Jobs and political participation - Evidence from a Field Experiment in Ethiopia[*]

Lovise Aalen[1], Andreas Kotsadam[2], Janneke Pieters[3], and Espen Villanger[1]

[1]Christian Michelsen Institute
[2]Ragnar Frisch Centre for Economic Research
[3]Wageningen University

February 17, 2021

Abstract

We identify the effects of employment on political participation by collaborating with 27 large companies in Ethiopia to randomly assign jobs to equally qualified female applicants. The job offers increase formal employment and earnings but we can clearly reject any positive effects on political participation. We find no effects on political interest, raising issues, or protest activity and we find negative effects on participation in community meetings. We further find that job offers reduce internal and external political efficacy, suggesting that employment may actually be politically disempowering. Our qualitative data suggest that several features of the jobs may contribute to lower political efficacy directly.

1 Introduction

Women have lower rates of political participation than men in most developing countries. A prominent explanation for this gender gap is the resource hypothesis, proposing that men have more resources in terms of education and income and therefore have the capacity to participate more (Brady et al., 1995; Isaksson et al., 2014; Verba et al., 1997; Schlozman et al., 1994, 1999). There is a strong expectation in the literature that formal employment, in particular, will increase women's political participation by increasing incomes, changing gender attitudes and identities, increasing their decision-making power within the household, giving them access to networks, and making them informed about their interests (Isaksson et al., 2014; Iversen and Rosenbluth, 2008; Robinson and Gottlieb 2019; Ross, 2008). On the other hand, employment is time consuming and may therefore reduce participation (Isaksson et al., 2014; Schlozman et al., 1999).

We identify the effects of women's formal employment on various forms of political participation in Ethiopia using a large scale field experiment where job offers were randomly assigned to qualified job applicants. We collected survey data before randomization, after 6 months, 12 months, 18 months, and finally after around three years. We find that the job offers increase formal employment and earnings, even after three years. We are clearly able to reject that the job offers have any positive effect on political participation and we even find that they reduce women's participation in community meetings.

Our short run analyses suggest that a likely mechanism for reduced political participation is increased time pressure, as we find reductions in leisure time and in time spent on social and religious activities for women offered a job. In the longer run, there is still a significant negative effect of the job offer on participation in meetings and no positive effect on other measures of participation, including political interest, raising issues, attending protests and intentions to vote. Contrary to the short-run, however, the longer run results show no effect of job offers on women's time use. In investigating the

mechanisms more fully we find a negative effect on internal and external political efficacy.

Our survey results are complemented by qualitative interviews, highlighting several aspects of the jobs that may be directly disempowering. In particular, workers are tightly controlled and often verbally abused; their tasks are repetitive; there are few chances of occupational advancement; and there is a clear gender hierarchy in the factories. The qualitative data, as well as data from interviews with the women's partners, also show that community meeting attendance is perceived as important. We therefore argue that employment likely has a politically disempowering effect on the women in our context.

Our paper contributes to a large literature on the determinants of political participation and most closely to the literature on its relationship with employment. The correlation between employment and participation is positive for both men and women and in both developed and developing countries (Aalen et al., 2019; Ross, 2008; Schlozman et al., 1999; Isaksson et al., 2014). Using data from the Afrobarometer, we show that this correlation holds in Ethiopia as well.

There is only limited evidence, however, for the *causal* impacts of employment. In many previous studies, which rely on comparisons between workers and non-workers, findings may be confounded by reverse causality and omitted variable bias. Without a credible design for causal inference it is difficult to rule out that political participation increases employment opportunities or that there is some third variable driving both employment and political participation (such as available time, aspirations, or social norms).

A number of previous studies employ research designs more suited to identify the causal effects of employment on political participation (see Margalit (2019) for an excellent and up to date overview). These studies are mostly from developed countries and most of them examine negative shocks, such as unemployment (Emmenegger et al., 2017; Guiso et al., 2017; Margalit, 2013; Burden and Wichowsky, 2014) and labor market

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disadvantage (Emmenegger et al., 2015). They yield mixed results: Emmenegger et al. (2015) and Margalit (2013) find no effects of unemployment on voter turnout. Burden and Wichowsky (2014) find that higher unemployment rates stimulate voter turnout, while Guiso et al. (2017) and Marx and Nguyen (2016) find that unemployment reduces political participation. Examining positive shocks, Brunner et al. (2011) and Charles and Stephens (2013) find that increased local level employment reduces voter turnout.[1]

We contribute to this literature in several ways. First of all, we identify the causal effect of employment on political participation using random assignment of job offers. While longitudinal data analysis allows controlling for time-invariant unobserved individual characteristics, and thus provides more compelling evidence than simple cross-sectional correlations, there are reasons to be worried that time variation in employment may still be endogenous. Most importantly, there may be unobserved factors that affect both changes in employment and changes in political participation, such as health shocks or onset of alcohol abuse at the individual level or housing market insecurities at the aggregate level. In fact, we demonstrate in our data that different research designs not relying on randomization produce biased estimates in our setting.

Secondly, we identify the effects of employment at the individual level. When identification relies on variation at higher levels of aggregation (such as regional exposure to global economic shocks) it is not possible to separate the effects of individual level employment from sociotropic concerns based on area level employment, such as area level inequality or concerns about the job status of neighbors. While the effects of aggregate economic conditions on political participation may also be of interest, we explicitly focus on the impact of an individual's own employment status, which is more clearly linked to the theoretical mechanisms of the resource hypothesis.

Thirdly, we are able to identify effects on a wide spectre of political behavior and

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[1]There is also a literature investigating other types of resources than employment. Two noteworthy examples are Gottlieb (2016) and Croke et al. (2016), whom both find decreased political participation of increased information and education respectively.

over a longer time period. The former is important when studying gender differences as such differences are usually larger for more regular and time consuming aspects of inter-electoral participation than for voter turnout (Carpena and Jensenius, 2020; Isaksson et al., 2014). It is also important in electoral authoritarian settings where voting may be perceived as mandatory yet inconsequential. The analysis over a longer time period is an important aspect as the effects of economic shocks on e.g., political attitudes are often transient (Margalit, 2019).

Finally, we analyze the effects of employment in an understudied context, namely in a low-income country in an early stage of export-led industrialization. The jobs are characterized by low wages, long work hours, and poor working conditions. While these jobs may offer less empowerment than the average job in an advanced economy, they reflect the typical reality for many blue collar workers - especially women - in low- and middle-income countries in initial phases of industrial development.

2 The Setting and Field Experiment

Ethiopia is one of the least developed countries in the world, with two thirds of employment in agriculture, only ten percent in industry, and 24 percent in services in 2019 (ILO, 2020). The Ethiopian manufacturing sector is growing quickly, however, and the Ethiopian Government is actively accommodating foreign direct investors through tax incentive schemes and the establishment of industrial parks. According to its national Growth and Transformation Plan for the period 2015/16 to 2019/20, the country aims to become the main manufacturing hub in Africa, with a focus on labor-intensive export-led manufacturing. An important goal is for this sector to stimulate formal employment, especially for youth and women.

For this study, we worked with 27 shoes and garment factories located in industrial parks across five different regions of Ethiopia: Tigray, Amhara, Oromia, SNNP, and

Dire Dawa. The location of the industrial parks is shown in Figure 1. The initial data collection was centered around a study of intimate partner violence (see Kotsadam and Villanger (2020)) and therefore data collection was restricted to women living with a partner at that time.
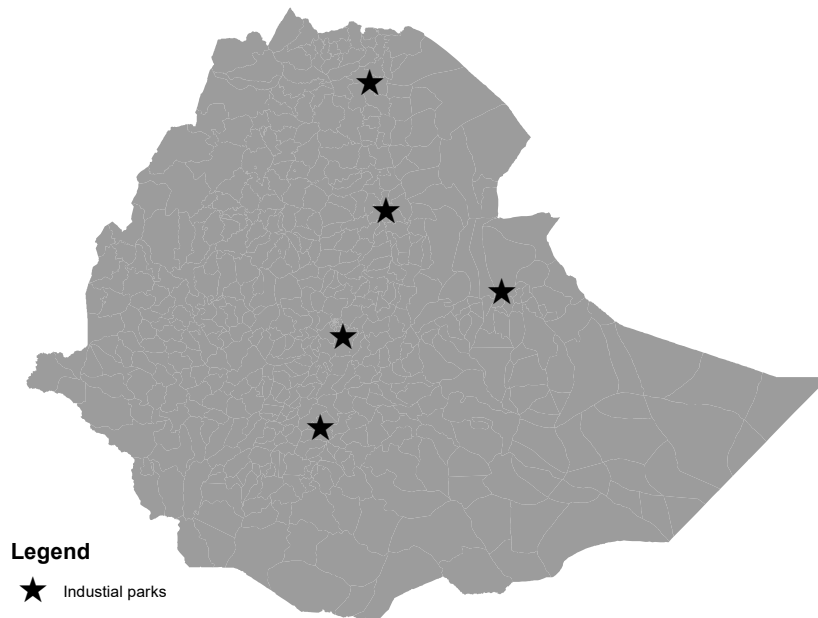

Legend

★ Industial parks

Figure 1: Industrial Parks

The factories hire in bulk. Applicants are asked to gather on a specific day and are screened for eligibility using verbal and physical tests. The factories involved in this study first assessed all job applicants and determined each applicant's eligibility for the job. From the pool of eligible candidates we created lists of women who were living with their partner. Women not living with a partner were hired using normal procedures.

From each factory's lists of eligible partnered applicants we randomly assigned half to receiving a job offer in the given factory (treatment) and the remaining women to a control group, in which no one received a job offer. The randomization was possible since the number of qualified applicants far exceeded the number of available jobs and the experiment did not change the number of jobs offered to women. The applicants on the lists were informed about the procedure before the randomization was conducted. The applicants perceived it as fair that everyone that is equally qualified gets an equal chance

of getting the job offer. We regard the randomization to be ethically justified for the same reason and we also note that the procedure eliminates the scope for hiring based on good looks, corruption and sexual extortion, which we heard stories about during our field visits. The research has IRB approval from The Norwegian Center of Research Data (Number 55793). See Appendix Section A.11 for a detailed discussion of ethical considerations and principles.

2.1 The jobs

In our sample, factory workers earned on average 1021 ETB per month in Wave 2 (around 43 USD according to the 2017 average exchange rate).[2] The women usually work for at least 8 hours per day and 6 days per week. Only 57 percent of factory workers reported being happy working at the factory and 23 percent reported health problems due to work. These results echo the results in Blattman and Dercon (2018), who randomize job offers in five Ethiopian factories and find that self reported poor health doubled in the treatment group.[3] Qualitative interviews with the women in our study by Halvorsen (2020) suggests it is ultimately women's own decision to accept the job offer and to continue or quit their job.

In our qualitative data, collected through semi-structured interviews, workers report heavy workloads under stressful and unhealthy conditions (see Appendix Section A.10 for a description of the qualitative methodology). Some women express that they would not be working in the factories if they had alternatives: "They make us work like donkeys. They profit and we suffer. I work for the sake of my child. If it was not for him, I would have quit a long time ago" (worker, Eastern Industrial Zone, November 2018).

A common understanding among the workers is that unless production targets are

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[2]For comparison, the estimated poverty line in 2016 was 7184 ETB per adult equivalent per year.

[3]They find that most people quit and that there was no effect on employment after a year. As compared to the firms in Blattman and Dercon (2018), our firms are more geographically spread and our sample includes mostly married women, whereas their sample includes mostly single men and women.

reached, they cannot leave work, even if official working hours are over. As explained by a worker in Eastern Industrial Zone: "It is compulsory to stay and finish the target. No one has ever dared to skip overtime before this is done".

In many factories, the workers' mobility is tightly controlled. Workers explain how they have to keep time when going to the toilet and that there are few breaks: "We do not get breaks and are under constant pressure to work." (worker, Dukem, November 2019). Women also report that they were not allowed to talk to their co-workers during working hours. In one factory, the manager explained how he uses a dog whistle to keep workers in line for lunch.

Knowledge about the harsh working conditions in the factories gradually reached the communities in which the workers live, and this has affected the way neighbors perceive the factory women and how factory women perceive themselves. As explained by one garment worker in Hawassa: "My neighbors insult me because of my job. They know that we have to go to the toilet at a restricted time and that we are working for many hours without a break" (worker, Hawassa, July 2017).

Almost all women told stories about abusive behavior from their supervisors, who would shout at them, insult them, and sometimes even use physical force to make them work faster or punish them for mistakes. Workers see few opportunities to communicate complaints to their superiors, and one worker witnessed coworkers who complain being fired immediately (worker, Dukem, November 2019).

In addition to the abusive behavior of supervisors, factory managers also display derogatory and belittling attitudes towards the female workers, who they consider to be primitive and uncivilized. A manager complained that "As many of the female workers come from an agricultural background, the women are lazy, very casual, have never worked for a clock and have never had a plan in life" (manager, Hawassa, June 2017). These attitudes and the supervisors' abuse are likely counterproductive for the development of

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the worker's self-esteem and confidence.

The nature of the work may also contribute to a lack of positive self-image. Most women do the same operations every day, and the only training they receive is on-site training in the first few days of their employment, restricted to the specific task they are going to perform. There are few opportunities for career advancement.

The lack of promotion opportunities is reinforced by a clear gendered division of labor. Many of the supervisors, trainers, and quality assurers are men. Women are seen as better at taking orders, especially from men. A factory manager in Hawassa explains: "Men do better than women in directing the women. When men direct women, they respect them well" (July 2017).

The managers express a hostile attitude towards trade unions and even though Ethiopian labor law prescribes the establishment of trade unions, unions are active in only two of the 27 firms in our sample (we describe the factories in Appendix section A.4). Government officials and representatives from the Confederation of Ethiopian Trade Unions tell us that labor laws are not enforced for fear of investors leaving the country. This skewed power balance also means that most factories are in practice exempted from basic labor regulations. Even if Government inspectors reveal violations of health and security requirements, for instance, they are unlikely to take the cases to court: "We don't want to sue the investors. You know our country is poor, so we need our daily bread. If for example a Chinese or Korean investor is taken to court, this may scare more investors away" (labor inspector, Dukem, November 2019).

2.2 Political context

Ethiopia is an electoral authoritarian regime where elections have been used to sustain the incumbent's power rather than opening up for competition between political parties. In the last elections in 2015, the ruling party won all seats in the House of People's Representatives (Arriola and Lyons, 2016). The ruling party, Ethiopian People's Revolutionary

Democratic Front (EPRDF), is a coalition of four ethnically based parties. Since it took power in 1991 up until 2018, the coalition was dominated by one of these, the Tigray People's Liberation Front (TPLF). Through a centralised party organization, EPRDF kept control of all regional governments around the country, and through repressive laws and harassment they restricted the space for opposition, free press and an independent civil society. Through fine-masked local administrative and political networks, the party kept control of the grassroots (Aalen and Tronvoll, 2009).

In 2018, a new alliance between two other parts of the EPRDF, the Oromo Democratic Party (ODP) and the Amhara Democratic Party (ADP), emboldened by resilient anti-regime protests since 2015, ended TPLF's dominance. A new party chairman and Prime Minister, Abiy Ahmed from ODP, introduced a range of democratizing reforms, described by observers as the start of a democratic transition (Temin and Badwaza, 2019). Among the reforms were the release of political prisoners, an invitation of banned opposition in exile back to the country, a revision of some of the repressive laws, and an opening up for independent media.

The initial optimism has recently been replaced by a concern for the political future of the country. While Abiy Ahmed has dissolved the EPRDF and established a new national, non-ethnic party, the Ethiopian Prosperity Party (PP), competing Oromo groups and the TPLF oppose the turn away from ethno-regional politics. The planned 2020 elections have been postponed and intra-Oromo and inter-ethnic conflicts threaten the political stability of the country.

2.3   Political participation at the local level in Ethiopia

The smallest administrative unit in Ethiopia, called the kebele, which corresponds to a neighborhood or small village, is central for local politics in Ethiopia. The kebele is a main service provider in the local communities, distributing for instance the right to public housing, access to health programs, and access to subsidized fertilizer, sugar or

cooking oil. The kebele office is also where most people sort out their bureaucratic affairs such as obtaining birth and marriage certificates and ID cards (Treiber, n.d.).

The kebele is a key area for citizen participation in Ethiopia. It is an arena for formal political participation through the kebele council, whose members are elected in local elections every five years.[4] Since 2005, the number of council members has increased dramatically from 15 to a maximum of 300 members, a substantial share of the average kebele population of around 4000 people (Aalen and Tronvoll, 2009). As the EPRDF has the majority in all kebele councils around the country, this formal participation has clearly been limited to the confines of the ruling party. For non-elected ordinary citizens, the kebele is the most important place for interaction with the government. Citizens are regularly called to meetings where the government informs and mobilizes support for new policies and guidelines, and where people can express their opinions and concerns. While kebele meetings are important for access to information and influencing local decision making, people may feel pressured to attend. A widespread popular perception has been that if people do not keep friendly terms with the kebele they lose benefits distributed by the kebele (Pausewang et al., 2002).

Our qualitative interviews confirm that kebele meetings are perceived as important for the women, and that these should not be missed. Attending kebele meetings can help facilitate a good relationship to the local administration, and thereby ease access to their services. Many of the women interviewed explain that they feel they missed economic opportunities because they had no time to attend kebele meetings due to their long factory work hours.

In addition to the kebele meetings, there are many other types of local community meetings in Ethiopia, most of which take place in local organizations that are either part of structures under the kebele or independent from the government and the party. In

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[4]The last kebele elections were held in 2013. Due to the uncertainty of the political transition in 2018, the elections have been postponed together with the national elections.

Appendix Section A.8 we describe these networks in more detail and we also show the evolution of membership in these networks over time.

Our qualitative data show that similar to the kebele meetings, the meetings in the sub-kebele structures are perceived as important for the women. Testimonies from women active in the sub-kebele networks tell how members are informed about practices and policies that can improve their lives.

2.4 Political participation and employment in the Afrobarometer

To investigate the correlation between political participation and employment in Ethiopia, and to compare Ethiopia to other African countries, we explore data from the fifth wave of the Afrobarometer (with data collected in 2013).[5] We see in Panel A of Table 1 that employed African women (defined as working for cash payment) are more likely to participate in politics and are more interested in politics. In Panel B we find the same sign of the correlations for women in Ethiopia, except for protest activity, which is negatively correlated with employment in Ethiopia. Hence, in line with the resource hypothesis, the correlation between employment and political participation is positive in Africa, and Ethiopia does not differ much from other African countries in this regard. In Appendix Section A.6 we further show that the gender gaps in political participation and employment in Ethiopia are also similar to those in Africa in general.

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[5]Ethiopia was not part of other Afrobarometer waves. The Ethiopian version of the 2013 Afrobarometer has been criticized and there has been caution against comparing democracy related indicators with the same indicators in other countries (Mattes and Teka, 2016). It is argued that Ethiopians interpret democracy differently. In addition, a high share of the respondents report that they think the survey enumerators in the Afrobarometer are part of the state. This implies that we should be cautious interpreting answers to sensitive questions, such as protests.

Table 1: Women's employment and political participation -Afrobarometer.

(a) Women in all countries.

	(1) Community	(2) Interested	(3) Raised	(4) Voted	(5) Protest
Has paid employment	0.021***	0.080***	0.024***	0.031***	0.016***
	(0.0068)	(0.0068)	(0.0068)	(0.0055)	(0.0037)
Control mean	0.53	0.52	0.47	0.80	0.07
N	26674	26496	26609	23392	26232
R-squared	0.00	0.01	0.00	0.00	0.00
Sample	Women	Women	Women	Women	Women

(b) Women in Ethiopia.

	(1) Community	(2) Interested	(3) Raised	(4) Voted	(5) Protest
Has paid employment	0.12***	0.065***	0.091***	0.099***	-0.044**
	(0.029)	(0.024)	(0.030)	(0.025)	(0.020)
Control mean	0.60	0.77	0.51	0.77	0.14
N	1151	1149	1133	1031	1087
R-squared	0.01	0.01	0.01	0.02	0.00
Sample	Women	Women	Women	Women	Women

Notes: Results from Afrobarometer round 5. Robust SE in parentheses.

3   Survey data

We surveyed women to collect baseline data (Wave 1) before they started working. The dates for the baseline data collection vary, depending on when the firms hired. Some of the firms hired new batches of workers several times during the period. We collected the first follow up data (Wave 2) around 6 months after the first interview, and subsequent follow up data at 12, 18, and 34 months after baseline (Waves 3-5). We show the number of interviews over time by wave in Figure 2. Out of the 1463 randomly assigned women in our baseline sample we managed to interview 1262 in the second wave and 1054 in the fifth and final wave. We show in Appendix Tables A1 and A2 that attrition is unrelated to treatment status. The only variable correlated with attrition is age, with older women being more likely to remain in the sample. The main reason for attrition is that respondents move to other areas.

Our survey data is rich both in depth and scope. In the following, we briefly describe the main variables used in our analysis. Full details concerning measurement and
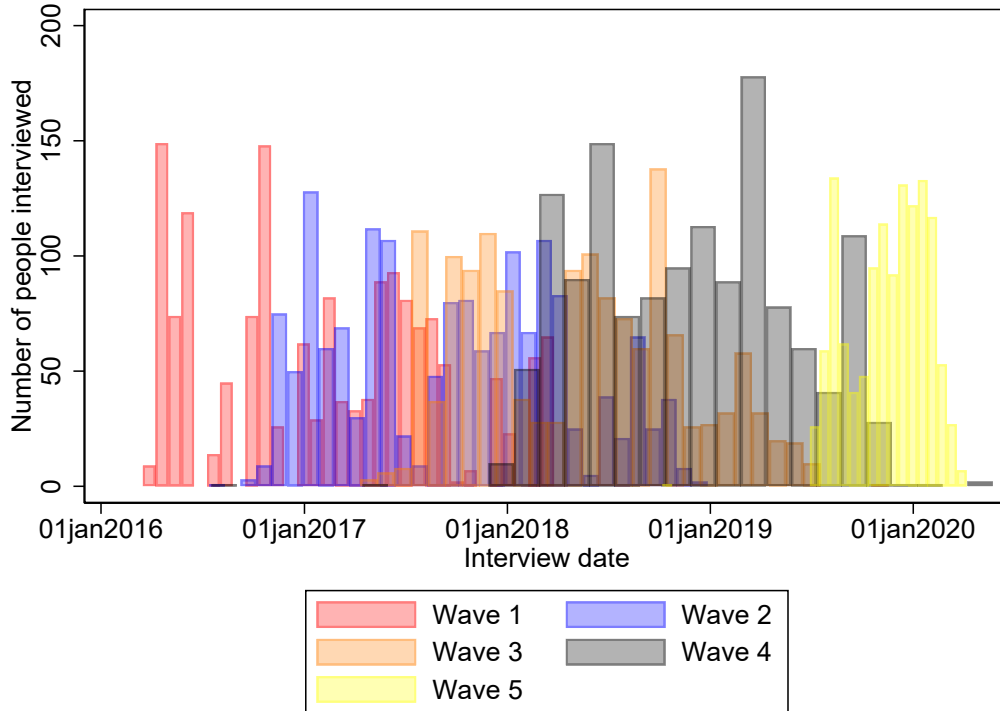
Figure 2: Interviews over time by waves

definition of all variables are given in Appendix Table A3. First of all, the surveys included a set of questions about political participation. Our main dependent variables are *Community meeting* and *Raised issue*, which capture whether the respondent attended a community meeting or got together with others to raise an issue during the past year. The community meetings are broadly defined and explained by the enumerators to be any type of organized meeting in the community or at work. As such, these meetings include kebele meetings, but also meetings in other organizations. We separately analyse the binary variable *Kebele meetings*, which captures whether the respondent attends meetings in the kebele. The fourth political participation variable is *Interested*, a binary variable capturing self-reported interest in politics and government.

To test different mechanisms we collected information on potential mediators for the effects on political participation. The surveys included questions about time use, including the respondents' weekly hours spent on *Social and religious activities* and *Leisure*

time.[6] We further create measures of employment and earnings during the past 6 months. *Any wage job* is a binary variable that captures whether the respondent had any formal wage employment during the past six months (except at baseline, when it measures whether the respondent ever had a formal wage job in the past). *Earnings* measures total earnings from wage employment (in Birr) during the last six months.

Finally, our analysis includes a number of individual level control variables. These are age, measured in years; religion, categorized into Orthodox Christians (the majority, and including the few people answering Catholic or Other), Muslim, and Protestant; and education, categorized into low (<10 years), medium (10 years), and high (>10 years). All values are taken from the baseline survey, so that any changes in the sample mean over time are due to changes in the composition of the sample (attrition).

Table 2 provides descriptive statistics for our sample at baseline and in waves 2 and 5. At baseline, almost 50 percent of the sample attended a community meeting last year, and the number attending kebele meetings is very similar (the correlation between the two types of meetings is 0.63). Around one in four of the respondents have raised an issue last year and around two in five are interested in politics. Comparing across the three survey waves, we see that the political outcome variables are relatively stable over time, with the exception of kebele meeting attendance, which has declined.

Women spend more than 6 hours per week on social and religious activities, and 33 hours on leisure, but both decline over time. We see large increases in employment and earnings over time. The average age in our sample is around 25 years. Most women are Orthodox Christian (the reference group for religion). Half of the women have 10 years of education, while 22 per cent has higher education.

In Appendix Section A.3 we compare our sample to women age 20-30 in the 2016 Demographic and Health Survey (DHS) who live in urban areas close to the factories

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[6]We report results for all categories of time use in the Appendix and we additionally measure female empowerment and attitudes toward gender equality (see Appendix Section A.7).

Table 2: Descriptive statistics for various waves

	Baseline		Wave 2		Wave 5	
	(1)		(2)		(3)	
	Mean	SD	Mean	SD	Mean	SD
Political outcome variables						
Community meeting last year	0.48	(0.50)	0.47	(0.50)	0.47	(0.50)
Kebele meetings	0.52	(0.50)	0.48	(0.50)	0.42	(0.49)
Interested in politics	0.36	(0.48)	0.33	(0.47)	0.35	(0.48)
Raised issue last year	0.28	(0.45)	0.22	(0.41)	0.32	(0.47)
Other outcome variables						
Social and religious activities last 7 days (hours)	6.56	(6.64)	5.53	(5.60)	4.69	(4.18)
Leisure time last 7 days (hours)	33.67	(22.86)	26.87	(21.22)	27.70	(19.42)
Employment and income variables						
Any wage job ever/last 6 months*	0.31	(0.46)	0.50	(0.50)	0.41	(0.49)
Earnings from wage job last 6 months (in Birr)	862	(2658)	2114	(3244)	2999	(5488)
Main baseline variables (constant over time)						
Treatment	0.49	(0.50)	0.49	(0.50)	0.49	(0.50)
Any formal wage job (ever)	0.31	(0.46)	0.31	(0.46)	0.31	(0.46)
Age	24.68	(6.02)	24.91	(6.14)	25.37	(6.33)
Muslim	0.14	(0.35)	0.14	(0.35)	0.15	(0.36)
Protestant	0.22	(0.42)	0.23	(0.42)	0.26	(0.44)
Medium education	0.51	(0.50)	0.51	(0.50)	0.50	(0.50)
High education	0.22	(0.41)	0.21	(0.41)	0.22	(0.41)
N	1463		1262		1054	

Notes: All variables are measured by wave except for the *Main baseline variables*, which are only measured at baseline and held constant. * Employment in wave 1 is defined as ever having had a wage job.

in our study, and to those who live outside of these factory areas. Compared to the DHS factory area sample, women in our study are less likely to have either low or high education and less likely to have ever had a formal wage job. Our sample therefore has a relatively large share of women seeking formal employment for the first time. The difference with non-factory area women is much larger, however. Clearly, our sample is not representative of the population of Ethiopian women age 20-30, but somewhat similar to the population of women age 20-30 living near the factories.

To test for baseline balance between treatment and control group, we regress $Treatment_i$ on the baseline control variables and baseline values of political participation, both individually and jointly, while controlling for the blocking variable (*List*, i.e. the firm hiring batches). The results, in Table 3, show that being Muslim is statistically significantly correlated with treatment. When we include all variables at the same time (in column 2) an F-test indicates that the variables do not jointly predict treatment status (F=0,54 p=0.48). We therefore view the randomization as successful in creating balance. In Appendix Table A5 we show that the same control variables strongly predict our main outcome variables, in particular the baseline values of the outcome variables. Reassuringly, the only control variable correlated with treatment (being Muslim), is not significantly correlated with political participation.

In wave 5 we added new questions to the survey (detailed definitions are given in Appendix Table A4). All new variables and coding choices were described in a pre-analysis plan that was filed before data collection started.[7]

New outcome variables include *Intend to vote*, which captures whether the respondent would vote if there was an election next week, and *Protest last year* and *Express last year*, which capture whether the respondent attended a demonstration or protest march, or expressed a political opinion through social media.

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[7]We provide an anonymized version of the pre-analysis plan in Appendix Section A.12 for the reviewers.

Table 3: Balance tests.

	(1)	(2)
	Treatment	
Community meeting last year	-0.0022	0.024
	(0.030)	(0.040)
Kebele meetings	-0.027	-0.042
	(0.031)	(0.037)
Interested in politics	-0.0036	-0.0029
	(0.034)	(0.034)
Raised issue	-0.016	-0.023
	(0.033)	(0.040)
Any formal wage job (ever)	0.039	0.038
	(0.033)	(0.033)
Age	0.00063	-0.00088
	(0.0030)	(0.0032)
Muslim	-0.13**	-0.12**
	(0.054)	(0.058)
Protestant	0.12*	0.096
	(0.065)	(0.068)
Medium education	-0.061*	-0.069
	(0.034)	(0.051)
High education	0.056	-0.0037
	(0.037)	(0.056)
No. of observations	NA	1262
R-squared	NA	0.07
F-test	NA	0.51
P-value of F-test	NA	0.48

Notes: All variables are measured at baseline. Column 1 shows coefficients when we include the variables one by one and column 2 is from a regression with all variables included. All regressions control for List fixed effects. Robust SE in parentheses.

In addition to new outcome variables, we added several questions to explore potential mechanisms. Based on respondents' agreement to the statement *"Sometimes politics and government seem so complicated that a person like me cannot really understand what is going on"*, we create a binary measure of *Internal efficacy*. Similarly, a binary measure of *External efficacy* is based on the statement *"People like me don't have any say in what the government does"*. We also ask respondents what topics they discuss with friends (they can list up to three topics). Based on this question we create two binary variables, *Discuss narrow politics* and *Discuss broad politics*. The former captures whether they discuss political issues, while the latter captures whether they discuss political issues, women's rights, and/or grievances and dispute resolution (excluding grievances related to their jobs).

To better understand community meeting participation, we ask women whether they feel pressured to attend these meetings nowadays (*Pressured now*) and whether they felt pressured before Abiy came to power *Pressured before Abiy*. We ask the latter question because of the political liberalization and shift in power in 2018 (see Section 2.2). To measure the importance of community meetings, we also ask whether people who cannot attend the meetings lose influence in the community (*Lose influence* and *Lost influence before Abiy*).

Finally, in Wave 5, we included two additional (pre-registered) employment variables. These are *Any factory job* (a dummy variable equal to one if the respondent has worked in a factory job for at least one month since the baseline survey) and *Months factory job* (the total number of months worked in a factory job since baseline).

Summary statistics for the variables added in Wave 5 are reported in Table 4. Almost all women state they intend to vote in the next election, and around 20 percent had protested during the last year. Political efficacy is low in the sample, as a majority of women think that politics is too complicated to understand and that they have no say in politics. One fourth of the women discuss politics with friends when we define it

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narrowly and only slightly more when we define it broader. Around one fourth of the women feel pressured to attend community meetings and around one in five feel that they lose influence if they do not attend. These numbers are very similar if we ask about how they felt before the regime change. Around half of the women had a factory job for at least one month since baseline, and the average woman had 9 months of factory employment.[8]

Table 4: Descriptive statistics for variables collected in Wave 5

	(1)	
	Mean	SD
Newly collected outcomes		
Intend to vote	0.95	(0.23)
Protested last year	0.19	(0.39)
Express last year	0.18	(0.38)
Internal efficacy	0.38	(0.48)
External efficacy	0.47	(0.50)
Discuss narrow politics	0.25	(0.43)
Discuss broad politics	0.27	(0.44)
Feel pressured to attend meetings	0.23	(0.42)
Felt pressured before Abiy	0.26	(0.44)
Lose influence if not attending meetings	0.17	(0.37)
Lost influence before Abiy	0.18	(0.39)
New employment variables		
Months with factory job	9.33	(13.11)
Any month with factory job	0.48	(0.50)
N	1054	

Notes: Data for these variables was only collected in Wave 5.

In Wave 5 we asked the women if they consented to us contacting their partners. Almost all women consented and shared their partner's phone number. We then conducted a phone survey with the partners in May to July 2020 - this was not included in our pre-analysis plan. In total we managed to interview 718 partners, whom we asked about their own political participation, among others. We show the summary statistics for the partners in column 1 of Table 5. Statistics for the women whose partners participated in the phone survey are in column 2, and in column 3 we report the within-couple gender gaps. For all types of participation except intention to vote, we see that partners

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[8]We also collected data on political priorities and preferences. We describe these in Appendix Section A.9. We further planned to collect data on *Party choice* but this was deemed too sensitive to ask at the time of the survey.

participate more than the women.

In the partners phone survey we also took the opportunity to ask some more questions about community meetings. As shown at the bottom of Table 5, they consider the main benefits of attending community meetings (including kebele meetings) to be informed about security issues (97 percent), to have a say in local politics and be informed about government policies (91 percent each), to get other information about political issues (85 percent), and to a lesser extent to get access to economic benefits. We also asked the partners to list the three main issues that are discussed during local meetings and the modal topic listed first was "development related issues" (23 percent). Other major topics include "grievances and dispute resolution" (20 percent) and "political issues" (20 percent).

Table 5: Descriptive statistics for women in wave 5 and their partners in the phone survey.

	Partners		Women		Gap	
	(1)		(2)		(3)	
	Mean	SD	Mean	SD	Mean	SD
Community meeting last year	0.77	(0.42)	0.48	(0.50)	0.29	(0.66)
Raised issue last year	0.73	(0.44)	0.32	(0.47)	0.41	(0.62)
Interested in politics	0.79	(0.41)	0.37	(0.48)	0.42	(0.64)
Kebele meetings	0.84	(0.37)	0.44	(0.50)	0.39	(0.60)
Intend to vote	0.91	(0.28)	0.96	(0.20)	-0.05	(0.35)
Protested last year	0.51	(0.50)	0.20	(0.40)	0.31	(0.66)
Internal efficacy	0.51	(0.50)	0.40	(0.49)	0.09	(0.59)
External efficacy	0.76	(0.43)	0.50	(0.50)	0.18	(0.61)
Main benefit of attending meetings						
Have my say in local affairs/ decision making	0.91	(0.28)				
Get access to economic benefits such as work, credit or cheap goods	0.74	(0.44)				
Get information about new taxes or government policies	0.91	(0.29)				
Be informed about local security issues	0.97	(0.18)				
Get information about political issues or election campaigns	0.85	(0.36)				
N	718		718		718	

Notes: The sample of women only includes those whose partners participated in the phone survey. Questions about the main benefits of attending community meetings were only asked to the partners. The question wording is: "What are, in your opinion, the main benefit of attending kebele/community meetings?". The mean values reflect the share of respondents answering yes.

4   Empirical specification

Our main specification is the following intention to treat (ITT) model:

$$(1) \quad Y_{i,t1} = \alpha Y_{i,t0} + \beta Treatment_i + \gamma X_{i,t0} + \delta List_i + \epsilon_{it},$$

where i indexes individuals, $t0$ refers to baseline values, and $t1$ is the first follow up (Wave 2). We analyze outcomes in subsequent waves separately. $Treatment_i$ is a dummy variable equal to 1 if the woman was randomized to get the job offer and zero if not. The main coefficient of interest is β, which captures the so called ITT effect, or the effect of receiving the randomly assigned job offer on the outcome of interest. We always include list fixed effects (blocking variables) as women are randomized within this unit. We include control variables in some specifications to see if we can increase precision. In particular we include the baseline value of the outcome variable and the vector of individual level controls X (described in the data section). We use heteroscedasticity robust standard errors in all estimations. In Appendix Section A.2 we also describe and present results from instrumental variables regressions.

5 Results

In this section, we first show the effects of treatment on employment and earnings. Next, we move on to present the main results on the impact of treatment on political participation, and finally we explore potential mechanisms.

5.1 Effects on employment and earnings

Using waves 2 and 5, Table 6 shows the effects on employment and earnings after six months (Panel A) and 34 months (Panel B). In Wave 2 we see a large effect on the probability of having had any wage job during the last six months and a large effect on earnings. While 29 percent in the control group have had such a job, this share increases to 69 percent for the treatment group.[9] In the longer run, we still see an effect on wage employment and earnings (Panel B). The treatment effects are now smaller, which

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[9]The main reasons for treatment group women not starting the job were working hours and pay, which they did not fully know before applying for the job. Some control group women found jobs in similar factories, or they found "work in another persons business or shop".

reflects turnover among treatment group women as well as continued job search efforts among control group women. Indeed, by the time of the fifth survey, more control group women reported having a wage job and their average earnings were almost twice as high compared to wave two.

In Panel B of Table 6 we present treatment effects on variables only collected in Wave 5. These variables are based on the total number of months employed in a factory since baseline. We see that since baseline, women in the treatment group worked in a factory around 9 months more than women in the control group. They are also 37 percentage points more likely to ever have worked in a factory since baseline. The control group has also worked in factories (29 percent of them) and they have worked for 5 months on average. Figure 3 shows the evolution of employment over time, across all survey waves.

Table 6:   Effects of treatment on employment and earnings.

(a)   Panel A: Wave 2 (first follow up)

	(1)	(2)
	Any wage job	Earnings from wage job
Treatment	0.40***	1726.8***
	(0.025)	(172.6)
Control mean	0.29	1292.02
N	1262	1262
R-squared	0.29	0.22
Controls	Block	Block

(b)   Panel B: Wave 5 (fourth follow up)

	(1)	(2)	(3)	(4)
	Any wage job	Earnings from wage job	Months with factory job	Any month with factory job
Treatment	0.12***	946.5***	9.13***	0.37***
	(0.030)	(332.2)	(0.78)	(0.028)
Control mean	0.35	2509.87	4.98	0.29
N	1054	1054	1054	1054
R-squared	0.15	0.14	0.27	0.31
Controls	Block	Block	Block	Block

Notes: Both employment and earnings refer to the last six months. All regressions control for List fixed effects. Robust SE in parentheses.
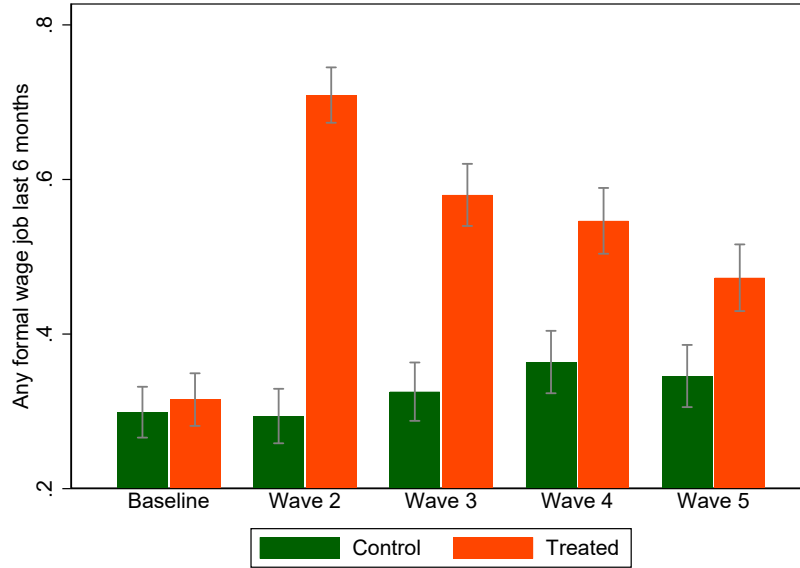
Figure 3: Evolution of employment over time for treatment and control
In Wave 1, Any wage job last six months is replaced by ever having had a wage job.

5.2 Effects on political participation and potential mechanisms

In Table 7 we show the effects of treatment on our main political participation indicators. For each dependent variable, we first show results without any controls other than the block variables, and then we add baseline control variables including the baseline value of the dependent variable. Starting with wave 2 in Panel A, we see a clear negative effect of the job offer on attendance of community meetings, and a similar effect on kebele meetings (columns 1-4). The coefficients correspond to a twenty percent decline in meeting participation relative to the control group mean. We find no effect on political interest (columns 5 and 6) and a small negative but statistically insignificant effect on raising an issue (columns 7-8). Panel B shows results for the final wave. There is still a negative effect of treatment on meeting attendance in the longer run. The effect is smaller and less precisely estimates compared to wave 2, but still quite large (11 to 16 percent of the control group mean). In Panel C we report all treatment effects in the pooled sample across waves 2-5. This increases power substantially and now also the negative effects on raising issues are statistically significant. Adding wave fixed effects is inconsequential to

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the results.

Table 7: Effects of treatment on participation and interest.

(a) Panel A: Wave 2 (first follow up)

	(1) Community meeting	(2) Community meeting	(3) Kebele meeting	(4) Kebele meeting	(5) Interested	(6) Interested	(7) Raised issue	(8) Raised issue
Treatment	-0.12***	-0.12***	-0.100***	-0.090***	-0.00056	0.0052	-0.034	-0.031
	(0.028)	(0.027)	(0.028)	(0.027)	(0.026)	(0.025)	(0.023)	(0.023)
Control mean	0.52	0.52	0.52	0.52	0.34	0.34	0.23	0.23
N	1262	1262	1261	1261	1260	1260	1262	1262
R-squared	0.07	0.17	0.09	0.20	0.17	0.21	0.11	0.17
Controls	Block	Controls	Block	Controls	Block	Controls	Block	Controls

(b) Panel B: Wave 5 (fourth follow up)

	(1) Community meeting	(2) Community meeting	(3) Kebele meeting	(4) Kebele meeting	(5) Interested	(6) Interested	(7) Raised issue	(8) Raised issue
Treatment	-0.052*	-0.056*	-0.079**	-0.072**	-0.0038	-0.0051	-0.045	-0.045
	(0.031)	(0.031)	(0.031)	(0.030)	(0.027)	(0.027)	(0.028)	(0.028)
Control mean	0.49	0.49	0.45	0.45	0.35	0.35	0.33	0.33
N	1054	1054	1054	1054	1054	1054	1054	1054
R-squared	0.11	0.13	0.10	0.16	0.29	0.29	0.14	0.16
Controls	Block	Controls	Block	Controls	Block	Controls	Block	Controls

(c) Panel C: Waves 2-5

	(1) Community meeting	(2) Community meeting	(3) Kebele meeting	(4) Kebele meeting	(5) Interested	(6) Interested	(7) Raised issue	(8) Raised issue
Treatment	-0.10***	-0.10***	-0.097***	-0.097***	-0.0024	-0.0025	-0.059***	-0.059***
	(0.020)	(0.020)	(0.022)	(0.022)	(0.017)	(0.017)	(0.017)	(0.017)
Control mean	0.55	0.55	0.51	0.51	0.40	0.40	0.31	0.31
N	4564	4564	4563	4563	4562	4562	4564	4564
R-squared	0.06	0.06	0.07	0.07	0.17	0.19	0.07	0.08
Controls	Block	Block	Block	Block	Block	Block	Block	Block
Wave f.e.	No	Yes	No	Yes	No	Yes	No	Yes

Notes: All regressions control for List fixed effects. Robust SE in parentheses, clustered at the individual level in panel C.

In Figure 4 we summarize the difference between treated and control women graphically, also including waves 3 and 4. Interestingly we see that in the control group, more women start attending community meetings between baseline and the first follow up, while in the treatment group, attendance declines. With respect to kebele meetings we also see a large decline between baseline and Wave 2 in the treatment group, while the control group attendance declines only marginally. Political interest and raising of issues drop slightly after baseline, for both treatment and control group women. For several outcomes, we can see an upward trend from Wave 2 to Wave 4, followed by a large drop

between Waves 4 and 5. The spike in interest and raising issues in Wave 4 is likely driven by the political protests and reforms occurring during this period, which seems to lead to a short run surge in activism and political interest.
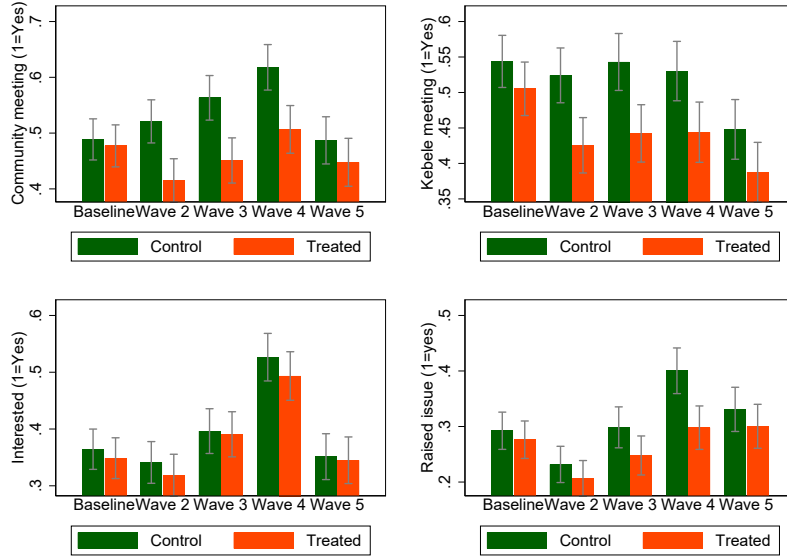

Figure 4: Main political outcomes over time for treatment and control

To assess potential mechanisms for the negative effect of the job offers on community and kebele meeting attendance we estimate the treatment effect on time use. Working reduces the time left to do other activities, and indeed in Panel A of Table 8 we see a negative effect of treatment on leisure time and on time spent on social and religious activities in Wave 2. In the longer run, however, there is no effect of treatment on time use (Panel B).[10]

In Panel B of Table 8 we further show that treatment reduces both *Internal* and *External political efficacy.* The effect is particularly strong for internal political efficacy, where treatment leads to a reduction of 0.078 or almost 20 percent of the control group

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[10]In Appendix Table A6 we show the effects on all time use variables in Wave 2 for completeness. We see that paid work hours and travel time increase, while household work hours and time spent on personal care decrease. We also show the complier average causal effect of leisure time on community meetings by instrumenting leisure with treatment (Appendix Table A9). We see a statistically significant effect in wave 2 but not in wave 5. The estimates are relatively imprecise and we also note that the exclusion restriction for the IV regression is unlikely to hold as employment likely has effects on meeting attendance that do not only run via leisure time.

Table 8: Effects of treatment on time use and political efficacy.

(a) Panel A: Wave 2 (first follow up)

| | (1) | (2) | (3) | (4) |
	Leisure	Leisure	Social and religious activities	Social and religious activities
Treatment	-3.36***	-2.91***	-0.60**	-0.52*
	(1.15)	(1.10)	(0.28)	(0.28)
Control mean	28.50	28.50	5.97	5.97
N	1261	1261	1262	1262
R-squared	0.17	0.25	0.17	0.20
Controls	Block	Controls	Block	Controls

(b) Panel B: Wave 5 (fourth follow up)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
	Leisure	Leisure	Social	Social	Internal	Internal	External	External
Treatment	-1.01	-0.65	0.11	0.10	-0.080***	-0.078***	-0.054*	-0.049*
	(1.12)	(1.11)	(0.23)	(0.23)	(0.027)	(0.027)	(0.028)	(0.028)
Control mean	28.36	28.36	4.62	4.62	0.42	0.42	0.51	0.51
N	1054	1054	1054	1054	1044	1044	1046	1046
R-squared	0.27	0.29	0.24	0.26	0.28	0.29	0.30	0.30
Controls	Block	Controls	Block	Controls	Block	Controls	Block	Controls

Notes: The time use variables are measured as number of hours in a week. Social is short for "Social and religious activities" in Panel B. All regressions control for List fixed effects. Robust SE in parentheses.

mean. Interestingly, we do not find that general depression is affected, if anything we find that women randomly assigned to jobs are more likely to say that they never sad, worried, tense, or anxious (see Appendix Figure A1.

In addition to investigating the longer run mechanisms we also pre-registered an analysis of the effect of treatment on *Vote intentions* and *Protest*. In Table 9 we show that there is no effect on either outcome. For *Vote intentions*, this is likely driven by very little variation in voting intentions as 95 percent of the control women report that they would vote. We stated in the pre analysis plan that if 95 percent or more of the respondents answer the same we will not regard a variable as a main outcome. We still present the results here for completeness.[11] In Appendix Table A8 we show that there are no treatment effects on political participation or efficacy for the male partners.

In Appendix Section A.5 we estimate the effect of wage employment in our sample,

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[11]We have also analyzed effects on membership in various networks but do not find effects on these variables (see Section A.8). Neither do we find any effect on discussing politics with friends (see Table A7). Finally, as seen in Appendix Section A.9, we find no effects on political preferences. We have also tested for heterogeneous effects by interacting the baseline control variables with treatment but there were no significant interaction effects.

Table 9: Effects of treatment on vote intentions and protest.

	(1) Intend to vote	(2) Intend to vote	(3) Protest last year	(4) Protest last year
Treatment	-0.0085	-0.0096	-0.0098	-0.0096
	(0.015)	(0.015)	(0.023)	(0.023)
Control mean	0.95	0.95	0.20	0.20
N	1054	1054	1054	1054
R-squared	0.04	0.04	0.17	0.18
Controls	Block	Controls	Block	Controls

Notes: All regressions control for List fixed effects. Robust SE in parentheses.

using various different models that make no use of the randomized treatment assignment. Even the results that exploit individual level fixed effects differ substantively from our ITT estimates reported above, which we know are unbiased by design. This clearly indicates that without exogenous variation, even panel data analysis cannot purge the bias in the relationship between employment and political outcomes in our setting.

In Appendix Section A.3 we show that there are negative treatment effects in each of the five factory areas in our sample, although the treatment effect is not statistically significant in Tigray. In Appendix Section A.4 we discuss differences between the factories.

6 Discussion

The negative effects of the job offers on meeting participation can be interpreted as a disempowering effect of employment. In particular, several features of the jobs may explain why the jobs lead to lower political efficacy. In the short run, the long working hours significantly affected time use, and this may have had persistent effects via habit formation if women who stop attending meetings start to think of themselves as less politically able. This could explain why the decline in participation persists, even though women's time use is no longer affected in the longer run. There may also be more direct effects of the jobs on women's confidence and political efficacy due to the belittling attitudes of factory managers, abuse of workers, and the clear gender hierarchy in the workplace. Given the magnitudes of our estimates, which show a relatively large reduction

in political efficacy, it seems unlikely that the initial impact on time use accounts for the entire long run reduction in efficacy.

Another possibility is that the meetings are of little value to the participants and that non-participation is actually a sign of empowerment. Both cross-national and case studies from authoritarian contexts show that political participation could be less a consequence of individual factors and behavior than in democracies. Instead, it could be a result of regime-directed mobilization (Bahry and Silver 1990), where participation is deliberately used to strengthen authoritarian rule instead of giving citizens the chance to influence decision making (Aalen and Muriaas 2018, Benton 2016). Croke et al. (2016) find that increased education in Zimbabwe caused a decline in political participation and argue that a likely mechanism is deliberate disengagement. We find it unlikely that non-participation is a sign of political empowerment in our context, as both the partner phone survey and the qualitative interviews reveal that the meetings are valued and considered important. Furthermore, in Table 4 we saw that less than one in four women feel pressured to attend meetings.[12] Most importantly, the interpretation that less meeting activity signals increased political empowerment is inconsistent with the negative effects of employment on efficacy.

An alternative explanation could be that employment causes political disempowerment if women try to avoid breaking multiple gender stereotypes at the same time. That is, it could be that women compensate for the non-traditional act of working by undertaking fewer other activities outside of the household, in order to maintain some sort of equilibrium within their families or communities. This is what women indicate in qualitative interviews in Gottlieb (2016), who finds that a civic participation course reduced other types of political participation. We do not find any indications of such conscious self-limiting behavior in our qualitative or quantitative data. Furthermore, this mechanism does not align with the large decline in political efficacy that we document.[13]

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[12]There is no difference across treatment and control in the perceived pressure to attend meetings.

[13]A related argument is that employment may lead to a backlash whereby the partners try to re-

Finally, it could be the case that employment affects the demand for political participation. In particular, increased financial resources may make women less dependent on the benefits that may stem from attending community and kebele meetings. Earnings from the factory jobs may be a substitute for access to certain public services, in line with Dionne et al. (2014); Escribà-Folch et al. (2018), who find that remittances can substitute for political participation as people are less in need of patronage goods. Some of our results speak against this interpretation, however. First, we learned from the partner phone survey that information about security and political issues, as well as political influence, are considered the main benefits of meeting attendance - more than access to economic benefits. Second, if reduced demand were indeed the main channel, it would be difficult to explain the negative effect on political efficacy. Finally, we do not find any evidence that the job offers reduced women's perceived pressure to attend community meetings.

7   Conclusion

The present project is the first to identify the causal effect of employment on political participation at the individual level. We do so using a field experiment, in which we collaborated with 27 large companies in Ethiopia to randomly assign job offers to equally qualified female applicants. We collected baseline data and four waves of follow up data up to around 3 years after randomization.

We find that the job offers increase formal employment and earnings. Contrary to the positive impacts hypothesized in light of the resource theory of political participation, the job offers did not increase political participation in our setting. In particular, we find no effects on political interest, raising issues, discussing politics with friends, joining

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instate their dominance within the family by using violence. Several authors posit a relationship between gender inequality at home and political participation (Burns et al., 1997; Iversen and Rosenbluth, 2006; Khan, 2017). Hence, a household level backlash could explain our results. As shown in Kotsadam and Villanger (2020), however, the job offers had no effect on partner violence and we also show in Appendix Section A.7 that there is no effect of the job offers on bargaining power within the household.

networks, joining protests, or turnout intentions. And we find that the job offers reduced participation in community and kebele meetings. In the short run, job offers reduced leisure time and time spent on social and religious activities, suggesting that a likely mechanism for reduced political participation and interest is increased time pressure for the working women. Analyzing longer run impacts, however, we do not find an effect of the job offers on time use even though we still find that it increases employment and reduces participation in meetings.

We argue that the factory jobs lead to political disempowerment. This interpretation is mainly based on the finding that the job offers reduce women's political efficacy in the longer run (we only measured political efficacy in the last survey wave), and further supported by the qualitative evidence that women view community and kebele meetings as meaningful and important. Whether the long run reduction in political efficacy is a direct result of the nature of the jobs, or caused indirectly by the initial shifts in time use and reduced meeting attendance, is difficult to determine. Given the large reduction in political efficacy, it seems likely that the direct impact of the jobs accounts for at least some part of this effect.

By randomly assigning jobs we contribute more reliable empirical evidence to the literature on the effects of employment on political outcomes. We are able to eliminate the usual sources of bias that are present in non-experimental analyses. Using randomized variation at the individual level, we are able to control for sociotropic concerns, such as the level of employment, and omitted variables at the area level, such as levels of crime and insecurity. This is in contrast to studies that exploit area level changes to identify the effects of employment.

Given our research design, our results have strong internal validity. We urge for caution in generalizing the results, however, as there are several factors that may limit the external validity. First of all, we investigate the effects of employment for women in a context with major other (social, economic, and political) changes going on. During

the study period, Ethiopia started to allow opposition groups and released thousands of political prisoners. Although these changes affect both the treatment and control group, and we find similar treatment effects across survey waves, it is difficult to determine how these developments moderate our results. Secondly, the women we study all applied for factory work and all have a partner. In comparing our sample to women of a similar age in our areas using DHS data, we see that they are relatively similar in terms of education.

Thirdly, the types of jobs we investigate are likely to be less empowering than other jobs. Employment is thought to have positive effects on political participation by increasing political skills, increasing networks, providing money, and affecting attitudes (Brady et al., 1995; Schlozman et al., 1999). In laying out the mechanisms for why employment in the US may affect women's political participation, Schlozman et al. (1999) describe that civic skills will be improved as organizing meetings, giving presentations, supervising others, and developing leadership capacities are thought to be core factors correlated with being employed. While such factors are likely features of US white collar jobs, they are far from everyday working life in blue collar occupations, especially in newly industrializing settings. Similar to garment work in Asia, as documented by Evans (2017), the working conditions in the Ethiopian factories are poor and the occupations are both vertically and horizontally segregated by gender. All managers are men and our interviews with these managers suggest that they view women as submissive and docile. Such "gendered discourses of work" (Caraway, 2005) are likely to affect women's political efficacy. Furthermore, unions are rare and in our qualitative interviews the workers report managerial surveillance and fears of dismissal if they speak up against bad conditions.

We also contribute to the literature on the relationship between industrialization and political action. This question has attracted great interest among historians and social scientists since the 19th century. A classic marxist position is that industrialization creates widespread social grievances, and that this will ultimately lead to a revolution by workers (Marx and Engels, 2010/1848). Research in history and political sociology

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has extensively studied the early phases of industrialization in Europe and the US (e.g., Tilly, 2004; Korpi and Shalev, 1979), and shown plausible links between poor working conditions and political participation.

There may be several reasons for why this is not happening in our context. One reason is that the power balance in today's international political economy is dramatically different from that during the industrial revolution. In a highly competitive globalized world, putting pressure on firms may simply lead them to go elsewhere, which we were also told in the qualitative interviews. This is also in line with the fact that in our sample, trade unions are active only in two Ethiopian owned factories. We further know that current day mining-induced industrialization causes political mobilization in Sub-Saharan Africa (Kotsadam et al., 2017). As extraction of point-source resources cannot be moved to a different location, workers involved in these activities have more power. It may also be the case that more political organization will develop in our study's factories, but that it simply takes more time, for instance since the total number of workers are still few in these communities. To date, however, we find no differences in the effects across areas with many or few other working women or across older and newer factories.

We urge future studies to conduct similar field experiments in different settings so that we will learn whether the disappointing results we find are indeed general or whether there are important moderators that may unleash the empowering potential of employment that is so often assumed.

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Appendix

A.1 Single tables and figures referred to in the text

Table A1: Attrition.

	(1) Attrition	(2) Attrition
Treatment	-0.0030	-0.0028
	(0.018)	(0.018)
Meeting last year (B)		-0.017
		(0.019)
Interested in politics (B)		-0.016
		(0.022)
Any formal wage job (ever)		-0.012
		(0.020)
Age		-0.0075***
		(0.0020)
Muslim		-0.0073
		(0.036)
Protestant		-0.027
		(0.046)
Medium education		-0.012
		(0.034)
High education		0.010
		(0.037)
Control mean	0.14	0.14
N	1463	1463
R-squared	0.07	0.08
Controls	Block	Full

Notes: All regressions control block fixed effects. Robust SE in parentheses.

Table A2: Attrition Wave 5.

	(1) Attrition	(2) Attrition
Treatment	-0.0075	-0.0072
	(0.023)	(0.023)
Meeting last year (B)		-0.0052
		(0.024)
Interested in politics (B)		-0.0079
		(0.028)
Any formal wage job (ever)		0.013
		(0.026)
Age		-0.013***
		(0.0024)
Muslim		-0.0048
		(0.042)
Protestant		0.019
		(0.048)
Medium education		0.0049
		(0.040)
High education		0.015
		(0.044)
Control mean	0.28	0.28
N	1463	1463
R-squared	0.11	0.13
Controls	Block	Full

Notes: All regressions control block fixed effects. Robust SE in parentheses.

Table A3: Construction of main variables

Variable	Survey question	Coding
Political outcome variables		
Community meeting*	Attended a community meeting during the past year	0 if Not at all; 1 if Yes rarely, Yes sometimes, Yes several times, or Yes often
Kebele meetings	Do you attend meetings in the kebele?	0 if No; 1 if Yes
Raised issue	Got together with others to raise an issue during the past year	0 if Not at all; 1 if Yes rarely, Yes sometimes, Yes several times, or Yes often
Interested	How interested would you say you are in politics and government?	0 if Not at all interested or Not very interested; 1 if Somewhat interested or Very interested
Other outcome variables		
Social and religious activities	How many hours did you spend on social and religious activities over the last seven days?	Number of hours over the last 7 days
Leisure	How many hours did you spend on social and leisure time (watching TV, reading magazine, playing, exercising, recreation etc.) over the last seven days?	Number of hours over the last 7 days
Employment and income variables		
Any wage job	Baseline: Have you ever had a formal wage job in the past? Wave 2: Did you start working in Factory X? Have you had any other formal or salaried job with salary since the last interview? Wave 3-5: During the last six months, how much income did you obtain from factory job employment? How much from other wage employment?	0 if No; 1 if Yes 0 if No to both questions; 1 if Yes to one or both questions 0 if zero income; 1 if positive income
Earnings	During the last six months, how much income did you obtain from factory job employment? How much from other wage employment?	Total earnings reported
Baseline variables (constant over time)		
Any wage job ever	Have you ever had a formal wage job in the past?	0 if No; 1 if Yes
Age	Age	Reported age in years
Religion	Religion	Categories Orthodox Christian, Catholic or Other; Muslim; Protestant
Education	How many years of education have you completed?	Categories Low if <10 years; Medium if 10 years; High if >10 years of education

Notes: *Community meetings are broadly defined and explained to be any type of organized meeting in the community or at work. These can include kebele meetings.

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Table A4: Construction of Wave 5 variables

Variable	Survey question	Coding
Political outcome variables		
Intend to vote	Would you vote if there was an election next week?	0 if No; 1 if Yes
Protest last year	Did you attend a demonstration or protest march or public rally last year?	0 if Not at all; 1 if Yes rarely, Yes sometimes, Yes several times, or Yes often
Express last year	Did you express a political opinion through social media last year?	0 if Not at all; 1 if Yes rarely, Yes sometimes, Yes several times, or Yes often
Internal efficacy	Sometimes politics and government seem so complicated that a person like me cannot really understand what is going on.	0 if Strongly agree or Agree; 1 if Strongly disagree or Disagree
External efficacy	People like me don't have any say in what the government does.	0 if Strongly agree or Agree; 1 if Strongly disagree or Disagree
Discuss broad politics	If you think about your friends and the things you talk about, what are the topics you discuss?	1 if Political issues, Women's rights, or Grievances and dispute resolution; 0 otherwise
Discuss narrow politics	If you think about your friends and the things you talk about, what are the topics you discuss?	1 if Political issues; 0 otherwise
Community meetings variables		
Pressured now	Do you feel pressured nowadays to attend community meetings?	0 if Not at all; 1 if Yes rarely, Yes sometimes, Yes several times, or Yes often
Pressured before Abiy	Before the regime of Abiy, did you feel pressured to attend community meetings?	0 if Not at all; 1 if Yes rarely, Yes sometimes, Yes several times, or Yes often
Lose influence	Do you think that people that cannot attend community meetings due to work get less influence in the community?	0 if No; 1 if Yes
Lost influence before Abiy	before Abiy came to power, do you think that people that could not attend community meetings due to work got less influence in the community?	0 if No; 1 if Yes

Notes: Data for the variables described in this table was only collected in Wave 5.

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Table A5: Correlations between outcome variables in Wave 2 and baseline controls.

	(1) Community	(2) Kebele	(3) Interested	(4) Raise issue
Meeting last year (B)	0.12***	0.089**	-0.035	-0.057*
	(0.040)	(0.039)	(0.035)	(0.032)
Kebele meetings (B)	0.15***	0.25***	0.021	0.13***
	(0.036)	(0.036)	(0.033)	(0.029)
Interested in politics (B)	0.023	0.0055	0.17***	0.048*
	(0.032)	(0.032)	(0.032)	(0.028)
Raised issue last year (B)	0.11***	0.068*	0.14***	0.16***
	(0.038)	(0.038)	(0.036)	(0.034)
Any formal wage job (ever)	0.035	0.010	-0.062**	0.039
	(0.031)	(0.030)	(0.029)	(0.026)
Age	0.017***	0.012***	0.0032	0.0080***
	(0.0029)	(0.0029)	(0.0028)	(0.0025)
Muslim	0.063	0.093*	0.0062	0.082*
	(0.054)	(0.053)	(0.050)	(0.046)
Protestant	0.0033	-0.044	-0.063	0.024
	(0.064)	(0.065)	(0.064)	(0.066)
Medium education	-0.051	0.036	0.11**	0.022
	(0.049)	(0.048)	(0.044)	(0.043)
High education	-0.078	0.047	0.077	-0.0076
	(0.053)	(0.052)	(0.047)	(0.047)
N	1262	1261	1260	1262
R-squared	0.18	0.20	0.22	0.18
F-test	8.00	7.33	16.05	12.85
P-value of F-test	0.005	0.007	0.0001	0.0004

Notes: All control variables are measured at baseline. All regressions control for List fixed effects. Robust SE in parentheses.

Table A6: Effects on other aspects of time use. Wave 2

	(1) Paid work	(2) Unpaid	(3) HH work	(4) Sleeping	(5) Eating	(6) Personal care	(7) School	(8) Travel
Treatment	10.4***	-0.51	-1.99***	-0.54	-0.046	-0.28**	0.070	0.68***
	(1.33)	(0.34)	(0.65)	(0.44)	(0.16)	(0.12)	(0.27)	(0.23)
Control mean	18.30	1.46	28.04	57.88	6.27	4.27	1.26	4.65
N	1262	1262	1262	1262	1262	1262	1262	1262
R-squared	0.15	0.08	0.13	0.14	0.13	0.11	0.08	0.18
Controls	Block	Block	Block	Block	Block	Block	Block	Block

Notes: HH work is short for household work. All regressions control block fixed effects. Robust SE in parentheses.

Table A7: Effects of treatment on discussing politics with friends. Wave 5

	(1) Broad	(2) Broad	(3) Narrow	(4) Narrow
Treatment	-0.0056	-0.0057	-0.0032	-0.0040
	(0.029)	(0.029)	(0.028)	(0.028)
Control mean	0.27	0.27	0.25	0.25
N	834	834	834	834
R-squared	0.21	0.22	0.22	0.23
Controls	Block	Controls	Block	Controls

Notes: All regressions control for list fixed effects. Robust SE in parentheses.

Table A8: Effects on the male partners of having a partner offered a job.

	(1) Community	(2) Raise issue	(3) Interested	(4) Kebele	(5) Intend to vote	(6) Protest	(7) Internal	(8) External
Treatment	-0.010	-0.039	-0.023	-0.0012	-0.012	-0.011	-0.0049	-0.011
	(0.029)	(0.033)	(0.032)	(0.027)	(0.021)	(0.035)	(0.040)	(0.037)
Control mean	0.78	0.76	0.80	0.85	0.92	0.52	0.50	0.77
N	718	718	718	718	710	718	632	588
R-squared	0.31	0.20	0.07	0.22	0.07	0.26	0.19	0.14
Controls	Block	Block	Block	Block	Block	Block	Block	Block

Notes: The sample includes the men from the phone survey. All regressions control for List fixed effects. Robust SE in parentheses.
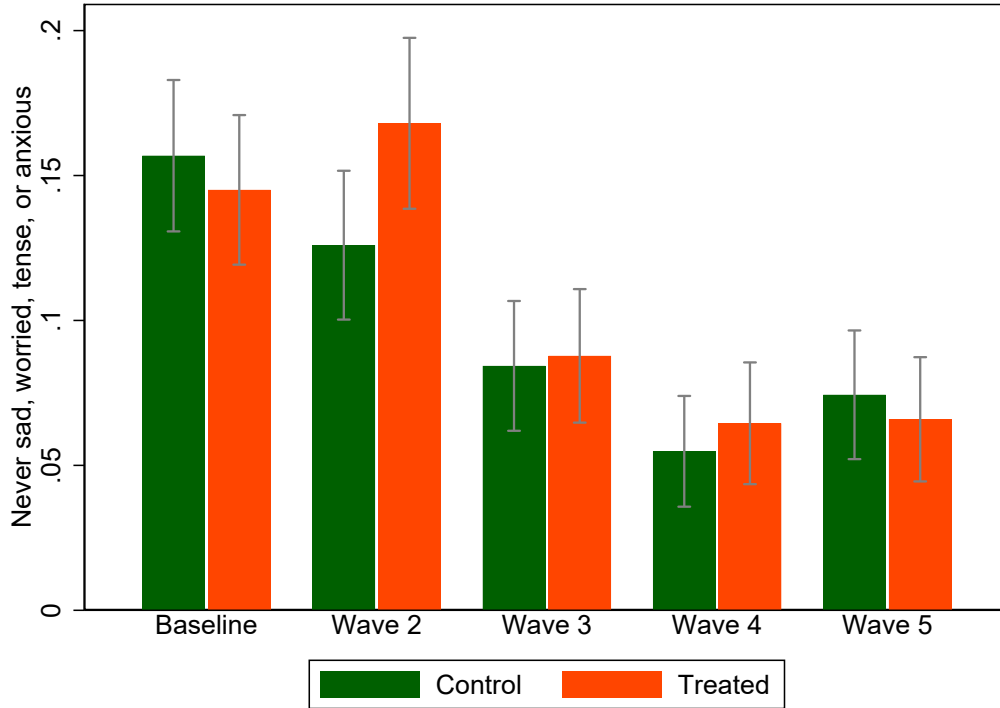
Figure A1: Treatment effects on signs of depression

Table A9: IV estimates of the effects
of leisure time in different waves.

	(1) Community meeting	(2) Community meeting
Leisure	0.034**	0.051
	(0.015)	(0.062)
Control mean	0.52	0.49
N	1261	1054
Controls	Block	Block
Wave	2	5

Notes: Leisure time is instrumented with treat-
ment. All regressions control for List fixed effects.
Robust SE in parentheses.

A.2 Instrumental variables regressions

As not all women that were offered a job start working, and as some women not offered a job at this time are able to find another job, we do not expect treatment to perfectly predict job status. To measure and to some extent account for imperfect compliance we also estimate an IV model of the following form:

(2) *Any wage job last 6 months*$_{i,t1} = \alpha Y_{i,t0} + \beta Treatment_i + \gamma X_{i,t0} + \delta List_i + \epsilon_{it}$;

(3) $Y_{i,t1} = \alpha Y_{i,t0} + \beta$ *Predicted(Any wage job last 6 months)*$_{i,t1} + \gamma X_{i,t0} + \delta List_i + \epsilon_{it}$

That is, we predict recent formal wage employment with the randomization and use the predicted values for formal employment in the second stage to calculate the local average treatment effect of having a formal job on political participation. We view the ITT specification as the main specification but the IV model is interesting as it scales the effect by the first stage effect that the randomization has on employment. In the longer run follow up we also pre-registered that we would instrument *Any factory job* and *Months factory job* with *Treatment.*

In Table A10 we show IV results for having had any wage job last 6 months. In Panel A we see that the short run causal effect of having had a wage job during the last six months is negative once we instrument it with random job offers (columns 1 and 2). Columns 3 and 4 shows that the effect on interest in politics is close to zero (but we cannot reject relatively large effects). In Panel B we show the longer run effects and they are very imprecisely estimated. In Table A11 we see that the effects of earnings and share of earnings are also statistically insignificant.

In Tables A12 and A13 we show results using our new pre-registered instrumental variables regressions and the outcome variables in Wave 5. These instruments have a stronger first stage and we see that the effects are more precisely estimated.

Table A10: IV estimates of the effects of being employed last six months.

(a) Panel A: Wave 2 (first follow up)

	(1) Community	(2) Community	(3) Interested	(4) Interested	(5) Raise	(6) Raise	(7) Kebele	(8) Ke...
Any wage job last 6 months	-0.29***	-0.29***	-0.0014	0.011	-0.086	-0.080	-0.25***	-0.2...
	(0.071)	(0.069)	(0.063)	(0.063)	(0.057)	(0.058)	(0.069)	(0.0...
Control mean	0.52	0.53	0.34	0.34	0.23	0.24	0.52	0.54
N	1262	1196	1260	1194	1262	1196	1261	119...
R-squared	0.49	0.55	0.44	0.46	0.30	0.35	0.52	0.58
Controls	Block	Full	Block	Full	Block	Full	Block	Ful...

(b) Panel B: Wave 5 (fourth follow up)

	(1) Community	(2) Community	(3) Interested	(4) Interested	(5) Raise	(6) Raise	(7) Kebele	(8) Kebele
Any wage last 6 months	-0.43	-0.41	-0.031	-0.083	-0.37	-0.42	-0.66**	-0.60**
	(0.26)	(0.27)	(0.22)	(0.23)	(0.25)	(0.26)	(0.28)	(0.28)
Control mean	0.49	0.49	0.35	0.36	0.33	0.34	0.45	0.46
N	1054	1002	1054	1002	1054	1002	1054	1002
R-squared	0.47	0.49	0.53	0.54	0.30	0.31	0.33	0.40
Controls	Block	Full	Block	Full	Block	Full	Block	Full

Notes: All regressions control for List fixed effects. Robust SE in parentheses.

Table A11: IV estimates for wave 5 using pre-registered specifications.

	(1) Community	(2) Community	(3) Community	(4) Community
Earnings from wage job last 6 months	-0.000055			
	(0.000035)			
Share of earnings from wage job		-0.81		
		(0.62)		
Earnings job last 6 months			-0.000058	
			(0.000043)	
Share of earnings				-0.97
				(1.37)
Control mean	0.49	0.49	0.49	0.50
N	1054	585	1054	801
R-squared	0.41	0.43	0.22	0.34
Controls	Block	Block	Block	Block

Notes: All regressions control for List fixed effects. Robust SE in parentheses.

Table A12: IV estimates for other variables. Instrumenting months with Treatment

	(1) Vote	(2) Protest	(3) Internal	(4) External
Months with factory job	-0.00093	-0.0011	-0.0088***	-0.0059*
	(0.0016)	(0.0025)	(0.0030)	(0.0031)
Control mean	0.95	0.20	0.42	0.51
N	1054	1054	1044	1046
R-squared	0.95	0.33	0.53	0.61
Controls	Block	Block	Block	Block

Notes: All regressions control for List fixed effects. Robust SE in parentheses.

Table A13: IV estimates for other variables. Instrumenting Any month with Treatment

	(1) Vote	(2) Protest	(3) Internal	(4) External
Any month with factory job	-0.023	-0.027	-0.22***	-0.15*
	(0.040)	(0.062)	(0.075)	(0.076)
Control mean	0.95	0.20	0.42	0.51
N	1054	1054	1044	1046
R-squared	0.95	0.33	0.54	0.62
Controls	Block	Block	Block	Block

Notes: All regressions control for List fixed effects. Robust SE in parentheses.

A.3 Heterogeneity across areas

To compare our sample to data from the DHS we use 50 km buffer zones around the industrial parks and spatially join all points from the GPS data in the DHS surveys to them.

(a) Industrial Parks with 50 km buffer zones

(b) Location of DHS clusters.
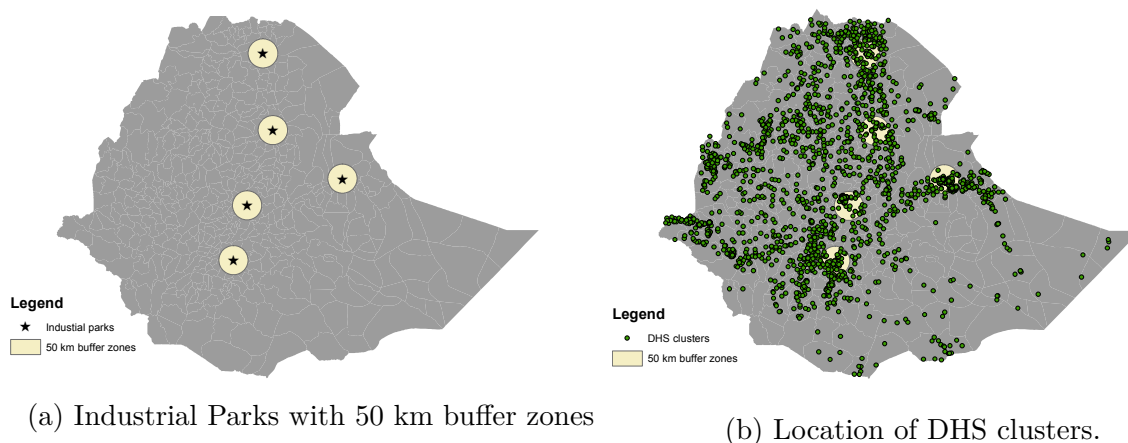
Figure A2: Industrial Parks and DHS data

Table A14 shows descriptive statistics for urban women in the factory areas and women outside factory areas. We note that the women in factory areas are relatively similar to the women in our sample but women in other areas of Ethiopia are different. Table A15 shows differences across factory areas, where Amhara stands out with lower levels of employment. In Table A16 we use data from our experiment again and we see that a negative treatment effect on community meetings in all areas, although it is not statistically significant in Tigray. In Table A17 we show that the experiment led to large increases in employment in all regions except Oromia, but there we only have 23 observations.

Table A14: Descriptive statistics for urban women in factory areas and women outside factory areas.

| | Factory areas | | | Other areas | | |
| | (1) | | | (2) | | |
	Mean	SD	N	Mean	SD	N
Medium education	0.156	(0.363)	1508	0.068	(0.251)	4513
High education	0.344	(0.475)	1508	0.083	(0.275)	4513
Any formal wage job (ever)	0.492	(0.500)	1508	0.187	(0.390)	4513
N	1508			4513		

Notes: The sample of women in factory areas includes only those between 20-30 years old living in urban areas within a 50 km radius from the factories. The women in the larger DHS sample include all women aged 20-30 years old living outside of the 50 km buffer zones. All data is from DHS 2016.

Table A15: Descriptive statistics for urban women by factory area.

| | Amhara | | Dire Dawa | | Oromia | | SNNP | | Tigray | |
| | (1) | | (2) | | (3) | | (4) | | (5) | |
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Medium education	0.304	(0.470)	0.149	(0.356)	0.157	(0.364)	0.174	(0.388)	0.158	(0.367)
High education	0.217	(0.422)	0.277	(0.448)	0.390	(0.488)	0.391	(0.499)	0.408	(0.495)
Any formal wage job (ever)	0.261	(0.449)	0.438	(0.496)	0.544	(0.498)	0.435	(0.507)	0.461	(0.502)
N	23		592		794		23		76	

Notes: The sample of women in factory areas includes only those between 20-30 years old living in urban areas within a 50 km radius from the factories. All data is from DHS 2016.

Table A16: Treatment effect on community meeting attendance in different areas.

	(1) Amhara	(2) Dire Dawa	(3) Oromia	(4) SNNP	(5) Tigray
Treatment	-0.20*	-0.12*	-0.47**	-0.23***	-0.042
	(0.12)	(0.066)	(0.19)	(0.059)	(0.040)
Mean dep. var in C group	0.58	0.52	0.60	0.57	0.48
No. of observations	70	251	23	305	613
R-squared	0.04	0.05	0.34	0.11	0.06
Controls	Block	Block	Block	Block	Block

Notes: The sample of women in factory areas includes only those between 20-30 years old living in urban areas within a 50 km radius from the factories. All data is from DHS 2016.

Table A17: Effects of job offers on employment in different areas.

	(1) Amhara	(2) Dire Dawa	(3) Oromia	(4) SNNP	(5) Tigray
Treatment	0.89***	0.41***	-0.27*	0.75***	0.22***
	(0.051)	(0.060)	(0.15)	(0.042)	(0.036)
Mean dep. var in C group	0.06	0.27	0.90	0.16	0.38
No. of observations	70	251	23	305	613
R-squared	0.80	0.18	0.46	0.60	0.23
Controls	Block	Block	Block	Block	Block

Notes: The sample of women in our areas includes only those between 20-30 years old living in urban areas within a 50 km radius from the factories. All data is from DHS 2016.

A.4   Heterogeneity across factories

Two of the factories have trade unions, Almeda Textiles and MAA Garment and Textiles. Both of these factories are Ethiopian owned and located in the Tigray region in Northern Ethiopia. Almeda Textiles, is owned by the EFFORT group – (the Endowment Fund for Re-habilitation of Tigray). EFFORT was established in the mid-1990s with resources accumulated and donated by the Tigray People's Liberation Front, and its corporate policy is still led by senior members of the TPLF (Vaughan and Gebremichael 2011). MAA Garment and Textiles belongs to the company Kebire enterprise that is owned by Saudi-Ethiopian business mogul Mohammed Al Amoudi. Al Amoudi has been one of the largest actors in the Ethiopian economy since the early 1990s, and is a close ally to the Ethiopian Government (Gebregziabher and Hout 2018). A common denominator for both these factories is thus that they have owners who are directly linked to the political leadership in the country. This close relationship facilitates a stronger political control of labor affairs in the factories. The establishment of organised unions may therefore not be perceived as a threat, but as a way of cooptation and control of the workers. Another interpretation is that these factories have no plans of stopping production and leaving the country. They may therefore have a longer time horizon and view workers rights as a productivity enhancing investment. They may also care more about the workers, for political or in-group altruistic reasons.

We conduct analyses by factory. For privacy reasons we restrict the factories to those where we have at least 20 individuals in our data. In Figure A3 we see that there are negative effects on meeting attendance in wave 2 in all but two factories, where the coefficient is very close to zero. These two factories are Almeda Textile and Velocity Textile. Both of these factories are in Tigray. Almeda Textile is also one of the two factories that have trade unions. Velocity Textile is owned by a company from United Arab Emirates. In Figure A4 we see that there are still negative effects in most areas, but now the effects at Velocity textile are even positive. In wave 5 we also investigate the

heterogeneity in the effects on internal political efficacy by factory and we see in Figure A5 that the effects are negative in most factories.
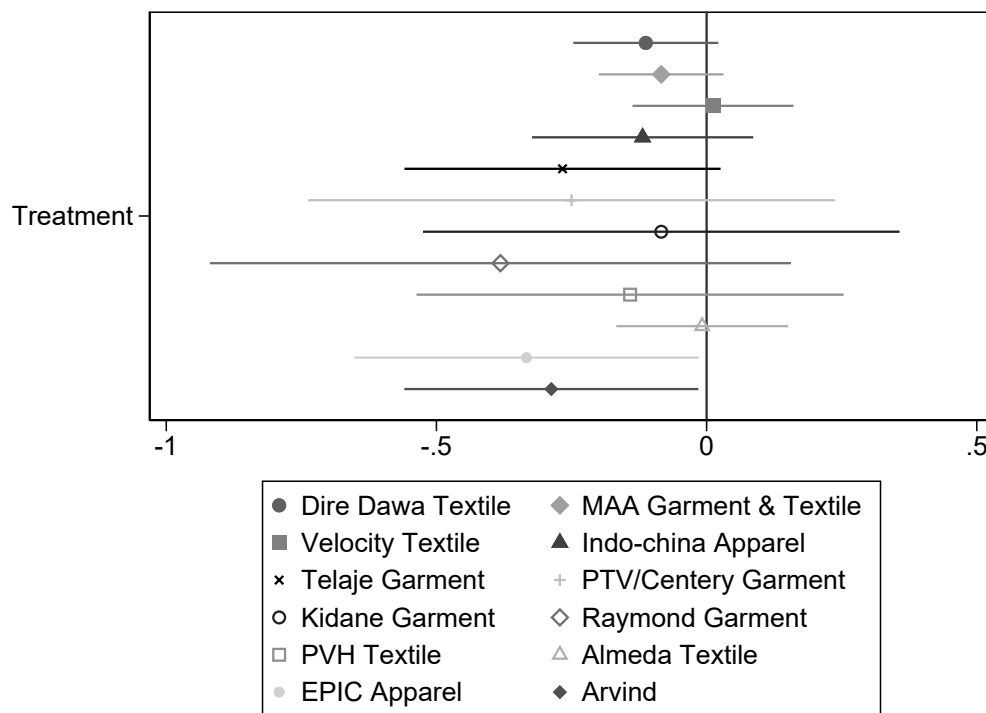

Figure A3: Treatment effects on community meeting attendance in different factories in wave 2
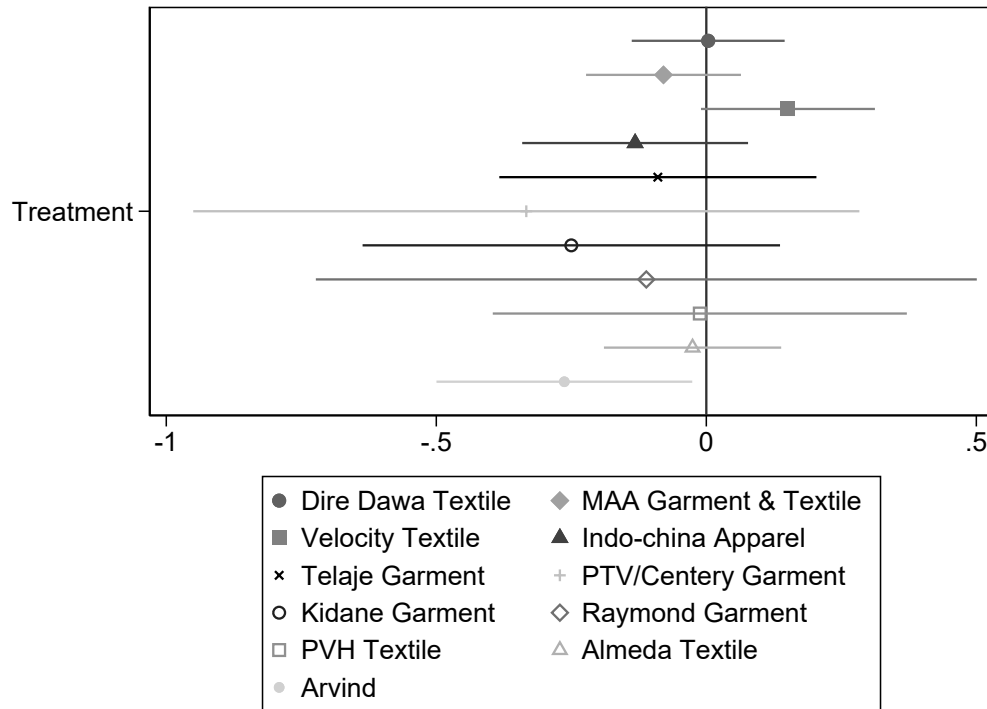
Figure A4: Treatment effects on community meeting attendance in different factories in wave 5
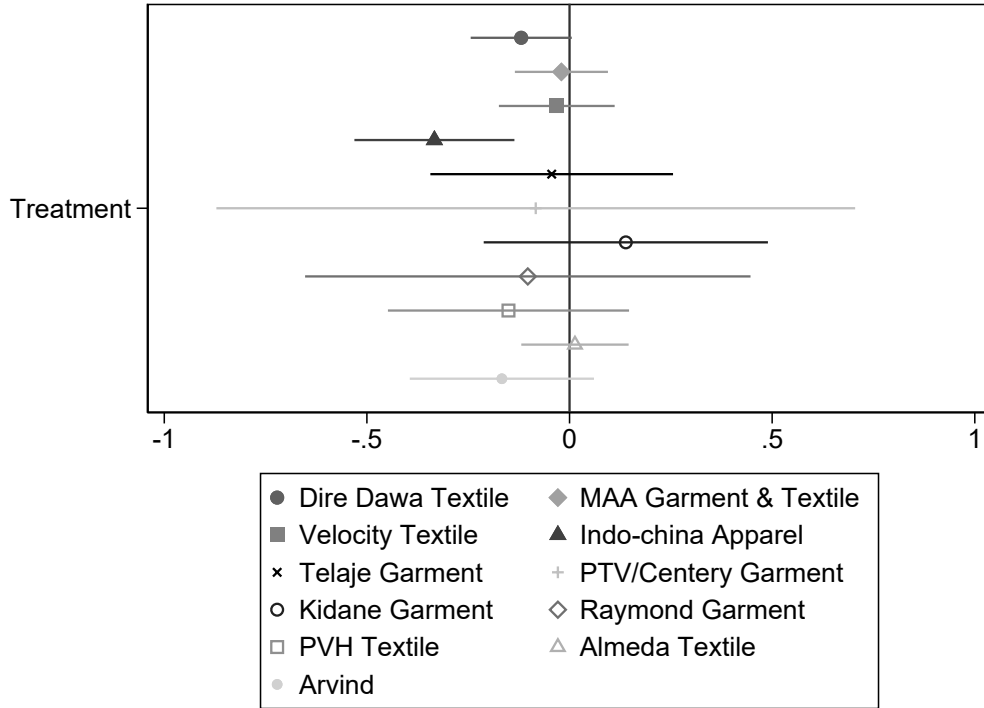
Figure A5: Treatment effects on internal political efficacy in different factories in wave 5

In Figures A6-A8 we show results by country of ownership of the factories. We see that the effects on meeting attendance is negative for all factory groups based on ownership except for a factory with owners in the United Arab Emirates. The results are similar in wave 5 albeit less precisely estimated for most groups. For internal political efficacy the effects are either negative or close to zero for all groups. We additionally explored whether there are differences across factories with a smaller or larger share of female workers (this ranges from 20-97 percent in our data), with women in management positions or not (9 percent of the factories have no women in management positions), and with different start years (ranging from 1986-2017). We found no statistically significant differences along any of these dimensions.
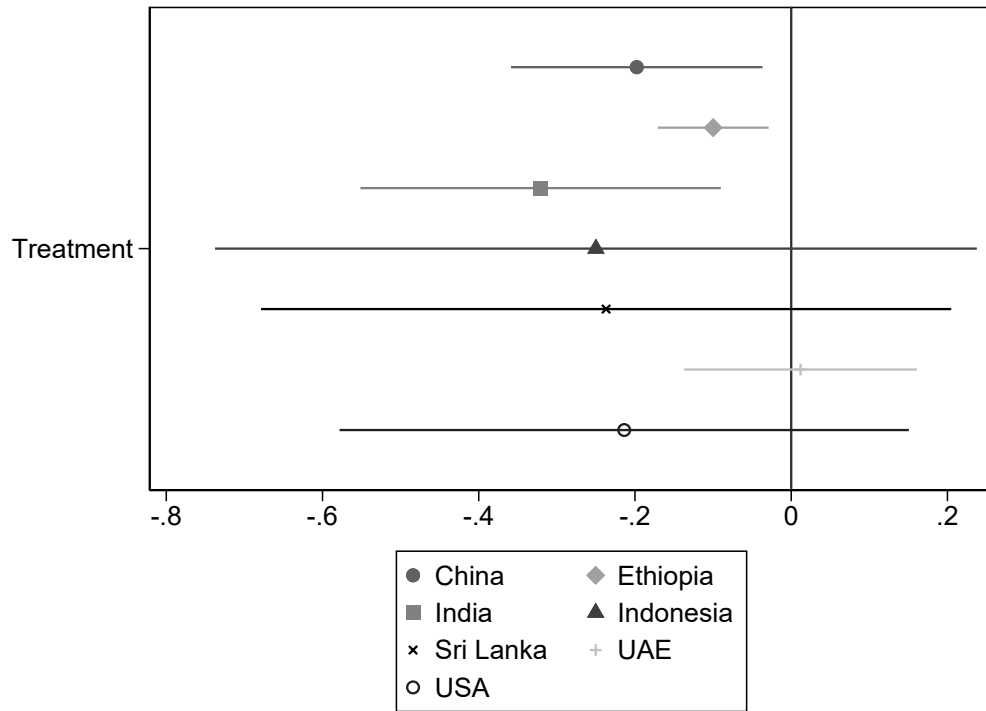
Figure A6: Treatment effects on community meeting attendance in wave 2 attendance by country of ownership
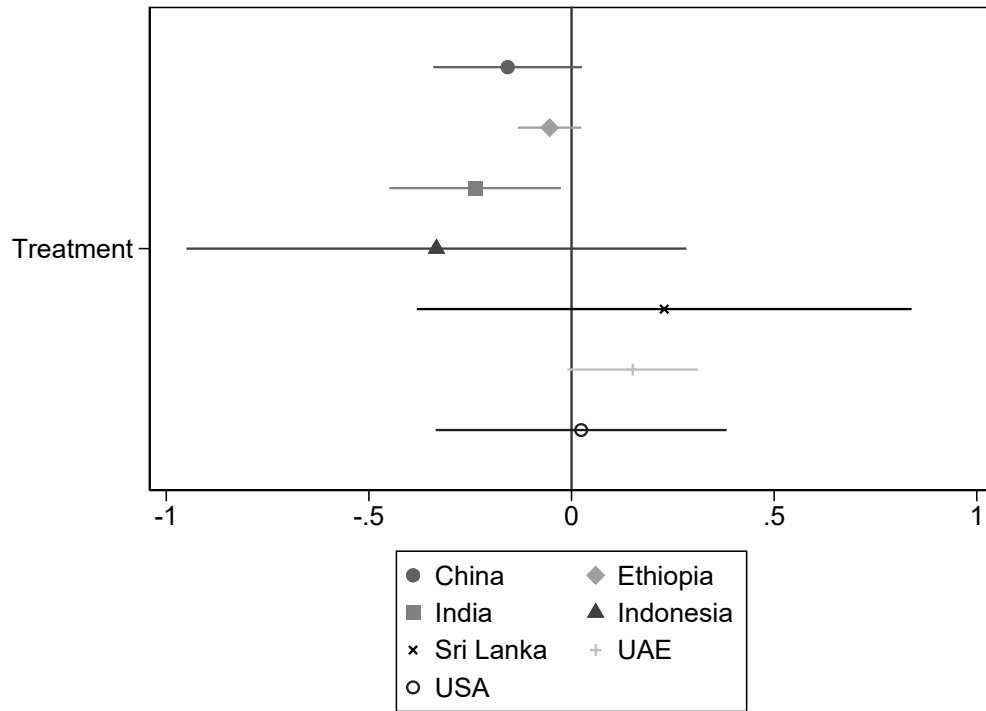
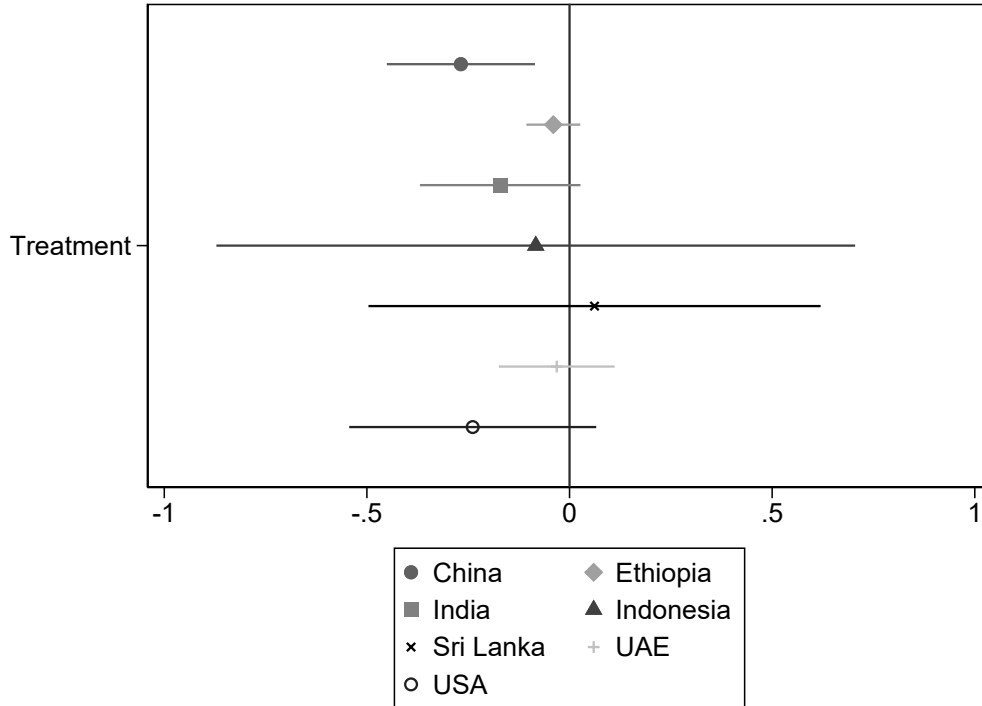Figure A7: Treatment effects on community meeting attendance in wave 5 by country of ownership

Figure A8: Treatment effects on internal political efficacy in wave 5 by country of ownership

A.5 The role of research design

Our results show that the job offers led to increased employment and earnings, but reduced political participation. We find no evidence, therefore, to support the prominent resource hypothesis by which employment would lead to an increase in political participation. On the contrary, our findings show that such employment leads to reductions in some measures of political participation, and to lower political efficacy. While previous studies, focusing mostly on high-income countries, show mixed results, our findings are in contradiction to some of those claiming to identify the causal effect of employment (such as Marx and Nguyen (2016) who find that job loss reduces political efficacy and participation). As our study is the first to exploit random variation in employment, we believe it is interesting to try and understand to what extent the differences in results are driven by research design.

Table A18: "Effects" of employment using pooled OLS and fixed effect models.

	(1) Community	(2) Interested	(3) Community	(4) Interested
Any wage last 6 months	-0.10***	-0.12***	0.018	-0.081**
	(0.027)	(0.027)	(0.038)	(0.037)
Control mean	0.55	0.40	0.55	0.40
N	2321	2320	2321	2320
R-squared	0.02	0.03	0.54	0.55
Individual FE	No	No	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes

Notes: Waves 2 to 5. Only control group individuals are included in the sample. All regressions control for wave fixed effects and SE are clustered at the individual level.

To asses this, we conduct a number of analyses. First of all, we have already shown in Table 1 that across the African countries and in Ethiopia, women with employment are more likely to participate in politics and are more interested in politics. Comparing women with a formal job to women without a formal job is likely to be misleading for causal inference for a number of reasons. We do not know if employment causes participation, if participation causes employment, or whether there are omitted factors that affect both employment and participation. One particularly relevant concern is the fact that women *applying* for jobs are different in many ways from women not applying for jobs.

To assess whether the correlation between employment and political participation is biased by employed women's willingness (and ability) to take up paid employment, we next assess the cross-sectional correlation in the control group of our study. We pool together waves 2-5 as any wage job during the last six months is consistently measured for these waves and we focus on the control group only. We find that in this subsample, the correlation between paid employment and political participation as well as political interest turns negative (see columns 1 and 2 of Table A18).

In addition to conditioning on willingness to work, we can make use of the panel nature of our data to assess the effect of these unobserved differences. We again pool all follow-up observations for the control group (Wave 2 up to Wave 5) and regress

political participation on employment, controlling for wave fixed effects and individual fixed effects. This means we now relate changes in political participation within the same individual to changes in their employment status. As such, the analyses control for all time-invariant differences between women, including innate ability or innate proclivity to work or participate in politics. The results, in columns 3 and 4 of Table A18, show that these individual fixed effects regressions yield no statistically significant effect of employment on meeting attendance and still a large negative effect on political interest. We can compare these to our analysis of treatment effects in the first follow-up wave, where we found a strong negative effect on participation in meetings and no effect on interest.

In Table A19 we show that the fixed effects estimations are not sensitive to controlling for time fixed effects and that there are no statistically significant effects in these models on raise issue and kebele meetings.

Table A19: "Effects" of employment using fixed effect models with and without wave fixed effects.

	(1) Community	(2) Community	(3) Interested	(4) Interested	(5) Raise	(6) Raise	(7) Kebele	(8) Kebele
Any wage last 6 months	0.022	0.0063	-0.062**	-0.091***	0.016	-0.0031	-0.027	-0.011
	(0.031)	(0.031)	(0.031)	(0.032)	(0.028)	(0.029)	(0.027)	(0.029)
Control mean	0.53	0.53	0.39	0.39	0.31	0.31	0.52	0.52
N	3024	3024	3023	3023	3024	3024	3024	3024
R-squared	0.47	0.47	0.43	0.45	0.42	0.44	0.56	0.56
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	No	Yes	No	Yes	No	Yes	No	Yes

Notes: All regressions control for individual fixed effects and SE are clustered at the individual level.

As these results differ substantively from the results when we exploit random assignment of jobs, they show that it is not enough to use panel data to get rid of the bias in the relationship between employment and political outcomes.

A.6 Gender gaps in the Afrobarometer

Table A20: Gender gaps in the Afrobarometer (all countries).

	(1) Community	(2) Interested	(3) Raised	(4) Voted	(5) Protest	(6) Paid Employment
Female	-0.11***	-0.12***	-0.13***	-0.032***	-0.036***	-0.14***
	(0.0042)	(0.0042)	(0.0043)	(0.0035)	(0.0025)	(0.0041)
Mean dep. for men	0.64	0.67	0.61	0.84	0.11	0.42
N	53619	53384	53523	47511	52801	53746
R-squared	0.01	0.02	0.02	0.00	0.00	0.02
Sample	All	All	All	All	All	All

Notes: Results from Afrobarometer round 5. Robust SE in parentheses.

Table A21: Gender gaps in Ethiopia in the Afrobarometer.

	(1) Community	(2) Interested	(3) Raised	(4) Voted	(5) Protest	(6) Paid Employment
Female	-0.15***	-0.11***	-0.19***	-0.071***	-0.046***	-0.19***
	(0.018)	(0.014)	(0.019)	(0.015)	(0.014)	(0.019)
Mean dep. for men	0.82	0.91	0.76	0.89	0.15	0.79
N	2339	2343	2309	2112	2217	2368
R-squared	0.03	0.02	0.04	0.01	0.00	0.04
Sample	All	All	All	All	All	All

Notes: Results from Afrobarometer round 5. Robust SE in parentheses.

A.7 Empowerment and attitudes toward gender equality

We create an empowerment index based on 12 different questions on intra-household decision making.[14] For each of the 12 questions we create a dummy variable which equals 1 if the partner has the final say or if the partner decides together with some other member of the household.[15] We then take the average of the 12 variables to get an index ranging between 0 and 1. The baseline average in our sample is 0.36 and we also show that the partners decide on around four of the items (Nr empowerment items). The survey also includes questions on a wider set of attitudes toward gender equality. We recode each of these questions into dummy variables so that 1 is a gender unequal attitude.[16] We again create an index by averaging across 11 questions on gender equality attitudes. The index average 0.126 at baseline, which indicates that women's attitudes were relatively pro-equality. We see that of the 11 items women on average disagree with only 1.3. Over time we note that time use on the measured activities have reduced somewhat and empowerment as well as gender equal attitudes have improved.

In Table A22 we see that there is no treatment effect on these variables.

A.8 Membership in various local organizations

As explained in the main text, political participation in Ethiopia at the local level is often in the kebele or in different sub-kebele networks. At the lowest level in the sub-kebele

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[14]We have 15 different questions in the survey on intra-household decision making. Not all questions apply to all people in the sample, however. For example, the decision to send a child to school has missing values for all individuals that do not have children. We therefore pre-registered that we would use the other 12 (Questions J1.03-J1.15 in the survey).

[15]If the individual decides together with the partner we code the variable as zero only if she has "a lot" of input into the decision (i.e. category 4 on the J1B questions) and otherwise as 1. All other values of J1A are coded as zero on the partner has the final say variables.

[16]See questions GA1-GA11 in the survey, we recode e.g. 1 or 2 to be 1 on statement GA1 and 3 or 4 on statement GA2.

Table A22: Effects of treatment on empowerment and attitudes toward gender equality.

(a) Panel A: Wave 2 (first follow up)

| | (1) | (2) | (3) | (4) |
	Empowerment	Empowerment	Gender equality	Gender equality
Treatment	-0.012	-0.0035	-0.0075	-0.0047
	(0.019)	(0.019)	(0.0074)	(0.0073)
Control mean	0.38	0.38	0.13	0.13
N	1041	1041	1260	1260
R-squared	0.22	0.30	0.16	0.21
Controls	Block	Controls	Block	Controls

(b) Panel B: Wave 5 (fourth follow up)

| | (1) | (2) | (3) | (4) |
	Empowerment	Empowerment	Gender equality	Gender equality
Treatment	-0.0080	-0.0099	-0.011	-0.0093
	(0.022)	(0.023)	(0.0072)	(0.0072)
Control mean	0.45	0.45	0.13	0.13
N	806	806	1037	1037
R-squared	0.36	0.37	0.17	0.19
Controls	Block	Controls	Block	Controls

Notes: All regressions control for List fixed effects. Robust SE in parentheses.

hierarchy, we find the One-to-Five networks where women from five households are organized into one group, lead by one of the women. Above the One-to-Five networks are the Development Teams which is a group of One-to-Five network leaders, representing about 30 households. Officially, both these networks were established to deepen development efforts (related to health, education, etc.) and promote agricultural productivity. In practice, however, they are also used for regime mobilization and control (Emmenegger 2016).

Another important arena for women's local participation are the Ethiopian Women's Associations, formally independent, but practically closely linked to the government and ruling party. The Women's Association has branches all over Ethiopia, and serves many of the same purposes as the One-to-Five networks and Development Teams, focusing on economic and social support for the members and as an information channels for the government. Many of its leaders are also leaders of the One-to-Five networks and Development Teams, demonstrating the close link between these on the ground. Springing out from the kebele and the sub-kebele structures, women may organize into microfinance

cooperatives. These are cooperative businesses with funding from public credit institutions at the zonal administration level. The kebele and the sub-kebele structures are instrumental in facilitating access to the loans, and the sub-kebele networks are often, but not always, the basis for organizing the cooperatives.

Among women's meeting arenas that are independent from government and party interference are the Mahiber (Christian prayer associations), the Idir (funeral associations sharing funeral costs and other social costs), and Equb (traditional saving associations). Membership in these associations is based on personal relationships and trust, shared congregation, neighborhood, or close acquaintances. Finally, there are user organizations which are often organized around using specific recourses such as irrigation water or fertilizers.

In Figure A9 we show membership in the different types of local organizations for the women in our sample, at baseline.[17]

The most common organization is the funeral association, Idir, of which around one third of the women are a member. Next are the religious groups: 15 percent are members of the Mahiber and slightly more women are members of other religious groups. Around 15 percent of the women were members of a One-to-Five network and slightly fewer are members of Women's Associations and savings groups (Equb). Almost 10 percent were members of Development Teams. Micro-finance cooperatives are less common, with only about five percent membership. and user groups are also not very common. Finally, very few women were member of a trade union linked to the national federation of trade unions and no one was a member of Informal Workers' Groups, which are local associations of workers.

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[17]In our survey we asked about the existence, membership, and roles in networks. The following 12 networks are included, in addition to an "other" category: Mahiber, Other religious associations, Women's association, Microfinance cooperative, One-to-five networks, Development teams, Idir, Equb, Trade union, Informal Workers group, Users association, customary institution. The first question is "Does this exist in your neighborhood or workplace? (Yes=1, No=0)". Then we ask: "Are you currently member of the specified network? (Yes=1, No=0)".
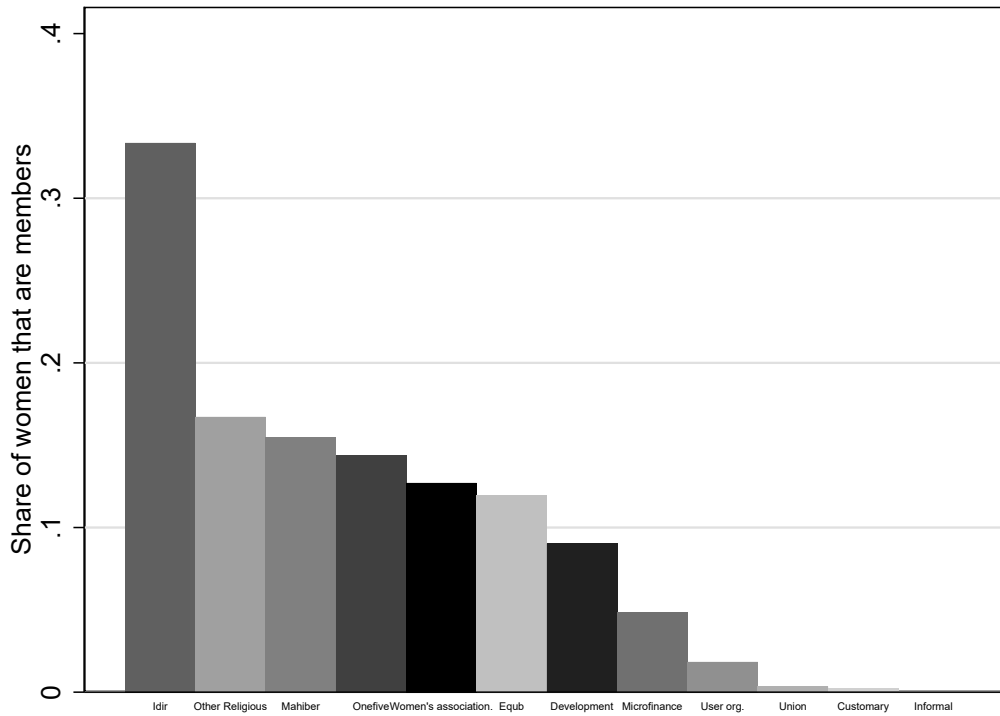
Figure A9: Membership in various organizations and networks at baseline

In Figure A10 we show the evolution of memberships in these organizations from baseline to the first follow up and in Figure A11 we show the effects of treatment on membership. We see that memberships in Idir is reduced while membership in Informal worker organizations increases as an effect of treatment. If we code membership in any organization we see that this is negative, but not statistically significantly so.
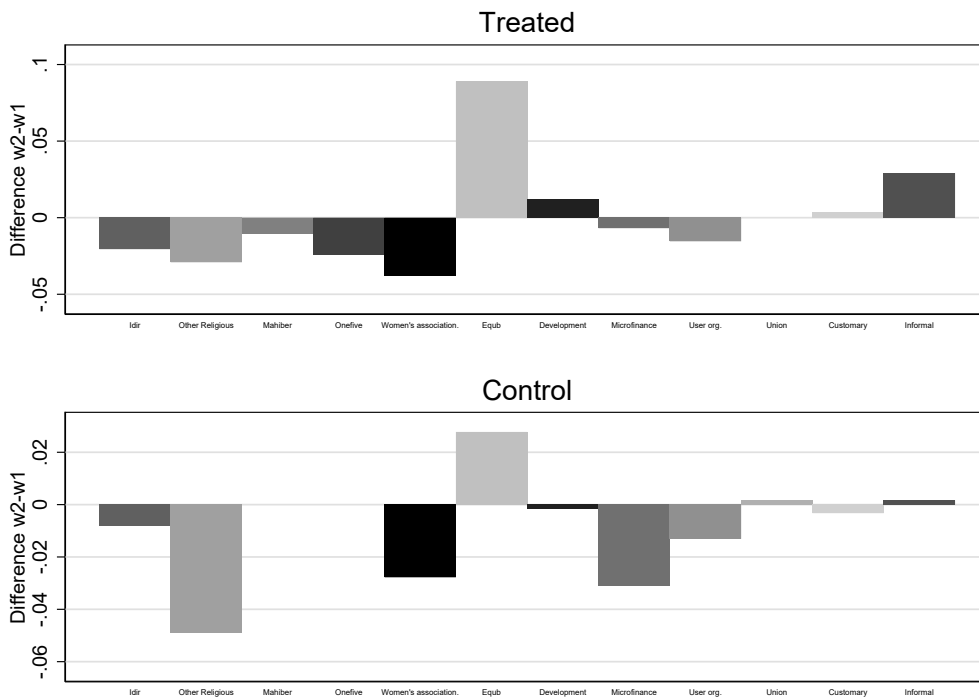
Figure A10: Evolution of membership from baseline to wave 2
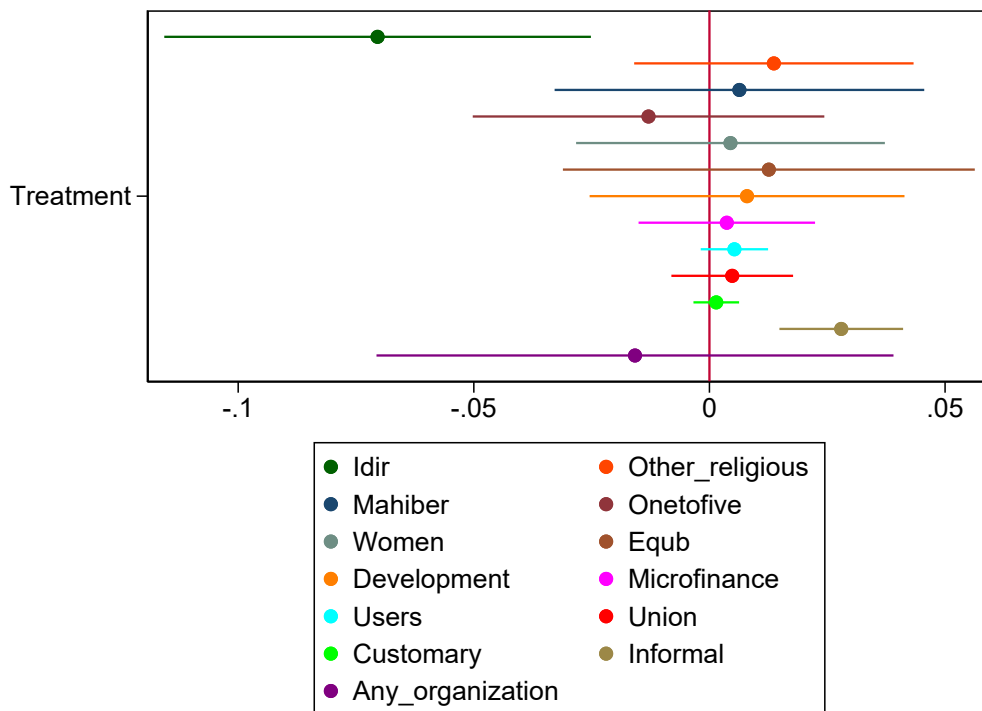
Figure A11: Treatment effects of membership in various organizations and networks in wave 2

A.9 Effects on political preferences

Our analysis speaks to the literatures on the role of material resources in the malleability of political preferences (Margalit 2013; Margalit 2019) and on the role of employment in explaining gender gaps in political preferences (Finseraas et al. 2012; Iversen and Rosenbluth 2006; Paxton 2007). Most of this literature is based on findings in western and developed countries and focuses on broad notions of political preferences, such as positioning on the left-right scale. In a recent contribution, however, Gottlieb et al. (2018) find that employment is correlated with a narrowing of the gender gap in Africa in two key policy domains: prioritization of infrastructure investment and access to clean water.

To investigate effects on political preferences we code the following variables:

Political priorities: We create a variable "Roads and Infrastructure" based on the following question: "If you could only choose between water or roads and infrastructure, which problem do you think is the most important one facing Ethiopia that the government should address?". The answer categories will be "1=Water", "2= Roads and Infrastructure", "3= I do not know". We will code the variable *Roads and Infrastructure* as 1 if the answer is 2 and as 0 if it is 1 or 3.

Redistribution: We also create three different variables on redistributive preferences and taxes. These questions are of the following form:

"Please say whether you strongly agree, agree, disagree, or strongly disagree with these statements (1 = Strongly agree 2 = Agree 3 = Disagree 4= Strongly disagree):

In Ethiopia, the national government should aim to reduce the economic differences between the rich and the poor.

In Ethiopia, the national government should have taxes on people owning houses to reduce the economic differences between the rich and the poor.

Table A23: Effects of treatment on political preferences.

	(1) Roads	(2) Reduce differences	(3) Tax houses	(4) Tax work
Treatment	-0.0091	-0.0081	0.0089	0.0092
	(0.026)	(0.023)	(0.017)	(0.019)
Control mean	0.29	0.38	0.39	0.39
N	1054	1048	1052	1052
R-squared	0.21	0.53	0.71	0.66
Controls	Block	Block	Block	Block

Notes: All regressions control for List fixed effects. Robust SE in parentheses.

In Ethiopia, the national government should have taxes on people working to reduce the economic differences between the rich and the poor."

These variables will be coded as binary variables: *Reduce differences*, *Tax houses*, and *Tax work.* The binary variables will be set in order to create 2 groups, as similar in size as possible, while respecting the order of the answers (from 1 to 4).

In contrast to the results in Gottlieb et al. (2018), in Table A23, we find that when employment is randomly induced it has no effect on any political preferences.

A.10   Qualitative method and details about interviews

The qualitative interviews were done during several rounds from 2017 to 2019, in three of the industrial sites that were part of the study: Hawassa, SNNPRS (June 27-July 7, 2017), Mekelle, Tigray (July 11-21, 2017) and Dukem, Oromia (November 1-10, 2018 and November 5-15, 2019). They represent the different types of context and organization in which the factories in our study are situated. Hawassa Industrial Park is the flagship of the government-owned parks. Established in 2017, the park accommodated 18 foreign companies within the textile and apparel industry from a variety of countries (US, India, China, Sri Lanka, Indonesia, UK, Belgium, France, Spain), and has since then expanded. The park is located in Hawassa, the capital of the multiethnic southern region (250 km from the national capital Addis Ababa). Workers come from a multitude of the 56 ethnic groups in the region, with a majority from the Sidama ethnic group, whose land the

park was build on. Eastern Industrial Zone is located in Dukem, 45 kilometres south of Addis Ababa, in Ethiopia's largest region, Oromia. Established in 2007, the zone was the first industrial park in Ethiopia. In contrast to Hawassa, it is privately owned by Chinese investors. It includes 107 companies, mainly from China, but also from the Netherlands, India and Ethiopia. Workers come from the Oromo and Amhara-dominated town of Dukem as well as from all over Oromia region. The workers interviewed in Tigray worked in different factories, including Ethiopian and foreign owned private companies in the vicinity of the regional capital Mekelle. The workers were almost exclusively from Tigray, Ethiopia's northernmost region (750 km from Addis Ababa).

The 28 factory workers interviewed for the qualitative data collection (6 in Mekelle, 8 in Hawassa and 14 in Dukem) were randomly drawn from the survey sample. In addition, leaders of Women's Associations, Development teams and One-to-Five networks in the local communities around factories and workers' homes were interviewed (11 interviews altogether, 4 in Mekelle, 3 in Hawassa and 4 in Dukem). The leaders were identified with the help of both the factory workers interviewed and officials at the local kebeles and at the municipality level. We further interviewed government officials from the regional administrations and local administrations around the factories (Dukem, Hawassa and Mekelle municipalities), from the Bureau of Industry and Trade; Labor and Social Affairs; and Women, Children and Youth Affairs (28 officials altogether). These are the offices most relevant to understanding the government's approach to industrial development. Similarly, representatives from the Confederation of Ethiopian Trade Unions (CETU) were interviewed both at the regional and municipality level (4 in total). Finally, we interviewed factory managers in Hawassa Industrial Park (4), in Eastern Industrial Zone in Dukem (3) and in Mekelle (3).

The majority of the interviews were done with the help of research assistants translating from Amharic, Tigrigna and Oromiffa into English. Interviews with some of the regional government officials and all the factory managers were done directly in English.

Written notes (if possible, word-for-word transcripts) were taken during the interviews.[18]

The factory worker interviews were both semi-structured (Leech 2002) and in-dept (Hesse-Biber 2010). Using an interview guide as a starting point (see below), the interviews were structured around certain predefined issues and had the aim of gaining rich qualitative data on the context in which the women work and live, particularly the mechanisms determining their political participation and interest. By using open-ended questions and by allowing the interviewee to go more in-depth into issues they found engaging and important, both the researcher and the interviewee could steer the dialogue, making the conversation more dynamic and rich than a structured interview with closed questions would allow. This allowed the researcher to look for patterns that emerged from the 'thick descriptions' recounted by the workers. To ensure privacy of the conversation, the interviews were either done at home in a separate room away from husbands and other family members or in places outside, such as a café or in a park, suggested by the interviewees.

The other interviews were also structured around predefined issues considered particularly relevant for the interviewee, and moved from general, non-sensitive issues to the more specific and sensitive topics regarding the conditions of female factory workers. Structuring the interviews in this way, it was easier to build trust, or what is described as building rapport - the degree of comfort in the interactions between the researcher and research participants (Given 2008). The interviews with the women's networks and association leaders started with general questions on their activities and mandates, gradually moving to the issue of factory workers' engagement. With government officials and trade union representatives, the interviews also started with questions on activities and mandates, thereafter moving to issues about labor conditions in the factories and their relationships to the investors. Finally, interviews with factory managers started with questions about the general conditions for establishing industries in Ethiopia, grad-

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[18]We decided against recording the interviews on tape, as our previous experience with tape recording in politically tense contexts suggests interviewees may refrain from addressing sensitive issues.

ually moving towards how they dealt with labor issues and conflict between workers and management.

Interview guide for qualitative interviews of female factory workers:

1. What did you do before you started working in the factory? Education?

2. How did you get to know about the factory job?

3. How long have you worked in the factory, and what are your tasks/position in the factory?

4. Monthly salary, working hours, overtime?

5. Do you have a written contract for your work?

6. Did you have any chances of promotion during your work in the factory?

7. What is a typical working day for you, from when you wake up until you go to bed?

8. How are conflicts between workers and management (on working conditions, salary etc.) solved in the factory?

9. Did you ever protest/raise an issues with the management related to salary or working conditions, together with co-workers?

10. Are you member of a trade union? Why/ why not?

11. Are you member of any other organization or meetings at the workplace (idir/ iqob, etc)? Why/ why not?

12. Are you member of any other networks outside of work? Why/ why not?

13. How interested are you in politics? If you had a chance, would you be active in local politics or organisations, or would you rather avoid this?

14. How long would you like to continue as a factory worker?

15. What other options do you have for salaried work – and in which way are they better/worse than the factory job?

A.11   Ethical considerations and principles

Our research has been undertaken with participants in a setting that requires careful planning to ensure ethical principles are not violated. There are several factors of our project and data collection that merit careful thought and discussion with respect to ethical considerations.

As we conducted a randomized field experiment, we start with laying out our reasoning of why we think randomization is ethically defensible in our context. As we describe in the paper, we worked with shoes and garment factories that frequently hire new workers in bulk. Applicants are typically asked to gather on a specific day and are screened for eligibility using verbal and physical tests. So far, this is standard operating procedure for the factories.

For each factory-hiring round, our intervention then consisted of compiling a list of all eligible women who were living with a partner, as the initial data collection was centered around a study of intimate partner violence. From the list of eligible partnered applicants we then randomly assigned half to receiving a job offer in the given factory and the remaining women to a control group, in which no one received a job offer.

There are several overarching aspects of the randomization that we discuss here, before turning to a more detailed discussion about data collection principles. First of all, we deem it to be ethically fair to randomize job offers to individuals with similar qualifications when not everyone can get a job. That is, the randomization was possible since the number of qualified applicants far exceeded the number of available jobs. We did not restrict the number of job offers in order to have a control group for the study.

Secondly, the applicants on the lists were informed about the procedure before the randomization was conducted and all consented to being part of the procedure. As we describe in the main text, the applicants perceived it as fair that everyone that is equally

qualified gets an equal chance of getting the job offer. We regard the randomization to be ethically justified for the same reason and we also note that the procedure eliminates the scope for discrimination based on good looks, corruption, and sexual extortion, which we heard stories about during our field visits.

Thirdly, the fact that we only randomize jobs to partnered women implies that we are only intervening in a small share of the actual hires. The factories generally prefer to hire non-partnered women as it is their impression that non-partnered women are able to work more overtime and are less likely to quit. Hence, if a factory wanted to hire 1000 workers but there were only 100 partnered women on the lists, then we randomly assigned 50 of the partnered women to receive a job offer and the remaining 950 jobs were allocated according to standard operating procedures at the factory. Therefore, women not living with a partner were hired using normal procedures. The share of partnered to non-partnered women in different places and over time varied and we did not record any data on this. We do know, however, that the vast majority of the women hired were not-partnered. Fourth, we took effort not to take advantage of the increased job probability for the consenting women. As factories prefer non-partnered women, it is likely that the women on the lists consented to the randomization since the probability of being hired was actually considerably larger if they were part of the experiment than if they were not. We made it clear, however, that consenting to the randomization was independent of consenting to the data collection so that we did not use the potential increased hiring probability as a power advantage to make people participate in the surveys. We describe issues of power and consent in detail below.

In the remainder of this section, we describe the standard of conduct and the ethical principles adhered to in this study. We explain the ethical challenges encountered during the research and how we handled those. A primary concern of our project has been to address the large power imbalance between the research team and the respondents. The people interviewed are less educated, most of them are poor and they live under vulnerable

conditions both in terms of economic and political risks. Ensuring their autonomy and minimizing negative effects on their wellbeing has been paramount to this study.

Consent

We sought consent from each person we interviewed, each time we interviewed them. Our surveys contain the following consent forms:

Read: Hello. My name is ... and I am working with the Ethiopian Development Research Institute (EDRI). As you know, in collaboration with CMI, a research institute from Norway, we are conducting a survey to study the lives of women seeking work in the industrial sector in Ethiopia. We now conduct the first follow-up of the study. Accordingly, I would like to ask you some questions about you and your household, in privacy, about your current work and time use, education, health, economic and family status. The purpose is to provide information about women in Ethiopia, and to write a paper about this. We are interviewing many women like yourself in several different areas, and no names or information to identify the persons will be available to anyone else than the research team. We would, therefore, kindly request you to participate in this survey. The survey usually takes between 60 and 90 minutes to complete. The purpose is not to offer you assistance, but we would like to offer you ETB 50 for your time if you complete the questionnaire. All your answers will be kept private and confidential. Only the researchers will have access to your "identifying information", such as your name. The information you provide will not affect your employment relationship in any way and will not be shared with your employer. Participation in this survey is voluntary, and if we should come to any question you don't want to answer, just let me know and I will go on to the next question; or you can stop the interview at any time. However, we hope you will participate in the survey since your views are important to our research. At this time, do you want to ask me anything about the survey? May I begin the interview now?

Signature of interviewer: .... Date: ....

In our study areas, people have been generally inclined to consent to interviews as long as their time is compensated. Given the power imbalances and the sensitivity of some questions due to protests against the government, we carefully designed the consent section to ensure participants were informed and that the interview was indeed conducted without any pressure to participate.

Enumerators were trained in what it means to acquire the consent of the respondents, about the power imbalance between the respondents and themselves/supervisor and that their higher socio-economic status in itself could put pressure on respondents to participate. The power imbalance is visible, local people could easily see that our survey team was of higher socio-economic status and we also asked them to dress down to reduce the gap.

Consent was asked repeatedly, at the start of each of the survey waves. In order to ensure that the enumerator had asked for the consent in the way specified, each enumerator had to sign the consent sheet with their name and date of the interview.

The main issue for the respondents is anonymity: they did not want anyone else to learn about their responses. We therefore ensured them that all information would be kept private and confidential and that only the researchers would get access to their identifying information such as their names. We also explicitly made clear that the information they provide would not affect their employment relationship in any way and that it would not be shared with their employer.

Power imbalances

When designing our research, we took into account that our target respondents were women from poor households, with relatively low education and likely not well-positioned to foresee how participating in the study might impose risks or benefits for themselves. From our previous similar studies of poor working women in Ethiopia (see for example Getahun and Villanger 2018), we knew that such power differentials between the research

team, including enumerators, were so large that care had to be taken to ensure respondents understand the purpose of the study, the voluntariness of participation and that declining to participate in the study would have no implications for themselves. During the preparation and implementation of the research we took several steps to ensure the power imbalances would not affect the respondents' voluntariness of consent, and that the likely risks and benefits were well explained. During the selection of enumerators, we only included those who had previous experience from interviewing people in similar imbalanced situations, and who were able to understand these issues and maintain a respectful and truthful approach to the respondents (assessed by role play and pilot interviews). The enumerators were trained also in explaining the consent form and tested to see that they were capable of respecting the autonomy of the respondent. During the data collection, we had supervisors on the ground that followed the enumerators and monitored whether this approach was also followed in practice.

When interviewing poor people, often under demanding circumstances, we consider it fair to compensate them for their time and effort. It was often inconvenient for the respondents to find a suitable time for the interview, especially for those who were working, since their work, transport back and forth from the factories as well as household chores and traditional responsibilities took up most of their time during the day. During the survey waves, we offered a compensation for participating in each interview of around 2 to 3 days of salary.

Risk of harm

Responses to some of the questions in our survey may be considered sensitive for some respondents. We took several concrete steps to account for this. First, we included in the consent section clear statements that respondents did not have to answer questions that they did not want to answer, and that if this happened, the enumerator would skip to the next questions. In addition, the consent section made clear that respondents could at any time stop the interview.

Another risk of inducing harm on the respondents could arise from information conveyed during the interview reaching other individuals, outside the research team. In addition to ensuring anonymity, which we discuss in the next section, it was therefore paramount that all interviews were conducted in privacy with one single respondent. If this could not be maintained, there were strict procedures of aborting the interview. This was stated on the front page of the questionnaires, and was covered intensively during the training of the survey team. In the case of aborting the interview, the enumerator would reschedule the interview to another time, and it could take place in a different location. We developed a more general protocol to ensure the safety of respondents, enumerators and research team, and all our project decisions were made in a way to comply with this objective. This protocol addresses the ethical issues that apply to our research, in particular in relation to survey questions about intimate partner violence The protocol used for the first follow up survey is shown in Section A.11.1 below.

The survey with the women's partners also applied strict safety measures, for the same reasons as stated above. In particular, each respondent was asked for consent that we could contact and interview her husband. In the case where consent was not obtained, her partner was not contacted, and the partner interview was not conducted. In addition, we did not ask any questions about domestic violence to the partners so that they would not know that the women had been asked about this.

Additionally, some controversial topics in the surveys could also pose a risk for the survey team. By asking sensitive questions on for instance protest participation, the enumerators could be accused of collecting information on activities that were considered illegal under a state of emergency that was imposed twice in the data collection period. The main risk mitigating strategy was to postpone data collection during the most intense protests. In addition, during the states of emergency, we took out survey questions that were considered to impose a risk to the enumerators or respondents, and rather relied on retrospective information about these issues once the situation had calmed. The

sensitivity regarding the information collected also influenced our choice of local research partner. In order to ensure both access and context sensitivity, we collaborated with PSI, which is a semi-autonomous research institute that produces research for the government. Close collaboration in the design of the study, and during implementation, ensured the political buy-in necessary to conduct this type of research under such circumstances without imposing any large risks for any involved parties – either for respondents, their families and friends, survey team or researchers.

Confidentiality

The project has high standards for ensuring anonymity of respondents. Assurances of confidentiality and anonymity is always explained to respondents during the initial phase before we ask for their consent.

Abiding by the formal regulations of processing of personal data ensures that we keep our promise of confidentiality and anonymity in practice. Data privacy is maintained through data management and storage in accordance with the General Data Protection Regulation (GDPR) for collecting personal data that applies in all EU/EEA countries. Moreover, we are also in compliance with the Personal Data Act of Norway. In line with these regulations, the project has been registered with the Data Protection Services for Research at NSD (the Norwegian Centre for Research Data) together with documenting which personal data we are processing, for what purpose and how our data processing will be handled. Based on our documentation, our project has been assessed by the NSD and they have confirmed that our project is in accordance with data protection legislation. A key feature is restricted access to identifying information. In our project, only the data manager has access to names of the respondents, which is stored separately from the database and not shared with anyone else. The anonymized version of the data is only shared with researchers in the team, and only for analytical purposes. After the analytical work has been completed, the names and identifying information will be deleted.

Regulations

Our research has been carried out in compliance with the Norwegian regulations as well as the regulations and permitting requirements in Ethiopia. In Ethiopia we conducted the research jointly with the Policy Studies Institute (PSI), which was established by the Government and is a semi-autonomous research institute handling the permissions required for conducting our research in Ethiopia.

A.11.1 Protocol for the enumerators used for the first follow up

Empowerment and jobs: Impacts on partner violence against women in Ethiopia. Women in the Developmental State

Intimate Partner Violence protocol.

First follow-up survey enumerators.

October 2016

Our work complies with the ethical and methodological requirements of conducting an intimate partner violence (IPV) study as described by WHO (2001). Ensuring the safety of respondents, enumerators and research team direct all of our project decisions. To this end, this protocol address the ethical issues that applies to our research. The protocol contains the steps that we will take to minimize the inherent risks involved in IPV studies, particularly to avoid that the research in itself leads to more violence. We apply an accurate descriptions of the violence in our questionnaire in order to maximize disclosure (Ellsberg et al. 2001) and we ask about a wide range of abusive acts prevalent in these societies using indicators of internationally validated standardized IPV measures (Domestic violence module for the baseline is enclosed in Annex1). We have based our questions and sequencing on the WHO Violence Against Women Instrument (Ellsberg and Heise 2002) and the Conflict Tactics Scales (Strauss 1979 and Hindin et al 2008). Hence, three categories of violence are included, emotional, physical and sexual, in addition to

controlling and manipulating behaviors such as threats. It is generally recognized that research on IPV has a particularly important responsibility to develop an impeccable study design and be conducted in the best possible way. It is ethically unacceptable to have a research design and implementation that is not able to deliver important and credible evidence. All research team members have previous in-depth experience with IPV research and individual data collection. Moreover, we will disseminate the findings broadly, and in particular to policy maker so that the right action can be taken to reduce IPV. Our study is on the methodological research frontier. It includes research to quantify the under-reporting of violence and will hence provide a more accurate picture of the prevalence and pave the way for prevalence studies to use similar tools to increase disclosure rates. Our aim is to bring prevention of IPV to the forefront in Ethiopian policy discussions and ignite future research and intervention development for its abolishment.

Training of data collecting personnel and research team

All involved data collecting personnel and research team will get a hard and soft copy of the WHO 2001 Putting Women First: Ethical and Safety Recommendations for Research on Domestic Violence Against Women. It is a requirement for participating in this study that this report has been read and understood. We have developed a detailed training module for enumerators, survey supervisors and research team in the protocol and its practical implications. This includes role-play of the interview setting using the actual IPV questionnaire to visualize the necessary actions to be taken in various scenarios and implications of encountering the specific challenges described in the protocol.

Moreover, only female enumerators with documented experience in interviewing respondents about sensitive issues will be hired for the project. The enumerators will be supported in the field by well-qualified and trained supervisors with the responsibility of overseeing safety of enumerators and respondents. Debriefing will also take place on a regular basis, but also as needed, to share experiences and to provide an outlet for emotional stress from recording the stories of violence and abuse.

Privacy procedures

Our research has a specific procedure to be followed to ensure privacy of information during the interview. First and foremost, the interview will not be conducted if 100 percent privacy cannot be ensured. This means that only the enumerator and the respondents will be present during the interview. Moreover, in case privacy is interrupted, the interview will be aborted unless privacy is restored. The enumerators and data collection supervisors will be instructed both by training and in writing on the front cover of the questionnaire that they need to find a place, and if necessary – at a new time, where they can be alone during the interview and to ensure complete privacy throughout the interview (see Annex 1). Each enumerator needs to sign off before the interview that this has been achieved. If privacy cannot be maintained, the interview will not be conducted. Moreover, no one else than the respondent will be informed that parts of the survey includes IPV questions. The IPV module is part of a larger survey that enables the respondent to explain safely to others the purpose of the interview. Moreover, the IPV module will only be conducted at the end of the larger survey after an initial consent procedure explaining the sensitivity of the questions. In addition, only one woman in each household will be interviewed so that it will not be revealed to other household members that part of the interview could entail disclosure of IPV.

Referral and information about redress mechanisms

Studies of IPV in our settings are likely to encounter situations where respondents disclose criminal acts that have taken place and not been reported. Our research contains procedures for providing information about available support and redress mechanisms to victims of IPV. However, since we do not know about the circumstances for such redress, it will always be up to the victim to report or address the violence. The enumerators will provide all the respondents with information about contact details for locally accessible IPV support services for referral, irrespective of whether they have been abused or not. The IPV module will be rounded off by the enumerator explaining that the Ethiopian

penal code (2005) makes it a criminal offense for a husband to act violent against his wife, and that the information provided in the interview will be used to reduce violence against women generally.

Protection of confidentiality of the women.

We have several measures to protect the confidentiality of the women respondents. No enumerators will conduct interviews in their own community. Moreover, each of them will have to guarantee that they will ensure privacy and not disclose any personal information to others. The anonymity and confidentiality of personal respondent information will be preserved in the data analysis by using identity codes instead of names and the data handling and storage will be secured. All identifying information will be destroyed after the endline.

Confirmation I hereby confirm that I have read and understood this protocol and will abide by all the procedures. I also confirm that I have received a hard copy of WHO (2001) and have read and understood the content.

Date and place:........................

Function:........................

Name:........................

Signature:........................

A.12 Anonymous version of the pre-analysis plan

Pre analysis plan for medium run results for the project "Jobs and political participation - Evidence from a Field Experiment in Ethiopia"

Abstract

The present project is the first to identify the individual level effects of jobs on female political participation using a field experiment. We identify the effects of jobs on political participation by collaborating with 25 large companies in Ethiopia to randomly assign jobs to equally qualified applicants. We already have some data from 6, 12, 18 months follow ups. The job offers increase formal employment, earnings, and earnings shares within couples in the short run. We also find that job offers reduce the participation in community meetings but does not affect political interest. A likely mechanism for our results is increased time pressure for the women who got the jobs. Consistent with such an interpretation we find reductions in leisure time and in time spent on social and religious activities. Another hypothesis could be that participation in meetings are not meaningful and that non-participation is actually empowering. We will now collect longer run data on the same and new outcomes. In particular, we will collect data on contentious political action (protests), political preferences, and voter turnout (both intentions and actual turnout in the 2020 election). The changes in Ethiopia over the period also makes it interesting to see if jobs have a different relationship with political participation before and after the liberalization. In this plan we describe the analytic decisions that will be made in the analysis of the data in the project. That is, we describe the hypotheses to be tested and how they will be tested. The description includes how the variables are coded, how we will deal with attrition and missing values, and how the estimation equations will look like. We also conduct a power analysis which suggests that we are able to identify relatively small effects. All deviations from the plan will be highlighted in the final paper.

1. Introduction

Women have substantially lower rates of political participation than men in most developing countries. A prominent explanation for this is the resource hypothesis which claims that men have more material resources in terms of education and income and therefore have the capacity to participate more.

There is a strong expectation in the literature that employment, in particular, will bring about changes in women's agency in multiple arenas of life, particularly by enabling and motivating them to participate politically. In developed democracies, the correlation between employment and participation is clear (Ross 2008, Schlozman, Burns, and Verba 1999). The higher percentage of women working, the more women tend to vote or become representatives in various political forums. Formal employment is argued to affect female political participation by changing women's gender attitudes and identities, by increasing their decision-making power within the household, giving them access to networks and making them informed about their interests (Isaksson et al., 2014; Iversen & Rosenbluth, 2008; Ross, 2008).

A growing body of literature on developing, and/or authoritarian contexts presents a slightly different picture. In general, the factors driving women's political participation in developing countries are much less clear than in the developed world, and the evidence is mixed, and scarce (Coffe and Bolzendahl 2010, Isaksson et al. 2014). In contrast to Western societies, research from Sub-Saharan Africa show that poor, less educated citizens may participate on an equal level as others (Bratton 1999). Both cross-national and case studies from authoritarian contexts show that participation could be less a consequence of individual factors and behavior than in democracies. Instead, it could be a result of regime-directed mobilization (Bahry and Silver 1990), where participation is deliberately used to strengthen authoritarian rule instead of giving citizens the chance to influence decision making (Aalen and Muriaas 2018, Benton 2016). Research from Ethiopia before the political transition in 2018 shows that women with formal employment are *participating* equally in politics, but are actually less *interested* in politics than those without (Aalen, Kotsadam and Villanger 2019).

There is, however, limited evidence for the causal impacts of employment. Previous studies, both from the developed and developing contexts and from across political regime types, often just compare workers to non-workers. In this way, the findings may be confounded by reverse causality and omitted variable biases. That is, without a credible design for causal inference it is difficult to rule out that political participation creates jobs or that there is some third variable

driving both employment and political participation. The present project is the first to identify the individual level effects of jobs on female political participation using random assignment of jobs.

2. The field experiment, data and main empirical strategy

We have been collaborating with large companies that were hiring new workers. The companies first assessed all job applicants and determined whether each applicant was eligible for the job or not. Then, from the pool of eligible candidates at each company, a list of all women with a partner[*] was compiled (this is our study sample), from which the company randomly selected half to receive a job offer. The randomization was done using computers and the applicants were informed about the procedure before the randomization was conducted.

Pre- and post- longitudinal data have already been collected using a detailed structured survey instrument. The women were interviewed before they started working and the first follow up data collection was around 6 months after the first interview. The dates for the baseline data collection varies and depend in particular on when the firms hired. Some of the firms hired new batches of workers several times during the period. In addition to questions on politics, the survey instrument includes modules gathering demographic and background information, including poverty measures and other socio economic variables, and a module on domestic violence and female empowerment.

Variables measuring political participation, interest, and preferences

The surveys included a set of questions about political participation. These will be coded as follows:

Participation last year: In the previous surveys we have two questions about the respondent's participation during the last year. These questions were asked as follows: "Here is a list of actions that people sometimes take as citizens. For each of these, please tell me whether you, personally, have done any of these things during the past year.

Attended a community meeting

Got together with others to raise an issue"

---

[*] The data collection initially focused on impacts on intimate partner violence, which is why the study population consists of partnered women.

The answers can be "1=Yes, often", "2=Yes, several times", "3= Yes, sometimes", "4=yes, rarely", and "5=Not at all". We create the dummy variables *Meeting last year* and *Raise issue last year* as 1 if the answer is 1-4 and 0 if it is 5. We will also reverse code the variables and investigate the effects on the continuous versions.

Interested: Based on the question "How interested would you say you are in politics and government?" The possible answers are "1= very interested", "2=somewhat interested", "3= not very interested", and "4= not at all interested". We create a dummy variable, *Interested*, that equals 1 if the answer is 1 or 2 and 0 if the answer is 3 or 4. We will also reverse code the variable and investigate the effects on the continuous versions.

Networks: The survey asked about the existence, membership, and roles in networks. The following 12 networks are included, in addition to an "other" category: Mahiber(tswa…etc), Other religious associations, Women's association, Microfinance cooperative, One-to-five networks, Development teams, Idir, Equb, Trade union, Informal Workers group, Users association, customary institution. The first question is "Does this exist in your neighborhood or workplace? (Yes=1, No=0". Then the women are asked: "Are you currently member of the specified network? (Yes=1, No=0)". Another question of interest is "Do you have an important role in the network? (Yes=1, No=0)". Based on these questions we create dummy variables for each network type X for membership. *Member X* equals 1 if member, 0 if not or if the network does not exist in the area (since this may be endogenous). We also create dummy variables for each network type X for leadership/important role. *Leader X* equals 1 if the role is important, 0 if not or if the network does not exist in the area. We then create two count variables, theoretically ranging from 0 to 13, for the number of networks *(Number of networks – member*, and *Number of networks – leader)*. Based on these count measures we also create two dummy variables, *Member of any network* and *Leader of any network*, which equal one if the corresponding count variables are positive. We also create the variables *Network X exists*, which equals 1 if the network exists in the area 0 if not.

Kebele meetings: Based on the question, "Do you attend meetings in the Kebele?" The possible answers are "Yes", and "No". We create a dummy variable, *Kebele meetings*, that equals 1 if yes and 0 if no.

In the new survey we will add the following questions:

Intend to vote: Based on the question, "Would you vote if there was an election next week?". The answers will be Yes or No and we will dummy code them so that *Intend to vote* equals 1 if yes and 0 if no. **This will be our main dependent variable.**

Party choice: If there was an election next week, which party would you vote for? We will create dummy variables for EPRDF, Ethiopia Citizens for Social Justice, MEDREK, Oromo Liberation Front, NAMA, and Other. We will code non-voters as zero.

Protest: We will add "Attended a demonstration or protest march" and "Expressed a political opinion through social media" to the questions about political participation last year and we will code the variables, *Protest last year* and *Express last year*, the same way as we coded the other two variables.

Voted in election X: Based on the questions: "Did you vote in the election year X", where X will be 2005, 2010 and 2015. The answers will be Yes or No and we will dummy code them so that *Voted in election X* equals 1 if yes and 0 if no. If we conduct a survey after the planned 2020 election we also add a question about that election in the follow up wave.

Pressured now: Based on the question, "Do you feel pressured nowadays to attend community meetings?" The possible answers are "Yes, often", "Yes, several times", "Yes, sometimes", "Yes, rarely", and "Not at all". We create a dummy variable, *Pressured now*, that equals 1 for any of the yes alternatives and 0 for "Not at all".

Pressured before: Based on the question, " What about before the regime of Abiy, did you feel pressured to attend community meetings? " The possible answers are "Yes, often", "Yes, several times", "Yes, sometimes", "Yes, rarely", and "Not at all". We create a dummy variable, *Pressured now*, that equals 1 for any of the yes alternatives and 0 for "Not at all".

Lose influence: Based on the question, "Do you think that people that cannot attend community meetings due to work get less influence in the community?" The possible answers are "Yes", and "No". We create a dummy variable, *Lose influence*, that equals 1 if yes and 0 if no.

Lost influence before: Based on the question, "What about before Abiy came to power , do you think that people that could not attend community meetings due to work got less influence in the community? " The possible answers are "Yes", and "No". We create a dummy variable, *Lost influence before*, that equals 1 if yes and 0 if no.

Political priorities: We create a variable "*Roads and Infrastructure*" based on the following question: "If you could only choose between water or roads and infrastructure, which problem do you think is the most important one facing Ethiopia that the government should address?". The answer categories will be "1=Water", "2= Roads and Infrastructure", "3= I do not know". We will code the variable *Roads and Infrastructure* as 1 if the answer is 2 and as 0 if it is 1 or 3.

Discuss politics with friends: We ask respondents, "If you think about your friends and the things you talk about, what are the topics you discuss?" They can choose up to three things. If they answer something coded as *11=political issues* we dummy code a variable Discuss narrow politics to be equal to one. If they answer something coded as either *11=political issues, 6=women´s rights, or 13= grievances and dispute resolution* we dummy code a variable Discuss broad politics to be equal to one.

Redistribution: We will also create three different variables on redistributive preferences and taxes. These questions are of the following form:

"Please say whether you strongly agree, agree, disagree, or strongly disagree with these statements
(1 = Strongly agree   2 = Agree   3 = Disagree   4= Strongly disagree)

In Ethiopia, the national government should aim to reduce the economic differences between the rich and the poor

In Ethiopia, the national government should have taxes on people owning houses to reduce the economic differences between the rich and the poor

In Ethiopia, the national government should have taxes on people working to reduce the economic differences between the rich and the poor."

These variables will be coded as binary variables: *Reduce differences*, *Tax houses*, and *Tax work*. The binary variables will be set in order to create 2 groups, as similar in size as possible, while respecting the order of the answers (from 1 to 4). For example, if 25% answer 1, 25% answer 2, 25% answer 3 and 25% answer 4, then the binary variable will be equal to 1 if the respondent answers 1 or 2 and zero otherwise. On the other hand, if for example 20% answer 1, 10% answer 2, 5% answer 3 and 65% answer 4, then the binary variable will be equal to one of the respondent answers 1, 2 or 3. We will only consider the answers of the respondents in the control group when creating the coding rules for the binary variables.

Political efficacy: We create a measure of *Internal efficacy* based on the question. "Sometimes politics and government seem so complicated that a person like me cannot really understand what is going on". Our measure of *External efficacy* is based on the question: "People like me don't have any say in what the government does". Both of these questions have the answer categories *1 = Strongly agree   2 = Agree   3 = Disagree 4= Strongly disagree* and will be coded using the same rules as for coding the Redistribution variables.

<u>*Main independent variable*</u>

Treatment

The main independent variable is treatment status which equals 1 if the respondent was randomly assigned to a job offer and zero otherwise. We will also investigate the effects of jobs and then we code the following variables:

<u>*Background variables*</u>

We will retain the continuous coding of age in years.

From the baseline data we will use the answer to the survey question: "Have you ever had a formal salaried job with salary before?" From this we create the variable *Any formal wage job* which equals one if the answer is yes.

We dummy code the religious affiliation of our respondents. The majority are Orthodox Christians and we let that be the base category (together with the few people answering Catholic or Other and create dummies for the other two main denominations (Muslim and Protestant).

We recode the years of schooling variable into low (<10 years), medium (10 years), and high (>10 years) and use low education as the base category.

<u>*Main estimation equations:*</u>

There is a lot of uncertainty in how much the treatment will bite in terms of predicting formal employment at the fourth follow up. In particular, some people that are not assigned to treatment will find other jobs and people assigned to job quit. It is also unclear whether having had any factory work ever, months of factory work, or factory work during the last six months are most important in affecting political participation. Our main estimation will be intention to treat analyses (ITT), where we regress our outcome variables on the Treatment indicator, controlling for the outcome as recorded at baseline if it exists (and other baseline characteristics). We will

also explore results where we use instrumental variable analyses (IV), instrumenting various aspects of employment with the randomized treatment.

The main estimating equation to estimate the medium run outcomes is the following intention to treat model:

$$y_{i,t} = aTreated\_Job_i + y_{i,t0} + x_{it0}\beta + c_l + e_{i,t}$$

Where $y_{i,t}$ corresponds to *Political participation, interest, or preferences* (e.g. Intend to vote, Protest etc.) for woman *i* in time period *t* (e.g. t4 is the fourth follow up). This is regressed on a dummy variable that equals one if the woman was randomized to get the job and zero if not. This captures the so called intention to treat effect and it gives us an estimate of the effect of being randomized into treatment. As long as treatment status is randomly assigned we do not expect any other differences between treated and control women. We will test if the groups are similar and also include control variables in order to increase power and precision. In particular we include *Political participation* at baseline (*t0*) for the variables that were included at baseline, list fixed effects (blocking variables) as women are randomized within this unit, and a vector of individual level controls X. For the vote intention variable we can include answers to the questions about voting in previous elections. As employment may affect reporting, however, we will first have to check that there is balance on the variables between treatment and control. This balance test will be important as a check of reporting bias more generally.

We will experiment with the control variables to see if we can get more power by adding the other participation at baseline variables as controls in addition to the specific Y used and we will also see if we can improve precision in the estimates by picking optimal controls from the total list of controls using LASSO (Belloni et al. 2014; Ahrens et al. 2018). The main specification will, however, be the one with only the list fixed effects as controls. We use robust standard errors.

The vector of individual level controls are all taken from the baseline survey and are: *Age*, *Muslim, Protestant, Any formal wage job, Medium education,* and *High education*. We have already tested for baseline balance on these variables in the samples we have up until now and it seems balanced.

3. Data and empirical strategy for the exploratory analysis of potential mechanisms

We have chosen to call our tests of mechanisms exploratory as there are many different tests that can be done and as it is difficult to exhaust the list ex-ante. We want to stress that even if

the list we give here turns out to be complete, the analysis will have to be seen as exploratory as the number of hypotheses is large.

While our primary outcome is Intend to vote, we will also explore effects on all other variables measuring political participation, interest, and preferences. Some of these are secondary outcomes and some of them will be used more to dig into the mechanisms behind possible effects (e.g., the internal and external efficacy variables). Some variables will also be used to measure changes over time in the liberalization efforts that have been rolled out in Ethiopia. This may be important as political participation may be differentially affected by employment under more or less autocracy. For instance, we will use whether the network exists in the area to investigate the changes over time in government run networks. In particular "One-to-five networks" and "Development teams" were closely linked to the Government control structures before the liberalization. By investigating participation in such networks before and after the liberalization we can gain insights into how meaningful vs mandated participation is affected differently. We also ask about the meaningfulness of meetings now and retrospectively.

We will test the effects of Treatment on various measures of employment and earnings. The most important measures will be *Any_factory_job (*Dummy variable equal to one if the respondent has worked in a factory job for at least one month since the first interview) *Months_factory_job* (in months), and *Any formal wage job (last 6 months)* a dummy variable based on earnings from any wage job (where 1 equals positive earnings).

When we find reduced form effects of treatment on our outcomes we will also scale the effects using an IV estimation to account for imperfect compliance. We use an IV model of the following form:

$$Any\ factory\ job_{it4} = aTreated\_Job_i + c_l + e_{i,t} \qquad (2)$$

$$y_{i,t4} = Predicted(Any\ factory\ job)_{it4} + c_l + e_{i,t} \qquad (3)$$

We will also test the IV regressions with the variables, *Months_factory_job* and *Any formal wage job (last 6 months)* to explore whether these are relevant channels.

In order to test mechanisms we will also regress treatment on income, time use, empowerment, and attitudes toward gender equality. If there is an effect on these variables we can include them as controls in the intention to treat analysis to investigate the effect of treatment over and above the effect it has on the intermediate variables. Such type of mediation analysis should not be seen as causal, however, as there are likely to be many variables correlated with the mediators

that are not included in the analysis. For the first six months follow up we know that there were effects on income and time use, but not on empowerment or attitudes. By interacting treatment with the baseline characteristics included as controls and with the mechanism variables at baseline, we also explore the extent to which the effect differs across different types of women. In the process of testing mechanisms we will code some new variables.

Income and income shares:

We will create a measure from the survey section on intra household income where we have net income for the respondent from:

1. Factory job employment
2. Other wage employment
3. Self-employment
4. Remittances
5. Government or NGO transfer
6. Other (specify)………

Adding up 1 and 2 will be useful for assessing whether treatment affected earnings from wage employment during the last 6 months. Investigating 1 will also be informative for indicating how much the treatment affected earnings from factory jobs in total.

The questions about incomes is also asked about the woman's partner and we will create a variable of her share of couple income by adding 1-3 for her and him and then take her income divided by their total income.

Empowerment

We have 15 different questions in the survey on intra-household decision making. Not all questions apply to all people in the sample, however. For example, the decision to send a child to school has missing values for all individuals that do not have children. To create an empowerment measure we restrict ourselves to the 12 measures that have fewer missing responses (03-15 below). For each of these measures we create a dummy variable for whether the partner has the final say (i.e., it is reverse coded empowerment). As seen, this corresponds

to the value of 2 in code B. We code it as one if the partner has the final say, and also if the partner decides together with some other member of the household. If the respondent woman decides together with her partner we code the variable as zero only if she has "a lot" of input into the decision (i.e. category 4 on the J1B questions) and otherwise as 1. All other values of J1A are coded as zero on the partner has the final say variables. We then add the 12 variables together and divide by 12 to get an index ranging between 0 and 1.

Section J. Intra-household Decision Making and Domestic Responsibility Allocation

J1. Who in your household usually has the final say about the following decisions?

	Decision-Making	J1A Household Member Use Code B	J1.B if she is **not** a sole decision maker, How much **input does the respondent** have in this decision? Code : 1=no input 2=little 3=some 4=a lot
01	Whether to send or not send children to school		
02	What to do if a child falls sick		
03	What to do if the respondent falls sick		
04	Whether to have children or to have more children		
05	Which family planning methods to use		
06	Whether or not you should earn money outside the house		
07	Whether you can visit your family or relatives?		
08	The use of the wife's earned income		
09	The use of the man's /husband's earned income		
10	Purchase of small daily food purchases		
11	Purchase of bulk or expensive food items		
12	Large purchases of items like furniture, cattle, TV, or other assets		
13	Purchase of children clothing and shoe		
14	Weather to open bank account or borrow money		
15	Whether to start a new business		

Code B: 01 = Respondent   02=husband/partner 03=other female member 04=other male member 12=respondent and husband jointly 13=respondent and other female jointly 14=respondent and other male member 23=husband and other female jointly 24=husband and other male jointly 34=other male and female member

Attitudes towards gender equality:

The survey also includes questions on a wider set of attitudes toward gender equality. The 11 statements are given below and we code them so that 1 is non-gender equal (e.g. 1 or 2 on statement 1 and 3 or 4 on statement 2). We will investigate whether employment affects these

variables individually as well as together by creating an index where we add the dummies together and divide by 11.

	Enumerator: "I will read some statements about men and women. Please say whether you strongly disagree, disagree, agree or strongly agree with these statements." (1=strongly agree 2=agree 3=disagree 4=strongly disagree)	
1	It is better to send a son to school than it is to send a daughter	|____|
2	It is okay for women to work outside of the home	|____|
3	It is okay for women to earn more money than men.	|____|
4	Women have a right to decide what to do with the money they earn.	|____|
5	It is okay for women to travel or to leave the house for several nights to do business.	|____|
6	Men should be responsible to help with childcare when his wife is busy with business or factory job	|____|
7	Men should be responsible to help with domestic duties when his wife is busy with business or factory job	|____|
8	The important decisions of the family should be made by the men of the family only.	|____|
9	A wife should tolerate being beaten by her husband/partner to keep the family together	|____|
10	Woman should seek help if she encounters sexual harassment	|____|
11	Woman should seek legal recourse if she encounters sexual harassment	|____|

Time use

There is a question about time use: "How many hours did you or your family member spend on the following activities over the last seven days?"

1.	Paid work /income generating activities
2.	Work outside home but unpaid (Apprenticeship, work at family business & farm etc.)
3.	Work inside the home(unpaid)*
4.	Sleeping
5.	Eating and drinking
6.	Personal care
7.	School (include homework)
8.	Travel time
9.	Social and religious activities
10.	Leisure time(watching TV, reading magazine, playing, exercising, recreation etc.)

In particular, we will use the values for 3 (household work) 9 (social and religious) and 10 (leisure) to see how they are affected.

Longer term effects and further heterogeneity

We will also do the analysis separately for our different areas by splitting them into over and below median levels number of women working at baseline. We will here also incorporate contextual information on female employment from the DHS surveys conducted within buffer zones of 50 km from the factories. Geocoded data on protests will also be added.

Data collection is intended to continue over time in order to follow the same sample. The analysis at these later points will be similar albeit the first stages are likely to be weaker.

4. Threats to the design and power analysis

Threats to the design

Attrition: Attrition from the sample will be investigated. In particular, we will check whether attrition is related to treatment status by the following regression:

$$Attrition_i = aTreated\_Job_i + c_l + e_{i,t}$$

Where $Attrition$ is dummy equal to 1 if individual i is not answering the fourth (or later) survey. Unless the difference between treatment and control is significantly different from zero at the 5 percent significance level, all estimations will proceed without any adjustment for attrition. If there is a statistically significant difference we will employ Lee bounds (Lee 2009). We will do a new test of balance with the sample from follow up 4 by regressing $Treated\_Job_i$ on the variables one by one and together while controlling for the blocking variable c_l. As many variables are tested we do not necessarily expect all of them to be statistically insignificant. We will also include all variables at the same time and we will deem the randomization as successful if the F-test shows that the control variables cannot predict treatment status together.

Limited variation: In order to limit noise caused by variables with limited variation, questions for which 95 percent of the observations have the same value within the relevant sample (for both treatment and control together) will be omitted from the analysis. If there is limited variation in the main outcome variable, vote intention, we will have *Protest last year* as our main outcome variable.

Missing values on independent variables: If we have missing values on variables we will code the variables as zero and include dummy variables controlling for missing status so that we do not lose observations.

Possibly weak first stages and heterogeneity in the first stage relationship: We are genuinely uncertain about the strength of the first stage in our study. It may furthermore be the case that the first stage is stronger in some areas than in others due to e.g. less other options. It is difficult to come up with a decision to handle this ex-ante but we will check the first stage in each of the 5 regions and run a separate analyses in the region where the first stage is strongest as well as one where we drop the region with the weakest first stage. Other eventualities are likely to come up during the analyses and we will have to highlight them separately in the paper.

Power

Our main dependent variable will be *Intend to vote* and our main specification is the ITT with only the blocking variables as controls. We want to ensure that our sample is large enough to detect quite small effects and we also want to assess the sensitivity of the project with respect to sample size and number of factories. We therefore calculate a set of minimal detectable

effects (MDE). Such calculations hinge on the number of women surveyed and the amount of variance we can control for with the help of covariates.

We expect to have between 1000-1300 women still included in the study with around 50 percent assigned to treatment. As vote intention is a binary variable we have to make some assumptions about the share intending to vote in the control group. We expect this level to be relatively high at around 85 percent. With a total of 1000 women we will then have an MDE of 6 percentage points. If the share intending to vote in the control group is instead 90 percent we get an MDE of 5 percentage points.

5. Archive

The pre-analysis plan is archived before any follow up data from the fourth round is received. We archive it at the registry for randomized controlled trials in economics held by The American Economic Association: https://www.socialscienceregistry.org/ on June 6 2019. We will start data collection for the fourth follow up after June 12 2019. The data collection from the other three rounds is still ongoing so we do not expect the fourth round to be finished until May 2020. Conditional on funding, we will continue to collect several waves of data.

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