

Jobs and Intimate Partner Violence - Evidence from a Field Experiment in Ethiopia

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July 19, 2021

Abstract

We identify the effects of employment on Intimate Partner Violence (IPV) by collaborating with 27 large companies in Ethiopia to randomly assign jobs to equally qualified female applicants. The job offers increase employment, total hours worked, income, earnings, and earnings shares within couples in the short and medium run but we can reject relatively small effects in any direction on our main outcome, physical IPV. In the short run, job offers reduce emotional abuse by 26 percent, but in the case of women with low bargaining power at baseline, there are indications that job offers increase physical abuse.

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I Introduction

Intimate partner violence (IPV) entails large costs in terms of health, productivity, and fear.¹ In addition to being harmful to the victim, IPV has also been shown to entail substantial costs to society and to affect children (Carrell and Hoekstra, 2010; Pollak, 2004; Doyle and Aizer, 2018; Aizer, 2011). Fearon and Hoeffler (2014) estimate that the global costs of IPV amount to over 5 percent of World GDP and that the costs of IPV in Sub-Saharan Africa amount to almost 15 percent of the regional GDP. Improved employment opportunities for women has been shown to increase their human capital, delay fertility, mobilize career aspirations, and is generally believed to increase female empowerment (Jensen, 2012; Heath and Mobarak, 2015). The effects of women's employment on IPV are, however, ambiguous. On the one hand, employment may reduce women's risk of IPV by increasing their bargaining power and improve outside options. On the other hand, it may fuel aggressive responses from partners viewing their status as threatened or by partners intending to extract some of the extra resources brought by the job.

We investigate the effects of women's employment on IPV in Ethiopia using a large scale pre-registered randomized field experiment. Qualified female job applicants were randomly assigned to job offers which substantially increased earnings and job probabilities in our 6 months, 12 months, and 18 months follow-up surveys. The job offers also increased the total hours worked (at the expense of leisure time) and total income. We can reject relatively small effects on physical abuse in either direction. We find that being offered a job decreases emotional violence after 6 months but our longer term results suggest that this effect is unstable over time. We find short run heterogeneous effects whereby women with low bargaining power at baseline experience increased abuse if they are randomly assigned a job offer. This is consistent with the theoretical models

¹We mainly use the terms IPV or abuse in this paper and we take it to physical violence against women perpetrated by their partners. When we do not refer to physical abuse we will explicitly label the violence with other terms, such as emotional violence or controlling behavior.

in Eswaran and Malhotra (2011) and Tauchen et al. (1991) as well as with the empirical results in Heath (2014). Apart from the differential effects with respect to baseline bargaining power we do not find any heterogeneity. In particular, we do not find that the effects are moderated by previous levels of abuse or previous employment, nor robustly by community level factors. Furthermore, there is no heterogeneity with respect to baseline empowerment in the longer run.

Our paper contributes to a rapidly growing literature on IPV in economics. Economists have investigated a range of different determinants of IPV such as education (Erten and Keskin, 2018; Gulesci et al., 2018), property rights (Amaral, 2017), culture and social norms (Alesina et al., 2016; Tur-Prats, 2019), divorce laws (Brassiolo, 2016; Stevenson and Wolfers, 2006; Garcia-Ramos, 2017), weather shocks (Miguel, 2005; Cools et al., 2019; Abiona and Koppensteiner, 2016; Sekhri and Storeygard, 2014) and gender ratios (Amaral and Bhalotra, 2017). They have also investigated the effects of interventions to reduce partner violence, such as female police stations (Amaral et al., 2018), mandatory arrest laws and no drop policies (Iyengar, 2009; Aizer and Dal Bo, 2009), gender and entrepreneurship training (Green et al., 2015; Bulte and Lensink, 2018), awareness raising (Villanger, 2020), and edutainment (Banerjee et al., 2018; Green et al., 2020). There is also a literature on the male motives of partner violence, focusing on expressive factors such as relieving frustration (Tauchen et al., 1991), information asymmetries and signaling (Anderberg et al., 2018, 2016), emotional cues (Card and Dahl, 2011) and instrumental reasons such as resource extraction (Bloch and Rao, 2002).

By estimating the causal effects of jobs on IPV, our paper is most closely related to the literature on female employment and IPV. In particular, we provide strong evidence for the non-existence of large average individual level effects in our setting. Previous studies from various contexts² that have investigated the question with quasi-experimental methods

²The US (Aizer 2010), Spain (Tur-Prats 2017), Sweden (Ericsson, 2020), the UK (Anderberg et al. 2016), Mexico (Davila, 2018), Turkey (Erten and Keskin, 2020), and India (Amaral et al., 2015; Chin, 2012).

have all investigated the effects of employment at the aggregate level with mixed results. These studies rely on stronger identifying assumptions than we do.³ There are also randomized experiments on related areas of study, such as the effects of cash transfers (e.g. Haushofer et al. (2019); Hidrobo et al. (2016); Heath et al. (2020); Angelucci (2008)) and microcredit (Pronyk et al. 2006). These studies often find that increased resources to women reduce IPV or that it has no effect.⁴ Haushofer et al. (2019) are able to dig further into husbands' motives for IPV by exploiting a large randomized cash transfer in Kenya that is sometimes given to the husband and sometimes given to the wife. With the exception of sexual violence, they find that husbands use physical abuse instrumentally to extract resources from the wives. Cash transfers and microcredit are, however, likely to have other effects than formal employment has. Women's employment directly challenges men's breadwinner status and provides access to social networks (Cools and Kotsadam, 2017). In addition, working outside of the home may affect time spent together and time spent on other tasks, such as household work, which may affect conflict propensities.

Access to a wide battery of moderators at baseline, as well as measures of likely important mediators such as empowerment and attitudes (which are highly correlated with abuse), enables us to test a larger set of mechanisms than previous studies on jobs and IPV. In doing so we also obtain results that speak to the literature on other effects of female employment apart from IPV (see Heath and Jayachandran (2016) for an overview of this literature). For instance, and in contrast to the observational study by Getahun and Villanger (2018) and studies relying on changes in labor market conditions at a more aggregate level, such as Atkin (2009) and Majlesi (2016), we do not find that women's bargaining power increases with job offers.

³Most previous studies use Bartik instruments. The identifying assumptions in such models have been scrutinized lately and re-analyses of papers using the method have shown that identification usually comes from the industry shares (Goldsmith-Pinkham et al., 2020). In addition, Adao et al. (2019) show that inference in popular Bartik designs is problematic due to correlated residuals across areas with similar industry shares.

⁴Across the 56 quantitative outcomes included in a review by Buller et al. (2018), more than half were statistically insignificant.

We are further able to investigate the role of relative earnings within the household as well as spending behavior. It does not seem to be the case that women’s relative income affects the risk of abuse, not even if she starts earning more than her partner or if the partner is unemployed. In contrast to the results in Haushofer et al. (2019), exploiting the panel feature of our data to investigate how changes in abuse affect changes in spending, we do not find that abuse seems to be used instrumentally by the husband to alter the wife’s spending behavior (as changes in abuse are not correlated with her spending more).

Our results also speak to the larger literature on the effects of industrialization on individual wellbeing. Blattman and Dercon (2018) find that industrial job offers in Ethiopia did not increase wages or even the probability of being employed after one year.⁵ In contrast, we find that the job offers increase earnings and that there are still differences in employment probabilities over time. As such, our results are more in line with results from observational studies, and in particular with Getahun and Villanger (2018) who find that employment in Ethiopian flower farms increased welfare for rural women.

II Employment and IPV

The correlation between individual level female employment and IPV is generally positive in Sub-Saharan Africa (Guarnieri and Rainer, 2018) and even more so in areas with higher acceptance of abuse (Cools and Kotsadam 2017), and in countries with less gender equality (Heise and Kotsadam 2015). The literature using quasi experimental designs has found that local level female employment reduces abuse in the US and the UK (Aizer, 2010; Anderberg et al., 2016), and increases abuse in Mexico (Davila, 2018) and in areas of Spain with stronger male breadwinner norms (Tur-Prats, 2017).

⁵They found that an entrepreneurial program had larger effects on employment in the short run, but going back to the sample five years later they found complete convergence in employment across all groups over time (Blattman et al., 2019). As compared to the five firms they study, our firms are more geographically spread and our sample includes mostly married women, whereas their sample includes mostly single men and women.

Theories on the effects of employment on IPV also point in different directions, for instance depending on whether violence is seen as expressive or instrumental. In bargaining models of the household that consider violence to be expressive, so that men get increased utility from abusing, employment and increased female resources are seen as protective as they improve women's outside options (Manser and Brown 1980; McElroy and Horney 1981; Lundberg and Pollak 1996; Farmer and Tiefenthaler 1997; Pollak 2005; Anderberg et al. 2016). If violence is instrumental, however, an increase in women's resources may yield a higher risk of abuse despite initially increased female bargaining power. The reasons are that violence may be used to counteract the concomitant increase in female power and because there are more resources to "extract" from female hands (Eswaran and Malhotra 2011; Heise and Garcia-Moreno 2002; Heath 2014). A condition is that the increase in bargaining power still leaves her below her exit point, for instance due to the exit point being too far away to start with. Heath (2014) finds a positive correlation between employment and IPV in Bangladesh only for women with low education or who were younger when they married. The result is consistent with the baseline level of bargaining power being an important moderator for the effects of employment.

Relative resources between spouses are likely to matter for identity reasons, especially if women start earning more than their partners. Such atypical roles may lead to status inconsistencies and, hence, threaten male identity (Akerlof and Kranton, 2000; Bertrand et al., 2015). Theories of male identity and IPV stress that his aggressive behavior is triggered when his breadwinner status is threatened (Hornung et al., 1981; Jewkes, 2002; Macmillan and Gartner, 1999), especially for men with conservative gender norms (Atkinson et al., 2005; Angelucci, 2008).

The effects of female employment are generally thought to be moderated by macro level factors, such as acceptance of divorce, the share of women working, male identity norms, and the degree of acceptance of abuse in society. One possible reason for the positive correlation between employment and IPV in developing countries is that partnership

dissolution may be costlier for financial or social reasons and therefore the outside option is practically non-existent or further away (Bhalotra et al., 2018; Doyle and Aizer, 2018). This is for instance the reason provided by Bulte and Lensink (2018), whom conduct an evaluation of a gender and entrepreneurship training in Vietnam and find that it increased IPV. They argue that the results are driven by increased female incomes in combination with a large stigma associated with divorce, which leaves little real outside options. Vyas and Watts (2009) point to a pioneering hypothesis whereby the risk of IPV may be largest for the women that start taking the first jobs in an area because they break with norms about women’s roles. Consistent with this, Heise and Kotsadam (2015) find that the positive association between abuse and working for cash is strongest in countries where fewer women work. Cools and Kotsadam (2017) argue that community level attitudes toward abuse are also likely to be important by giving a sort of impunity to husbands that want to reinstate their power within the household. They find a larger positive correlation between working and abuse for women in areas where wife-beating is considered more acceptable. Kotsadam et al. (2017) find that mining increases female employment and that it leads to higher levels of IPV in areas with higher levels of acceptance. This is also consistent with the finding by Tur-Prats (2017) that the response to better labor market conditions for women is increased violence in parts of Spain with a traditional nuclear family tradition and no effects in areas of Spain with a traditional stem family tradition. She interprets her results in an identity framework where men loose identity utility if their breadwinner role is threatened in traditional cultures. The effects of employment on IPV are thus argued to be context dependent.

III The Context and The Field Experiment

Ethiopia is one of the least developed countries in the world, with a majority of the population working in agriculture. The culture is generally described as patriarchal and there is a widespread acceptance of IPV (Kedir and Admasachew, 2010). While women’s

legal rights with respect to divorce and civil liberties are formally equal to men's, informal rules and adverse cultural norms affect family relations and in practice women often lose their property when divorcing (CEDAW, 2011). Using data from the world values survey (WVS) and from the Demographic and Health Surveys (DHS) we show in Figure 1 that Ethiopia scores low on acceptability of divorce and high on acceptance of abuse.⁶ According to the theories outlined in Section II, both of these factors would lead us to expect that the effects of employment on IPV would be more negative in Ethiopia than in many other places.

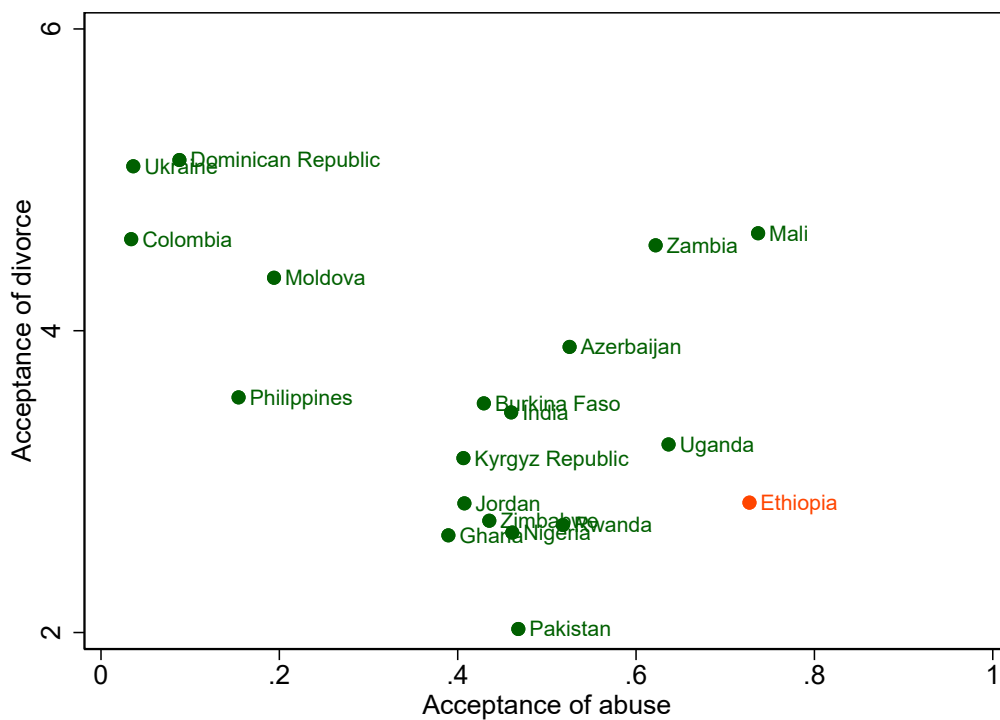


Figure 1: Comparison of Ethiopia to other countries
Own calculations based on data from the WVS and the DHS, see footnote.

The Ethiopian manufacturing sector is growing quickly and the Ethiopian Government

⁶The question in the WVS is "Please tell me for each of the following statements whether you think it can always be justified, never be justified, or something in between, using this card." The answers are given on a scale from 1-10, where 1 is never justified and 10 is always justified. The mean across all 100 countries in the WVS is 4.7 and in Ethiopia it is 2.9. In fact, only 10 countries have a lower score. The acceptance of abuse variable in the DHS data is based on the same questions we have for acceptance of abuse and is equal to one if abuse is accepted in at least one of the cases. The sample of DHS countries are those included in Heise and Kotsadam (2015) and DHS data for Ethiopia in 2000, 2005, and 2011 is added (adding 2016 data for Ethiopia does not change the ranking of the countries).

is actively accommodating foreign direct investors. One way of doing so is to build industrial parks to provide economies of scale for the potential investors. We work with 27 firms within such industrial parks. More specifically, our intervention centers on shoes and garment factories in five different regions: Tigray, Amhara, Oromia, SNNP, and Dire Dawa. In the factories we study, people earn on average 1021 ETB (around 38 dollars) per month and they usually work for 8 hours per day, 6 days a week. The location of the industrial parks are shown in Figure 2a.

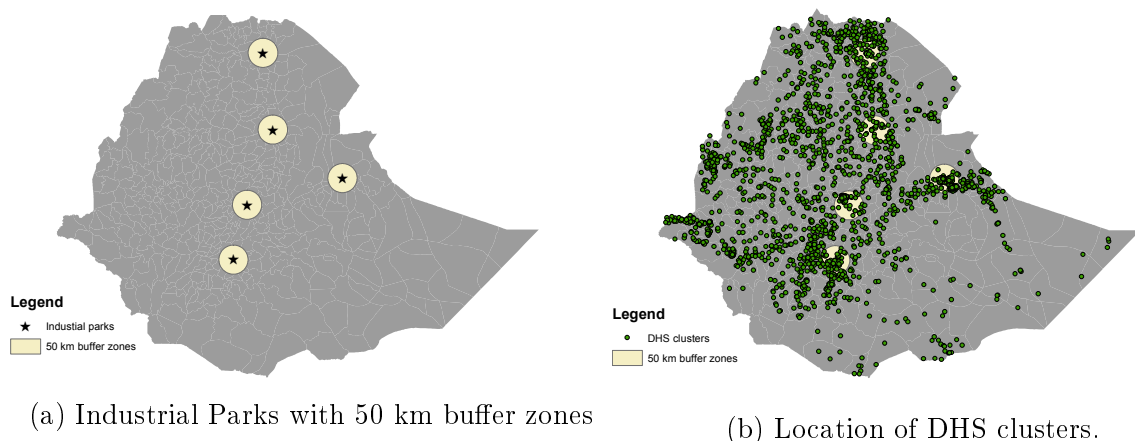


Figure 2: Industrial Parks and DHS data

The factories' standard procedure of hiring is to advertise bulks of positions by posting on the front gate, by word of mouth, and on local job boards. The applicants are asked to gather on a specific day and are screened for eligibility using verbal and physical tests. The companies we collaborate with were hiring new workers and were willing to slightly alter their recruitment process. They first assess all job applicants and determine whether each applicant is eligible for the job or not. Then, from the pool of eligible candidates, we create lists of women having partners. From the lists with eligible and partnered entry-level applicants, we randomly assign around half (depending on the number of available positions and the number of available partnered women) to either receiving a job offer in the given factory (treatment) or to a control group. The randomization is possible since there is large surplus demand for jobs. The randomization was done using computers and the lists were sent back via email. The applicants are informed about the procedure

before the randomization is conducted.

IV Data and empirical strategy

The women were interviewed by female enumerators from an independent survey team before they started working. This baseline data collection took place between March 2016 and March 2018, depending on when the firms were hiring. The first follow-up data collection was conducted around 6 months after the first interview.⁷ We received the first follow-up data for a limited sample of women in November 2017 and the pre-registration was uploaded before any follow-up data was received. The survey contains modules gathering demographic and background information, including measures of earnings and other socio economic variables. We developed a comprehensive module for IPV containing questions on both attitudes and experience with IPV. We also include questions on female empowerment similar to the questions in the Demographic and Health Surveys (DHS).

We interviewed 1871 partnered women at baseline. Of these, 374 were not randomly allocated to jobs due to a misunderstanding in one place and due to internet problems during the state of emergency in another. We still collected data for these women but we do not include them in our main analysis.⁸ Out of the 1463 randomly assigned women in our baseline sample we managed to interview 1262 for the first follow-up (619 treated and 643 control women). We show in Appendix Table A1 that attrition is unrelated to treatment status. The only variable correlated with attrition is age: older women are less likely to attrite. When interacting treatment status with all the covariates (which was not pre-registered) we see that one coefficient is statistically significantly different at the 5 percent level, namely that acceptance of abuse if the woman goes out without telling her partner is more positively correlated with attrition for the treatment group.

⁷There is some variation in timing due to a state of emergency and insecurities in some areas at some points in time.

⁸The results including these women are very similar and none of the conclusions change if we do include them as we show in the Appendix Section A.6.

All other acceptance interactions point in the other direction, however. The interaction between Treatment and having worked before is also statistically significant at the 10 percent level. In all, we conclude that selective attrition does not seem to be a problem for the analysis.

Our main specification is:

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

where i indexes individuals, $t0$ refers to baseline values, and $t1$ is the first follow-up. We will also show results for $t2$ and $t3$, that is for the more medium run follow-up surveys. $Y_{i,t1}$ will most often be a measure of abuse (see below). $Treatment_i$ is a dummy variable equal to 1 if the woman was randomized to get the job offer and zero if not. This captures the so called intention to treat effect and it gives us an estimate of the total effect of being randomized to get a job offer. We always include $List_i$, which are list fixed effects (blocking variables) as women are randomized within this unit. As long as treatment status is randomly assigned we do not expect any baseline differences between treated and control women. We include control variables in some specifications to see if we can increase precision. In particular we include *Abuse last 3 months* at baseline and a vector of individual level baseline controls $X_{i,t0}$ (described below). We use robust standard errors.⁹

Our main outcome variable, *Abuse last 3 months*, is set equal to one for women who answer that they had a partner doing one of the following to them during the last 3 months prior to being interviewed: Pushing, shaking, slapping, throwing something, twisting an arm, striking with a fist or something that could cause injury, or kicking or dragging (any of which is classified by the DHS as "less severe violence"), attempting to strangle or burn, threatening with a knife, gun, or other type of weapon, and attacking with a knife, gun, or other type of weapon (any of which is classified by the DHS as "severe violence"),

⁹There is no need to cluster the standard errors at the factory level since the randomization is at the level of the individual (Abadie et al., 2017).

or physically forcing intercourse or any other sexual acts, or forcing her to perform sexual acts with threats or in any other way (any of which is classified by the DHS as "sexual violence").

It is important to apply accurate descriptions of the violence that has occurred in order to maximize disclosure (Ellsberg et al. 2001) and we therefore ask about a wide range of abusive acts using indicators of internationally validated standardized IPV measures. We base the questions and sequencing on the WHO Violence Against Women Instrument (Ellsberg and Heise 2002) and the Conflict Tactics Scales (Straus 1979; Hindin et al. 2008). Using a modified Conflict Tactics Scale (CTS) has several advantages compared to many other datasets on violence (see Kishor (2005) for an extensive overview). A characteristic of CTS is that it uses several different questions regarding specific acts of violence. In this way the measure is less likely to be polluted by different understandings of what constitutes violence. CTS is also argued to reduce underreporting, as it gives respondents multiple opportunities to disclose their experiences of violence (Kishor 2005; La Mattina 2017).

In Table 1 we see that around 29 percent of the women in the sample have ever been abused and around 13 percent have been so during the last three months before the first follow-up. Notably, we see that the rate of recent abuse in the full sample of treatment and control individuals has decreased from 19 to 13 percent from baseline to the first follow-up. In addition to our main outcome we also measure emotional violence and controlling behaviors. The questions about emotional violence are the same as in the DHS surveys and are coded as one if the partner humiliated, threatened or insulted the woman.¹⁰ We follow Heise and Kotsadam (2015) and create a variable for the number of controlling issues last 3 months by adding the number of positive responses to questions regarding jealousy, controlling and manipulating behaviors.¹¹

¹⁰See survey questions 13-15b in the survey provided in Appendix Section A9 for exact wordings.

¹¹See questions 7b-11b.

Table 1: Descriptive statistics

	(1)	
	Mean	SD
<i>Physical abuse variables (1st follow up)</i>		
Abuse	0.290	(0.454)
Abuse last 3 months	0.129	(0.336)
Less severe	0.260	(0.439)
Less severe last 3 months	0.109	(0.311)
Severe	0.018	(0.134)
Severe last 3 months	0.003	(0.056)
Sexual	0.092	(0.289)
Sexual last 3 months	0.036	(0.187)
<i>Other outcome variables (1st follow up)</i>		
Emotional	0.399	(0.490)
Emotional last 3 months	0.177	(0.382)
Nr of control issues	1.010	(1.549)
Nr control last 3 months	0.376	(0.862)
Empowerment index	0.361	(0.327)
Nr empowerment items	4.334	(3.919)
Equality index	0.126	(0.136)
Nr equality items	1.260	(1.361)
<i>Employment and income variables (1st follow up)</i>		
Any wage job last 6 months	0.498	(0.500)
Earnings from wage job last 6 months (in Birr)	2114	(3244)
Share of earnings from wage job	0.320	(0.395)
Earnings last 6 months (in Birr)	2818	(3919)
Share of earnings	0.349	(0.395)
Income last 6 months (in Birr)	3434	(4116)
Share of income	0.229	(0.274)
She earns more than him	0.249	(0.433)
<i>Main baseline variables</i>		
Treatment	0.490	(0.500)
Abuse last 3 months	0.193	(0.395)
Any formal wage job (ever)	0.308	(0.462)
Age	24.909	(6.139)
Justified: goes out	0.300	(0.459)
Justified: neglects ch	0.352	(0.478)
Justified: refuses sex	0.151	(0.358)
Justified: argues	0.197	(0.398)
Justified: burns food	0.196	(0.397)
Muslim	0.140	(0.347)
Protestant	0.229	(0.420)
Medium education	0.514	(0.500)
High education	0.215	(0.411)
Father beat mother	0.351	(0.477)
<i>N</i>	1262	

Notes: All variables are measured at the first follow-up except for Treatment and the baseline controls. The share variables refer to woman's share of the total couple earnings and income.

We measure female empowerment with questions on intra-household decision making (see Seymour and Peterman (2018) for a recent review and discussion about such measures). We create an empowerment index based on 12 different questions on intra-household decision making.¹² 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.¹³ We then add the 12 variables together and divide by 12 to get an index ranging between 0 and 1. The survey also includes 11 questions on a wider set of attitudes toward gender equality. We recode each of these questions into dummy variables so that 1 is gender unequal.¹⁴ We again create an index where we add the dummies together and divide by 11.

The vector of individual level controls are all taken from the baseline survey. Employment at baseline is based on the answer to the survey question: "Have you ever had a formal job with salary before?". From this we create the variable *Any formal wage job (ever)*, which equals one if the answer is yes. Table 1 shows that around 31 percent of women have ever had a formal job at any time before the survey.

We also collected data on attitudes toward IPV by asking the same questions as the main ones used in the DHS surveys. For each of the five variables we code them as one if the respondent agrees that a husband is justified in beating his wife in the five following situations: She goes out without telling him, she neglects the children, she argues with him, she refuses to have sex with him, or she burns the food. Following previous research (e.g. Cools and Kotsadam 2017) we also create a variable *Father beat mother*, which is equal to one if the respondent answers yes to the question: "As far as you know, did your

¹²We 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 12 questions that were more likely to apply to everyone (questions J1.03-J1.15 in the survey).

¹³If 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.

¹⁴See 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.

father ever beat your mother?".

We include a set of demographic variables. We retain the continuous coding of age in years and 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.

We test for baseline balance on these variables both individually and together by regressing $Treatment_i$ on the variables one by one while controlling for the blocking variables (Lists). As many variables are tested we do not necessarily expect all of them to be statistically insignificant. We see in column 1 of Table 2 that some of the variables are correlated with treatment status so that showing results both with and without controls will be useful. In column 2 we see that being Muslim and having seen your father abuse your mother are statistically significantly correlated with treatment when we include all variables at the same time. Most importantly, however, we find that the variables cannot predict treatment status together in an F-test ($F=1.26$ $p=0.26$). We therefore view the randomization as successful. In columns 3 and 4 we test how the same control variables predict IPV at follow-up and we note that they do ($F=3.43$ $p=0.06$), but that IPV at baseline is the only strong predictor. We note that Muslim, which is the variable with the strongest imbalance in treatment probability, is not correlated with Abuse.

In the Appendix Section A.1 we compare data from our survey to data from the DHS. The rates of IPV are similar and comparing our data to the same areas in the DHS, the numbers are similar also with respect to employment. We also show that there is variation across our study areas with respect to levels of abuse, employment, divorce rates, and acceptance of abuse as measured in the DHS. We later use these data to explore heterogeneous treatment effects.

Table 2: Balance tests and predictions of control variables.

	(1)	(2)	(3)	(4)
	Treatment		Abuse last 3 months 1st follow up	
Abuse last 3 months (B)	-0.0057 (0.037)	0.0069 (0.037)	0.19*** (0.032)	0.19*** (0.032)
Any formal wage job (ever)	0.00063 (0.0030)	-0.0018 (0.0031)	0.017 (0.022)	0.0077 (0.022)
Age	-0.065** (0.032)	-0.053 (0.039)	-0.0012 (0.0019)	-0.0012 (0.0019)
Justified: goes out	-0.053* (0.032)	-0.040 (0.040)	0.014 (0.022)	-0.0051 (0.025)
Justified: neglects ch	-0.068* (0.041)	-0.050 (0.049)	0.016 (0.021)	-0.0072 (0.027)
Justified: refuses sex	-0.015 (0.037)	0.040 (0.045)	-0.0091 (0.028)	-0.060* (0.032)
Justified: argues	-0.015 (0.039)	0.041 (0.049)	0.041 (0.026)	0.036 (0.030)
Justified: burns food	0.039 (0.033)	0.040 (0.033)	0.046* (0.027)	0.040 (0.033)
Muslim	-0.13** (0.054)	-0.13** (0.058)	0.0036 (0.037)	-0.0012 (0.038)
Protestant	0.12* (0.065)	0.11 (0.067)	0.0064 (0.042)	0.034 (0.042)
Medium education	-0.061* (0.034)	-0.068 (0.051)	-0.010 (0.022)	-0.029 (0.029)
High education	0.056 (0.037)	-0.010 (0.056)	-0.0059 (0.024)	-0.019 (0.032)
Father beat mother	-0.059* (0.031)	-0.064** (0.031)	0.026 (0.021)	0.019 (0.021)
No. of observations	1262	1262	1262	1262
R-squared		0.08	0.11	0.11
F-test		1.26		3.43
P-value of F-test		0.26		0.06

Notes: Columns 1 and 3 show coefficients when we include the variables one by one. All regressions control block fixed effects. Robust SE in parentheses.

A) Employment and income variables

We have several measures that enable us to investigate the effects of job assignment on job take-up and earnings. In the 6 months follow-up analysis we create a variable, *Any wage job last 6 months*, which equals one if the respondents answer affirmatively on either one of the two questions: "Did you start working at Factory X" (the one where the respondent applied) or "Have you had any other formal salaried job with salary since the last interview". For the later follow-up analyzes (at 12 and 18 months) we instead create a dummy variable based on earnings from any wage job (where 1 equals positive earnings).¹⁵

As not all women 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:

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

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

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 Abuse last 3 months. It should be noted that the exclusion restriction need not hold for variables such as earnings and income shares as it is likely that getting a job affects a persons identity in addition to the effects it has on income. We therefore pre-specified that the intention to treat specification is the main specification. The IV models should rather be seen as explorative tests of mechanisms for the results.

¹⁵This was not pre-specified in the analysis plan but we change it anyway as it makes little sense to continue to base the variable on whether they started working at the factory.

V Main results

We start by showing the effects of the randomization on employment related variables in Table 3. We see a large effect on the probability of having had any wage job during the last six months. While 29 percent in the control group have had such a job, this share increases to 69 percent for the treatment group. We also see large effects on earnings and on the woman’s share of couple earnings and incomes. The women’s earnings from wage jobs is more than doubled (column 2), her share of within couple earnings is increasing (column 3), and the probability that she earns more than her partner increases from 18 percent to 32 percent (column 4). In Appendix Table A2 we show the first stages on more employment variables (with and without the full set of baseline controls) and we note in particular that incomes from any source are also higher in the treatment group. In Appendix Table A35 we also see that job offers lead to more total hours worked overall, mainly at the expense of leisure time. As we show later, there are no effects of the job offers on partners working hours, job probabilities, or earnings.

Table 3: First stages: Effects of treatment on employment and earnings.

	(1)	(2)	(3)	(4)
	Any wage job	Earnings from wage job	Share of wage earnings	She earns more
Treatment	0.40*** (0.025)	1726.8*** (172.6)	0.19*** (0.025)	0.14*** (0.028)
Mean dep. var in C group	0.29	1292.02	0.23	0.18
No. of observations	1262	1262	930	930
R-squared	0.29	0.22	0.22	0.16
Controls	Block	Block	Block	Block

Notes: All regressions control block fixed effects. Robust SE in parentheses. The outcomes refer to the last six months. Share of wage earnings refers to her share of the total couple wage earnings during the last six months. She earns more is a dummy for whether the woman has higher earnings than her partner.

In Table 4 we show the effects of job offers (Treatment) on IPV. Treatment is not statistically significantly related to physical abuse and the coefficients are close to zero in the first follow-up data. In column 1 we show the results from our main specification, which only includes the list fixed effects. The coefficient for Treatment is 0.01 and conducting an equivalence test with two one-sided t-tests (TOST), we can reject effects

more negative than -0.043 and more positive than 0.023.¹⁶ Hence, we can reject relatively small effects in any direction. The results are very similar if we add the vector of individual level baseline controls, as we show in column 2.¹⁷

Exploring other types of violence, we see in columns 3 and 4 that there is a negative effect on emotional violence.¹⁸ This effect is large and suggests that emotional violence is reduced with 5.3 percentage points (26.5 percent from the mean in the the control group). In Appendix Table A7 we show that the estimated effect on emotional violence seems to be driven by all three components (humiliation, threats, and insults) being reduced. We find no statistically significant effect on controlling behavior. In general the control variables do not do much to affect the estimates, but they do not affect the standard errors much either.¹⁹

Table 4: Reduced form estimates. The effects of treatment assignment on various forms of violence.

	(1)	(2)	(3)	(4)	(5)	(6)
	Abuse	Abuse	Emotional	Emotional	Controlling	Controlling
Treatment	-0.0100 (0.020)	-0.012 (0.019)	-0.053** (0.022)	-0.054** (0.022)	-0.021 (0.050)	-0.024 (0.049)
Mean dep. var in C group	0.13	0.13	0.20	0.20	0.38	0.38
No. of observations	1262	1262	1262	1262	1261	1261
R-squared	0.06	0.11	0.07	0.09	0.06	0.09
Controls	Block	Full	Block	Full	Block	Full

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

In Table 5 we show results for our main variable to be instrumented, "Any wage job last 6 months". In columns 1 and 2 we show the OLS relationships between baseline wage

¹⁶Alternatively, the 95 percent confidence interval is [-.049, .029], which is very similar. We prefer the one sided equivalence tests for conceptual reasons as one tests whether effects are larger than a highest value and lower than a lowest value. In practice, it makes little difference. If we pool all post treatment waves together in order to maximize power we can reject effects more negative than -0.031 and more positive than 0.013, see Appendix Table A29. We did not pre-register to pool the waves.

¹⁷Breaking the effect down by different components of physical abuse we see in Appendix Table A3 that there does not seem to be any effect on less severe, severe, or sexual abuse.

¹⁸While we pre-registered the analyses of the other types of violence we still view the results as exploratory as these are not our main outcome.

¹⁹In Appendix Table A4 we show that the results are also similar when using an "optimal" set of controls, using a double-debiased LASSO regularization approach (Belloni et al., 2014). Notably, the only selected control variable for the abuse regression is abuse at baseline. This analysis was not pre-specified.

job and abuse. We note that the correlation is positive, as in previous literature focusing on Africa and as in the DHS survey for Ethiopia in 2016 (where women employed last year have a 2 percentage points higher IPV rate last year). In columns 3 and 4 we show the first stage relationships again for completeness. In columns 5 and 6 we show the causal effects of having had a wage job during the last six months on abuse when it is instrumented by the randomized job offer. We see that the coefficient is negative but it is not statistically significant. In Appendix Table A5 we present the results from IV models with other employment related variables.

In Appendix Section A.2 we present longer term results. Importantly, attrition is still unrelated to Treatment and there is still a first stage effect of Treatment on employment and earnings. There is no effect on IPV in any of the follow-up surveys and the effect on emotional abuse is not present after 12 months but is there after 18 months as well.

VI Mechanisms and heterogeneity

There may be several reasons why employment does not affect IPV. It could be that employment does not affect important mediators such as empowerment and gender attitudes, or that it affects different types of women in opposite directions and that the effects cancel out on average. It may also be that female employment at the individual level is not important on its own, but that it is the relative position within couples that matters.

In this section we explore these mechanisms.²⁰

²⁰In the Appendix Section A.3 we show non pre-registered results that abuse does not seem to be instrumentally used for resource extraction in our setting. We find that job offers increase female expenditures on both private and public goods. Using our longitudinal data we also note, however, that changes in abuse correlate with reduced spending on household as well as male goods. In Appendix Section A.4 we investigate the role of time use for abuse in our setting. This analysis was not pre-registered. First we show that travel time is positively affected by treatment. We also show that it is positively correlated with abuse, so it does not give any support for a exposure reduction theory of violence. We find that women do less household work if they are randomly assigned to a job offer but, again, household work is not correlated with abuse. Hence, abuse does not seem to be used instrumentally in order to punish women for doing less household work.

Table 5: Correlations and effects of wage jobs on abuse last 3 months.

	(1) OLS	(2) OLS	(3) First stage	(4) First stage	(5) IV	(6) IV
Any wage job last 6 months (B)	0.054** (0.026)	0.050* (0.026)				
Any wage job last 6 months					-0.025 (0.049)	-0.031 (0.049)
Treatment			0.40*** (0.025)	0.39*** (0.026)		
Mean dep. var in C group	0.13	0.13	0.29	0.29	0.13	0.13
No. of observations	1262	1262	1262	1262	1262	1262
R-squared	0.09	0.11	0.29	0.31	0.06	0.11
Controls	Block	Full	Block	Full	Block	Full

Notes: Robust SE in parentheses. (B) refers to baseline such that columns 1 and 2 refer to any wage job last 6 months as measured at baseline. The First stage regression has Any wage job last 6 months (at follow-up) as the outcome variable. The IV results are results from two stage least squares regressions where Treatment is used to instrument for any wage job last 6 months at follow-up.

A) Effects of job offers on potential mediators

Empowerment, attitudes toward gender equality, attitudes toward abuse, and controlling behavior are factors that are likely mediators for how employment could impact abuse. We show in Table 6 that there is indeed a positive correlation between these variables and abuse at baseline (except for the gender equality index for which the correlation is very small and not statistically significant). The indices are coded such that higher values imply less female empowerment ("He has final say over...") and less gender egalitarian attitudes.

In Table 7 we see that there is no treatment effect on any of these variables.²¹ In Appendix Tables A6a and A6b we show the estimated effects on answers to each of the questions that comprise the empowerment and equality indices. We see that there is only one statistically significant effect of job offers out of all the gender equality variables. Women in the treatment group are 4.5 percentage points more likely to agree that "It is okay for women to travel or to leave the house for several nights to do business". The limited effects on these potential mediators is a likely explanation for the lack of effects of Treatment on IPV.

²¹In Appendix Table A8 we see very similar results when we control for the full set of baseline variables.

Table 6: Correlation at baseline between abuse and potential moderators.

	(1) Abuse	(2) Abuse	(3) Abuse	(4) Abuse
Equality index (B)	0.058 (0.076)			
Empowerment index (B)		0.084** (0.041)		
Acceptance index (B)			0.080*** (0.024)	
Nr of control issues (B)				0.096*** (0.0088)
Mean dep. var in sample	0.19	0.18	0.19	0.19
No. of observations	1260	935	1262	1262
R-squared	0.09	0.11	0.10	0.21
Controls	Block	Block	Block	Block

Notes: All regressions control block fixed effects. Robust SE in parentheses. The indices for female empowerment and equality are reverse coded so that higher values imply less empowered or equal. The sample includes everyone without missing values of the outcome at baseline and follow-up.

Table 7: Reduced form effects on potential mediators.

	(1) Equality index	(2) Empowerment index	(3) Acceptance index	(4) Nr controlling issues
Treatment	-0.0075 (0.0074)	-0.012 (0.019)	-0.034 (0.027)	-0.020 (0.087)
Mean dep. var in sample	0.13	0.38	0.44	1.05
No. of observations	1260	1041	1262	1262
R-squared	0.16	0.22	0.15	0.13
Controls	Block	Block	Block	Block

Notes: All regressions control block fixed effects. Robust SE in parentheses. The indices for female empowerment and equality are reverse coded so that higher values imply less empowered or equal.

B) Heterogeneity with respect to baseline characteristics and across areas

Despite the fact that there is no average effect of employment on IPV or on any of the likely mediators, it may be the case that heterogeneity in the effects across women go in different directions so that the average effect becomes close to zero. As discussed in Section II, the level of bargaining power is likely to be an important moderator for the effects of employment and we present the heterogeneity for the empowerment index in Table 8.²² We see that there is some indication for the effects being more negative for women with lower bargaining power at baseline. The effect is only statistically significant (and only at the 10 percent level, $p=0.082$) when we add the baseline controls, however. In column 2 we see that Treatment is correlated with a decline of abuse of 4.5 percentage points for women with a high degree of bargaining power (where the index is zero so that the partner does not have sole decision making power over any of the issues) but is 9 percentage points higher for women where the partner has total decision making power (index=1). The Appendix Tables A9 to A14 also show the heterogeneity results for all baseline variables and the different components of the empowerment index with and without controls. Investigating the different components of the empowerment index we see that the result is driven by decision making power over starting a business, opening a bank account, and contraception use.²³

We find no evidence of effect heterogeneity with respect to any of the baseline control variables (see Appendix Tables A9 and A10). That is, there is no statistically significant difference in the effects for women of different ages, religion, or education levels. Neither is there any difference for women with different attitudes towards domestic violence or

²²Note that we, as pre-registered, code missing values on control variables as zero and add dummy variables for missing variables in order to not reduce the sample unnecessarily. When the same variables are used as outcomes, however, no recoding of missing values is done.

²³In addition, we have tested whether there are heterogenous effects across couples with larger and smaller differences in age and education at baseline, and we did not find any such heterogeneity. Neither do we find any statistically significant heterogeneity if we use controlling behavior or the gender equality index at baseline and interact it with treatment.

whom had different experiences with their fathers abusing their mothers. We further note that there is no difference in the effects for women who had been employed before or not, nor between women that had recently been abused before or not.²⁴ In total, we note that there is very limited evidence for heterogeneous treatment effects, with the exception for heterogeneity with respect to baseline empowerment. In Appendix Section A.2 we further show that there is no treatment effect heterogeneity with respect to baseline empowerment after 12 or 18 months.

The effects of jobs on IPV are likely to differ in different settings. Studies using observational data from more developed countries find that female employment is protective in areas with relatively greater gender equality in terms of attitudes and more liberal divorce laws and practices (the UK and the US versus Mexico and traditional areas of Spain). In Appendix Section A.1 we present results moderated by area levels of divorce rates, abuse, female employment, and acceptance of abuse. We find some differences across areas but no difference is statistically significant when we include all characteristics at the same time. Neither do we find any effects in any specific region, nor when excluding the region with the weakest first stage relationship.

C) Relative employment and relative income

Theoretically, it is often stressed that relative resources within the couple are important (e.g. Cools and Kotsadam 2017). There are different theories on the role of relative resources which yield different predictions. For instance, according to the marital dependency theory, a woman's lower relative income may lead her to be economically dependent on her male partner and thereby increase her risk of abuse as she is less likely or able to

²⁴We also tested whether there was a difference in effects between those that had ever been abused or not. In the theoretical model of Anderberg et al. (2016), such a situation offers the most interesting case in terms of revealing information about husband type. The prediction is that men will be less likely to signal that they are of the abusive type in situations where women have a better outside option. This would also be consistent with Tankard et al. (2019) who find that a savings intervention in Colombia reduced the risk of IPV only for women never abused at baseline. We find no difference in the effects across these groups.

Table 8: Heterogeneity in reduced form effects by the baseline empowerment index. Dependent variable is Abuse last 3 months.

	(1)	(2)
	Abuse	Abuse
Treatment	-0.038 (0.027)	-0.045* (0.027)
Empowerment index	-0.0063 (0.039)	-0.030 (0.038)
Empowerment*Treatment	0.079 (0.053)	0.090* (0.052)
Mean dep. var in C group	0.13	0.13
No. of observations	1262	1262
R-squared	0.07	0.12
Controls	Block	Full

Notes: All regressions control block fixed effects. Robust SE in parentheses. The index for female empowerment is reverse coded so that higher values imply less empowerment.

exit the relationship. On the other hand, identity based theories focus more on status inconsistencies and stress that a woman's higher relative income may lead to more abuse because it threatens the male breadwinner identity.

We start by investigating the differential effects of Treatment on IPV as moderated by the woman's partner's employment status in Table 9. In column 1 we include Treatment, a dummy variable for whether the husband has a wage job at the first follow-up, and their interaction. We see that there is a negative correlation between having a husband with a wage job and abuse for the control group women but this negative correlation is canceled out for the treated women. This may suggest that the status component of relative earnings matter since husbands with a job having wives without a job maintain their breadwinning status, while in households where both have job the husbands' status could be undermined. The interaction term is not statistically significant, however.

We proceed to investigate the relationship between relative earnings and abuse. In column 2 we use his earnings during the last 6 months (in 1000 Birr) and interact it with treatment. We see that having a partner with higher earnings is correlated with lower rates of abuse for the control group but again less so for the treatment group. For the

control group, having a husband with 1000 Birr higher earnings is correlated with 0.35 percentage points lower risk of abuse. The mean of husband earnings is around 12,000 Birr and has a standard deviation of 13,000 Birr (in both baseline and follow-up). Hence a standard deviation change in husband earnings predicts quite substantial differences in abuse rates (4.55 percentage points). Using the baseline earnings of the husband instead, we see in column 3 that the pattern is similar but the precision is lower. These results indicate that the relative protection of having a husband earning more money is reduced for women being offered a job.

Taken together, we see no evidence for the marital dependency theory as there is a negative correlation between partner working as well as partner earnings and abuse for the control group. This correlation is smaller for the treatment group but it does not turn into a positive correlation. On the other hand, there is no clear evidence for the status inconsistency theory either. While the association between his earnings and abuse seem less negative for the treatment group it is also the case that treatment is not significantly correlated with more abuse for women without a working partner, as this theory would predict.

The status inconsistency experienced by the husband should be largest in the cases where she earns more than him. In order to investigate this more closely we proceed as is in Bertrand et al., (2015) and test whether there is a discontinuity at the point where they earn the same (0.5) in the share of the couple earnings distribution. Column 4 of Table 9 shows the results for the full sample (of both treated and control women). We see that there is a positive correlation between her share of earnings and abuse for women in couples where she earn less than him and a negative correlation between her share of earnings and abuse for women in couples where she earn more than him. The coefficient for "She earns more than him" is positive, indicating that there is a jump in the probability of being abused at the threshold of her earning exactly the same. None of the coefficients in column 4 are, however, statistically significant. A disadvantage

Table 9: Relative employment, identity and IPV.

	(1)	(2)	(3)	(4)	(5)	(6)
	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse
Treatment	-0.038 (0.032)	-0.038 (0.028)	-0.033 (0.027)		0.021 (0.027)	0.0076 (0.034)
Husband has a job	-0.052* (0.028)					
Husband job*Treatment	0.053 (0.040)					
Husband earnings 6 months		-0.0035*** (0.0011)				
Husband earnings*Treatment		0.0025** (0.0013)				
Husband earnings 6 months (B)			-0.0014* (0.00075)			
Husband earnings (B)*Treatment			0.0025* (0.0014)			
Share of earnings from wage job				0.042 (0.096)	0.016 (0.10)	-0.069 (0.15)
She earns more than him				0.13 (0.21)	0.22 (0.21)	0.76* (0.44)
She earns more*Share of earnings				-0.11 (0.24)	-0.094 (0.23)	-0.57 (0.47)
She earns more*Treatment					-0.13** (0.063)	-0.87* (0.48)
She more*Share*Treatment						0.65 (0.54)
Share of earnings*Treatment						0.13 (0.21)
Mean dep. var in C group	0.13	0.13	0.13	0.13	0.13	0.13
No. of observations	1231	1222	1252	931	930	930
R-squared	0.07	0.07	0.07	0.08	0.08	0.09
Controls	Block	Block	Block	Block	Block	Block

Notes: All regressions control block fixed effects. Husband earnings are in 1000 Birr's. When we use baseline variables we indicate this with (B). Robust SE in parentheses.

of pooling the treated and control women together is that the earnings share may be endogenous with respect to both his and her income. In column 5 we therefore interact treatment with the discontinuity variable in order to introduce exogenous variation in her earnings.²⁵ We note that there is a difference whereby women that are randomly assigned to job offers who earn more than their partners are less likely to be abused compared to women who earn more than their partners in the control group. That is, being randomly assigned a job offer drives the correlation of earning more towards zero. In column 6 we also interact the forcing variable and its interaction with the discontinuity with treatment as well. We then note that there is a jump in the probability of abuse at the margin where she starts to earn more than him for the control group but that this effect disappears for the treatment group (both statistically significant at the 10 percent level).

These results do not give much support for the status inconsistency theory, rather they seem more consistent with relative income having no impact on abuse once selection effects are controlled for by randomizing job offers.²⁶ This interpretation is also consistent with the IV results in Table A5, in particular the finding that she earning more than him has a very small and statistically insignificant effect on abuse once instrumented with treatment.

VII Addressing reporting issues: results from list experiments

Reported abuse is a function of both abuse and the propensity to report it, and we cannot separately identify the two. When asking about experience with IPV we worry that individuals may conceal their experiences in order to conform to social norms or because they are ashamed. If such social norm bias is related to employment it can seriously undermine the credibility of our self-reported measures. While we believe that

²⁵His earnings response may theoretically be affected by treatment, but we do not find any effects of her treatment status on her partners earnings or job probability.

²⁶In Appendix Tables A15 to A17 we show that the results for the relative employment and earnings regressions are similar and that the conclusions stay the same if we also control for baseline abuse.

under reporting may occur in our data we still think that the problem is limited due to the careful data collection. One indication of this is the high actual reported prevalence and the high acceptance of violence in the data. In any case, there exist no available data on IPV from other sources (e.g. from the police or hospitals) at the local level in Ethiopia. Even if such data would exist, it is unlikely that reporting bias would be lower. Using DHS data, Palermo et al. (2014) show that there is much larger underreporting to formal sources than in surveys. In fact, only 7 percent of the women that reported IPV in the DHS surveys had reported to a formal source.

In order to investigate the issue of underreporting and social desirability bias we randomly divided a sample (see below) into two groups and asked respondents to count the number of true statements on a list that either includes a sensitive statement or not, in a so-called "list experiment". By comparing the number of statements reported as true across the two groups we get a measure without any specific individual having revealed their own status. By also asking a question about the sensitive statement directly to the list control group we can assess the degree of underreporting by comparing the results when using the two different ways of asking. The degree of underreporting can then also be compared across subgroups of e.g. those offered a job and not or those employed and non-employed. Three papers use list experiments to investigate underreporting of IPV across subgroups and none of them find it to be correlated with employment (Peterman et al., 2018; Agüero and Frisancho, 2017; Joseph et al., 2017). Bulte and Lensink (2018), however, evaluate an empowerment course and find that using list experiments affects the conclusions.

We conduct the list experiment on a sample of 367 women (254 of which are in our main sample) that were participating in an empowerment course after the first follow-up survey in January-April 2018. At the final day of the course we had them answer a questionnaire. The data collection started with a detailed instruction of how to answer the questions (see Appendix Figure A1). In Figure 3 we show the control and treatment

questions when the variable of interest is "My partner sometimes hits me". The control questions include four statements that we are not interested in and that are used only to get an average to compare the other group with. The treatment list includes the same questions and adds the question of interest. The control questions are created to avoid ceiling and floor effects and to include items that are negatively correlated so as to increase power (Glynn, 2013). To take a concrete example, let us say that the list control group answers that two of the four statements are true on average and the list treatment group answers that 2.5 of the statements are true on average. Since the only difference between the two groups are the extra question on IPV we would infer that 50 percent of the individuals in the list treatment group had experienced IPV.

We also included another list in order to measure "Partner punched last 3 months". The list treatment group got the list shown in Appendix Figure A2 and the list control group got a list without item 2.

1. You have been to Addis Ababa 2. You cannot borrow money from anyone outside your family if you need it 3. You have friends that you can always depend on if you need money 4. You have been to the cinema	Number of true statements	1. You have been to Addis Ababa 2. You cannot borrow money from anyone outside your family if you need it 3. You have friends that you can always depend on if you need money 4. Sometimes your partner hits you when he gets angry 5. You have been to the cinema	Number of true statements
(a) Questions to the list experiment control group.		(b) Questions to the list experiment treatment group.	

Figure 3: List experiment for the question "Partner sometimes hits"

In Table 10 we show the results of the list experiments. We see that individuals getting the list with the additional question about partner sometimes hitting answer 0.18 more true statements on average. The interpretation from this is that 18 percent of the individuals have partners that sometimes hit them. When asking the question directly to the control group we see that 15 percent answer that they have partners that sometimes hit them. While lower, the difference is not large nor statistically significant. For the list experiment with "been punched by your husband in the last three months" we get a larger difference but it is not statistically different either. We see that people in the list control group answer that around 1.5 of the four control items are true on average for

both lists.

Moving over to differences in reporting across subgroups we split the samples into those offered a job (treated) and not (control) and into those employed at baseline or not. As seen in Figure 4, which shows the point estimates and 95 percent confidence intervals, there does not seem to be a difference for the statement "partner sometimes hits" for any of these groups. An important caveat to these analyses is that jobs may affect the control items as well so the results should be interpreted with care. Another disadvantage is that the list experiment leads to relatively noisy estimates. Appendix Figure A3 shows the same type of figure for the second list experiment.

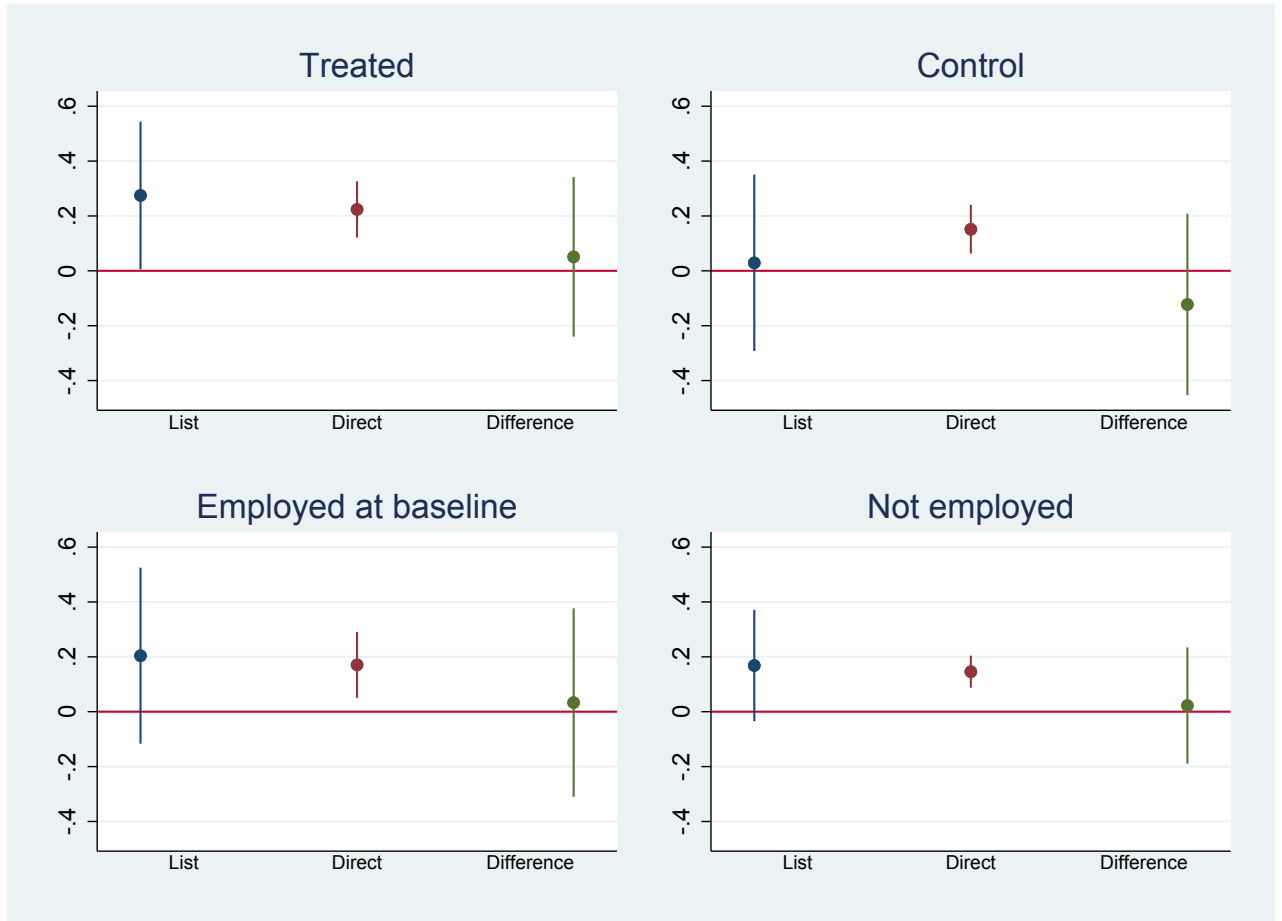
While we can never completely rule out that being offered a job affects reporting, we find the results reassuring. In addition, we are not particularly worried about researcher demand effects whereby the respondents would answer the questions in a way to try to please the enumerators. First of all, neither the enumerators nor the respondents had any reason to believe that the main interest lies in investigating IPV. The survey was framed as one "to study the lives of women seeking work in the industrial sector in Ethiopia". The survey is also long (it takes between 60 and 90 minutes to complete the interviews) and only a small subset of the questions are about IPV.

In our data, abuse decreases for both treatment and control women from baseline to the first follow-up. We do not know why abuse has declined in our sample. It may be that

Table 10: List experiment.

	(1)	(2)
	Partner hits sometimes	Partner punched last 3 months
List treatment	0.18** (0.087)	0.11 (0.092)
Mean nr answers in C group	1.49	1.50
Mean direct question in C group	0.15	0.06
SE mean direct question in C group	(0.026)	(0.018)
No. of observations	367	367
R-squared	0.01	0.00
Controls	None	None

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



Notes: Treated and control refers to the randomization of job offers in the field experiment. List refers to the estimated prevalence of having partner sometimes hitting in the list experiment. Direct refers to the prevalence when using a direct survey question. Difference refers to the difference between asking in the list experiment minus asking directly. 95 percent confidence intervals are shown.

Figure 4: List experiment: "Partner sometimes hits" by sub-groups

general changes in Ethiopian society and in our areas in particular (such as high growth, increased male and female employment rates, and political liberalization) reduce IPV.²⁷ It may also be that reporting of abuse decreases when women are interviewed several times. We do not believe this to be the case for several reasons. First of all, we would expect more reporting over time as the women build up a relationship with the enumerators. Secondly, previous studies have not found any evidence for such survey effects, even when explicitly testing for them (Haushofer et al., 2019). For social desirability to affect the internal validity of our conclusions it would have to be the case that abuse either increases, or decreases less, in the treatment group but that they do not want to tell us (anymore) or that abuse decreases in the treatment group but the control group do not want to tell us that they are still abused. As we do not observe any effects of treatment on the acceptance of abuse we find such effects particularly unlikely.

VIII Conclusion

Intimate partner violence (IPV) is harmful and costly for society (Fearon and Hoeffler, 2014). It is related to a host of negative outcomes for the women who are abused and people around them (Carrell and Hoekstra, 2010; Pollak, 2004; Doyle and Aizer, 2018; Aizer, 2011). IPV is prevalent in all societies, but the level and the degree to which it is considered acceptable vary greatly (Cools and Kotsadam, 2017). In Ethiopia, data from the Demographic and Health Survey in 2016 shows that 16 percent of women had been physically abused by their partners in the last year. Acceptance levels, i.e. the degree to which a husband is perceived as justified in beating his wife, is also very high in Ethiopia, with more than half of the women in 2016 finding it acceptable under at least one condition.

Most previous evidence on employment and IPV is based on correlational studies

²⁷The political liberalization during this time were extraordinary and rapid. For instance, Ethiopia saw the release of thousands of political prisoners, opposition groups and media were allowed to work more freely, and a peace deal with Eritrea led to the Nobel Peace Prize to the new Prime Minister.

(see e.g. Cools and Kotsadam 2017; Heise and Kotsadam 2015). While correlations are illustrative they do not tell us whether employment affects IPV, whether IPV affects employment, or whether there is some other factor that affects both employment and IPV. Recent literature has also investigated the effects of contextual level employment level using Bartik instruments, finding that when labor markets have better conditions for women, abuse decreases in the US and in the UK (Aizer, 2010; Anderberg et al., 2016), but increases in Mexico and in areas of Spain where men are traditionally breadwinners (Davila, 2018; Tur-Prats, 2017).

We identify the individual level effects of formal employment on IPV by randomly assigning job offers to equally qualified applicants, in collaboration with large companies in Ethiopia. We find no effect of being offered a job on physical abuse, despite finding large effects on the probability of working, on time use, earnings, and on incomes. We find that job offers reduce emotional violence in the short run but the longer term results suggest that this effect is not stable over time. We find some indications of heterogeneous effects whereby women with low bargaining power at baseline seem to experience increased abuse in the short run if randomly assigned a job offer. There are no effects of job offers on attitudes toward gender equality, attitudes toward abuse, female empowerment, or controlling behavior. The lack of effects on these potential mediators are a likely explanation for the limited effects on abuse.

In investigating the effects of job offers and of abuse on spending patterns, it does not seem as if abuse is instrumentally used to extract resources in our setting. Being offered a job increases spending but abused women spend less on household goods and male goods. Neither does it seem to be the case that status inconsistencies trigger abuse for the women offered a job. In particular, job offers are not correlated with abuse for women with partners that are not working. In general, relative incomes within the household do not seem to matter much for abuse once we use the random assignment of jobs to control for selection effects. It is difficult to know why there is a correlation between

employment and abuse in the cross-section but our results suggest that it may be driven by selection rather than being a causal relationship. In addition, the margin we study the effects at is one where everyone apply for a job, it could be the case that it is the decision to apply that causes violence. It could also be that contextual level employment is more important than individual level employment. In a bargaining framework, improved employment opportunities increase the bargaining power of all women, including those who are currently not employed, and hence the contextual level of employment may be what determines outside options and threat points (Aizer, 2010). In any case, our results speak against the theories focusing on individual level or couple level resources.

The context under which we are investigating the effects is one where we should expect the increases in abuse following job offers to be large. Acceptance of abuse is high and acceptance of divorce is low in Ethiopia. Finding that job offers do not increase abuse in such a setting is comforting and we view it as possible that job offers could be protective in other settings with different moderating macro level factors. We strongly urge future studies to conduct similar field experiments in different settings so that we will learn whether there is no relationship overall or whether our results stand out in some way.

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Online Appendix

Table A1: Attrition.

	(1) Attrition	(2) Attrition	(3) Attrition (Treatment*X)
Treatment	-0.0030 (0.018)	-0.00050 (0.018)	
Any formal wage job (ever)		-0.015 (0.020)	-0.071* (0.041)
Age		-0.0075*** (0.0020)	0.0018 (0.0032)
Justified: goes out		0.024 (0.026)	0.11** (0.052)
Justified: neglects ch		0.020 (0.027)	-0.0040 (0.055)
Justified: refuses sex		0.041 (0.032)	0.069 (0.066)
Justified: argues		0.0035 (0.029)	-0.076 (0.058)
Justified: burns food		-0.023 (0.030)	-0.096 (0.060)
Abuse last 3 months		0.0098 (0.023)	0.013 (0.048)
Muslim		-0.011 (0.035)	0.057 (0.057)
Protestant		-0.025 (0.047)	0.0038 (0.048)
Medium education		-0.012 (0.034)	-0.0030 (0.051)
High education		0.013 (0.037)	0.070 (0.058)
Father beat mother		-0.017 (0.020)	0.051 (0.039)
Mean dep. var in C group	0.14	0.14	0.14
No. of observations	1463	1463	1463
R-squared	0.07	0.08	0.10
Controls	Block	Full	Full

Notes: All regressions control block fixed effects. In column 3 the coefficients presented are interaction terms where the variables are interacted with Treatment. The base coefficients are included in the model but not presented in the figure. Robust SE in parentheses.

Table A2: First stages: Effects of treatment on employment and earnings.

(a) Controlling for block fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Any wage job	Any factory job	Earnings from wage job	Share	Earnings	Share	Income	Share	She earns more
Treatment	0.39*** (0.026)	0.36*** (0.024)	1664.9*** (169.2)	0.19*** (0.025)	1283.6*** (205.7)	0.065*** (0.021)	1182.2*** (223.1)	0.065*** (0.016)	0.14*** (0.028)
Mean dep. var in C group	0.29	0.16	1292.02	0.23	2171.34	0.32	2824.93	0.19	0.18
No. of observations	1262	1262	1262	930	1262	1222	1259	1210	930
R-squared	0.31	0.31	0.26	0.25	0.16	0.26	0.14	0.17	0.18
Controls	Full	Full	Full	Full	Full	Full	Full	Full	Full

(b) Controlling for the full set of baseline controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Any wage job	Any factory job	Earnings from wage job	Share	Earnings	Share	Income	Share	She earns more
Treatment	0.39*** (0.026)	0.36*** (0.024)	1664.9*** (169.2)	0.19*** (0.025)	1283.6*** (205.7)	0.065*** (0.021)	1182.2*** (223.1)	0.065*** (0.016)	0.14*** (0.028)
Mean dep. var in C group	0.29	0.16	1292.02	0.23	2171.34	0.32	2824.93	0.19	0.18
No. of observations	1262	1262	1262	930	1262	1222	1259	1210	930
R-squared	0.31	0.31	0.26	0.25	0.16	0.26	0.14	0.17	0.18
Controls	Full	Full	Full	Full	Full	Full	Full	Full	Full

Notes: All regressions control block fixed effects. In Panel B, the full set of baseline controls are included. Robust SE in parentheses. Earnings from wage job only includes earnings from wage jobs and the share of within couple in column 4 refers to her share of such earnings. Earnings refers to earnings from any source and income include remittances and transfers in addition. The share in column 6 refers to her share of earnings and the share in column 8 refers to her share of incomes. She earns more is a dummy for whether the woman has higher earnings than her partner.

Table A3: Reduced form estimates. The effects of treatment assignment on various forms of violence.

	(1)	(2)	(3)	(4)	(5)	(6)
	Less severe	Less severe	Severe	Severe	Sexual	Sexual
Treatment	0.0013 (0.018)	0.0023 (0.018)	-0.00065 (0.0028)	-0.00078 (0.0024)	-0.0091 (0.011)	-0.011 (0.011)
Mean dep. var in C group	0.11	0.11	0.00	0.00	0.04	0.04
No. of observations	1262	1262	1262	1262	1262	1262
R-squared	0.04	0.09	0.08	0.09	0.08	0.10
Controls	Block	Full	Block	Full	Block	Full

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

Table A4: Reduced form estimates with optimal controls. The effects of treatment assignment on various forms of violence.

	(1)	(2)	(3)
	Abuse	Emotional	Controlling
Treatment	-0.0093 (0.019)	-0.053** (0.021)	-0.021 (0.049)
Mean dep. var in C group	0.13	0.20	0.38
No. of observations	1262	1262	1261
R-squared			
Controls	Optimal	Optimal	Optimal

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

Table A5: Instrumental variables estimates on abuse last 3 months. Other employment related variables instrumented with treatment.

	(1) Abuse	(2) Abuse	(3) Abuse	(4) Abuse	(5) Abuse	(6) Abuse	(7) Abuse
Earnings from wage job last 6 months	-0.0000058 (0.000011)						
Share of earnings from wage job		0.0011 (0.12)					
Earnings job last 6 months			-0.0000073 (0.000014)				
Share of earnings				-0.12 (0.32)			
Income last 6 months					-0.0000079 (0.000016)		
Share of income						-0.10 (0.29)	
She earns more than him							0.0015 (0.17)
Mean dep. var in C group	0.13	0.13	0.13	0.13	0.13	0.13	0.13
No. of observations	1262	930	1262	1222	1259	1210	930
R-squared	0.06	0.07	0.06	0.05	0.06	0.06	0.07
Controls	Block	Block	Block	Block	Block	Block	Block

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

Table A6: Reduced form effects on components of empowerment and equality.

(a) Reduced form effects on female empowerment ("He has final say over...").

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sick		Nr ch	contrac	earn outs	visit rel	her earn	his earn	small p	bulk p	large p	bank	business
Treatment	-0.014 (0.025)	0.018 (0.027)	0.015 (0.021)	-0.028 (0.025)	-0.0082 (0.026)	-0.0081 (0.024)	-0.023 (0.026)	-0.010 (0.014)	-0.021 (0.025)	-0.036 (0.027)	-0.028 (0.027)	-0.021 (0.027)
Mean dep. var in C group	0.31	0.37	0.16	0.30	0.32	0.22	0.68	0.08	0.29	0.52	0.53	0.54
No. of observations	1262	1249	1254	1261	1261	1119	1200	1261	1260	1261	1254	1253
R-squared	0.14	0.16	0.15	0.14	0.15	0.14	0.18	0.13	0.15	0.16	0.20	0.20
Controls	Block	Block	Block	Block	Block	Block	Block	Block	Block	Block	Block	Block

(b) Reduced form effects on gender equality (Higher values=unequal).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
School		Earn	Decide	Leave	childcare	hh work	Men decide	Tolerate	harassment	sex. harassment
Treatment	0.0069 (0.0093)	-0.013 (0.020)	-0.0047 (0.020)	-0.045** (0.020)	0.016 (0.012)	0.0032 (0.014)	-0.0041 (0.017)	-0.0079 (0.026)	-0.0056 (0.015)	-0.019 (0.015)
Mean dep. var in C group	0.03	0.16	0.15	0.18	0.05	0.07	0.15	0.35	0.08	0.09
No. of observations	1260	1262	1262	1262	1262	1262	1262	1261	1262	1262
R-squared	0.20	0.16	0.13	0.10	0.05	0.10	0.24	0.17	0.19	0.20
Controls	Block	Block	Block	Block	Block	Block	Block	Block	Block	Block

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

Table A7: Reduced form effects on the components of emotional violence.

	(1)	(2)	(3)
	Humiliated	Threatened	Insult
Treatment	-0.020 (0.013)	-0.011 (0.0078)	-0.037* (0.021)
Mean dep. var in C group	0.07	0.02	0.18
No. of observations	1261	1262	1262
R-squared	0.06	0.05	0.07
Controls	Block	Block	Block

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

Table A8: Reduced form effects on potential mediators. Results with full set of baseline controls.

	(1)	(2)	(3)	(4)
	Equality index	Empowerment index	Acceptance index	Nr controlling issues
Treatment	-0.0055 (0.0074)	-0.0061 (0.019)	-0.021 (0.026)	-0.042 (0.086)
Mean dep. var in sample	0.13	0.38	0.44	1.05
No. of observations	1260	1041	1262	1262
R-squared	0.18	0.23	0.25	0.16
Controls	Full	Full	Full	Full

Notes: All regressions control block fixed effects. Robust SE in parentheses. The indices for female empowerment and equality are reverse coded so that higher values imply less empowered or equal.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse
Treatment	-0.0030 (0.023)	-0.0099 (0.023)	0.0056 (0.024)	-0.0084 (0.021)	-0.0075 (0.021)	-0.00067 (0.021)	-0.026 (0.019)	0.024 (0.075)	-0.0016 (0.021)	-0.0019 (0.022)
Any wage job ever (B)	0.030 (0.031)									
Justified: goes out		0.012 (0.031)								
Justified: neglects ch			0.037 (0.030)							
Justified: refuses sex				-0.0039 (0.036)						
Justified: argues					0.046 (0.037)					
Justified: burns food						0.069* (0.038)				
Abuse last 3 months							0.14*** (0.041)	-0.00051 (0.0027)		
Age										
Muslim									0.028 (0.046)	0.026 (0.049)
Protestant										
*Treatment	-0.024 (0.042)									
*Treatment		0.0021 (0.043)								
*Treatment			-0.044 (0.041)							
*Treatment				-0.014 (0.055)						
*Treatment					-0.011 (0.052)					
*Treatment						-0.048 (0.052)				
*Treatment							0.091 (0.061)			
*Treatment								-0.0014 (0.0029)		
*Treatment									-0.059 (0.057)	
*Treatment										-0.040 (0.050)
Mean dep. var in C group	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13
No. of observations	1262	1262	1262	1262	1262	1262	1262	1262	1262	1262
R-squared	0.06	0.06	0.06	0.06	0.06	0.06	0.11	0.06	0.06	0.06
Controls	Block	Block	Block	Block	Block	Block	Block	Block	Block	Block

Table A10: Heterogeneity in reduced form effects by baseline control variables. Dependent variable is Abuse last 3 months.

	(1) Abuse	(2) Abuse	(3) Abuse	(4) Abuse	(5) Abuse	(6) Abuse	(7) Abuse	(8) Abuse
Treatment	-0.032 (0.028)	-0.0058 (0.022)	-0.019 (0.023)	-0.014 (0.026)	-0.041 (0.025)	-0.034 (0.023)	-0.023 (0.026)	-0.015 (0.025)
Medium education	-0.033 (0.030)							
High education		0.0041 (0.034)						
Father beat mother			0.011 (0.028)					
She sick				-0.0026 (0.029)				
nr ch					-0.0092 (0.030)			
contraception						-0.042 (0.029)		
earn outside							-0.055** (0.026)	
visit relatives								0.0013 (0.029)
*Treatment	0.043 (0.039)							
*Treatment		-0.018 (0.045)						
*Treatment			0.029 (0.041)					
*Treatment				0.0090 (0.039)				
Treatment					0.069 (0.039)			
Treatment						0.079 (0.043)		
*Treatment							0.032 (0.039)	
*Treatment								0.013 (0.041)
Mean dep. var in C group	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13
No. of observations	1262	1262	1262	1262	1262	1262	1262	1262
R-squared	0.06	0.06	0.06	0.06	0.07	0.06	0.06	0.06
Controls	Block	Block	Block	Block	Block	Block	Block	Block

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

Table A11: Heterogeneity in reduced form effects by empowerment variables. Dependent variable is Abuse last 3 months.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse
Treatment	-0.026 (0.022)	-0.046 (0.031)	-0.015 (0.021)	-0.025 (0.024)	-0.024 (0.028)	-0.055* (0.028)	-0.061** (0.029)	-0.038 (0.027)	-0.038 (0.027)
use her earnings	-0.036 (0.030)								
use his earnings		-0.0030 (0.031)							
small purchases			-0.0049 (0.036)						
bulk purchases				0.0054 (0.028)					
large purchases					0.026 (0.029)				
bank						0.0026 (0.030)			
business							-0.012 (0.029)		
index								-0.0063 (0.039)	
nr items									-0.00053 (0.0032)
*Treatment	0.073 (0.045)								
*Treatment		0.057 (0.039)							
*Treatment			0.036 (0.054)						
*Treatment				0.037 (0.039)					
*Treatment					0.022 (0.038)				
*Treatment						0.076** (0.038)			
*Treatment							0.082** (0.039)		
*Treatment								0.079 (0.053)	
*Treatment									0.0066 (0.0044)
Mean dep. var in C group	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13
No. of observations	1262	1262	1262	1262	1262	1262	1262	1262	1262
R-squared	0.07	0.07	0.06	0.06	0.06	0.07	0.07	0.07	0.07
Controls	Block	Block	Block	Block	Block	Block	Block	Block	Block

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

Table A12: Heterogeneity in reduced form effects by baseline control variables. Dependent variable is Abuse last 3 months.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse
Treatment	-0.0021 (0.023)	-0.011 (0.022)	0.0030 (0.023)	-0.0095 (0.021)	-0.011 (0.021)	-0.0012 (0.021)	0.025 (0.075)	-0.0046 (0.021)	-0.0070 (0.021)
Any wage job last 6 months (B)	0.024 (0.031)								
Justified: goes out		-0.0035 (0.031)							
Justified: neglects ch			0.013 (0.034)						
Justified: refuses sex				-0.053 (0.038)					
Justified: argues					0.040 (0.039)				
Justified: burns food						0.068* (0.041)			
Age							-0.00045 (0.0027)		
Muslim							0.020 (0.046)		0.046 (0.049)
Protestant									
*Treatment	-0.032 (0.042)								
*Treatment		-0.0049 (0.043)							
*Treatment			-0.045 (0.040)						
*Treatment				-0.018 (0.053)					
*Treatment					-0.0079 (0.050)				
*Treatment						-0.057 (0.050)			
*Treatment							-0.0015 (0.0030)		
*Treatment								-0.054 (0.056)	
*Treatment									-0.024 (0.049)
Mean dep. var in C group	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13
No. of observations	1262	1262	1262	1262	1262	1262	1262	1262	1262
R-squared	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11
Controls	Full	Full	Full	Full	Full	Full	Full	Full	Full

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

Table A13: Heterogeneity in reduced form effects by baseline control variables. Dependent variable is Abuse last 3 months.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse
Treatment	-0.033 (0.028)	-0.0050 (0.022)	-0.024 (0.023)	-0.020 (0.026)	-0.041 (0.025)	-0.038* (0.023)	-0.028 (0.025)	-0.017 (0.024)
Medium education	-0.050 (0.035)							
High education		-0.0028 (0.040)						
Father beat mother			0.0015 (0.028)					
She sick				-0.019 (0.028)				
nr ch					-0.017 (0.029)			
contraception						-0.048* (0.029)		
earn outside							-0.069*** (0.026)	
visit relatives								-0.0028 (0.029)
*Treatment	0.041 (0.038)							
*Treatment		-0.032 (0.045)						
*Treatment			0.035 (0.041)					
*Treatment				0.016 (0.038)				
Treatment					0.064 (0.039)			
*Treatment						0.087** (0.042)		
*Treatment							0.039 (0.038)	
*Treatment								0.013 (0.040)
Mean dep. var in C group	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13
No. of observations	1262	1262	1262	1262	1262	1262	1262	1262
R-squared	0.11	0.11	0.11	0.11	0.11	0.12	0.12	0.11
Controls	Full	Full	Full	Full	Full	Full	Full	Full

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

Table A14: Heterogeneity in reduced form effects by empowerment variables. Dependent variable is Abuse last 3 months.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse	Abuse
Treatment	-0.030 (0.022)	-0.046 (0.030)	-0.017 (0.021)	-0.034 (0.024)	-0.029 (0.028)	-0.063** (0.028)	-0.062** (0.029)	-0.045* (0.027)	-0.045* (0.027)
use her earnings	-0.042 (0.030)								
use his earnings		-0.018 (0.030)							
small purchases			-0.015 (0.033)						
bulk purchases				-0.019 (0.027)					
large purchases					0.0093 (0.029)				
bank						-0.015 (0.030)			
business							-0.023 (0.029)		
index								-0.030 (0.038)	
nr items									-0.0025 (0.0031)
Treatment	0.075 (0.044)								
*Treatment		0.053 (0.038)							
*Treatment			0.034 (0.050)						
*Treatment				0.051 (0.039)					
*Treatment					0.027 (0.038)				
*Treatment						0.084** (0.038)			
*Treatment							0.080** (0.038)		
Treatment								0.090 (0.052)	
Treatment									0.0075 (0.0043)
Mean dep. var in C group	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13
No. of observations	1262	1262	1262	1262	1262	1262	1262	1262	1262
R-squared	0.12	0.12	0.11	0.11	0.11	0.12	0.12	0.12	0.12
Controls	Full	Full	Full	Full	Full	Full	Full	Full	Full

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

Table A15: Relative employment and IPV. Controlling for baseline violence.

	(1) Abuse	(2) Abuse	(3) Abuse	(4) Abuse	(5) Abuse	(6) Abuse
Any wage job last 6 months	0.034* (0.020)	0.035* (0.021)			0.035* (0.020)	0.038* (0.021)
Any wage last 6 months (B)		-0.00055 (0.025)				0.0098 (0.026)
Husband has a job			-0.024 (0.020)	-0.0026 (0.021)	-0.025 (0.020)	-0.0065 (0.021)
Husband has a job (B)				-0.053** (0.021)		-0.050** (0.021)
Abuse last 3 months (B)	0.19*** (0.031)	0.19*** (0.032)	0.20*** (0.033)	0.20*** (0.032)	0.20*** (0.033)	0.20*** (0.032)
Mean dep. var in sample	0.13	0.13	0.13	0.13	0.13	0.13
No. of observations	1262	1246	1231	1224	1231	1214
R-squared	0.11	0.11	0.11	0.12	0.12	0.12
Controls	base viol	base viol	base viol	base viol	base viol	base viol

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

Table A16: Relative earnings and IPV. Controlling for baseline violence.

	(1) Abuse	(2) Abuse	(3) Abuse	(4) Abuse
Treatment	-0.024 (0.031)	-0.00022 (0.032)	-0.034 (0.028)	-0.035 (0.026)
Husband has a job	-0.039 (0.028)			
Husband job*Treatment	0.031 (0.039)			
Husband has a job (B)		-0.054* (0.027)		
Husband job (B)*Treatment		-0.0098 (0.038)		
Husband earnings 6 months			-0.0032*** (0.0011)	
Husband earnings*Treatment			0.0023* (0.0013)	
Husband earnings 6 months (B)				-0.0014* (0.00077)
Husband earnings (B)*Treatment				0.0027* (0.0014)
Abuse last 3 months (B)	0.20*** (0.033)	0.18*** (0.031)	0.20*** (0.033)	0.19*** (0.032)
Mean dep. var in C group	0.13	0.13	0.13	0.13
No. of observations	1231	1255	1222	1252
R-squared	0.12	0.12	0.12	0.11
Controls	base viol	base viol	base viol	base viol

Notes: All regressions control block fixed effects. Husband earnings are in 1000 Birr's. Robust SE in parentheses.

Table A17: Identity and IPV. Controlling for baseline violence.

	(1)	(2)	(3)
	Abuse	Abuse	Abuse
Share of earnings from wage job	0.0049 (0.096)	-0.016 (0.099)	-0.15 (0.15)
She earns more than him	0.069 (0.20)	0.15 (0.20)	0.74* (0.41)
She earns more*Share of earnings	-0.025 (0.23)	-0.015 (0.22)	-0.51 (0.44)
She earns more*Treatment		-0.11* (0.063)	-0.93** (0.45)
Treatment		0.019 (0.026)	-0.0023 (0.032)
She more*Share*Treatment			0.66 (0.51)
Share of earnings*Treatment			0.21 (0.20)
Abuse last 3 months (B)	0.21*** (0.038)	0.21*** (0.038)	0.21*** (0.038)
Mean dep. var in C group	0.13	0.13	0.13
No. of observations	930	930	930
R-squared	0.13	0.13	0.14
Controls	base viol	base viol	base viol

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

A.1 Comparison with DHS data and heterogeneity across areas

Using DHS data for the years 2000, 2005, 2011, and 2016 we show descriptive statistics in all years and in 2016 separately in Table A18. Questions on experience with IPV were only included in 2016 but we can see that attitudes have changed considerably to not accept IPV and female wage employment has remained relatively stable.

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 (see Figure 2b). In Table A19 we compare the factory areas included in our analysis to the rest of the data from Ethiopia in the DHS 2016. We see that the rates of IPV are similar but that acceptance is lower and employment higher in the factory areas. Comparing our baseline data in Table 1 with the factory areas in the DHS we see that the numbers are similar.

We can also investigate the variation across the different areas. As seen in Table A20, there is some variation across our study areas with respect to important variables such as levels of abuse, employment, divorce rates, and acceptance of abuse. As pre-registered, we will later use the variation across areas to investigate if there are different effects of job offers in areas with high and low values on these different macro-level characteristics.

Using the DHS data we investigate whether the effect of job offers varies across our areas. To link the results more closely to factors we think are important we spatially merge our factory area buffers with data from the DHS surveys and aggregate a set of macro level factors for different areas. We focus on divorce rates, levels of abuse, female employment, and acceptance of abuse in the areas. In Table A21 we show results from models where we interact Treatment with having above median values on the variable of interest from the DHS data. In column 1 we see that the effect of job offers is negative in areas with low divorce rates (statistically significant at the 10 percent level), and statistically

Table A18: Descriptive statistics DHS

	All years			2016		
	(1)			(2)		
	Mean	SD	N	Mean	SD	N
Abuse	0.234	(0.424)	4727	0.234	(0.424)	4727
Abuse last year	0.159	(0.366)	4720	0.159	(0.366)	4720
Justified: goes out	0.471	(0.499)	61002	0.374	(0.484)	15533
Justified: neglects ch	0.527	(0.499)	61059	0.416	(0.493)	15552
Justified: argues	0.467	(0.499)	60876	0.367	(0.482)	15546
Justified: refuses sex	0.387	(0.487)	59638	0.317	(0.465)	15415
Justified: burns food	0.455	(0.498)	61082	0.330	(0.470)	15556
Any formal wage job (ever)	0.229	(0.420)	61576	0.233	(0.423)	15683
Divorced	0.055	(0.228)	61635	0.056	(0.230)	15683
<i>N</i>	61635			15683		

Notes: Data from the DHS surveys for the years 2000, 2005, 2011, and 2016.

Table A19: Descriptive statistics from the DHS in 2016 for our areas and for the rest of Ethiopia

	Factory areas			Other areas		
	(1)			(2)		
	Mean	SD	N	Mean	SD	N
Abuse	0.249	(0.433)	1193	0.229	(0.421)	3534
Abuse last year	0.161	(0.368)	1189	0.159	(0.365)	3531
Justified: goes out	0.242	(0.428)	4904	0.435	(0.496)	10629
Justified: neglects ch	0.277	(0.448)	4910	0.480	(0.500)	10642
Justified: argues	0.219	(0.414)	4907	0.435	(0.496)	10639
Justified: refuses sex	0.187	(0.390)	4882	0.378	(0.485)	10533
Justified: burns food	0.193	(0.395)	4908	0.393	(0.489)	10648
Any formal wage job (ever)	0.369	(0.483)	4935	0.171	(0.377)	10748
Divorced	0.064	(0.245)	4935	0.052	(0.223)	10748
<i>N</i>	4935			10748		

Notes: Data from the DHS surveys for the year 2016. The factory areas are all DHS points that are located within 50 kilometer from our factory areas. The other areas are all other areas in the 2016 Ethiopia DHS.

Table A20: Descriptive statistics from the DHS across the different factory areas in 2016

	Dire Dawa		Eastern		Hawassa		Kombolcha		Mekelle	
	(1)		(2)		(3)		(4)		(5)	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Abuse	0.277	(0.448)	0.226	(0.419)	0.302	(0.461)	0.111	(0.317)	0.190	(0.394)
Abuse last year	0.202	(0.402)	0.111	(0.315)	0.217	(0.414)	0.093	(0.293)	0.083	(0.276)
Justified: goes out	0.291	(0.455)	0.120	(0.325)	0.466	(0.500)	0.409	(0.493)	0.316	(0.466)
Justified: neglects ch	0.285	(0.451)	0.170	(0.376)	0.503	(0.501)	0.434	(0.497)	0.492	(0.501)
Justified: argues	0.249	(0.433)	0.100	(0.300)	0.430	(0.496)	0.321	(0.468)	0.415	(0.493)
Justified: refuses sex	0.253	(0.435)	0.057	(0.232)	0.377	(0.485)	0.314	(0.466)	0.263	(0.441)
Justified: burns food	0.228	(0.420)	0.062	(0.242)	0.468	(0.500)	0.352	(0.479)	0.345	(0.476)
Any formal wage job (ever)	0.338	(0.473)	0.467	(0.499)	0.219	(0.414)	0.195	(0.397)	0.254	(0.436)
Divorced	0.069	(0.253)	0.060	(0.237)	0.017	(0.129)	0.107	(0.310)	0.086	(0.281)
<i>N</i>	2039		1963		356		159		418	

Notes: Data from the DHS surveys for the year 2016. The 5 different areas consist of the DHS points that are located within 50 kilometer from the factory cluster in the area.

significantly more positive in areas with higher divorce rates. This is surprising to us as we would expect the opposite. We also see that job offers have a statistically significantly more negative effect on IPV in areas where there is more IPV. We see some support for the effect being negative in areas with relatively less acceptance of abuse (we have here taken the share of individuals agreeing that IPV is justified in at least one of the situations) but the coefficient is only statistically significant at the 10 percent level. We find no statistically significant difference between areas with high or low female employment and, if anything, the effect is more positive in areas where many women work, in contrast to the pioneering hypothesis.

There are many other differences across areas and the heterogeneity results by no means show a causal effect of the moderators. In Table A22 we standardize the variables to have mean zero and a standard deviation of 1 and run the same type of regressions. We see that none of the interaction terms is statistically significant when we include them all in the same regression. This analysis was not pre-specified.

Table A21: Heterogeneity across areas above and below DHS 2016 characteristics

	(1)	(2)	(3)	(4)
	Abuse	Abuse	Abuse	Abuse
Treatment	-0.061*	0.028	-0.044	-0.073*
	(0.032)	(0.026)	(0.030)	(0.044)
<i>Above median values of ... in the area</i>				
Divorce	0.039			
	(0.14)			
Abuse		-0.046		
		(0.10)		
Employment			0.052	
			(0.10)	
Acceptance				0.14
				(0.093)
*Treatment	0.089**			
	(0.040)			
*Treatment		-0.078**		
		(0.040)		
*Treatment			0.063	
			(0.040)	
*Treatment				0.080
				(0.049)
Mean dep. var in C group in below median areas	0.16	0.11	0.16	0.17
No. of observations	1262	1262	1262	1262
R-squared	0.07	0.06	0.06	0.06
Controls	Block	Block	Block	Block

Notes: All regressions control block fixed effects. Robust SE in parentheses. The data on contextual variables comes from the DHS survey for the year 2016 and are based on averages for all individuals within 50 kilometer from the factory cluster in the area. We here further create dummy variables for whether the average is above the median or not of all the factory areas.

Table A22: Heterogeneity across areas with standardized DHS 2016 characteristics

	(1)	(2)	(3)	(4)	(5)
	Abuse	Abuse	Abuse	Abuse	Abuse
Treatment	-0.012 (0.020)	-0.013 (0.020)	-0.012 (0.020)	-0.012 (0.020)	-0.014 (0.020)
<i>Standardized values of ... in the area</i>					
Divorce	-0.13 (0.14)				-0.023 (0.075)
Abuse		-0.49 (0.62)			-0.011 (0.10)
Employment			0.030 (0.055)		0.037 (0.083)
Acceptance				-0.42 (0.49)	0.018 (0.033)
*Treatment	0.032 (0.021)				0.060 (0.060)
Treatment		-0.039 (0.020)			0.059 (0.11)
*Treatment			0.032 (0.020)		0.032 (0.068)
Treatment				0.038 (0.021)	0.051 (0.032)
Mean dep. var in C group	0.13	0.13	0.13	0.13	0.13
No. of observations	1262	1262	1262	1262	1262
R-squared	0.06	0.06	0.06	0.06	0.07
Controls	Block	Block	Block	Block	Block

Notes: All regressions control block fixed effects. Robust SE in parentheses. The data on contextual variables comes from the DHS survey for the year 2016 and are based on averages for all individuals within 50 kilometers from the factory cluster in the area. All contextual level variables are standardized to have mean zero and standard deviation 1.

We also pre-specified that we would conduct analyses separately in each of the 5 regions and run a separate analysis in where we drop the region with the weakest first stage. These results are presented in in Table A23 and A24. We note that there is no statistically significant effect on abuse in any of the specifications.

Table A23: Heterogeneity across areas in effects on "Any formal employment"

	(1)	(2)	(3)	(4)	(5)
	Amhara	Dire Dawa	Oromia	SNNP	Tigray
Treatment	0.89*** (0.051)	0.41*** (0.060)	-0.27* (0.15)	0.75*** (0.042)	0.22*** (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: All regressions control block fixed effects. Robust SE in parentheses.

Table A24: Heterogeneity across areas in effects on Abuse

	(1)	(2)	(3)	(4)	(5)	(6)
	Amhara	Dire Dawa	Oromia	SNNP	Tigray	Excl. Oromia
Treatment	0.032 (0.083)	-0.061 (0.047)	-0.20 (0.14)	-0.049 (0.046)	0.028 (0.026)	-0.0063 (0.020)
Mean dep. var in C group	0.11	0.17	0.20	0.15	0.11	0.13
No. of observations	70	251	23	305	613	1239
R-squared	0.01	0.02	0.17	0.09	0.08	0.06
Controls	Block	Block	Block	Block	Block	Block

Notes: All regressions control block fixed effects. Robust SE in parentheses. In column 6 we exclude Oromia (the region with the weakest first stage).

A.2 Longer term results: 12 and 18 months follow-up

In this section we present results from medium term follow-up surveys. The results are commented on in the text.

Table A25: Attrition after 12 and 18 months.

	(1)	(2)	(3)	(4)
	12 months		18 months	
	Attrition	Attrition	Attrition	Attrition
Treatment	-0.020 (0.021)	-0.018 (0.021)	-0.0075 (0.023)	-0.0036 (0.023)
Any formal wage job (ever)		0.0040 (0.024)		0.0052 (0.026)
Age		-0.010*** (0.0022)		-0.0090*** (0.0025)
Justified: goes out		0.020 (0.030)		0.061* (0.034)
Justified: neglects ch		0.030 (0.031)		0.060* (0.033)
Justified: refuses sex		0.016 (0.037)		0.025 (0.042)
Justified: argues		-0.0024 (0.033)		-0.0035 (0.038)
Justified: burns food		0.0019 (0.036)		-0.017 (0.041)
Abuse last 3 months		0.026 (0.028)		0.012 (0.031)
Muslim		-0.020 (0.041)		-0.046 (0.045)
Protestant		-0.060 (0.049)		-0.061 (0.054)
Medium education		-0.016 (0.037)		0.0037 (0.040)
High education		0.022 (0.041)		0.035 (0.044)
Father beat mother		-0.020 (0.022)		-0.028 (0.025)
Mean dep. var in C group	0.21	0.21	0.27	0.27
No. of observations	1463	1463	1463	1463
R-squared	0.07	0.09	0.07	0.09
Controls	Block	Full	Block	Full

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

Table A26: First stages after 12 and 18 months: Effects of treatment on employment and earnings.

(a) First stages after 12 months.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Any wage job	Any factory job	Earnings from wage job	Share	Earnings	Share	Income	Share	She earns more
Treatment	0.24*** (0.028)	0.26*** (0.026)	1661.1*** (200.2)	0.15*** (0.027)	1219.0*** (233.4)	0.063*** (0.023)	967.4*** (266.0)	0.048*** (0.019)	0.10*** (0.031)
Mean dep. var in C group	0.33	0.18	1841.30	0.30	2862.79	0.36	3664.87	0.25	0.26
No. of observations	1174	1174	1174	866	1174	1161	1174	1141	866
R-squared	0.19	0.25	0.18	0.21	0.10	0.18	0.07	0.15	0.17
Controls	Block	Block	Block	Block	Block	Block	Block	Block	Block

(b) First stages after 18 months.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Any wage job	Any factory job	Earnings from wage job	Share	Earnings	Share	Income	Share	She earns more
Treatment	0.17*** (0.031)	0.23*** (0.027)	1392.1*** (250.7)	0.097*** (0.029)	1047.1*** (302.7)	0.016 (0.025)	948.5*** (347.4)	0.048*** (0.021)	0.057* (0.032)
Mean dep. var in C group	0.36	0.19	2437.13	0.35	3689.51	0.41	4545.74	0.28	0.30
No. of observations	1074	1074	1074	794	1074	1066	1074	1053	794
R-squared	0.15	0.25	0.14	0.20	0.09	0.18	0.08	0.17	0.19
Controls	Block	Block	Block	Block	Block	Block	Block	Block	Block

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

Table A27: Reduced form estimates after 12 and 18 months. The effects of treatment assignment on various forms of violence.

(a) Reduced form estimates after 12 months.												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Abuse	Abuse	Less severe	Less severe	Severe	Severe	Sexual	Sexual	Emotional	Emotional	Controlling	Controlling
Treatment	-0.0017 (0.017)	-0.00028 (0.017)	0.0053 (0.016)	0.0083 (0.016)	0.0018 (0.0033)	0.0021 (0.0037)	-0.0089 (0.0084)	-0.010 (0.0089)	0.0035 (0.020)	0.0071 (0.020)	0.018 (0.045)	0.014 (0.045)
Mean dep. var in C group	0.09	0.09	0.08	0.08	0.00	0.00	0.03	0.03	0.12	0.12	0.24	0.24
No. of observations	1174	1174	1174	1174	1174	1174	1174	1174	1174	1174	1174	1174
R-squared	0.07	0.11	0.06	0.11	0.02	0.04	0.07	0.08	0.09	0.13	0.05	0.08
Controls	Block	Full	Block	Full	Block	Full	Block	Full	Block	Full	Block	Full
(b) FReduced form estimates after 18 months.												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Abuse	Abuse	Less severe	Less severe	Severe	Severe	Sexual	Sexual	Emotional	Emotional	Controlling	Controlling
Treatment	-0.016 (0.018)	-0.014 (0.017)	-0.026 (0.016)	-0.024 (0.016)	0 (.)	0 (.)	0.011 (0.0083)	0.0097 (0.0083)	-0.047** (0.020)	-0.044** (0.020)	0.028 (0.036)	0.021 (0.036)
Mean dep. var in C group	0.10	0.10	0.10	0.10	0.00	0.00	0.01	0.01	0.14	0.14	0.16	0.16
No. of observations	1073	1073	1073	1073	1073	1073	1073	1073	1073	1073	1073	1073
R-squared	0.06	0.10	0.06	0.11	.	.	0.04	0.05	0.10	0.14	0.09	0.11
Controls	Block	Full	Block	Full	Block	Full	Block	Full	Block	Full	Block	Full
Notes: All regressions control block fixed effects. Robust SE in parentheses.												

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

A.3 Expenditures and IPV

To further investigate different theories on the motivations for abuse we move on to investigate the correlation between abuse and spending patterns. In particular, we want to investigate if abuse is used instrumentally by the husband to alter the wife's spending behavior. We start by investigating the effect of treatment on the spending of the women. Starting with the effects of job offers on various infrequent expenditures ("In the last six months, how much of the purchase of X was financed from your income(s)?"), we see in Panel a of Table A30 that treatment causes women to spend more on women's and children's schooling and health. Note that spending on men's school is zero for everyone so we include it in the tables for completeness (as a comparison, spending on women's school is zero for 95 percent of the respondents). In Panels b and c of Table A30, we investigate the effects on frequent expenditures ("In the last month...") and we see that treatment causes women to spend more on a mix of items that are more likely private (women's care items, transportation, and mobile) and more public for the household (rent, charcoal, fuel, toiletries, children's care items, social and religious expenditures, and food). To some extent these expenditure increases are matched by a similar reduction in the partner's spending on the same items (in particular clothing, rent, toiletries and care items) as we show in Tables A32a to A32c.

Moving over to the relationship between abuse and spending behavior we run regressions of abuse during the last three months on spending, controlling for follow-up and baseline earnings, baseline spending, and baseline values of abuse. As such we can see the relationship between changes in abuse and spending, but also if this effect differs by treated and control women. In Table A31 we see that, for the control group, increases in abuse are correlated with a reduction in her spending on men's clothing, health, and care, and on toiletries and fuel.²⁸ As such, we immediately see that changes in abuse are

²⁸Looking at men's spending in the same way we see that abuse is correlated with husbands in the control group spending less on women's clothing, toiletries, mobile, remittances, and men's care (see Tables A32 to A33).

Table A28: 12 and 18 Months Heterogeneity in reduced form effects by the empowerment index. Dependent variable is Abuse last 3 months.

	(1)	(2)	(3)	(4)
	12 months		18 months	
	Abuse	Abuse	Abuse	Abuse
Treatment	-0.013 (0.024)	-0.017 (0.023)	0.0028 (0.025)	-0.0012 (0.025)
Empowerment index	-0.0035 (0.034)	-0.025 (0.033)	0.036 (0.038)	0.015 (0.037)
Empowerment*Treatment	0.033 (0.046)	0.046 (0.044)	-0.048 (0.049)	-0.035 (0.048)
Mean dep. var in C group	0.09	0.09	0.10	0.10
No. of observations	1174	1174	1073	1073
R-squared	0.08	0.11	0.06	0.10
Controls	Block	Full	Block	Full

Notes: All regressions control block fixed effects. Robust SE in parentheses. The index for female empowerment is reverse coded so that higher values imply less empowerment. Note that female empowerment is measured at baseline.

Table A29: Effects when pooling waves 2-4 together. Dependent variable is Abuse last 3 months.

	(1)	(2)
	Abuse	Abuse
Treatment	-0.0094 (0.013)	-0.0093 (0.013)
Control mean	0.11	0.11
N	3509	3509
R-squared	0.04	0.04
Controls	Block	Block
Wave f.e.	No	Yes

Notes: All regressions control block fixed effects. SE clustered at the individual level in parentheses.

not correlated with her spending more on her partner, which weakens the interpretation of domestic violence being instrumental to extract resources. There is only weak evidence that the relationship between abuse and spending is different for treatment and control women. The interaction between treatment and abuse is only statistically significant in two out of 25 cases.²⁹

²⁹Neither do we find any effects if we investigate the threat of violence instead of actual abuse.

Table A30: Jobs and her spending

(a) Effects of job offer on her spending on infrequent expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
M clothing	W clothing	C clothing	M health	W health	C health	C school	M school	W school	
Treatment	2.34 (4.02)	63.4*** (19.0)	27.0*** (9.11)	-1.58 (1.46)	14.6 (10.6)	7.87** (3.70)	8.32 (16.5)	0 (.)	37.7** (16.8)
Mean dep. var in C group	4.59	123.19	26.42	2.29	7.81	4.17	33.60	0.00	38.29
No. of observations	1256	1262	1262	1261	1262	1262	1262	1261	1262
R-squared	0.11	0.12	0.12	0.04	0.02	0.07	0.04	.	0.07
Controls	Block	Block	Block	Block	Block	Block	Block	Block	Block

(b) Effects of job offer on her spending on frequent expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rent	Water	Electricity	Charcoal	Fuel	Toiletries	M care	W care	
Treatment	21.7*** (8.02)	1.69 (1.42)	0.24 (1.19)	10.4** (4.90)	1.86** (0.87)	10.7*** (2.54)	-0.56 (0.59)	9.80*** (3.41)
Mean dep. var in C group	37.03	9.34	4.93	36.13	1.59	20.92	0.96	28.58
No. of observations	1262	1261	1262	1261	1262	1259	1214	1260
R-squared	0.09	0.19	0.10	0.08	0.08	0.10	0.02	0.15
Controls	Block	Block	Block	Block	Block	Block	Block	Block

(c) Effects of job offer on her spending on frequent expenditures continued.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
C care	Recreation	Social	Transportation	Mobile	Remittance	Food	Tobacco and alco	
Treatment	3.24** (1.51)	0.79 (1.12)	9.56* (5.27)	8.43** (4.15)	5.31*** (1.65)	-8.04 (6.11)	51.1*** (15.7)	-0.52 (1.87)
Mean dep. var in C group	4.80	2.68	17.71	19.39	14.96	20.00	152.15	3.12
No. of observations	1262	1262	1262	1261	1258	1262	1262	1241
R-squared	0.08	0.13	0.05	0.16	0.11	0.05	0.13	0.07
Controls	Block	Block	Block	Block	Block	Block	Block	Block

Notes: In the column titles, M is short for Men's, W for Women's, and C for Children's. All regressions control block fixed effects and baseline values of the outcome variable. Robust SE in parentheses.

Table A31: Abuse and her spending

(a) Abuse and her spending on infrequent expenditures

	(1) M clothing	(2) W clothing	(3) C clothing	(4) M health	(5) W health	(6) C health	(7) C school	(8) M school	(9) W school
Treatment	-1.83 (4.78)	22.4 (22.1)	7.89 (10.7)	-3.40 (2.91)	12.9 (12.5)	3.31 (3.56)	-2.47 (18.1)	0 (.)	26.7 (19.3)
Abuse last 3 months	-9.81** (4.96)	-2.11 (36.8)	-2.20 (15.1)	-3.48* (1.91)	1.83 (8.03)	-6.08 (4.50)	29.6 (27.2)	0 (.)	-3.76 (23.9)
Treatment*Abuse	-5.02 (12.4)	-14.1 (54.2)	38.4 (31.4)	2.67 (2.25)	-12.7 (16.9)	19.9* (11.6)	-70.0** (34.4)	0 (.)	9.16 (53.8)
Mean dep. var in C group	4.64	122.85	26.39	2.32	7.81	4.15	33.97	0.00	38.71
No. of observations	1239	1245	1245	1244	1245	1245	1245	1244	1245
R-squared	0.12	0.14	0.13	0.05	0.02	0.08	0.06	.	0.07

(b) Abuse and her spending on frequent expenditures

	(1) Rent	(2) Water	(3) Electricity	(4) Charcoal	(5) Fuel	(6) Toiletries	(7) M care	(8) W care
Treatment	-2.93 (8.21)	-0.034 (1.59)	-2.20 (1.61)	1.18 (5.49)	0.78 (1.05)	3.88 (3.07)	-0.61 (0.67)	-1.07 (4.01)
Abuse last 3 months	9.82 (19.1)	-1.35 (3.23)	-0.14 (2.03)	0.086 (9.98)	-1.68* (0.99)	-6.88* (4.17)	-1.33* (0.70)	-8.52* (5.00)
Treatment*Abuse	39.2 (31.2)	0.14 (3.67)	2.39 (2.86)	6.47 (15.9)	3.09 (2.25)	5.47 (6.91)	0.50 (0.74)	16.6** (8.31)
Mean dep. var in C group	36.26	9.37	4.98	35.89	1.61	20.81	0.97	28.49
No. of observations	1245	1244	1245	1244	1245	1242	1198	1243
R-squared	0.16	0.21	0.13	0.11	0.09	0.15	0.02	0.21
Controls								

(c) Abuse and her spending on frequent expenditures continued

	(1) C care	(2) Recreation	(3) Social	(4) Transportation	(5) Mobile	(6) Remittance	(7) Food	(8) Tobacco and alco
Treatment	1.46 (1.78)	0.44 (1.09)	1.81 (4.23)	3.87 (4.87)	0.21 (1.91)	-12.3* (6.30)	-1.56 (19.3)	-0.73 (2.50)
Abuse last 3 months	1.02 (2.22)	-1.72 (2.49)	5.63 (8.89)	-0.52 (7.16)	-5.81** (2.59)	-19.1** (8.84)	25.5 (34.7)	-4.57 (3.04)
Treatment*Abuse	1.56 (4.14)	1.76 (2.98)	-22.2* (12.8)	-4.96 (10.5)	3.84 (3.69)	13.4 (10.2)	1.00 (47.5)	4.35 (3.75)
Mean dep. var in C group	4.80	2.71	17.83	19.30	14.87	19.98	152.15	3.16
No. of observations	1245	1245	1245	1244	1241	1245	1245	1224
R-squared	0.10	0.14	0.08	0.18	0.19	0.06	0.22	0.08
Controls								

Notes: In the column titles, M is short for Men's, W for Women's, and C for Children's. All regressions control block fixed effects, follow-up and baseline earnings, baseline values of the outcome variable, and baseline values of abuse. Robust SE in parentheses.

Table A32: Jobs and his spending

(a) Effects of job offer on his spending on infrequent expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
M clothing	W clothing	C clothing	M health	W health	C health	C school	M school	W school	
Treatment	3.95 (38.4)	-88.9** (39.7)	-42.8* (22.7)	-21.2 (15.7)	-11.8 (17.6)	8.60 (9.10)	-24.2 (40.7)	-25.0 (24.3)	-14.5 (15.3)
Mean dep. var in C group	453.28	374.75	299.05	53.30	62.07	28.57	350.63	82.04	69.02
No. of observations	1256	1262	1262	1261	1262	1262	1262	1260	1262
R-squared	0.13	0.11	0.26	0.07	0.05	0.07	0.29	0.08	0.14
Controls	Block	Block	Block	Block	Block	Block	Block	Block	Block

(b) Effects of job offer on his spending on frequent expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rent	Water	Electricity	Charcoal	Fuel	Toiletries	M care	W care	
Treatment	-19.6 (17.9)	0.71 (2.73)	-2.12 (2.27)	-10.5 (6.69)	0.014 (1.32)	-9.69*** (3.66)	-5.54** (2.44)	-8.22** (3.32)
Mean dep. var in C group	308.45	19.42	17.01	110.79	4.14	57.50	33.33	37.76
No. of observations	1262	1261	1262	1261	1261	1259	1181	1260
R-squared	0.40	0.16	0.16	0.18	0.14	0.18	0.12	0.18
Controls	Block	Block	Block	Block	Block	Block	Block	Block

(c) Effects of job offer on his spending on frequent expenditures continued.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
C care	Recreation	Social	Transportation	Mobile	Remittance	Food	Tobacco and alco	
Treatment	-2.90 (2.56)	-4.31 (6.37)	-13.6 (25.1)	5.30 (8.06)	-1.87 (6.04)	4.72 (12.3)	-42.9 (32.6)	-8.43 (11.1)
Mean dep. var in C group	17.79	29.79	97.68	73.69	84.69	33.90	909.48	83.90
No. of observations	1262	1262	1262	1261	1250	1262	1262	1235
R-squared	0.13	0.14	0.05	0.06	0.24	0.10	0.29	0.20
Controls	Block	Block	Block	Block	Block	Block	Block	Block

Notes: In the column titles, M is short for Men's, W for Women's, and C for Children's. All regressions control block fixed effects and baseline values of the outcome variable. Robust SE in parentheses.

Table A33: Abuse and his spending

(a) Abuse and his spending on infrequent expenditures

	(1) M clothing	(2) W clothing	(3) C clothing	(4) M health	(5) W health	(6) C health	(7) C school	(8) M school	(9) W school
Treatment	-5.54 (45.3)	-79.1* (45.4)	-31.9 (26.1)	-39.3** (16.6)	-9.15 (16.8)	5.61 (11.7)	0.47 (46.6)	-14.5 (25.8)	-7.15 (17.1)
Abuse last 3 months	-20.6 (70.1)	-117.0** (59.6)	-37.2 (39.0)	-20.9 (20.8)	47.7 (42.4)	-10.4 (8.74)	60.8 (73.4)	-5.80 (47.4)	4.69 (32.1)
Treatment*Abuse	42.5 (97.4)	58.1 (85.8)	25.7 (58.5)	44.5 (31.7)	0.21 (67.4)	57.6** (29.2)	-64.8 (103.0)	-25.9 (60.4)	23.7 (47.3)
Mean dep. var in C group	456.00	376.84	301.58	53.24	62.52	27.97	353.86	80.58	69.78
No. of observations	1239	1245	1245	1244	1245	1245	1245	1243	1245
R-squared	0.13	0.11	0.27	0.07	0.05	0.08	0.30	0.09	0.15

(b) Abuse and his spending on frequent expenditures

	(1) Rent	(2) Water	(3) Electricity	(4) Charcoal	(5) Fuel	(6) Toiletries	(7) M care	(8) W care
Treatment	-7.69 (20.8)	3.13 (3.22)	-0.62 (2.47)	0.50 (7.74)	1.04 (1.45)	-2.08 (4.26)	-7.32** (2.85)	-2.48 (3.90)
Abuse last 3 months	-32.1 (31.0)	-0.51 (4.47)	-2.10 (3.19)	0.87 (13.7)	0.34 (2.22)	-11.9** (5.72)	-9.36** (3.78)	-1.88 (6.64)
Treatment*Abuse	8.98 (49.3)	0.75 (5.97)	-1.55 (4.53)	-4.06 (20.0)	1.28 (4.54)	0.97 (8.23)	4.66 (5.71)	-1.14 (8.21)
Mean dep. var in C group	310.35	19.54	17.12	111.54	4.19	57.96	33.41	38.10
No. of observations	1245	1244	1245	1244	1244	1242	1165	1243
R-squared	0.41	0.16	0.17	0.19	0.15	0.22	0.13	0.20
Controls								

(c) Abuse and his spending on frequent expenditures continued

	(1) C care	(2) Recreation	(3) Social	(4) Transportation	(5) Mobile	(6) Remittance	(7) Food	(8) Tobacco and alco
Treatment	-1.64 (2.67)	-10.6 (9.25)	-6.06 (29.4)	9.64 (10.7)	2.43 (7.97)	6.68 (14.8)	-11.2 (36.2)	-6.76 (12.8)
Abuse last 3 months	-3.76 (3.51)	-5.82 (11.3)	-42.9 (27.6)	-16.0 (14.3)	-16.9* (9.18)	-22.9* (13.0)	-90.1 (75.6)	48.0 (38.8)
Treatment*Abuse	0.74 (6.46)	13.8 (18.9)	18.3 (40.9)	6.12 (19.3)	6.44 (15.1)	13.8 (17.7)	-10.6 (95.3)	-32.3 (40.7)
Mean dep. var in C group	17.94	30.12	98.68	74.19	85.19	34.28	912.57	84.61
No. of observations	1245	1245	1245	1244	1233	1245	1245	1218
R-squared	0.14	0.14	0.05	0.06	0.25	0.10	0.30	0.21
Controls								

Notes: In the column titles, M is short for Men's, W for Women's, and C for Children's. All regressions control block fixed effects, follow-up and baseline earnings, baseline values of the outcome variable, and baseline values of abuse. Robust SE in parentheses.

A.4 Time use and IPV

There are other theories of abuse that may also be investigated in our setting. We present a set of auxiliary (and not pre-registered) results in Table A34.³⁰ A popular theory in criminology is exposure reduction (Dugan et al., 2003). At a most general level it is surely true that if the couple never meets (e.g. due to incarceration or migration) it is unlikely that he will abuse her. We believe that exposure reduction due to her getting a job is less likely to matter in our setting. We can investigate this to some extent by looking at travel time since that should be related to exposure. We see in column 1 that travel time is positively affected by treatment. We also see that it is positively correlated with abuse, however. One reason for travel time being positively correlated with abuse could be that abuse is more likely if women do less household work. We find that women do less household work if they are randomly assigned to a job offer (column 3). Household work is, however, not correlated with abuse. As such, it does not seem to be the case that abuse is used instrumentally in order to punish women for doing less household work. A possible reason for this could be that the husbands are not affected that much as they do not have to do more household work (column 5) and the household work is still being done, but by the eldest daughter (column 6).

³⁰In these regressions we control for the baseline values of the outcome variable and also baseline values of the main variable of interest when the outcome is abuse.

Table A34: Time use and IPV

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Travel time	Abuse	hh work	Abuse	husb. hh work	Abuse	Daughter hh work	Abuse
Treatment	0.65*** (0.23)		-1.68*** (0.64)		0.27 (0.31)		1.43** (0.69)	
Travel time		0.0051** (0.0025)						
Household work				0.00050 (0.00078)				
Husband hh work						-0.00021 (0.0022)		
Eldest daughter hh work								0.0013 (0.0023)
Mean dep. var in C group	4.65	0.13	28.04	0.13	1.96	0.13	3.45	0.12
No. of observations	1262	1262	1262	1262	963	963	387	387
R-squared	0.20	0.11	0.16	0.11	0.14	0.12	0.42	0.23
Controls	Block	Block	Block	Block	Block	Block	Block	Block

Notes: All regressions control block fixed effects and baseline values of the outcome variable. Columns 2, 4, 6, and 8 control for baseline values of the main outcome variable. Robust SE in parentheses.

Table A35: Additional time use variables

	(1)	(2)	(3)
Treatment	Hours worked 9.29*** (1.33)	Leisure -3.14*** (1.10)	Social and religious activities -0.55** (0.28)
Mean dep. var in C group	18.30	28.50	5.97
No. of observations	1262	1260	1262
R-squared	0.17	0.24	0.20
Controls	Block	Block	Block

Notes: All regressions control block fixed effects and baseline values of the outcome variable.
Robust SE in parentheses.

A.5 List experiment

Here we present the instructions for the list experiment and results for our second list experiment. The instructions in Figure A1 were read and explained to the participants in addition to them being told to read them on their own.

We have some questions about your life, and I will now explain how you should respond to the:

Now I'm going to read some statements about many different things. Some of these statements will be true and some will not. After I read all statements, please tell me HOW MANY of them are true for you.

And this is important: I don't want to know which ones, just how many.

Let's try with an example first. Suppose I read you 4 statements. After I read each statement, I want you to count with your fingers if it is true, and keep track **without showing to me**. I will then ask you how many are true. Let me demonstrate first.

NOTE: READ THE STATEMENTS BELOW AND SHOW THEM HOW YOU ARE COUNTING:

1. I went to the movies with my best friend on Sunday
2. I had dinner last night
3. I can speak English
4. Addis Ababa is the capital of Ethiopia

I will now read you the statements. As I'm reading, count with your fingers without showing me (**ENUMERATOR LOOK THE OTHER WAY**). At the end, you'll tell me how many are true for you.

1. You went to the movies with your best friend on Sunday
2. You had dinner last night
3. You can speak English
4. Addis Ababa is the capital of Ethiopia

How many of these statements are true?

Note that I do not know which of the statements that are true if you just give me the number. I only know how many. In this module, you will have several examples of this, remember to only tell us how many things are true so that we can not know which ones are true.

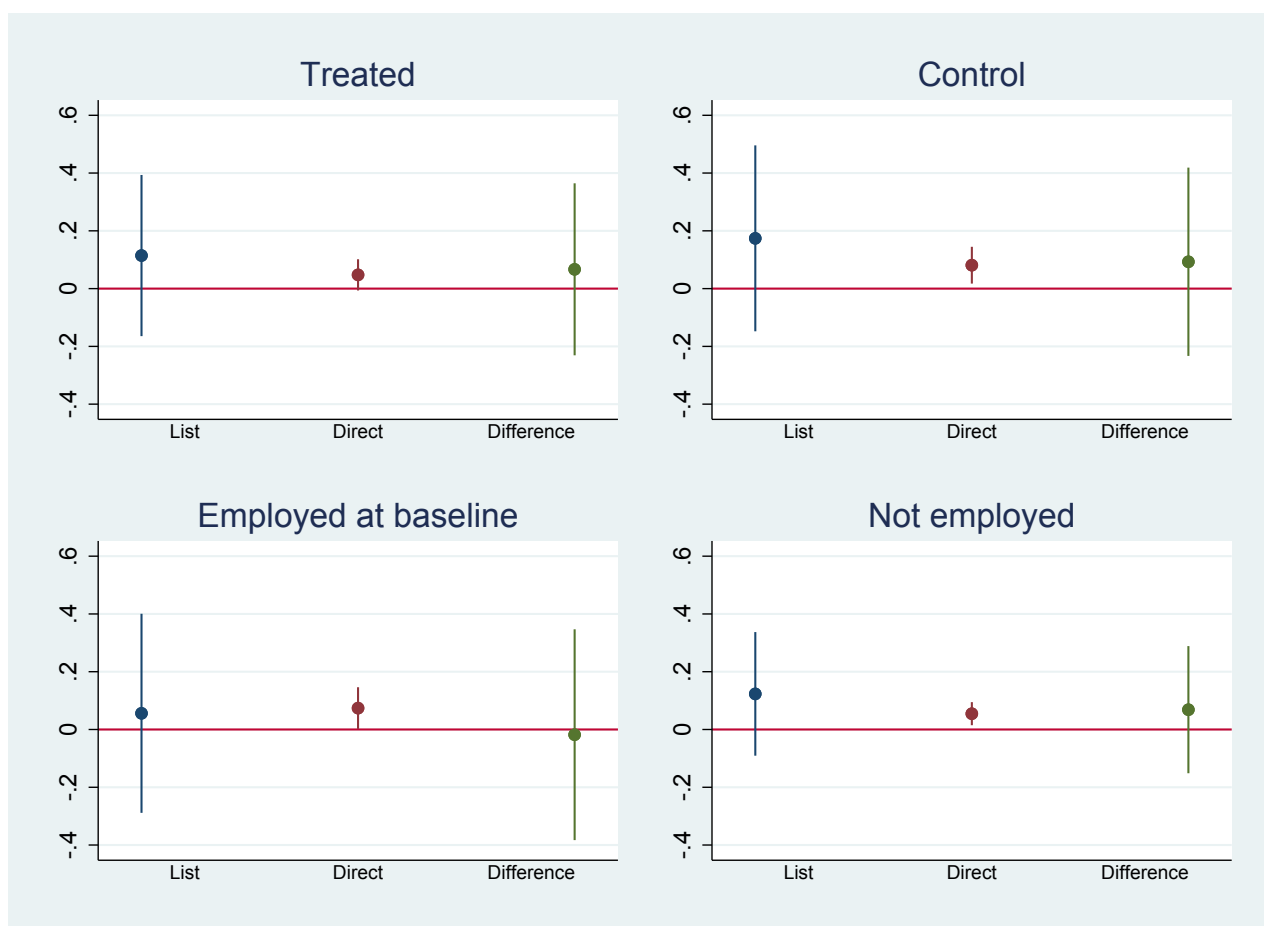
Figure A1: Instructions for the list experiment

LIST 3: ***DURING THE LAST 3 MONTHS YOU HAVE:***

1. Attended a religious service, except for a special occasion like a wedding or funeral
2. Been punched by your husband
3. Not travelled anywhere
4. Travelled to Addis
5. Had personal belongings such as money or a mobile phone stolen from you or from your house

Number of true statements |__|

Figure A2: List experiment: "Partner punched last 3 months"



Notes: Treated and control refers to the randomization of job offers in the field experiment. List refers to the estimated prevalence in the list experiment. Direct refers to the prevalence when using a direct survey question. Difference refers to the difference between asking in the list experiment minus asking directly. 95 percent confidence intervals are shown.

Figure A3: List experiment: "Partner punched last 3 months" by sub-groups

A.6 Full sample analysis

Table A36: Descriptive statistics for the full sample

	(1)	
	Mean	SD
<i>Physical abuse variables (Endline)</i>		
Abuse	0.291	(0.454)
Abuse last 3 months	0.131	(0.338)
Less severe	0.252	(0.434)
Less severe last 3 months	0.109	(0.312)
Severe	0.019	(0.137)
Severe last 3 months	0.003	(0.057)
Sexual	0.104	(0.306)
Sexual last 3 months	0.038	(0.192)
<i>Other outcome variables (Endline)</i>		
Emotional	0.396	(0.489)
Emotional last 3 months	0.176	(0.381)
Nr of control issues	0.991	(1.554)
Nr control last 3 months	0.313	(2.700)
Empowerment index	0.371	(0.319)
Nr empowerment items	4.447	(3.831)
Equality index	0.124	(0.134)
Nr equality items	1.237	(1.339)
<i>Employment and income variables (Endline)</i>		
Any wage job last 6 months	0.493	(0.500)
Earnings from wage job last 6 months	2290.810	(3779.633)
Share of earnings from wage job	0.334	(0.402)
Earnings last 6 months	2950.784	(4313.908)
Share of earnings	0.341	(0.389)
Income last 6 months	3610.956	(4858.260)
Share of income	0.227	(0.272)
She earns more than him	0.264	(0.441)
<i>Main baseline variables</i>		
Treatment	0.503	(0.500)
Abuse last 3 months	0.197	(0.398)
Any formal wage job (ever)	0.289	(0.454)
Age	24.967	(5.767)
Justified: goes out	0.300	(0.458)
Justified: neglects ch	0.351	(0.477)
Justified: refuses sex	0.166	(0.372)
Justified: argues	0.210	(0.407)
Justified: burns food	0.208	(0.406)
Muslim	0.120	(0.325)
Protestant	0.273	(0.446)
Medium education	0.506	(0.500)
High education	0.236	(0.425)
Father beat mother	0.363	(0.481)
N	1514	

Notes: All variables are measured at follow-up except for Treatment and the baseline controls.

Table A37: Reduced form estimates. The effects of treatment assignment on various forms of violence in the full sample.

	(1) Abuse	(2) Abuse	(3) Less severe	(4) Less severe	(5) Severe	(6) Severe	(7) Sexual	(8) Sexual	(9) Emotional	(10) Emotional	(11) Controlling	(12) Control
Treatment	-0.0031 (0.018)	0.00016 (0.018)	0.00021 (0.017)	0.0054 (0.016)	-0.0017 (0.0026)	-0.0023 (0.0026)	-0.0016 (0.0099)	-0.0013 (0.0100)	-0.044** (0.020)	-0.045** (0.020)	0.050 (0.053)	-0.0039 (0.057)
Mean dep. var in C group	0.13	0.13	0.11	0.11	0.01	0.01	0.04	0.04	0.19	0.19	0.22	0.22
No. of observations	1514	1514	1514	1514	1514	1514	1514	1514	1514	1514	1514	1514
R-squared	0.06	0.13	0.04	0.10	0.08	0.09	0.09	0.11	0.08	0.11	0.07	0.09
Controls	Block	Full	Block	Full	Block	Full	Block	Full	Block	Full	Block	Full

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