bulky, and thus prone to taxation by armed groups. As a result, the price of coltan does not offer a useful benchmark.

Table B.9: Replication: Communities Supported the Militias

	Militia from Village			Militia from		
	All	Raia	Non Raia	Outside	Foreign	Army
A. Protection of the Community: # Episodes	134	39	95	134	248	189
Population Perceived Chapter's Security as Effective	0.72	0.95	0.63***	0.29***	0.40***	0.83*
A Chapter Member Attacked Villagers	0.31	0.13	0.39***	0.71***	0.74***	0.13
B. Support from the Community: # Episodes	134	39	95	134	248	189
Some Villagers Opposed the Chapter	0.17	0.05	0.21**	0.24***	0.29***	0.06
Parents Encouraged Their Children to Join the Chapter	0.42	0.63	0.37**	0.16***	0.18***	0.20***
Chief Encouraged the Youth to Join the Chapter	0.47	0.64	0.43**	0.14***	0.19***	0.23***
Chief or Relative is the Chapter's Leader	0.41	0.62	0.32***	0.00***	0.01***	0.01***
Chief was Forced to Support the Chapter	0.26	0.08	0.33***	0.55***	0.72***	0.10
C. Members' Motivations: # Participants	364	243	121	51	39	17
Social Motivations, Intrinsic (Social Emotions)	*0.66*	*0.64*	*0.70*	*0.43****	*0.25*	*0.00*
For Revenge	0.13	0.14	0.10	0.07	0.00	0.00
For Community Protection	0.54	0.50	0.61**	0.36	0.25	0.00
Social Motivations, Extrinsic (Social Incentives)	*0.20*	*0.21*	*0.17*	*0.21*	*0.75****	*0.00*
For Status	0.04	0.06	0.01**	0.04	0.50***	0.00
Social Pressure	0.13	0.15	0.09*	0.07	0.25	0.00
Social Coercion	0.02	0.00	0.07***	0.14***	0.00	0.00
Private Motivations	*0.14*	*0.14*	*0.13*	*0.36****	*0.00*	*1.00***
For Money	0.03	0.04	0.03	0.29***	0.00	1.00***
For Private Protection	0.10	0.11	0.10	0.07	0.00	0.00

Notes: This table replicates Table 2.1 by including extra village sample from North Kivu. *Militia from Village* reports the sample of individuals who joined a militia chapter formed in the village of survey. *Raia* and *Non-Raia* report the sample of individuals who joined in Raia and other militia chapter formed in the village of survey that is not Raia, respectively. *Militia from Outside* reports the sample of individuals who joined a militia chapter formed outside of the survey village. *Foreign* reports the sample of individuals who joined a foreign armed group. *Army* reports the sample of individuals who joined Congolese national army. For motives, we classify all the answers into the seven groups: Revenge (to avenge; following an incident with family or community), to protect the community, status (to become a military; to be feared), social pressure (social pressure; convinced by family, villager, or other civilian; everybody participated), social coercion, for money (for financial advantage; there is no other opportunities), private protection (private protection; to find refuge; to protect own goods). Units for the number of observations are reported in the panel headers. We indicate the significance of differences compared to Column 2 with stars at 1, 5, or 10% significance levels (*, **, *** respectively).

Table B.10: Replication: The Victims are More Likely to Join Militia Village Chapters

	Participants				Non-participants		
	Militia from Village			Militia from	Living in the Same:		
	All	Raia	Non-Raia	Outside	Village	Chiefdom	Territory
# Participants/Indiv-Year Obs.	364	243	121	51	7351	49927	58207
Past Victimization	*0.25*	*0.26*	*0.23*	*0.04***	*0.04**	*0.05*	*0.05*
By Foreign armed group	0.24	0.25	0.23	0.04**	0.03**	0.04***	0.03***
By Congolese Militia	0.03	0.03	0.03*	0.02	0.01	0.01**	0.01*
Past Participation	*0.16*	*0.20*	*0.08****	*0.10****	*0.03****	*0.03****	*0.03****
In Militia Village Chapter	0.10	0.15	0.00***	0.02***	0.01***	0.01***	0.01***
In Raia Mutomboki or Mayi-Mayi	0.09	0.14	0.00***	0.00***	0.01***	0.00***	0.00***
In Militia Formed Outside Village	0.01	0.02	0.01	0.04	0.00	0.00	0.00

Notes: This table replicates Table 2.2, Panel B, by including extra village sample from North Kivu. *Militia from Village* reports the sample of individuals who joined a militia chapter formed in the village of survey. *Raia* and *Non-Raia* report the sample of individuals who joined in Raia and other militia chapter formed in the village of survey that is not Raia, respectively. *Militia from Outside* reports the sample of individuals who joined a militia chapter formed outside of the survey village. We compare participants in militia chapters versus those where respondents do not participate in any militia chapter contemporarily (living in the same village, same chiefdom, or the same territory). We indicate the significance of differences compared to Column 2 with stars at 1, 5, or 10% significance levels (*, **, *** respectively). When calculating differences, we include year fixed effects, control for all variables in Table B.6, Panels B–D, and cluster at two-way at the individual respondent and the village*year level.

Table B.11: Replication: The State Vacuum Caused the Rise and Growth of the Militia

	(1)	(2)	(3)	(4)	(5)
	Presence in the Village			Active Combatants	
	National Army	Militia Village Chapter		Militia Village Chapter	
	Stock	Stock	Inflow	Inflow	Individual
Vacuum 1 [Sun City Peace]	-10.28**	10.88**	0.19	6.40**	2.19***
	(4.67)	(5.41)	(2.22)	(2.47)	(0.81)
Vacuum 2 [Regimentation]	-15.59***	27.15***	34.25***	34.20***	16.42***
	(5.71)	(4.21)	(3.37)	(4.02)	(2.53)
Observations	4,505	4,505	4,505	4,398	54,558
R-squared	0.48	0.29	0.16	0.21	0.17
Village FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Individual FE					Y
Clustered at Individual-level					Y
Pre-Vacuum 1 Shabunda mean	0.00	41.30	4.35	0.00	0.00
Pre-Vacuum 2 Shabunda mean	76.09	0.00	0.00	0.00	0.00
P-value: Vacuum 1, Clustered at:					
Village	0.03	0.05	0.93	0.01	0.01
Village & Chiefdom-post Vacuum	0.01	0.00	0.86	0.00	0.00
Village & Chiefdom-year	0.08	0.08	0.93	0.14	0.12
P-value: Vacuum 2, Clustered at:					
Village	0.01	0.00	0.00	0.00	0.00
Village & Chiefdom-post Vacuum	0.00	0.00	0.00	0.00	0.00
Village & Chiefdom-year	0.23	0.11	0.02	0.01	0.07

Notes: This table replicates Table 2.3 by including extra village sample from North Kivu. It presents the estimates of Equation 2.2, where the dependent variables are (in decimal digits): an indicator for whether there is presence of the Congolese national army in the village, for whether there is a militia village chapter (stock), for whether there is a new militia chapter (inflow), for whether there is new militia village chapter combatants in the village (inflow), and for whether the respondent of the household survey was participating in a militia village chapter at that year, in Columns 1–5 respectively. The latter is estimated at the level of the individual respondent * year (n=15,106), while the former are from the village * year dataset (and thus indexed by jt rather than ijt). Column 1–4 control for village fixed effects and year fixed effects; standard errors are clustered at the village-level. Column 5 controls for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the village-level and individual-level. Table notes below the regression coefficients report p-values calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level.

Table B.12: Replication: The Rise of the Raia is Driven by Extrinsic and Intrinsic Social Motivations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Social Motivations					Private Motivations	
		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	0.52	0.16	-0.23	0.06	0.17*	0.10	0.05	0.23*
	(0.42)	(0.11)	(0.26)	(0.04)	(0.09)	(0.11)	(0.04)	(0.13)
Vacuum 2 [Regimentation]	12.74***	1.91***	6.25***	0.67***	2.09***	-0.00	0.53***	1.38***
	(2.03)	(0.36)	(1.14)	(0.19)	(0.63)	(0.01)	(0.18)	(0.34)
Observations	29,035	29,035	29,035	29,035	29,035	29,035	29,035	29,035
R-squared	0.18	0.08	0.13	0.11	0.08	0.06	0.06	0.07
Village FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y
Cluster at Individual-level	Y	Y	Y	Y	Y	Y	Y	Y
Pre-Vacuum 1 Shabunda mean	0.25	0.12	0.00	0.00	0.00	0.12	0.00	0.00
Pre-Vacuum 2 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 1, Clustered at:								
Resp. & Village	0.22	0.18	0.38	0.16	0.06	0.35	0.29	0.09
Resp. & Village & Chiefdom-post Vacuum	0.09	0.00	0.28	0.00	0.01	0.09	0.02	0.00
Resp. & Village & Chiefdom-year	0.20	0.11	0.42	0.01	0.00	0.21	0.03	0.02
P-value: Vacuum 2, Clustered at:								
Resp. & Village	0.00	0.00	0.00	0.00	0.00	0.85	0.00	0.00
Resp. & Village & Chiefdom-post Vacuum	0.00	0.00	0.00	0.00	0.00	0.47	0.00	0.00
Resp. & Village & Chiefdom-year	0.00	0.00	0.00	0.01	0.00	0.00	0.01	0.00

Notes: This table replicates Table 2.3, Panel B, by including extra household sample from South Kivu. It presents the estimates of Equation 2.2, where the dependent variables are (in decimal digits): an indicator for whether the individual joins a militia village chapter (Column 1), for whether they joined it motivated by intrinsic social emotions (Columns 2–3), by extrinsic social incentives (Columns 4–6), or by private motivations (Column 7–8). All regressions control for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the level of village-level and individual-level. Table notes below the regression coefficients report p-values calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level.

Table B.13: Why the Predecessor Vacuum Caused the Raia Predecessor Only in Shabunda

	(1)	(2)	(3)	(4)	(5)
	Presence in the Village			Active Combatants	
	National Army	Militia Village Chapter		Militia Village Chapter	
	Stock	Stock	Inflow	Inflow	Individual
Vacuum 1 [Sun City Peace]	-9.50*	1.79	-3.35	-1.36	0.75
	(5.11)	(6.23)	(2.72)	(2.37)	(0.80)
Vacuum 1 X Stock of victimization	0.10	8.11**	2.64	8.67***	9.28***
	(3.02)	(3.56)	(1.84)	(1.72)	(2.58)
Vacuum 2 [Regimentation]	-31.56***	30.59***	34.25***	33.43***	16.96***
	(6.49)	(4.69)	(3.63)	(4.31)	(2.65)
Observations	2,411	2,411	2,411	2,411	15,106
R-squared	0.53	0.31	0.18	0.23	0.19
Village FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Individual FE					Y
Clustered at Individual-level					Y
Pre-Vacuum 1 Shabunda mean	0.00	41.30	4.35	0.00	0.00
Pre-Vacuum 2 Shabunda mean	76.09	0.00	0.00	0.00	0.00
P-value: Vacuum 1, Clustered at:					
Village	0.07	0.77	0.22	0.57	0.34
Village & Chiefdom-post Vacuum	0.00	0.51	0.03	0.46	0.16
Village & Chiefdom-year	0.09	0.77	0.23	0.68	0.41
P-value: Vacuum 1 X Victimization, Clustered at:					
Village	0.97	0.02	0.15	0.00	0.00
Village & Chiefdom-post Vacuum	0.89	0.00	0.00	0.02	0.00
Village & Chiefdom-year	0.97	0.05	0.00	0.09	0.09

Notes: This table presents the estimates of Equation 2.2 with the core sample, with an additional interaction between Vacuum 1 and the aggregate number of reported violent attacks on households by FDLR up to year t. Column 1–4 aggregate the total number of attacks at the village level; Column 5 aggregates at the household level. The dependent variables are (in decimal digits): an indicator for whether there is presence of the Congolese national army in the village, for whether there is a militia village chapter (stock), for whether there is a new militia chapter (inflow), for whether there is new militia village chapter combatants in the village (inflow), and for whether the respondent of the household survey was participating in a militia village chapter at that year, in Columns 1–5 respectively. Column 5 is estimated at the level of the individual respondent * year (n=15,106), while Column 1–4 are from the village * year dataset (and thus indexed by jt rather than ijt). Column 1–4 control for village fixed effects and year fixed effects; standard errors are clustered at the village-level. Column 5 controls for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the village-level and individual-level. Table notes below the regression coefficients report the p-values for the key coefficients, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level.

Table B.14: The Role of Victimization and Revenge in the Rise of the Raia—Within-Village Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Social Motivations					Private Motivations	
		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 X Victimization	8.85*	1.69	-1.11	1.04	2.45	2.67	-0.73	3.31
	(4.82)	(2.25)	(2.50)	(1.04)	(1.86)	(2.03)	(0.71)	(2.30)
Vacuum 2 X Victimization	-1.16	4.26	-7.52*	-2.67*	4.31*	-0.12	0.49	0.78
	(3.85)	(3.14)	(3.93)	(1.51)	(2.45)	(0.17)	(1.08)	(1.69)
Observations	14,991	14,991	14,991	14,991	14,991	14,991	14,991	14,991
R-squared	0.53	0.25	0.41	0.34	0.27	0.29	0.23	0.24
Pre-Vacuum 1 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 1 X Victimization, Clustered at:								
Resp. & Village	0.07	0.45	0.66	0.32	0.19	0.19	0.31	0.15
Resp. & Village & Chiefdom-post Vacuum	0.00	0.00	0.05	0.00	0.00	0.00	0.00	0.00
Resp. & Village & Chiefdom-year	0.25	0.45	0.70	0.26	0.06	0.01	0.22	0.24
P-value: Vacuum 2 X Victimization, Clustered at:								
Resp. & Village	0.76	0.18	0.06	0.08	0.08	0.50	0.65	0.64
Resp. & Village & Chiefdom-post Vacuum	0.40	0.00	0.00	0.00	0.00	0.00	0.04	0.15
Resp. & Village & Chiefdom-year	0.67	0.03	0.25	0.13	0.11	0.00	0.22	0.27

Notes: This table presents the estimates of Equation 2.2 using the core sample, in which the main independent variables are replaced with the following two indicators: $\mathbf{1}[V1_{jt} = 1] \times F_{ijt}$ and $\mathbf{1}[V2_{jt} = 1] \times F_{ijt}$, where F_{ijt} is an indicator taking value one if the household members of individual i in village j in year t have previously been victimized by the FDLR. The dependent variables are (in decimal digits): an indicator for whether the individual joins a militia village chapter (Column 1), for whether they joined it motivated by intrinsic social emotions (Columns 2–3), by extrinsic social incentives (Columns 4–6), or by private motivations (Column 7–8). All regressions include controls for village-year fixed effects and individual fixed effects; standard errors are clustered at the individual-level and village-level. Table notes below the regression coefficients report the p-values for the coefficients, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level.

Table B.15: The Regimentation Caused an Unprecedented Rise in Insecurity—Statistical Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	External Threats: Attacked				External Threats: FDLR Present			
	in Village	in Groupement	in Chiefdom	in Territory	in Village	in Groupement	in Chiefdom	in Territory
Vacuum 1 [Sun City Peace, initial]	-9.78**	-8.49***	-9.53***	-9.52***	-0.44	-0.55	-0.42	-0.21
	(3.94)	(1.94)	(1.35)	(0.41)	(4.60)	(1.81)	(0.60)	(0.48)
Vacuum 2 [Regimentation, initial]	21.60***	22.23***	22.13***	21.51***	37.27***	37.39***	37.51***	37.49***
	(8.12)	(2.52)	(1.34)	(0.52)	(7.35)	(5.22)	(1.86)	(1.42)
Observations	2,192	2,338	2,360	2,360	2,491	2,491	2,491	2,491
R-squared	0.11	0.19	0.41	0.68	0.45	0.51	0.57	0.65
Pre-Vacuum 1 Shabunda mean	15.56	15.76	15.56	15.56	4.35	4.35	4.35	4.35
Pre-Vacuum 2 Shabunda mean	17.78	17.47	17.78	17.78	13.04	13.04	13.04	13.04
P-value: Vacuum 1, Clustered at:								
Village	0.01	0.00	0.00	0.00	0.92	0.76	0.48	0.65
Village & Chiefdom-post Vacuum	0.02	0.02	0.02	0.00	0.79	0.72	0.84	0.90
Village & Chiefdom-year	0.01	0.01	0.01	0.00	0.89	0.85	0.90	0.94
P-value: Vacuum 2, Clustered at:								
Village	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Village & Chiefdom-post Vacuum	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Village & Chiefdom-year	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Notes: This table presents the estimates of Equation 2.2 using the core sample. The dependent variables are (in decimal digits): an indicator for whether the village is attacked by FDLR (Column 1) or whether the FDLR is present in the village (Column 5), percentage of villages in the same groupment, chiefdom, or territory that are attacked by FDLR (Columns 2–4) or see the presence of FDLR (Columns 6–8). The independent variables are defined as whether a village j in year t belongs to the initial years of the vacuums. All regressions control for village fixed effects and year fixed effects; standard errors are estimated at the village level. Table notes below the regression coefficients report the p-values for the coefficients, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level.

Table B.16: Replication: Unbundling the Extraordinary Rise of the Raia

	(1) Participate	(2) Participate	(3) Participate	(4) Participate	(5) Participate
Vacuum 1 [Sun City Peace]	2.19***	2.25***	2.22***	2.18***	2.25***
	(0.81)	(0.81)	(0.81)	(0.81)	(0.81)
Vacuum 2 [Regimentation]	16.42***	0.50	15.25***	14.93***	0.17
	(2.53)	(0.69)	(2.53)	(2.43)	(0.81)
Vacuum 2 X Insecurity (2010-11)		20.29***			18.63***
		(2.68)			(2.71)
Vacuum 2 X Past Participation (2003-05)			11.70***		6.68
			(4.49)		(4.63)
Vacuum 2 X Stock of Victimization				8.21***	5.27*
				(2.69)	(2.72)
Observations	54,558	47,702	54,558	54,558	47,702
R-squared	0.17	0.19	0.17	0.17	0.20
Pre-Vacuum 1 Shabunda	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 2, Clustered at:					
Resp. & Village	0.00	0.47	0.00	0.00	0.83
Resp. & Village & Chiefdom-post Vacuum	0.00	0.16	0.00	0.00	0.64
Resp. & Village & Chiefdom-year	0.07	0.64	0.05	0.06	0.90
P-value: Vacuum 2 < Vacuum 1	0.00	0.96	0.00	0.00	0.98

Notes: This table replicates Table 2.5, Panel A, by including extra village sample from North Kivu. We do not have information of past victimization in the extra household sample from South Kivu. We present the estimates of Equation 2.2 in which we have added controls $\mathbf{1}[V1_{jt} = 1] \times F_{ijt}$, where F_{ijt} is one of the following: (i) an indicator whether FDLR is present in nearby villages in the same Groupement during 2010–11, but not in own village (Insecurity, indexed by j), (ii) an indicator whether the respondent participated a militia village chapter during the first state vacuum induced by Sun-city peace agreement (Past participation, indexed by ij), and (iii) number of household-level FDLR attacks in the past (only in Panel A, indexed by ijt). Column 5, Panel A and Column 4, Panel B include all available controls in the same regression. The dependent variable is (in decimal digits) an indicator for whether the respondent of the household survey was participating in a militia village chapter in that year. All regressions control for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the village-level and individual-level. Table notes below the regression coefficients report the p-values for the coefficients of Vacuum 2, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level. In the last row, we compute p-values of rejecting the null hypothesis that the coefficient of Vacuum 2 is smaller than that of Vacuum 1.

Table B.17: Replication: Explaining Individual Motivations to Join the Militia with Community Insecurity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Social Motivations					Private Motivations	
		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	0.66	0.16	-0.13	0.06	0.16	0.15	0.05	0.23
	(0.43)	(0.11)	(0.27)	(0.04)	(0.10)	(0.10)	(0.04)	(0.14)
Vacuum 2 [Regimentation]	0.28	-0.11	-0.24	0.01	0.07**	0.04*	0.76	-0.03
	(0.93)	(0.12)	(0.27)	(0.01)	(0.03)	(0.02)	(0.82)	(0.08)
Vacuum 2 X Insecurity (2010-11)	13.63***	2.17***	7.08***	0.71***	2.17***	-0.04*	-0.25	1.50***
	(2.26)	(0.40)	(1.18)	(0.20)	(0.67)	(0.02)	(0.84)	(0.35)
Observations	27,068	27,068	27,068	27,068	27,068	27,068	27,068	27,068
R-squared	0.19	0.08	0.13	0.11	0.08	0.06	0.06	0.07
Pre-Vacuum 1 Shabunda mean	0.25	0.12	0.00	0.00	0.00	0.12	0.00	0.00
Pre-Vacuum 2 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 2, Clustered at:								
Resp. & Village	0.76	0.37	0.38	0.39	0.02	0.07	0.36	0.70
Resp. & Village & Chiefdom-post Vacuum	0.56	0.41	0.31	0.16	0.00	0.00	0.02	0.46
Resp. & Village & Chiefdom-year	0.95	0.86	0.93	0.98	0.93	0.11	0.12	0.96
P-value: Vacuum 2 X Insecurity, Clustered at:								
Resp. & Village	0.00	0.00	0.00	0.00	0.00	0.05	0.77	0.00
Resp. & Village & Chiefdom-post Vacuum	0.00	0.00	0.00	0.00	0.00	0.00	0.40	0.00
Resp. & Village & Chiefdom-year	0.09	0.05	0.11	0.16	0.11	0.03	0.49	0.13

Notes: This table replicates Table 2.5, Panel B, by including extra household sample from South Kivu. It presents the estimates of Equation 2.2, in which we have also added as a control, the following indicator: $\mathbf{1}[V2_{jt} = 1] \times F_j$, where F_j is an indicator taking value one if FDLR is present in nearby villages in the same Groupement during 2010–11, but not in own village j. The dependent variables are (in decimal digits): an indicator for whether the individual joins a militia village chapter (Column 1), for whether they joined it motivated by intrinsic social emotions (Columns 2–3), by extrinsic social incentives (Columns 4–6), or by private motivations (Column 7–8). All regressions include controls for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the individual-level and village-level. Table notes below the regression coefficients report the p-values for the key coefficients, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level.

Table B.18: Individual Motivations to Join the Militia Channeled Through Community Insecurity and Victimization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Social Motivations					Private Motivations	
		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	1.85**	0.45	0.01	0.16	0.23	0.45*	0.11	0.48
	(0.91)	(0.32)	(0.49)	(0.12)	(0.17)	(0.27)	(0.12)	(0.30)
Vacuum 2 [Regimentation]	0.46	-0.06	-0.25	0.03	0.10**	0.09*	0.77	-0.01
	(0.99)	(0.14)	(0.35)	(0.02)	(0.04)	(0.05)	(0.82)	(0.12)
Vacuum 2 X Insecurity (2010-11)	18.84***	2.15***	12.00***	1.19*	1.22**	-0.02	0.77	1.22**
	(2.67)	(0.69)	(2.09)	(0.64)	(0.55)	(0.02)	(0.97)	(0.49)
Vacuum 2 X Insecurity (2010-11) X Victimization	10.74**	4.30	2.44	-1.32**	4.69*	-0.49	-0.28	1.84
	(4.15)	(3.10)	(3.31)	(0.64)	(2.44)	(0.31)	(1.14)	(1.80)
Observations	13,982	13,982	13,982	13,982	13,982	13,982	13,982	13,982
R-squared	0.21	0.09	0.15	0.15	0.10	0.07	0.07	0.08
Pre-Vacuum 1 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 2, Clustered at:								
Resp. & Village	0.64	0.67	0.48	0.20	0.01	0.06	0.35	0.94
Resp. & Village & Chiefdom-post Vacuum	0.38	0.67	0.34	0.00	0.00	0.00	0.02	0.83
Resp. & Village & Chiefdom-year	0.92	0.90	0.93	0.93	0.85	0.15	0.07	0.98
P-value: Vacuum 2 X Insecurity, Clustered at:								
Resp. & Village	0.00	0.00	0.00	0.07	0.03	0.25	0.43	0.01
Resp. & Village & Chiefdom-post Vacuum	0.00	0.00	0.00	0.00	0.00	0.24	0.03	0.00
Resp. & Village & Chiefdom-year	0.10	0.02	0.13	0.18	0.16	0.12	0.19	0.04
P-value: Vacuum 2 X Insecurity X Victimization, Clustered at:								
Resp. & Village	0.01	0.17	0.46	0.04	0.06	0.11	0.81	0.31
Resp. & Village & Chiefdom-post Vacuum	0.00	0.00	0.01	0.00	0.00	0.00	0.29	0.00
Resp. & Village & Chiefdom-year	0.07	0.04	0.00	0.12	0.15	0.06	0.58	0.29

Notes: This table presents the estimates of Equation 2.2 using the core sample, in which we have also added as controls, the following indicators: $\mathbf{1}[V2_{jt} = 1] \times F_{ijt}$, where F_{ijt} is one of the following indicators: (i) whether FDLR is present in nearby villages in the same Groupement during 2010–11, but not in own village j (indexed by j), and (ii) whether the previous indicator equals one and the household members of individual i in village j in year t have previously been victimized by the FDLR (indexed by ijt). The dependent variables are (in decimal digits): an indicator for whether the individual joins a militia village chapter (Column 1), for whether they joined it motivated by intrinsic social emotions (Columns 2–3), by extrinsic social incentives (Columns 4–6), or by private motivations (Column 7–8). All regressions include controls for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the individual-level and village-level. Table notes below the regression coefficients report the p-values for the key coefficients, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level.

Table B.19: Types of Recruitment Campaigns in the Village: The Role of Village Chiefs

	Militia, From:			Foreign g.	Army	P-value
	Anywhere	Village	Outside			(4)-(1)
	(1)	(2)	(3)	(4)	(5)	(6)
# Village Chapter Episodes	129	39	90	52	127	136
Frequency (% Years of Village Chapter Episode)						
Recruitment Campaigns, All	84.65	94.40	60.29	26.81	7.70	0.00
, Private	13.39	12.87	14.68	0.00	0.16	0.00
, Circumventing Chief	20.66	14.87	35.16	9.95	1.16	0.04
, Coercing Chief	7.07	9.68	0.53	8.92	0.00	0.65
, Public Village Meetings	40.87	53.67	8.86	8.14	2.35	0.00
, Chief-Initiated	11.90	16.67	0.00	2.92	1.16	0.04
, Chief-Initiated or Public	48.35	64.15	8.86	9.81	3.50	0.00

Notes: Only the core sample from South Kivu is included. *Militia from Village* reports the sample of individuals who joined a militia chapter formed in the village of survey. *Raia* and *Non-Raia* report the sample of individuals who joined in Raia and other militia chapter formed in the village of survey that is not Raia, respectively. *Militia from Outside* reports the sample of individuals who joined a militia chapter formed outside of the survey village. *Foreign* reports the sample of individuals who joined a foreign armed group. *Army* reports the sample of individuals who joined Congolese national army. Units for the number of observations are reported in the panel headers. The numbers reported after the first row are the fractions of chapter episodes in which at least one recruitment campaign of each corresponding type takes place. We indicate the significance of differences compared to Column 2 with stars at 1, 5, or 10% significance levels (*, **, *** respectively).

Table B.20: Replication: The Extraordinary Rise of the Raia is in Part Channeled by Communities Institutions Responses to Insecurity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
					Social Motivations					Private Motivations	
	Campaign		Participation		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	Public	Public	General	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	1.37	1.42	0.52	0.52	0.16	-0.24	0.06	0.17*	0.10	0.05	0.23*
	(3.47)	(3.49)	(0.42)	(0.43)	(0.11)	(0.27)	(0.04)	(0.09)	(0.11)	(0.04)	(0.13)
Vacuum 2 [Regimentation]	7.69**	0.96	12.74***	7.68***	1.34***	3.93***	0.30*	0.56**	-0.00	0.62***	1.04***
	(3.36)	(6.90)	(2.03)	(1.66)	(0.35)	(0.97)	(0.17)	(0.24)	(0.02)	(0.22)	(0.39)
Vacuum 2 X Insecurity		8.26									
		(7.80)									
Vacuum 2 X Public Campaign				19.42***	2.24*	8.87**	1.42**	5.86***	0.00	-0.38	1.31
				(6.72)	(1.14)	(3.71)	(0.67)	(2.04)	(0.03)	(0.49)	(1.06)
Observations	4,392	3,899	29,035	28,784	28,784	28,784	28,784	28,784	28,784	28,784	28,784
R-squared	0.26	0.26	0.18	0.21	0.08	0.14	0.11	0.10	0.06	0.06	0.07
Pre-Vacuum 1 Shabunda mean	23.91	23.91	0.25	0.25	0.12	0.00	0.00	0.00	0.12	0.00	0.00
Pre-Vacuum 2 Shabunda mean	2.17	2.17	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 2, Clustered at:											
Resp. & Village	0.02	0.89	0.00	0.00	0.00	0.00	0.07	0.02	0.81	0.01	0.01
Resp. & Village & Chiefdom-post Vacuum	0.00	0.81	0.00	0.00	0.00	0.00	0.00	0.00	0.63	0.00	0.00
Resp. & Village & Chiefdom-year	0.25	0.83	0.00	0.01	0.00	0.02	0.03	0.01	0.50	0.05	0.01
P-value: Vacuum 2 X Insecurity/Campaign, Clustered at:											
Resp. & Village		0.29		0.00	0.05	0.02	0.04	0.00	0.92	0.44	0.22
Resp. & Village & Chiefdom-post Vacuum		0.07		0.00	0.00	0.00	0.00	0.00	0.86	0.10	0.00
Resp. & Village & Chiefdom-year		0.15		0.00	0.00	0.00	0.00	0.00	0.81	0.33	0.00

Notes: This table replicates Table 2.6. Column 1–2 includes extra village samples from North Kivu. Column 3–11 includes extra household samples from South Kivu. The table presents the estimates of Equation 2.2. In Column 2 and Column 4–11, we have also added as a control, the following indicator: $\mathbf{1}[V2_{jt} = 1] \times F_{jt}$, where F_{jt} is one of the following indicators: (i) whether FDLR is present in nearby villages in the same Groupement during 2010–11, but not in own village j (Column 2, Insecurity, indexed by j), and (ii) whether village j in year t has a public campaign (Column 4–11, Campaign, indexed by jt). The dependent variables are (in decimal digits): an indicator for whether the village has a public campaign (Column 1–2), for whether the individual joins a militia village chapter (Column 3–4), for whether they joined it motivated by intrinsic social emotions (Columns 5–6), by extrinsic social incentives (Columns 7–9), or by private motivations (Column 10–11). Column 1–2 control for village fixed effects and year fixed effects; standard errors are clustered at the village-level. Column 3–11 control for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the individual-level and village-level. Table notes below the regression coefficients report the p-values for the key coefficients, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level.

Table B.21: Chief-Initiated Campaigns Channel the Effect of Vacuum 2 on the Rise of the Raia

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
					Social Motivations					Private Motivations	
	Campaign		Participation		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	Public	Public	General	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	-1.24	-1.24	1.68*	1.63*	0.45	-0.17	0.17	0.25	0.38	0.11	0.48
	(0.92)	(0.94)	(0.91)	(0.92)	(0.32)	(0.50)	(0.12)	(0.16)	(0.28)	(0.12)	(0.30)
Vacuum 2 [Regimentation]	6.54**	5.73	16.96***	14.75***	2.17***	8.36***	0.79	1.34**	0.02	1.19**	1.07**
	(2.60)	(5.92)	(2.55)	(2.57)	(0.65)	(1.76)	(0.49)	(0.58)	(0.02)	(0.47)	(0.46)
Vacuum 2 X Insecurity		0.95									
		(6.58)									
Vacuum 2 X Chief-initiated Campaign				26.53**	1.19	16.64*	0.51	4.54	-0.18	2.02	1.99
				(12.12)	(2.38)	(8.66)	(1.48)	(3.04)	(0.17)	(2.48)	(1.97)
Observations	2,454	2,284	15,106	14,855	14,855	14,855	14,855	14,855	14,855	14,855	14,855
R-squared	0.23	0.23	0.19	0.20	0.08	0.15	0.14	0.09	0.06	0.07	0.08
Pre-Vacuum 1 Shabunda	2.17	2.17	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda	2.17	2.17	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 2, Clustered at:											
Resp. & Village	0.01	0.33	0.00	0.00	0.00	0.00	0.11	0.02	0.27	0.01	0.02
Resp. & Village & Chiefdom-post Vacuum	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.16	0.00	0.00
Resp. & Village & Chiefdom-year	0.03	0.07	0.01	0.02	0.00	0.02	0.07	0.04	0.00	0.05	0.00
P-value: Vacuum 2 X Insecurity/Campaign, Clustered at:											
Resp. & Village		0.89		0.03	0.62	0.06	0.73	0.14	0.29	0.42	0.31
Resp. & Village & Chiefdom-post Vacuum		0.70		0.00	0.05	0.00	0.06	0.00	0.00	0.00	0.00
Resp. & Village & Chiefdom-year		0.64		0.00	0.28	0.00	0.62	0.00	0.11	0.07	0.00

Notes: This table replicates Table 2.6 by replacing public campaign with chief-initiated campaign. It presents the estimates of Equation 2.2. In Column 2 and Column 4–11, we have also added as a control, the following indicator: $\mathbf{1}[V2_{jt} = 1] \times F_{jt}$, where F_{jt} is one of the following indicators: (i) whether FDLR is present in nearby villages in the same Groupement during 2010–11, but not in own village j (Column 2, Insecurity, indexed by j), and (ii) whether village j in year t has a chief-initiated campaign (Column 4–11, Campaign, indexed by jt). The dependent variables are (in decimal digits): an indicator for whether the village has a chief-initiated campaign (Column 1–2), for whether the individual joins a militia village chapter (Column 3–4), for whether they joined it motivated by intrinsic social emotions (Columns 5–6), by extrinsic social incentives (Columns 7–9), or by private motivations (Column 10–11). Column 1–2 control for village fixed effects and year fixed effects; standard errors are clustered at the village-level. Column 3–11 control for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the individual-level and village-level. Table notes below the regression coefficients report the p-values for the key coefficients, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level.

Table B.22: The Extraordinary Rise of the Raia is in Part Channeled by Communities Responses to Insecurity—By Victmization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			Social Motivations					Private Motivations	
			Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	General	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	1.68*	1.64*	0.45	-0.16	0.17	0.25	0.38	0.11	0.48
	(0.91)	(0.91)	(0.32)	(0.50)	(0.12)	(0.16)	(0.28)	(0.12)	(0.29)
Vacuum 2 [Regimentation]	16.96***	11.61***	1.95***	6.79***	0.05	0.73*	0.02	1.32***	0.89**
	(2.55)	(2.42)	(0.69)	(1.73)	(0.12)	(0.39)	(0.03)	(0.45)	(0.41)
Vacuum 2 X Public Campaign		28.22***	0.99	17.56**	5.98*	3.45	0.04	-0.40	0.83
		(10.72)	(1.80)	(8.01)	(3.47)	(2.43)	(0.03)	(1.17)	(1.57)
Vacuum 2 X Public Campaign X Victimization		15.60	4.13	-0.36	-6.11*	10.70**	-0.51	2.76	5.35
		(10.82)	(5.08)	(8.65)	(3.46)	(5.31)	(0.40)	(2.82)	(4.61)
Observations	15,106	14,855	14,855	14,855	14,855	14,855	14,855	14,855	14,855
R-squared	0.19	0.23	0.09	0.16	0.18	0.12	0.06	0.07	0.08
Pre-Vacuum 1 Shabunda	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
P-value: Vacuum 2, Clustered at:									
Resp. & Village	0.00	0.00	0.01	0.00	0.67	0.06	0.54	0.00	0.03
Resp. & Village & Chiefdom-post Vacuum	0.00	0.00	0.00	0.00	0.09	0.00	0.06	0.00	0.00
Resp. & Village & Chiefdom-year	0.01	0.02	0.00	0.03	0.59	0.03	0.58	0.07	0.00
P-value: Vacuum 2 X Insecurity, Clustered at:									
Resp. & Village		0.01	0.59	0.03	0.09	0.16	0.20	0.73	0.60
Resp. & Village & Chiefdom-post Vacuum		0.00	0.06	0.00	0.00	0.00	0.00	0.31	0.01
Resp. & Village & Chiefdom-year		0.00	0.17	0.00	0.00	0.00	0.09	0.58	0.20
P-value: Vacuum 2 X Insecurity X Victimization, Clustered at:									
Resp. & Village		0.15	0.42	0.97	0.08	0.05	0.20	0.33	0.25
Resp. & Village & Chiefdom-post Vacuum		0.00	0.00	0.83	0.00	0.00	0.00	0.00	0.00
Resp. & Village & Chiefdom-year		0.00	0.02	0.93	0.00	0.00	0.10	0.01	0.00

Notes: This table presents the estimates of Equation 2.2 using the core sample, in which we have also added as controls, the following indicators: $\mathbf{1}[V2_{jt} = 1] \times F_{ijt}$, where F_{ijt} is one of the following indicators: (i) whether village j in year t has a public campaign (indexed by jt), and (ii) whether the previous indicator equals one and the household members of individual i in village j in year t have previously been victimized by the FDLR (indexed by ijt). The dependent variables are (in decimal digits): an indicator for whether the individual joins a militia village chapter (Column 1–2), for whether they joined it motivated by intrinsic social emotions (Columns 3–4), by extrinsic social incentives (Columns 5–7), or by private motivations (Column 8–9). All regressions include controls for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the individual-level and village-level. Table notes below the regression coefficients report the p-values for the key coefficients, calculated from (i) clustering at village-level, (ii) clustering at village and chiefdom-post vacuums level, and (iii) clustering at village and chiefdom-year level.

Table B.23: Description of Migrants

	Nonmigrant	Migrant
# Individual-Year Obs.	18067	1389
A: Conflict Background		
Past Victimization by Foreign Armed Group	0.07	0.08
By Congolese Militia	0.03	0.03
Past participation in Militia Village Chapter	0.06	0.04**
In Raia Mutomboki or Mayi-Mayi	0.03	0.02**
In Militia Formed Outside Village	0.02	0.02
B: Demographic Characteristics		
In the Family of the Village Chief	0.10	0.11
Age in year t	26.58	26.52*
Married in year t	0.35	0.34
C: Productive Capacity in Nonviolent Sector		
Employed in year t-1	0.57	0.51***
In Mining Sector in year t-1	0.12	0.11
In Agricultural Sector in year t-1	0.39	0.34***
As a Civil Servant in year t-1	0.06	0.06
Father's Wealth Index	-0.20	-0.17
# Plots Owned in year t-1	0.50	0.48
Farm Animal Index in year t-1	0.08	0.04
Primary Education Complete	0.50	0.59***
Secondary Education Complete	0.13	0.19***
D: Average Increase in Future Assets		
# Plots Owned	0.18	0.25***
Farm Animal Index	0.22	0.31***

Notes: We use the core sample from South Kivu to present the descriptives. *Migrant*: Episodes where respondents just move to a new village. *Nonmigrant*: observations where respondents stay in the same village as in the previous year. We indicate the difference between Column 1 and 2 (P-value: *** 0.01, ** 0.05, * 0.10), computed after including year FE, and clustered two-way at the individual respondent and the village-year level.

Table B.24: Migration History of Participants

	Participants				Non-participants		
	Militia from Village			Militia from	Living in the Same:		
	All	Raia	Non-Raia	Outside	Village	Chiefdom	Territory
# Participants/Individual-Year Obs.	245	134	111	30	753	11092	12152
Past migration	*0.58*	*0.71*	*0.42*	*0.40****	*0.42*	*0.41*	*0.41*
Within sample villages	0.31	0.40	0.22	0.17*	0.18	0.14**	0.14***
From out-of-sample villages	0.33	0.43	0.21	0.20*	0.21	0.22	0.23
To out-of-sample villages	0.24	0.31	0.14	0.10	0.16**	0.16*	0.16*
Within out-of-sample villages	0.05	0.07	0.03	0.03*	0.04	0.06*	0.06*
Migration in the same year	*0.05*	*0.01*	*0.10*	*0.14****	*0.09*	*0.07****	*0.07***
Within sample villages	0.04	0.00	0.08	0.10	0.03	0.02**	0.02**
From out-of-sample villages	0.01	0.01	0.01	0.03***	0.03	0.03	0.03
To out-of-sample villages	0.00	0.00	0.01	0.00	0.03	0.01	0.02
Within out-of-sample villages	0.00	0.00	0.00	0.00	0.00	0.01	0.01

Notes: We use the core sample from South Kivu to present the descriptives. *Militia from Village* reports the sample of individuals who joined a militia chapter formed in the village of survey. *Raia* and *Non-Raia* report the sample of individuals who joined in Raia and other militia chapter formed in the village of survey that is not Raia, respectively. *Militia from Outside* reports the sample of individuals who joined a militia chapter formed outside of the survey village. For non-participants, we include individual-year observations where there is at least one contemporary participant in a militia living in the same village, same chiefdom, same territory, respectively. We indicate the significance of differences compared to Column 2 with stars at 1, 5, or 10% significance levels (*, **, *** respectively). When calculating differences, we include year fixed effects, control for all variables in Table B.6, Panels B–D, and cluster at two-way at the individual respondent and the village-year level. We are not able to provide more descriptives of migration in the future because the data collection of the core sample ended in 2013, while most participation in Raia Mutomboki happened in 2012.

Table B.25: Migration Analysis: State Vacuum and the Birth of The Raia

Panel A. Only Stayers Included

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Social Motivations					Private Motivations	
		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	1.78*	0.40	-0.22	0.18	0.16	0.50	0.18	0.58
	(1.01)	(0.30)	(0.55)	(0.17)	(0.16)	(0.39)	(0.17)	(0.41)
Vacuum 2 [Regimentation]	15.61***	2.11***	9.06***	0.71	1.20**	-0.04*	2.17***	0.72
	(2.61)	(0.74)	(1.87)	(0.51)	(0.60)	(0.02)	(0.70)	(0.55)
Observations	9,308	9,308	9,308	9,308	9,308	9,308	9,308	9,308
R-squared	0.18	0.08	0.13	0.07	0.07	0.07	0.08	0.07
Pre-Vacuum 1 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Panel B. Interacting Vacuums with Immigration Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Social Motivations					Private Motivations	
		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Vacuum 1 [Sun City Peace]	1.76*	0.43	-0.22	0.16	0.20	0.48	0.14	0.58
	(0.96)	(0.31)	(0.50)	(0.15)	(0.16)	(0.33)	(0.15)	(0.36)
Vacuum 2 [Regimentation]	16.99***	1.83***	9.82***	0.62	1.86***	0.00	1.91***	1.11**
	(2.65)	(0.65)	(1.87)	(0.45)	(0.65)	(0.02)	(0.63)	(0.54)
Vacuum 1 X Immigrant	-0.48	0.11	0.46	0.05	0.31	-0.56*	-0.12	-0.54
	(1.18)	(0.65)	(0.59)	(0.19)	(0.61)	(0.34)	(0.15)	(0.41)
Vacuum 2 X Immigrant	-0.13	1.52	-0.25	0.73	-0.48	-0.00	-1.92***	0.42
	(3.17)	(1.28)	(2.75)	(0.83)	(0.86)	(0.03)	(0.64)	(1.12)
Observations	15,074	15,074	15,074	15,074	15,074	15,074	15,074	15,074
R-squared	0.19	0.09	0.14	0.14	0.09	0.07	0.07	0.07
Pre-Vacuum 1 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Pre-Vacuum 2 Shabunda mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Notes: This table presents the estimates of Equation 2.2 using the core sample, where the dependent variables are (in decimal digits): an indicator for whether the individual joins a militia village chapter (Column 1), for whether they joined it motivated by intrinsic social emotions (Columns 2–3), by extrinsic social incentives (Columns 4–6), or by private motivations (Column 7–8). Panel A restricts the core sample to respondents who never left the village throughout 1995–2013. Panel B adds as a control $\mathbf{1}[V1_{jt}] \times F_{ijt}$ and $\mathbf{1}[V2_{jt}] \times F_{ijt}$, where F_{ijt} is an indicator of whether the respondent i in village j in year t recently moved from somewhere outside of our sample villages. All regressions control for village fixed effects, year fixed effects, and individual fixed effects; standard errors are clustered at the level of village-level and individual-level.

Table B.26: Migration Counterfactual Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Social Motivations					Private Motivations	
		Intrinsic (Social Emotions):		Extrinsic (Social Incentives):				
	General	Revenge	Community Protection	Status	Social Pressure	Social Coercion	Money	Private Protection
Estimated ATE from Table 2.3:								
Vacuum 1 [Sun City Peace]	1.68*	0.45	-0.14	0.17	0.25	0.38	0.11	0.48
	(0.91)	(0.32)	(0.50)	(0.12)	(0.16)	(0.28)	(0.12)	(0.30)
Vacuum 2 [Regimentation]	16.96***	2.26***	9.75***	0.83*	1.72***	0.01	1.36***	1.23***
	(2.55)	(0.62)	(1.80)	(0.45)	(0.58)	(0.02)	(0.47)	(0.45)
Estimated Treatment Effects on Immigrants from Outside of Sample from Table B.25, Panel B:								
Vacuum 1 X Immigrant ($\widehat{T}_1(B_m)$)	-0.48	0.11	0.46	0.05	0.31	-0.56*	-0.12	-0.54
	(1.18)	(0.65)	(0.59)	(0.19)	(0.61)	(0.34)	(0.15)	(0.41)
Vacuum 2 X Immigrant ($\widehat{T}_2(B_m)$)	-0.13	1.52	-0.25	0.73	-0.48	-0.00	-1.92***	0.42
	(3.17)	(1.28)	(2.75)	(0.83)	(0.86)	(0.03)	(0.64)	(1.12)
Counterfactual: Actual ATE = 0, Treatment Effects on Emigrates in Out of Sample $\in [-10\%, 10\%]$*:*								
P-value: $\widehat{T}_1(B_m) = T_1^{ct}(B_m)$	0.20	1.00	1.00	1.00	1.00	0.58	1.00	0.49
P-value: $\widehat{T}_2(B_m) = T_2^{ct}(B_m)$	0.00	0.89	0.00	1.00	0.07	1.00	0.00	0.90
Counterfactual: Actual ATE = 0, Treatment Effects on Emigrates in Out of Sample $\in [-20\%, 20\%]$*:*								
P-value: $\widehat{T}_1(B_m) = T_1^{ct}(B_m)$	0.54	1.00	1.00	1.00	1.00	1.00	1.00	1.00
P-value: $\widehat{T}_2(B_m) = T_2^{ct}(B_m)$	0.00	1.00	0.00	1.00	0.36	1.00	0.00	1.00
Counterfactual: Actual ATE = 0, Treatment Effects on Emigrates in Out of Sample $\in [-30\%, 30\%]$*:*								
P-value: $\widehat{T}_1(B_m) = T_1^{ct}(B_m)$	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
P-value: $\widehat{T}_2(B_m) = T_2^{ct}(B_m)$	0.00	1.00	0.00	1.00	1.00	1.00	0.11	1.00

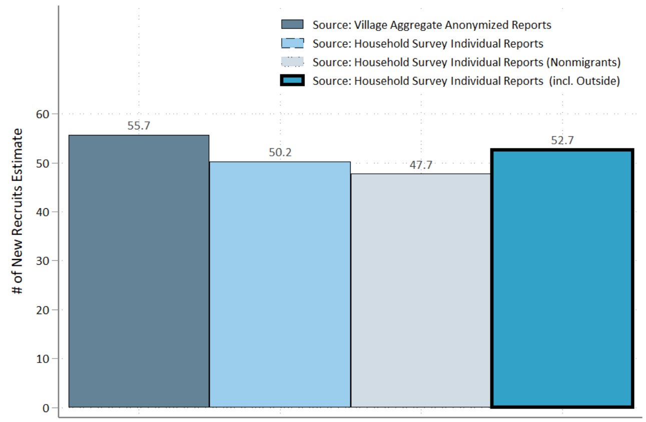
Notes: This table conducts a counterfactual exercise, using the estimates from Table 2.3 and Table B.25, Panel B. We assume the actual average treatment effect ATE is zero, and the treatment effects on the unobserved emigrates in out-of-sample villages ($T(A_m)$) are between $[-10, 10]$, $[-20, 20]$, and $[-30, 30]$ percentage points, respectively, to calculate the counterfactual treatment effects on the immigrants ($T^{ct}(B_m)$) following Equation B.1. We report the maximum p-values for different assumptions of $T(A_m)$.

Figure B.1: The Outraged Citizens

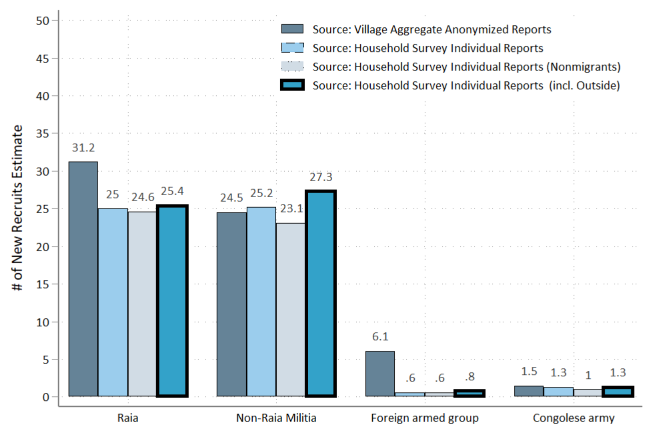
Notes. Source: Photography taken by Diana Zeyneb Alhindawi, which is publicly available at https://www.dianazeynebalhindawi.com. Zeyneb Alhindawi describes the image as follows: “Raia fighters gather, wearing leaves for camouflage, after going on a patrol through Lulingu’s surrounding areas [...] Dec. 27, 2013. Lulingu, South Kivu.”

Figure B.2: Cross-Validation of Participation Reports in the Data

Panel A. Participation in Militia Chapters



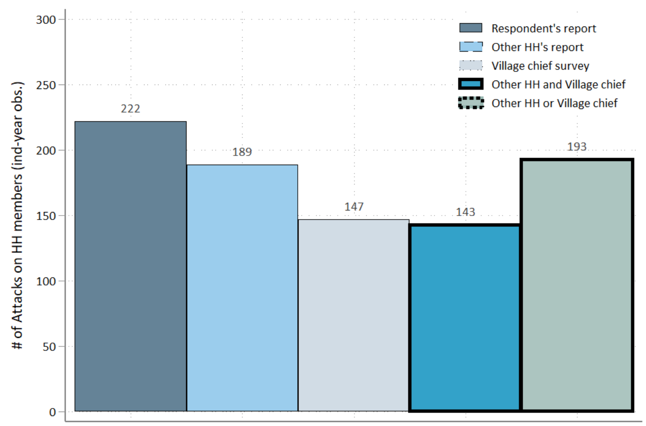
Panel B. Participation in Other Types of Armed Groups



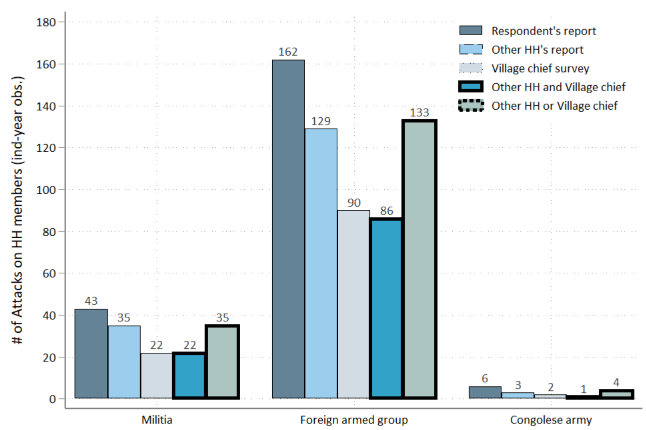
Notes. We use the core sample from South Kivu for the cross-validation exercise. Dark blue bar with solid outline on the left is the estimate obtained from the village aggregate anonymized reports. The aggregate village data report number of individuals who participated in an armed group for each village control episode. The second, light blue bar with dashed outline is the estimate obtained from the household individual report, restricted to enrollment during an episode in which an armed group controls the village. The third, gray bar with dotted contour, does the same as the previous but excludes from the estimation all respondent-year observations for years in which the respondent did not live in the village. The last, blue bar with solid thick contour, are the household reports, including those for participation events that took place outside of the recruitment obtained from the detailed data gathered separately for each village armed group control episode.

Figure B.3: Cross-Validation of Violent Events Reports in the Data

Panel A. All Violent Events on Household Members

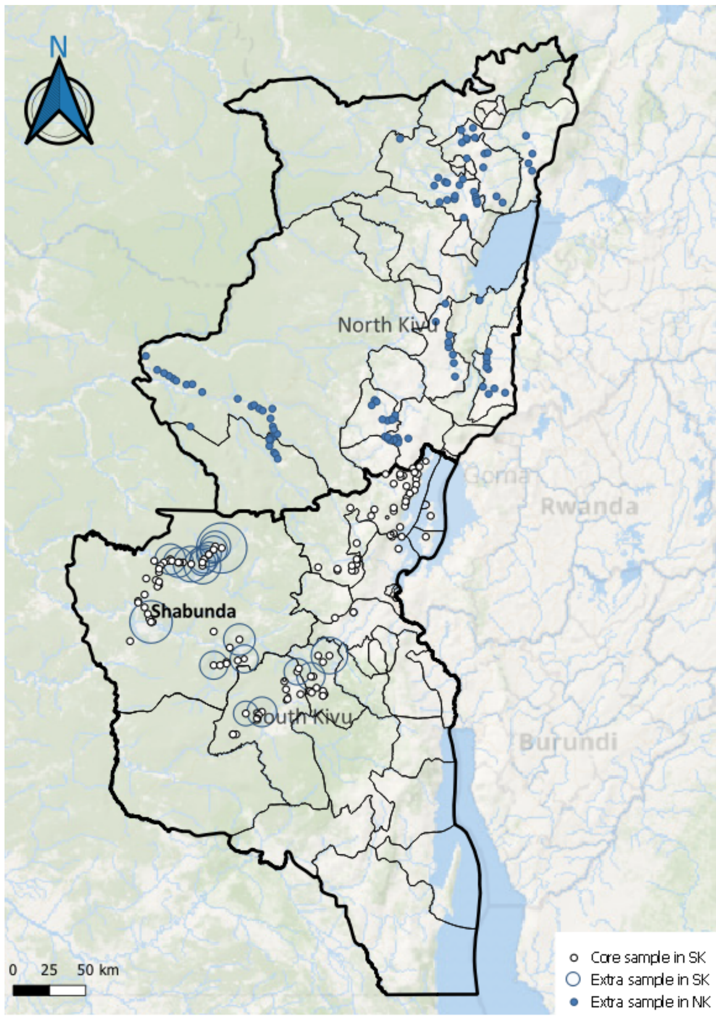


Panel B. Violent Events on Household Members by Armed Groups



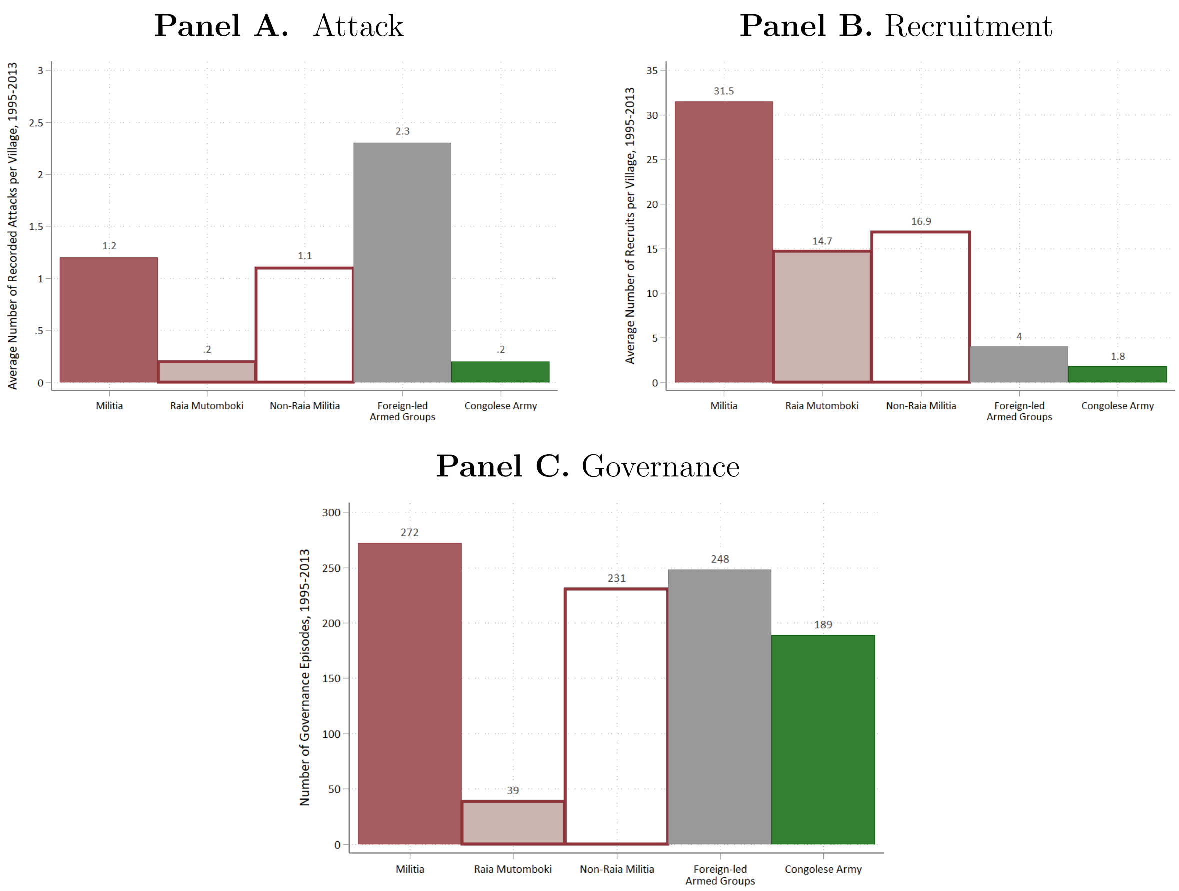
Notes. We exclude 46 attack events when respondents lived outside of the sample and thus cannot be validated using the village chief survey. The first, dark blue bar with solid outline, is the number of reported attacks on household between year 1995 and 2013 from respondents' report. The second, light blue bar with dashed outline, shows the number of attacks on household that are also reported by at least 1 other respondent observed in the sample who lived in the same village within 1 year ($t-1$, t, $t+1$). The third, gray bar with dotted contour, shows the number of attacks on household that are also reported in village chief survey within 1 year. The fourth, blue bar with thick solid contour, shows the number of attacks on household that are cross-validated by both village chief survey and at least 1 other contemporary respondent. The last, light green bar with thick short-dashed contour, shows the number of attacks on household that are cross-validated by either village chief survey or at least 1 other contemporary respondent.

Figure B.4: Study Samples



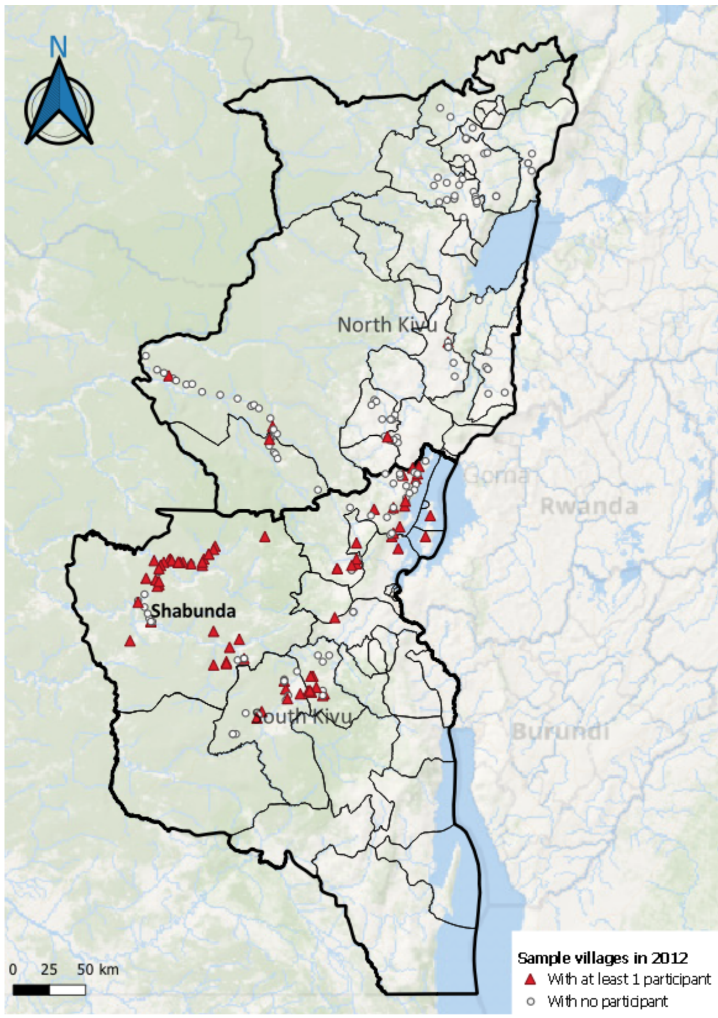
Notes. White dots are the core sample from South Kivu. We sample 8 households from each village in the core sample and collect information about household economic history, attack history, participation history, and the motives to participate in an armed group. For some of the villages in the core sample (white dots with blue circles), we sample extra households to collect information about participation history and motives to participate. Blue dots are the extra village sample from North Kivu; from each village, we sample 6 households and collect information about household economic history, attack history, and participation history.

Figure B.5: Replication: Militias Predominate in the Conflict



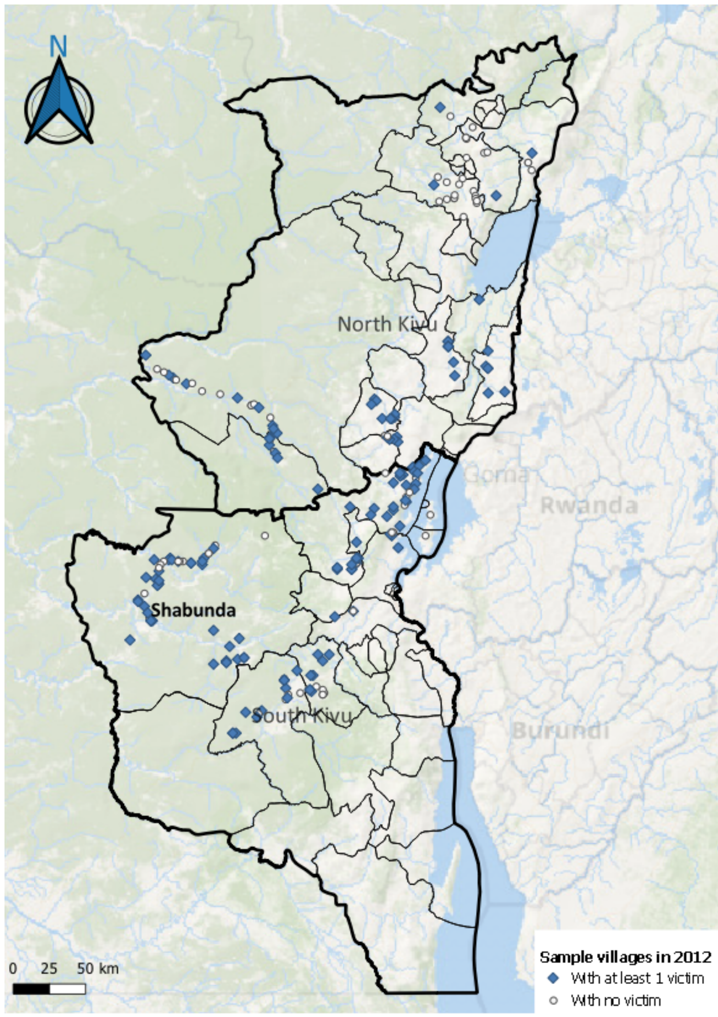
Notes. This figure replicates Figure 2.1 by including extra village sample from North Kivu. Panel A presents the average number of attacks per village by each type of armed group. Panel B presents the number of participants in each type of armed group for a village in the core sample from South Kivu. We first obtain the share of respondents who report to have participated in a group during an episode where an armed group controlled the village. Then, we use the village size we recorded in the village survey, and the number of surveyed villages (n=133) to construct a village-level estimate of the number of participants. The mean village size in the core sample is 203 households. Panel C presents the number of village governance episodes.

Figure B.6: Spatial Distribution of Participation Episodes in Militias



Notes: Red triangles are villages in 2012 where at least one respondent has participated in any armed group up to 2012.

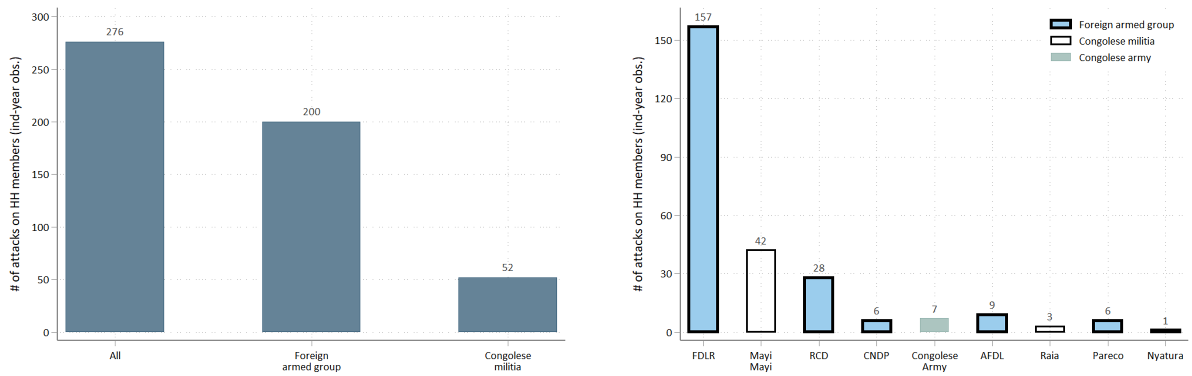
Figure B.7: Spatial Distribution of Attacks against the Sample Households



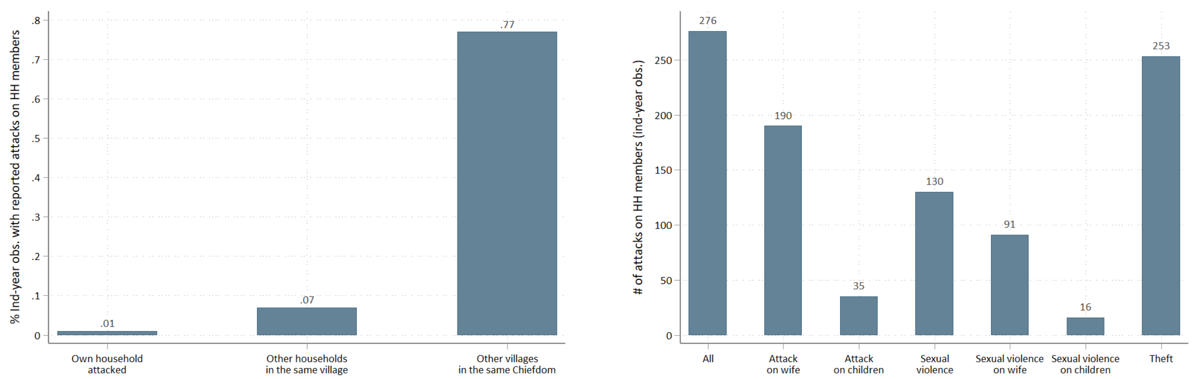
Notes: Blue diamonds are villages in 2012 where at least one respondent has experienced an attack on household up to 2012.

Figure B.8: Perpetrators and Targeted Persons in the Recorded Violent Attacks

Panel A. Perpetrators

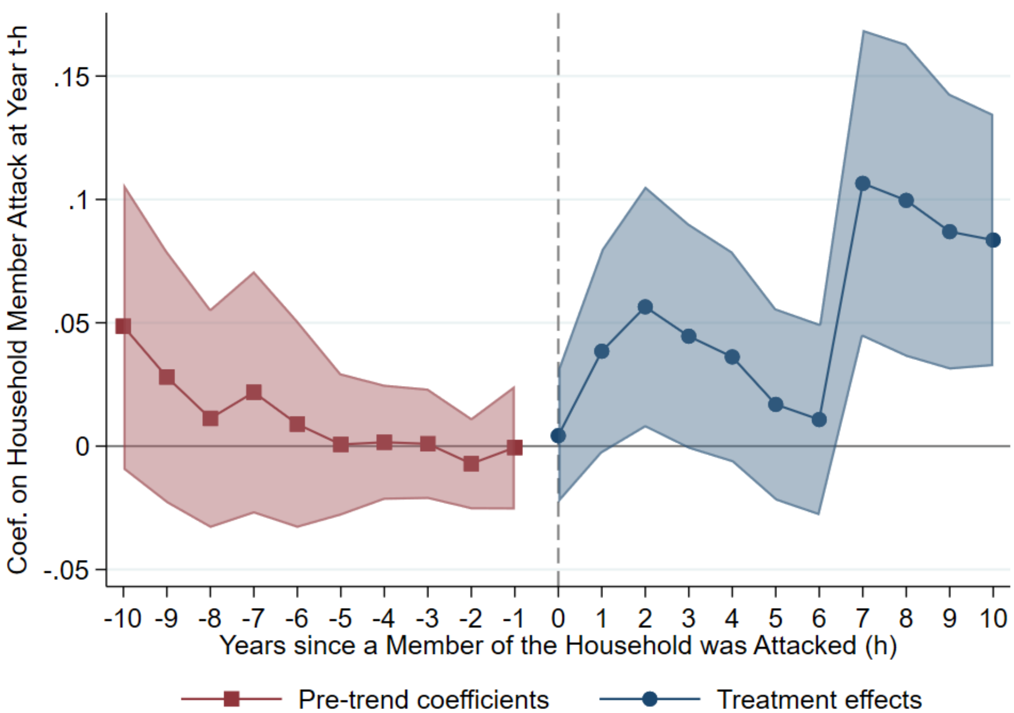


Panel B. Targeted Persons



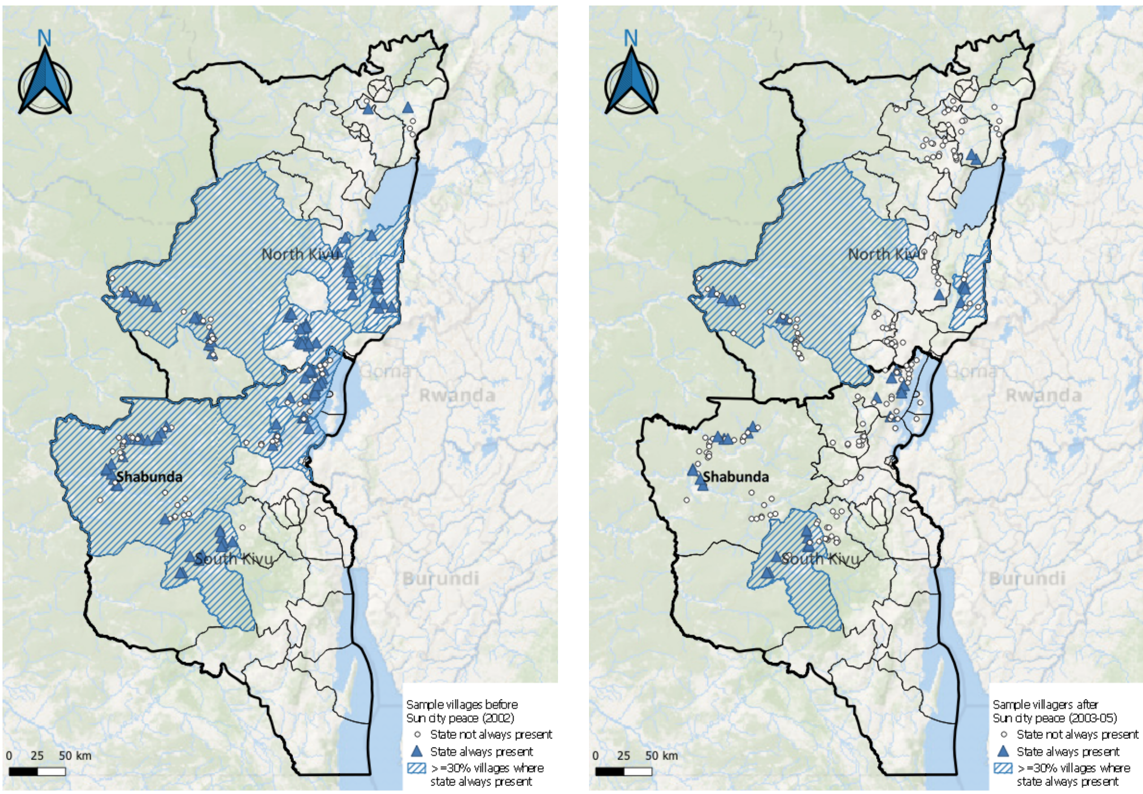
Notes: Panel A describes the perpetrators of the attacks. It uses the sample of 276 reported attacks that targeted the households of the respondents from South Kivu and shows their distribution by perpetrator. In the right quadrant, blue bars with solid outline refer to foreign-led armed groups; hollow bars refer to militias; green bar refers to Congolese national army. Panel B describes the individuals directly affected in each of these attacks. The left quadrant uses the whole sample of attacks to have happened on the household members of respondents, as well as other households in the same village and in the same Chiefdom. Based on this information, it shows the percentage of individual-year observations in which the own household was attacked, other households in the same village were attacked, or other villages in the same Chiefdom were attacked. The right quadrant decomposes all attacks on the household members by the type of actions that were conducted (not mutually exclusive), respectively: attack on the spouse, attack on children, attack with sexual violence, attack involving sexual violence on respondent's spouse, attack involving sexual violence on respondent's children, attack in which household property was stolen.

Figure B.9: Past Victims are Over-Represented in Militia Chapters Today—Dynamic Visualization



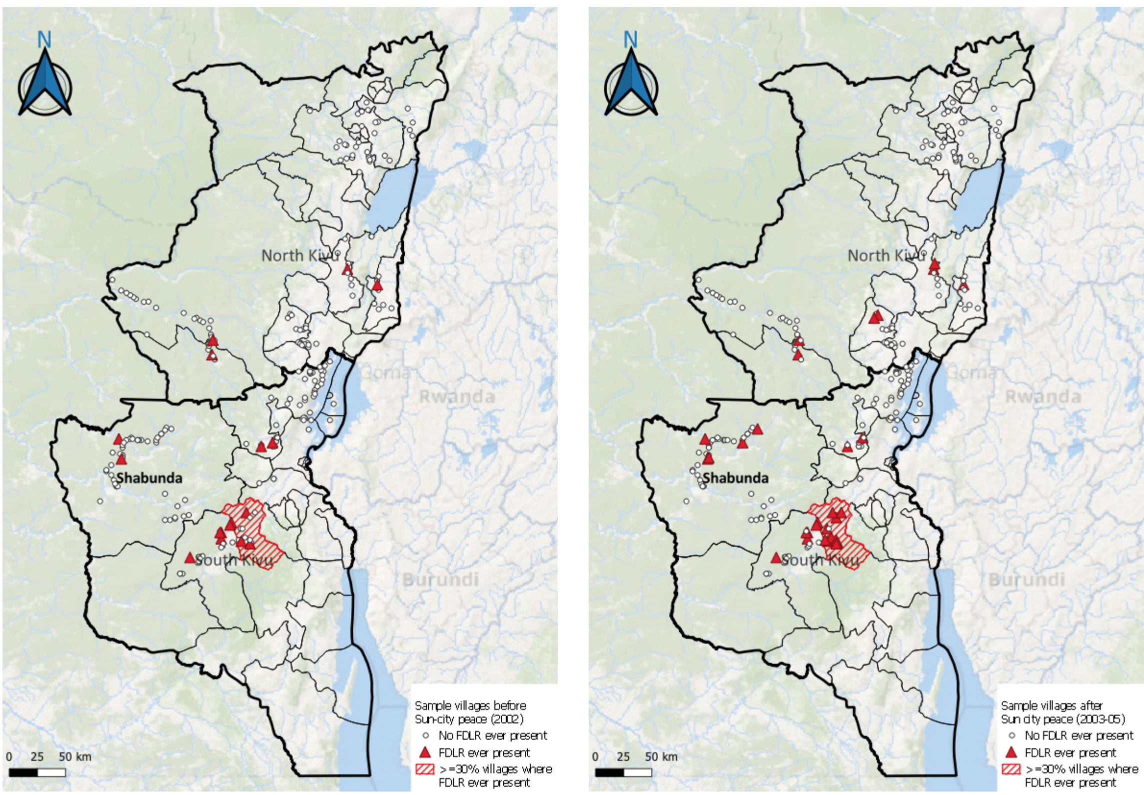
Notes: This figure shows Borusyak et al. (2024)'s event study estimators of the coefficients in Equation 2.1, γ_h, for $h \in [-10, 10]$. The dependent variable is an indicator taking value one in years and villages observations in which the respondent participates in a militia chapter formed in the village, and zero otherwise. We include observations between 1995 and 2013. All regressions include individual, village, year, and age fixed effects, and cluster two-way at the individual respondent and the village*year levels. We show 95% confidence intervals.

Figure B.10: The Predecessor Vacuum as a "State" Vacuum: Presence of the Regional Army RCD



Notes: This figure shows the presence of the Congolese army before and after the end of the Second Congo War, i.e. around the Sun-City peace agreement (2003). Since the peace agreement took place in 2003, our indicator of Congolese army presence in 2003 captures the presence of the Congolese army in the months of 2003 leading up to their removal. Thus, in the post-agreement map on the right, a blue triangle is a village where the Congolese army is always present between 2003–05, and a white dot is a village where Congolese army is not always present between 2003–05. The blue areas are chiefdoms where at least 30% are controlled by Congolese army; the cutoff 30% is selected because among cheifdoms where the Congolese army is present, on average, roughly 30% villages are controlled by the Congolese army.

Figure B.11: The Predecessor Vacuum Was Not a Security Vacuum: Presence of FDLR Predatory Group

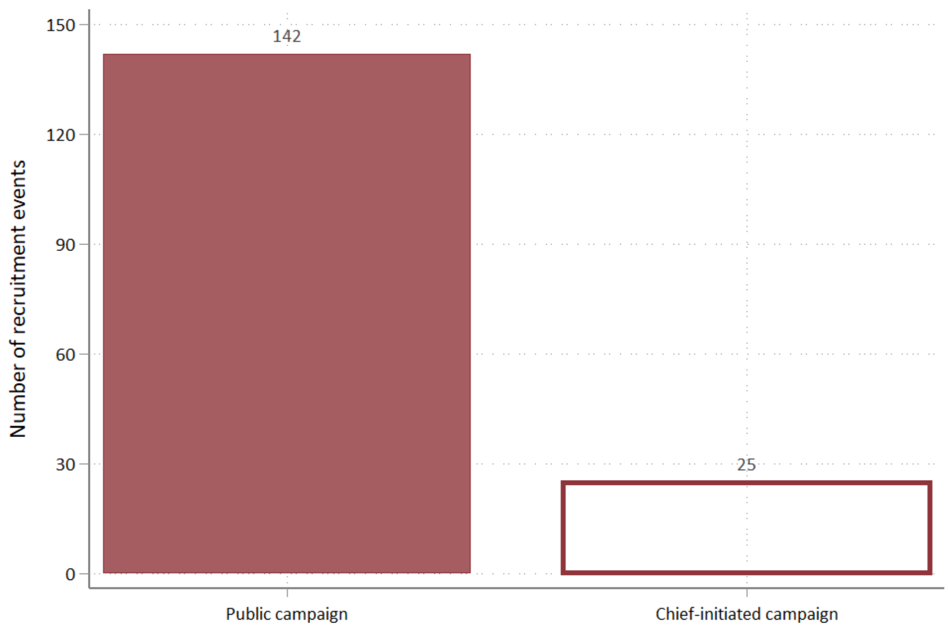


Notes: This figure shows the presence of the FDLR, a foreign-led predatory armed group known to be violent against civilians around the time of the Sun-City peace agreement (2003). In the post-agreement map on the right, a red triangle is a village where the FDLR is present in any year between 2003–05, and a white dot is a village where the FDLR is not ever present in any year between 2003–05. The red areas are chiefdoms where at least 30% are controlled by the FDLR.

Figure B.12: An NDC Recruitment Campaign Organized by a Village Chief

Notes: Sheka, former NDC's General, in a recruitment campaign organized by the village chief. The village chief presides over the campaign and sits at the back. Source: NDC media obtained by one of the authors.

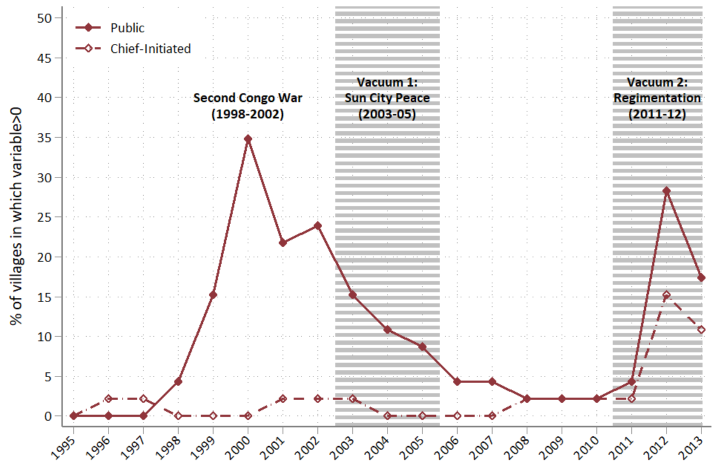
Figure B.13: Campaigns



Notes: This figure shows the frequency of different types of recruitment campaigns in the core sample. The recruitment data are collected from the village chief survey, where for each episode of armed group governance, we ask whether a public campaign or a chief-initiated campaign has taken place.

Figure B.14: The Regimentation Caused a Rise in Campaigns: Times Series

Panel A. Villages in Shabunda



Panel B. All Other Villages

Notes: We use both the core sample from South Kivu and the extra village sample from North Kivu to present the yearly trend. The red thick solid line shows the fraction of villages in which a public campaign takes place in each year. The red dashed line shows the fraction of villages in which a chief-initiated campaign takes place in each year. Panel A restricts the sample to Shabunda, the district affected by the Regimentation's induced state vacuum of 2011. Panel B shows the yearly trend for the remaining of the sample. Left and right grayed areas indicate years in which documented policy-driven state vacuums were associated to the rise of the first and the second, larger, Raia, respectively.

Appendix C

Additional Materials for Chapter 3

C.1 Data and Measurements

Perception questions. To guide through our conceptual classification of all job aspects, suppose a worker decides to stay for 2 periods. In the first period, she gets paid by $w + r(y)$, where w is the base salary, $r(y)$ is the wage premium determined by her production $y = y(\theta, e)$, which is itself a function of worker's productivity θ and effort e with a convex cost function $c(e)$. In the second period, she may be fired by the firm by a probability of $\delta(y)$ and enjoy zero utility. If she stays on the job, she has a probability of $p(y)$ to get promoted to upper-level positions where she enjoys a fixed salary w_H. If the worker is not promoted, she gets paid by a fixed salary $w_L < w_H$. Suppose there is no discount of future utility, and the amenities add a constant utility term a to the worker. The expected utility of working in the industrial park can be broken down into four parts:

$$\begin{aligned} U &= w + r(y) - c(e) + (1 - \delta(y))(p(y)w_H + (1 - p(y))w_L) \\ &= \underbrace{w}_{\text{(i)}} + \underbrace{p(y)(1 - \delta(y))(w_H - w_L)}_{\text{(ii)}} + \underbrace{r(y) + (1 - \delta(y))w_L}_{\text{(iii)}} + \underbrace{a}_{\text{(iv)}} - c(e) \end{aligned}$$

The first part (i), w, represents the entry-level career incentive. In the context of the industrial park, we ask each respondent to guess the base salary for all entry-level operators in the first month (1,000 ETB), and the percentage of new hires assigned to entry-level (90%). This does not depend on workers' effort level. There is a chance that high-productivity workers can be assigned to upper-level positions (mostly quality control team); given the small percentage of such workers, the entry-level salary is the same for most workers regardless of productivity.

The second part (ii), $p(y)(1-\delta(y))(w_H - w_L)$, relates to career ladder in the long run. $w_H - w_L$ is the salary premium of upper-level positions; $p(y)$ is the promotion likelihood to upper-level positions. To simplify the survey questions, we ask each respondent to guess the average salary of upper-level positions (2,413 ETB), and the percentage of entry-level operators being promoted to upper-level in one year (15%).

The third part (iii), $r(y) + (1-\delta(y))w_L$, relates to performance pay and bonus which depends on productivity and effort on the same level position. In particular, we model the wage premium $r(y)$ as a function of worker's production, which is a very common practice of firms to decide worker's performance pay. To simplify the question, we ask each respondent to guess how much a top-10% entry-level worker can earn more than average entry-level workers (400 ETB). The second component, $(1-\delta(y))w_L$, can be interpreted as the salary if the entry-level worker manages to stay in the firm. Most firms designs a tenure bonus for workers who stay more than one year. We thus ask each respondent how much more an entry-level worker can earn if she stays one year after (300 ETB). In addition, we ask workers how many of the 10 major firms provide attendance bonus, a major type of bonus relevant to workers' effort and all 10 major firms provide. We also collect respondents' perception of the likelihood of being fired in the first month $\delta(y)$ (10%).

The fourth part (iv), a, captures all utility terms regardless of workers' positions, productivity type, or effort. This includes: number of days per week workers are required to work (6 days), hours per day (8 hours), average overtime hours per week (7 hours), average minutes per day allowed during work (30 minutes), number of the 10 major firms providing free transportation instead of transport subsidies (4), and number of the 10 major firms providing free lunch instead of lunch subsidies (6).

Distribution of all these 14 perceptions is summarized in Table 3.1. We calculate relative perceptions as the difference of workers' answers to the benchmark divided by the benchmark. The benchmark information of average salary of upper-level, promotion likelihood, entry-level salary in the first month, and the percentage of new hires assigned to entry-level is calculated from the current worker survey conducted by EIC during October 2021 and February 2022. The rest of the benchmark information is calculated from the qualitative interview with 10 major firms during November and December 2021.

In general, workers have a roughly correct idea of jobs in the industrial park, but with great variations. Workers tend to underestimate the percentage of new hires assigned to entry-level positions, top performance salary premium, the number of firms providing attendance bonus, and overtime hours per week, but overestimate the hours per day required to work and number of firms providing free transportation. Interestingly, most workers guess correctly how many minutes of break per day allowed at work (30 minutes).

We chose to focus on career ladder for four reasons. First, workers learn about other job aspects fairly quickly on the first few days of work. Firms provide detailed information of entry-level career incentives, performance pay scheme, bonus, and amenities during orientation. The grading center is also giving out brochures to job applicants with basic information. Neither firms nor the grading center provides detailed information of the career ladder, partly because they think career ladder matters less for most entry-level workers.

Second, career ladder is listed as one of the most important job aspects for workers. Each respondent is asked to choose three most important job aspects from a list of options. Table C.1 shows the proportion of workers choosing which item as the first, second, and third most important job aspects. The last column shows the proportion of workers listing which item in the top 3 job aspects. In general, career ladder aspects (upper-level salary, chance of promotion to upper-level in one year) are listed consistently as the #3 or #4 aspects during job search. 34.7% workers listed upper-level salary in the top-3 job aspects, 35.9 % listed chance of promotion to upper-level in one year in the top-3 job aspects, right below "providing good benefits" (61.6%) and "entry-level salary in the first month" (46.1%), the two job aspects the grading center and firms have been trying very hard to inform all job applicants.

Third, it is difficult for workers to learn about true career ladder through their social network. During baseline survey, we asked each respondent the number of family and friends they know who worked in the industrial park before. In addition, we presented 5 names of treated workers in the previous two weeks and asked how many of these names they recognize. 39% of the respondents know at least one person who worked in the industrial park before or were treated during our survey. Figure C.1 compares the level of relative perceptions of these workers to those who know no one from the industrial park before. Indeed, networked workers have more correct perceptions on amenities. However, they have very similar levels of misperceptions on salary after promotion, promotion likelihood from entry-level, entry-level salary in the first month, the likelihood of being fired in the first month, and performance salary premium. They are even more biased in terms of the percentage of new hires assigned to entry-level, tenure bonus, or the number of firms providing attendance bonus. The evidence suggests workers may learn work amenities efficiently through social network, but not so in terms of career incentives in short or long run.

Last, misperceptions of career ladder are the main predictor of early turnover. Figure 3.2 regresses workers' quitting before signing a formal contract on the 14 relative baseline perceptions, only among control cohorts. Results suggest a significant negative correlation between early turnover and perceived salary after promotion: workers who overestimated salary after promotion by 1 standard deviation are 5.93% less likely to quit before signing a contract. This is the only negative correlation observed in the regression; in fact, workers with overoptimistic baseline perceptions in entry-level salary of the first month or number of firms providing attendance bonus are more likely to quit early, aligned with the fact that firms usually provide these two pieces of information on the first day of work and may dissuade these overoptimistic workers at the beginning. It is thus very likely that workers with overoptimistic baseline perceptions of career ladder may stay in the firm for too long, only to find out a lower salary after promotion after they sign a formal contract and quitting becomes more costly.

Details of other baseline characteristics.

Demographics. During baseline, we asked each respondent of their age, marital status, whether their family is from Hawassa where the industrial park is located, whether they speak Sidamanagna (the main local language) or Amharic (the national language) at home, and their religious belief.

Education and experience. We asked each respondent of their education background and work experience. Most respondents only graduate from 8th or 10th grade. 31% graduate from high school; another 31% either graduate from vocational training school (TVET) or are educated in the college. Only 17% have any previous work experience, 11% have any work experience in garment sector.

Skill measures. We conducted multiple tests in this following.

1. Memory test: Enumerator would read a series of numbers and ask the respondent to repeat. For example, given a random number series $\{8, 1, 4, 2, 5, 6, 7\}$, enumerator would first say 8 and ask the respondent to repeat 8. Then, enumerator would read $\{8, 1\}$ and ask the respondent to repeat again; if the respondent repeated them correctly, the enumerator would add the third number to the sequence, until the respondent cannot repeat correctly the number sequence. The average length of the number sequence the respondent can repeat correctly is 5.32.

2. Raven score: Enumerator would conduct a simplified 12-question standard Raven test with each respondent. For each question, respondent would be asked to select an object to fill in a simple geometric pattern. The average Raven score is 3.90.

3. Knowledge games: Enumerator asked two additional questions to test respondents' common knowledge, (i) what year Prime Minister Abiy Ahmed got Nobel Peace Prize (2019, or 2012 in Ethiopian calendar), and (ii) how many regions in Ethiopia (10, this is a very relevant question because the 10th region Sidama, where the industrial park is located, was only recently approved in 2019). 46% and 39% respondents answered correctly the first and second question, respectively. We then extract a principal component from the four measures above to construct a normalized cognitive score.

4. Dexterity games: Enumerator conducted two additional games to capture workers' dexterity skills, following the previous grading test conducted in the grading center. (i) Finger coordination: Respondent was asked to take one pin ball out from a case,

move it through a specific design, catch the pin ball with the other hand, and put it in another case. Enumerator then calculated the number of pin balls each respondent can relocate within 60 seconds. The average number is 34.80. (ii) Threading needles: Respondent was asked to thread three needles as fast as possible. Enumerator then calculates the number of needles respondent to thread within 60 seconds. The average number is 11.78. We then extract a principal component from these two measures to construct a normalized dexterity score.

Social network. We asked each respondent the number of family or friends they know who worked in the industrial park before and the number of family or friends who applied for the job together on the same day. On average, respondents know 2.30 people who worked in the industrial park before and was accompanied by 2.98 friends on the same day. Then, we presented 5 names of treated workers from the previous two weeks of survey and asked how many of these names each respondent recognized. On average, only 4.57% respondents recognize any treated worker from the list. We then extract a principal component from these three measures to construct a normalized social network score.

Career plan and motivations. We first asked each respondent whether they planned to start their own business within 5 years; on average, 54% respondents expressed having such a plan. We then asked how long each respondent planned to stay in the industrial park, and what are the three most important aspects during job search. On average, worker plans to stay for 3.75 years, and 20% workers care about long-run career ladder during job search. We then asked for the reasons why respondent applied for the jobs in the industrial park: because she wants to learn skills, because the future salary is attractive, or because the job is interesting. 89%, 48%, and 80% respondents agreed with the three reasons, respectively. We then extract a principal component from these six measures to construct a normalized intrinsic motivation score.

C.2 Additional Tables and Figures

Table C.1: Self-reported Importance of Job Aspects

	First (%)	Second (%)	Third (%)	Listed in top 3 (%)
Amenities, performance pay, and bonus				
Provide good benefits and bonus	13.1	28.1	20.4	61.6
Reasonable work hours	4.8	8.6	12.6	25.9
Interesting task	5.7	8.2	13.6	27.5
Skill development	4.8	6.1	11.7	22.6
Good management	9.7	6.4	9.0	25.1
Entry-level career incentives				
Entry-level salary in the first month	32.2	8.0	6.0	46.1
Entry-level salary after 1 year	2.2	2.4	1.4	6.0
Career progression				
Upper-level salary	11.0	13.4	10.3	34.7
Chance of promotion to upper-level in 1 year	8.9	13.7	13.3	35.9
Others	7.7	5.2	1.7	8.6

Notes: This table shows workers' ranking of job aspects during job search. Each respondent was asked to choose three most important job aspects from a list of options. The last column shows the percentage of workers choosing each item as one of the top 3 job aspects.

Table C.2: Balance Table: Baseline Misperceptions of Job Aspects

	Mean outcomes					Diff
	All	Control		Treated		
Observations		566		637		
A. Career progression						
Salary after promotion (10 USD)	0.24	0.17	(1.04)	0.30	(0.96)	0.13*
% entry-level workers promoted in one year	0.18	0.25	(1.10)	0.11	(0.90)	-0.14*
B. Entry-level career incentive						
Entry-level salary first month (10 USD)	0.23	0.29	(1.05)	0.19	(0.95)	-0.11
% new hires assigned to entry-level	-1.11	-1.14	(1.01)	-1.08	(0.99)	0.06
% new hires fired first month	0.12	0.16	(1.06)	0.08	(0.94)	-0.08
C. Performance pay and bonus						
Top performance salary premium	0.03	0.01	(0.99)	0.05	(1.01)	0.04
Tenure bonus, entry-level, one year	0.18	0.25	(1.07)	0.12	(0.93)	-0.13*
# of 10 major firms providing attendance bonus	-1.28	-1.26	(0.99)	-1.30	(1.01)	-0.04
D. Amenities						
Days per week required to work	-0.64	-0.63	(0.98)	-0.66	(1.02)	-0.03
Hours per day required to work	0.58	0.63	(1.02)	0.54	(0.99)	-0.09
Overtime hours per week	-0.57	-0.59	(1.05)	-0.55	(0.96)	0.04
Minutes of break per day allowed	0.04	0.06	(1.04)	0.02	(0.96)	-0.03
# of 10 major firms providing free transport	0.79	0.78	(0.98)	0.80	(1.02)	0.02
# of 10 major firms providing free lunch	0.11	0.10	(1.00)	0.12	(1.00)	0.03

Notes: This table shows balance between the baseline perceptions of treated and control workers. Baseline bias is the distance between the level of the prior belief and the level of the benchmark, divided by the standard deviation of the prior. Standard deviations in brackets. We compute the difference in the last column; standard errors are clustered at the cohort (day of hire) level. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C.3: Robustness: Functional Form of Perceptions

VARIABLES	(1) Quit early	(2) Quit early	(3) Quit early	(4) Quit early	(5) Quit early
Updated belief of upper-level salary	-0.005**	-0.010***			-0.011***
	(0.002)	(0.003)			(0.003)
	[0.015]	[0.002]			[0.002]
Updated belief of promotion likelihood			-0.005	-0.007	-0.006
			(0.004)	(0.006)	(0.005)
			[0.232]	[0.233]	[0.298]
Observations	1,165	1,165	1,167	1,167	1,165
R-squared	0.005	0.003	0.008	0.008	0.008
Specification	OLS	IV	OLS	IV	IV
Cluster	Cohort	Cohort	Cohort	Cohort	Cohort
Dep var mean	0.407	0.407	0.407	0.407	0.407
F-stat		76.39		562.9	11.44

Notes: This table reports main results with different functional forms of perceptions. In all specifications the dependent variable is a dummy variable equal to 1 if the worker left the industrial park prior to signing a formal contract, which occurs after completing the 45-day trial period. Updated belief of upper-level salary and promotion likelihood is the level, instead of natural logarithm, of the posterior belief of the after-promotion salary and the likelihood of being promoted. Columns 1 and 3 report OLS estimates and Columns 2, 4, and 5 report instrumental variables estimates. F-stat reports the first-stage F-statistic for IV estimations. Standard errors are clustered at the cohort (day of hire) level. See Section 3.5.4 for detailed discussion. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C.4: Robustness: Examining Exclusion Restrictions

VARIABLES	(1) Quit early	(2) Quit early	(3) Quit early	(4) Quit early	(5) Quit early	(6) Quit early	(7) Quit early
Updated belief of upper-level salary	-0.418**	-0.292*	-0.297*	-0.275**	-0.313*	-0.465**	-0.273
	(0.208)	(0.174)	(0.179)	(0.082)	(0.180)	(0.215)	(0.212)
	[0.044]	[0.093]	[0.098]	[0.010]	[0.081]	[0.030]	[0.197]
Observations	1,165	1,104	1,133	1,163	1,163	1,152	1,137
R-squared	0.000	0.159	0.052	0.002	0.005	0.034	0.035
Specification	IV	IV	IV	IV	IV	IV	IV
Cluster	Cohort	Cohort	Cohort	Firm	Cohort	Cohort	Cohort
Dep var mean	0.407	0.407	0.407	0.407	0.407	0.407	0.407
F-stat	33.33	302.7	349.9	32.89	385.5	31.13	281.4
Additional control	No	Baseline demo Demographics	Baseline Perceptions	Firm FE	Update on Own type	Information Retention	Update on Other perceptions

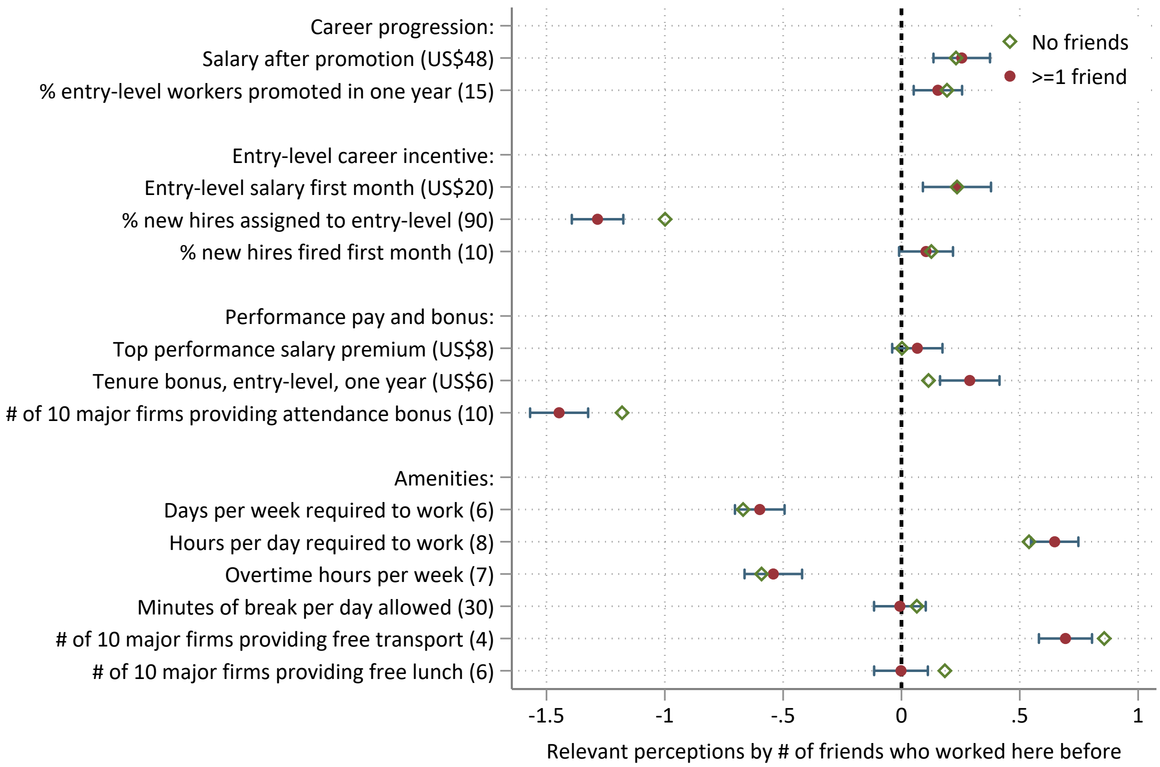
Notes: This table reports robustness check of the IV estimates of equation 3.2. In all specifications the dependent variable is a dummy variable equal to 1 if the worker left the industrial park prior to signing a formal contract, which occurs after completing the 45-day trial period. Updated belief of upper-level salary is the natural logarithm of the posterior belief of the after-promotion salary. F-stat reports the first-stage F-statistic for IV estimations. Column 1 reports our baseline specification. Column 2 includes all baseline workers' demographic controls. Column 3 includes all baseline workers' perceptions of the 14 job aspects. Column 4 includes fixed effects for the initially assigned firm and clusters standard errors at the firm level; for the workers who are not assigned a firm, we group them into one cluster. Column 5 includes interaction terms for the treatment indicator and two variables that capture workers' potential update of own type (expected own salary in one year divided by expected average salary in one year computed from workers' answers, expected own salary in one year compared to benchmark expected average salary in one year). Column 6 includes interaction terms for the treatment indicator and several variables that affect the retention of information (an indicator of having previous work experience, a standardized raven score, an indicator of having friends who will join the industrial park later-on, and an indicator of whether the worker reports joining the industrial park because they want to develop skills). Column 7 adds interaction terms of the treatment status with all other 13 baseline perceptions. Standard errors are clustered at the cohort (day of hire) level. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table C.5: Spillover

VARIABLES	(1) Quit early	(2) Quit early	(3) Quit early	(4) Quit early
Treated cohort * Baseline bias of upper-level salary	0.262**	-0.162		
	(0.121)	(0.200)		
	[0.035]	[0.422]		
Updated belief of upper-level salary			-0.385*	-0.370*
			(0.200)	(0.198)
			[0.055]	[0.061]
Know previous treated workers			-0.150***	
			(0.050)	
			[0.003]	
High network index				-0.118***
				(0.035)
				[0.001]
Observations	1,166	543	1,165	1,165
R-squared	0.003	0.007	0.005	0.011
Specification	RF	RF	IV	IV
Cluster	Cohort	Cohort	Cohort	Cohort
Sample	All	Control workers	All	All
Dep var mean	0.407	0.407	0.407	0.407
F-stat			38.03	34.10

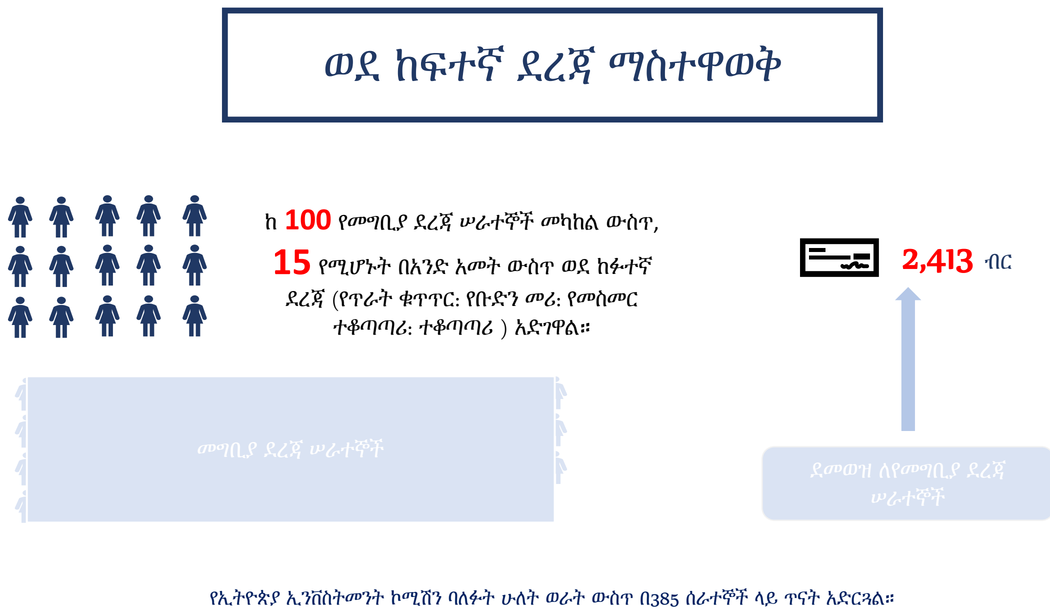
Notes: This table reports estimates of spillover effects. In all specifications the dependent variable is a dummy variable equal to 1 if the worker left the industrial park prior to signing a formal contract, which occurs after completing the 45-day trial period. Baseline bias is the natural logarithm of the baseline perception minus the logarithm of benchmark. Updated belief of upper-level salary is the natural logarithm of the posterior belief of the after-promotion salary. Knows previous treated workers is a dummy variable equal to 1 if the respondent knows workers who were previously treated. High network index is an index constructed using the principal component of the number of previous treated workers that the worker knows, number of friends who joined the industrial park before, number of friends who joined the industrial park today. Columns 1 reports OLS estimates of the reduced form, only including control workers. Columns 2 and 3 report IV estimates. F-stat reports the F-statistic for the excluded instruments in the first stage. Standard errors are clustered at the cohort (day of hire) level. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Figure C.1: Baseline Misperceptions by Social Network



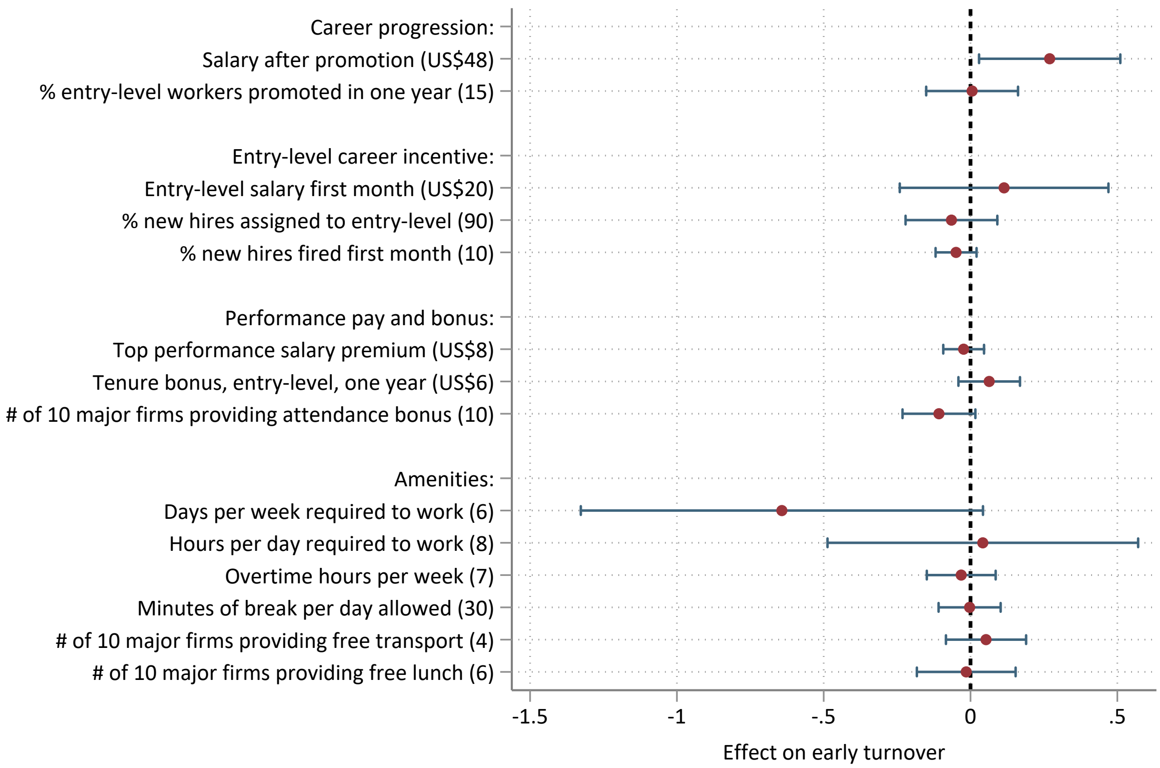
Notes: This figure shows the average of the 14 relative perceptions by social network. The hollow diamond dots are the average perceptions of workers who have no family or friends working in the industrial park before, nor do they recognize any of the 5 treated workers during baseline. The red solid dots are the average perceptions of workers who have at least 1 family member or friend working in the industrial park before, or they recognize at least 1 treated worker during baseline. All benchmark information is shown in the brackets on the vertical axis. Relevant perceptions are calculated as the difference between workers' perceptions and benchmark divided by the benchmark.

Figure C.2: Visualization for Information Treatment



Notes: This figure shows the visualization card enumerators used during the information treatment. The infographic on the left states that 15 out of 100 workers were promoted to an upper-level position (quality Control, team leader, line supervisor, supervisor) within one year. The infographic on the right states the average salary for an upper-level position. The bottom note states that this was estimated with a survey of 385 workers conducted by the Ethiopian Investment Commission.

Figure C.3: Placebo Test: Potential Update of Other Perceptions



Notes: This figure presents the results from the placebo test of the main result in Table 3.3, Column (2). We report the reduced-form estimate from Equations 3.2 and 3.3 where the dependent variable is the dependent variable is a dummy variable equal to 1 if the worker left the industrial park prior to signing a permanent contract, which occurs after completing the 45-day trial period, and the main independent variable is the interaction of treated cohort and baseline bias of each of the 14 job aspects. Baseline bias is the distance between the natural logarithm of the prior belief and the natural logarithm of the benchmark. Standard errors are clustered at the cohort (day of hire) level. We show the magnitudes and the 95% confidence intervals.