



Regular article

With or without him? Experimental evidence on cash grants and gender-sensitive trainings in Tunisia[☆]

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ABSTRACT

Is it possible to stimulate women's income-generating activities by relaxing their financial and human capital constraints? Does involving husbands help or hinder the effort? We examine these questions using a three-arm randomized-controlled trial with 2000 women in Tunisia. Women in the two treatment arms were offered a large cash grant (worth USD768 in PPP terms) and a gender-sensitive financial training. In one of the treatment arms, women were additionally encouraged to bring their male partner to the training. Two years after the program, we show that the treatments stimulated women's income-generating activities, but only when partners were not involved, and with no downstream effects on women's agency. Independently of partners' participation, impacts on household living standards were overwhelmingly positive, suggesting that the program was highly cost-effective. Overall, our results highlight the difficulty of stimulating women's agency in traditional societies, and suggest that involving men in women's empowerment programs can backfire.

Should husbands be involved in cash grant and training programs aiming at stimulating women's income generating activities (IGAs)?¹ Including or excluding husbands should have no impact on outcomes according to the unitary model of the household, which assumes that

households have only one set of preferences and that information flows without frictions within the household (Chiappori and Mazzocco, 2017). However, recent evidence shows that the unitary model is

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¹ In this paper, the terms *employment* and *income-generating activities* (IGAs) are used interchangeably and encompass both self-employment and wage-employment. Labor markets include markets for both self-employment and wage-employment.

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inconsistent with empirical observations. Spouses have different preferences and are willing to adopt costly strategies to hide their income, savings, or behavior from their partner or to control a greater share of resources (Anderson and Baland, 2002; Castilla and Walker, 2013; Schaner, 2015; Boltz et al., 2019; Castilla, 2019; Mani, 2020). Information does not flow seamlessly within the household (Nygqvist and Jayachandran, 2017; Apedo-Amah et al., 2020; Conlon et al., 2021; Rehman, 2023) and spouses behave differently when outcomes or behavior are hidden from their partner (Ashraf, 2009; Ashraf et al., 2014; Munro, 2018). Furthermore, spouses tend to under-contribute to public goods in experimental games (Hoel, 2015) and misallocate inputs within family farms (Udry, 1996), leading to inefficient outcomes.

In light of this evidence, excluding husbands from programs aiming at promoting women's IGAs may increase women's privacy and agency and lead to different outcomes, especially in contexts where women face a high risk of expropriation by other household members (De Mel et al., 2009; Jakiela and Ozier, 2016; Riley, 2020) and strong gender norms dictating the roles women can or cannot play (Field et al., 2010; Alesina et al., 2013; Bertrand et al., 2015; Delecourt and Fitzpatrick, 2021). On the other hand, involving men in carefully designed programs might strengthen women's agency if the intervention can rebalance power, information, or preferences within the household. Previous research in Côte d'Ivoire shows that women participating in gender dialogue groups were less likely to report economic abuse (Gupta et al., 2013). Bursztyn et al. (2020) also show that new information correcting beliefs on gender norms can empower women in labor markets.

We test these two opposite predictions using a randomized controlled trial (RCT) of a cash grant and gender-sensitive training program targeted at women in Tunisia. The RCT had two treatment arms. First, 1,000 women received an unrestricted cash grant worth USD 768 in PPP terms in 2018 (TND 634). This amount is large, about four times the median monthly income of respondents at baseline. These women also received a one-day financial training, which we label as *gender sensitive* because it included specific videos and exercises aiming to stimulate women's agency. While the cash grant was unrestricted, women were encouraged to invest the money in an IGA or in human capital to enhance their prospects in the labor markets.

The second treatment arm aimed at involving women's partners in the program to encourage gender dialogue and address some of the gender-specific barriers in the labor markets. A subset of cash grant recipients were encouraged to bring their male partners to the gender-sensitive financial training. The objective of the joint training was to actively engage male partners in the training program, in order to minimize resentment or backlash in response to women's empowerment and maximize impacts on women's IGAs.

We study the impacts of the treatments after two years. Results show that the cash grant and training program had positive impacts on women's likelihood of having an IGA (+3.3 p.p.) and on their income (+60%), but only for women who had to attend the training alone. The involvement of partners in the training appears to have backfired: compared to women who had to attend the training alone, women who could invite their male partner are significantly less likely to have an IGA,² they earn lower incomes, and they work fewer hours. We find no significant effects on an index of women's agency, which suggests that improvements in women's employment are not enough to empower them. While we find no significant effect on the IGAs of household heads, we find that other members in the households of treated women have more IGAs when the partners were not invited to the training. The cash grants and training program also had significant positive effects on participation in livestock farming (+4.7 p.p.) and small-scale agriculture (+2.5 p.p.), two occupations traditionally undertaken by

women at home. This latter result does not depend on male partners' participation in the program.

This evidence suggests that husbands' involvement in women's cash grant and training programs can affect how investments are made within households and be detrimental to the promotion of women's IGAs. At least two mechanisms could explain these results. First, men's involvement in the training may reduce women's privacy over the cash grant, giving them less agency to invest the grant in their own activities or that of other household members. Second, men involved in the training may have felt part of the program, giving them more legitimacy to influence how cash grants were spent. Given strong gender norms and the difficulty to hide income in this context, we conjecture that the second explanation is more plausible.

Beyond labor market outcomes, the impacts of the cash grant and training program were overwhelmingly positive, but without significant differences between the two treatment arms. The program had positive effects on household living standards, as measured by food consumption (+10.7%) and asset ownership (+0.13 SD). Women in the treatment groups were also more satisfied with their lives (+0.18 SD), and had better mental health (+0.07 SD), and better access to finance (+0.41 SD). Overall, the program was highly cost-effective: we estimate that its material benefits exceed program costs after only 1.2 years.

Our paper makes three main contributions to the academic literature. First, our finding that partners' involvement in the program backfired highlights the importance of considering intra-household dynamics and spousal information asymmetries (Baland and Ziparo, 2018). While most of the studies in this literature have been conducted in the context of lab or lab-in-the-field experiments (Munro, 2018), our paper exploits a large-scale RCT of a program providing cash grants and trainings in Tunisia. Our study complements the studies of Bulte et al. (2017, 2018) in Vietnam, which also evaluated a training program targeting women alone versus with their husband. Contrary to our study, their experiment did not include cash grants to relax financial constraints and, while their studies find that inviting male partners had little marginal impact, our experiment shows that involving men may backfire. Second, our study provides additional evidence that programs including large cash grants can have positive effects and be highly cost-effective, thereby complementing the literature on the impact of large one-off grants in developing economies (see e.g., Haushofer and Shapiro 2016, Crépon et al. 2020, Kondylis et al. 2021). Finally, and more generally, our study relates to the literature on the gender-specific constraints that women face, which limit their participation in labor markets (see Duflo 2012, Jayachandran 2021 for reviews). The limited and ambiguous results on women's IGAs call for further research on how to empower women in contexts where strong social norms shape women's opportunities and labor market outcomes (see e.g., Bertrand et al. 2015, Bursztyn et al. 2020, Field et al. 2021).

1. Context and interventions

Our experiment was implemented in Jendouba, one of the poorest governorates in Tunisia. The Jendouba governorate is administratively divided into 95 Imadas (localities), 15 of which are classified as urban, and 80 of which are classified as rural. Our RCT targeted 2,000 women in the 80 rural Imadas. These women were recruited from the treatment and control groups of another experiment, which evaluated the impact of a public-works program (The World Bank, 2017).³

The labor markets in rural Jendouba are highly segregated by gender. Female workers are typically working in agriculture as casual laborers (harvesting, weeding, sowing) (United Nations General Assembly, 2013; Zuccotti et al., 2018), or they are involved in small-scale kitchen gardens and livestock farming at home (Massin et al.,

² This result is observed for both wage- and self-employment.

³ See Appendix A for details on this prior program and its impact evaluation.

2016). By contrast, men tend to work in sectors requiring physical strength, such as construction work. Men are also in charge of specific tasks in agriculture, such as operating machines, transport, and portage. About 18% of households reported owning land at baseline and 49% of households were owning livestock (usually goats or chickens). About half of the women with an IGA at endline reported being self-employed (typically operating small shops or doing home production such as sewing), whereas the other half reported being wage employed or casual workers (mostly working in others' farms, with a few working as cleaning ladies).

Between September 2018 and January 2019, the 2,000 women of our study sample were randomly assigned to one of two treatment arms or to a control group. The design of the experiment is represented in Appendix Figure A.1.

Treatment 1: Cash Grant and Gender-Sensitive Training

One thousand women were offered a cash grant of TND 634 (USD 768 in PPP terms, USD 240 in nominal terms). This amount is relatively large: about four times the median monthly income of respondents with an IGA at baseline; and enough to purchase two goats.

The cash grants were unrestricted: the money could be spent on anything (Siu et al., 2023). Before receiving the cash grants, women had to participate in a gender-sensitive financial training which aimed at stimulating women's agency. The one-day training aimed at encouraging women to invest their money productively in physical capital (e.g., starting an IGA) or human capital (e.g., paying for vocational training of their choice). The training covered three main modules: i) financial planning and budgeting, ii) savings, and (iii) debt management. Each module included a series of videos, exercises, and guided discussions. Details about the training, including photos and links to the training material, are provided in Appendix A.

Women had to open a bank account or get a pre-paid ATM card to receive the cash grants (see Appendix A for more information). The unrestricted grants were delivered, in one lumpsum, to these accounts. The take-up of the cash grant intervention was very high (95%): 949 out of the 1,000 randomly selected women participated in the training sessions and received the cash grants.

Treatment 2: Joint Training with Male Partners

Half of the 1,000 women receiving cash grants (i.e., 500 women) were invited to bring their male partners to the financial training described above. Partners were asked to sit next to each other and do the exercises together. The six videos played during the training show two partners discussing, arguing about, and agreeing upon budget management and investments (see Appendix Figure A.3). The opinions of the women in the video were presented by the trainers as the best practice. Partners were invited to reflect together on the content of the videos and share their joint experience with the group.

The rationale for adding this component was to encourage gender dialogue and joint decision-making between the partners, in order to limit expropriation of grants by male partners and other household members, and mitigate any negative perceptions or resentments that male partners may have as a result of the cash grant program. Previous research by Gupta et al. (2013) in Côte d'Ivoire found that women participating in gender dialogue groups were less likely to report economic abuse.

The take-up rate of the joint training was also high (88%): 444 women attended the training with their partner and 34 women attended the training alone, out of 502 women randomly selected into this treatment group.⁴

⁴ We find no significant difference in the take-up of the cash grant intervention across the two treatment arms (p -value = 0.66).

2. Hypotheses

Our paper tests two series of hypotheses, which are based on our <https://doi.org/10.1257/rct.7136-1.1> [pre-analysis plan] (Gazeaud et al., 2021).

The first set of hypotheses is based on the naive assumption that the two treatment arms achieve their intended objectives. Previous research on cash grants to micro-entrepreneurs found positive impacts on a wide variety of business outcomes (De Mel et al., 2008; Fafchamps et al., 2014; McKenzie, 2017). Training programs also generate positive impacts, but effect sizes are relatively small (McKenzie, 2021). In light of this evidence, we hypothesize that the cash grant and training program relaxes women's financial and human capital constraints, and that this is sufficient to stimulate women's IGAs (H1.1) and enhance women's agency (H1.2). In line with the findings of Gupta et al. (2013), we also hypothesize that the joint training with partners is fostering gender dialogue and empowering women participants (H1.3) which should in turn stimulate their IGAs (H1.4).

The second series of hypotheses recognizes that relaxing financial and human capital constraints may be insufficient because women face gender-specific constraints, including high risk of expropriation by other household members (De Mel et al., 2009; Jakiela and Ozier, 2016; Riley, 2020). In contrast with Gupta et al. (2013), the joint training might reduce women's privacy and agency over the grant and affect how the money is invested, inverting H1.3 and H1.4. The cash grants to women could have impacts beyond recipient women and directly impact household IGAs and household well-being (H2.1). The effect of the cash grant and training program on the IGAs of the household head and other household members might depend on partners' participation in the training (H2.2). The program could also encourage migration if cash grants relax liquidity, credit, and risk constraints to migration (Gazeaud et al., 2023) (H2.3).

3. Empirical strategy

3.1. Data

Baseline data were collected between April 2016 and January 2017, which is 12 to 18 months before the distribution of cash grants. The endline survey was conducted between December 2020 and March 2021, which is between 2 and 2.5 years after the distribution of cash grants. Of the female participants, 91.2% were successfully surveyed at endline ($N=1,824$).⁵

The baseline and endline questionnaires included questions on household composition, economic activities, assets, consumption, women's agency, subjective well-being, mental health, migration, economic shocks, and coping strategies. Data collection was carried out in person and coordinated by a professional survey firm recruited by the World Bank.

3.2. Estimation of intent-to-treat effects

We estimate the intent-to-treat (ITT) effect of the cash grant and training program using a regression of the following form:

$$y_{i,t} = \beta_0 + \beta_1 T_i + \delta^T X_{i,t-1} + \mu_e + \varepsilon_{i,t} \quad (1)$$

where $y_{i,t}$ is the outcome of interest for unit i at endline t (where i is an individual or a household depending on the outcome); T_i is a dummy

⁵ At endline, we also sought to interview the male partners of the 1,000 women who received cash grant and training program. We successfully interviewed 83% of these ($N=830$) and use the data to construct one measure of women's agency. For male partners, we do not reject the hypothesis that attrition rates are equal in the two treatment groups (17.1% and 16.9% respectively).

indicating whether the unit i was randomly offered the cash grant and training program; $X_{i,t-1}$ is a vector of control variables measured at baseline; μ_e are enumerator fixed effects; and $\varepsilon_{i,t}$ is the disturbance term for the regression. The ITT effect of the cash grant and training program is given by the coefficient β_1 . The vector of control variables $X_{i,t-1}$ is selected using the double LASSO method of Chernozhukov et al. (2017).⁶

To test whether the cash grant and training program is more effective when partners are invited to the training, we estimate the following ITT specification:

$$y_{i,t} = \beta_0 + \beta_1 T_{i1} + \beta_2 T_{i2} + \delta^T X_{i,t-1} + \mu_e + \varepsilon_{i,t} \quad (2)$$

where T_{i1} is a dummy indicating whether the unit i was randomly offered the cash grant and training program without partners' involvement; T_{i2} is a dummy indicating whether the unit i was randomly offered the cash grant and the joint training with male partners; and $y_{i,t}$, $X_{i,t-1}$, μ_e and $\varepsilon_{i,t}$ are defined as above. The ITT effects of the cash grant and training program without and with partners are given by the coefficients β_1 and β_2 respectively. We then test $H_0: \beta_1 = \beta_2$ to study the significance of the marginal effect of joint participation into the training.

3.3. Outcomes

In the main analysis, we consider 37 outcomes of interest, which are classified into five categories.

1. IGAs: To study impacts on female participants, we consider dummy variables identifying whether they had an IGA at the time of the survey and before the COVID-19 pandemic, several measures of income and profit, the number of business employees, and hours worked. We also study various measures of involvement in agriculture and livestock ownership. To study impacts on other household members, we consider whether the household head and other household members have IGAs, and a measure of their income.
2. Women's empowerment and well-being: We consider two indices of women's agency (one using data from women and one using data from their male partners) and an index of women's access to finance. We also consider three subjective measures of current, past, and future life satisfaction, one measure of perceived relative wealth, and a general measure of mental health (the Mental Health Inventory-5, or MHI-5).
3. Household living standards: We study three aggregate measures of consumption (total, food, and non-food consumption), an index of asset ownership, and an index of asset purchases.
4. Shocks and coping strategies: We consider four variables, identifying whether households faced economic shocks or other shocks, and whether they used extreme coping strategies in response to these shocks.
5. Migration: We study a dummy equal to one if the household has moved to a different Imada, a dummy equal to one if at least one household member has migrated since 2019, and a dummy equal to one if at least one household member reported being likely or very likely to migrate to another city or governorate in the next 12 months.

The construction of these outcome variables is detailed in Appendix Table A.1. Descriptive statistics are shown in Appendix Table A.2.

⁶ We follow the approach outlined in Derksen et al. (2021) to select variables for the double LASSO procedure. We control for baseline levels of outcome variables when possible (McKenzie, 2012). As our impact evaluation builds on the sample of a previous experiment, we control for women's treatment status in that prior experiment in all regressions (see Appendix A for details on this prior cash-for-work program and its impact evaluation). Results are similar without control variables and enumerator fixed effects.

3.4. Internal validity

We discuss four threats to the identification of causal impacts: imbalances between the treatment and control groups, spillovers, attrition, and multiple testing.

Balance. The treatment and control groups are well balanced across a series of baseline demographic and economic characteristics (Appendix Table A.2). For all but one variable analyzed, the magnitude of the differences between the treatment and control groups is smaller than 0.1 standard deviations of the control group, which indicates good balance (Imbens and Rubin 2015, p. 310). Only two out of 75 differences are statistically significant at conventional thresholds. The p-values of omnibus F-tests of joint significance are above 0.90 for both treatment arms.

Spillovers. Important spillovers between the treatment and control groups are unlikely given the relatively small scale of the program—only 0.3% of the rural population of Jendouba received a cash grant.

Attrition. Attrition could be an issue in this study, especially since the endline survey took place 4 years after the baseline survey.

The overall attrition rate in our study is 8.8% (Appendix Table A.3), which is relatively low and on par with comparable studies in other developing country contexts. The attrition rate in the control group is 11.2%, which is significantly larger than the attrition rate of 6% and 6.8% observed in the two treatment groups. Differential attrition is mainly driven by higher migration rates in the control group. While the survey firm made its best efforts to track all households – even those that migrated within Tunisia – not all of them could be traced because some households had moved abroad, or because neighbors and community leaders did not know where the households had moved. The attrition differential is in itself an interesting result, which contrasts with the findings of Gazeaud et al. (2023) (more on this in Section 4). However, it could also undermine the internal validity of our analysis. We study whether the baseline characteristics of households are balanced across the control and the treatment groups after dropping attrited observations (Appendix Table A.4). Reassuringly, we find no evidence of serious imbalance, which suggests that the control group is a credible comparison group even if attrition is slightly higher in this group.

As a robustness check, we implement three methods that aim to address selection bias due to differential attrition.⁷ First, we use Inverse Probability Weighting (IPW), using baseline characteristics to estimate respondents' probability to participate in the endline survey, and then using the inverse of these probabilities as weight in the regressions. Second, we follow Lee (2009) and exclude 5% (48 observations) of the treatment group such as to fully close the attrition differential. Following Behaghel et al. (2015), we exclude the most-difficult-to-track respondents in the treatment group by exploiting the fact that respondents that are hard to track are typically interviewed much later than other respondents in the same village.⁸ Finally, we follow Kling and Liebman (2004) and estimate treatment effect bounds (KL bounds thereafter) by imputing to the attrited observations the mean outcome of their respective treatment arm plus or minus x SD. Following Baird et al. (2019), Özler et al. (2021), and Brune et al. (2022), we consider $x = 0.1$ and $x = 0.25$, and recognize that the latter assumption is considered as quite extreme (Blattman et al., 2020; Özler et al., 2021).

⁷ For these tests, we focus on T_i as we find no differential attrition between T_{i1} and T_{i2} .

⁸ Appendix Figure A.4 shows the distribution of interview dates relative to the median interview date in their Imadas. While 82% of interviews were done within 10 days of the median interview date, a series of interviews were done much later, typically because respondents were much more difficult to find and interview. For the treatment group, we trim 5% (48 observations) of the upper tail of this distribution in order to fully close the attrition differential.

Results are overall robust to using IPW, the trimming procedure of Behaghel et al. (2015), and the moderate KL bounds (± 0.1 SD). This suggests that attrition is unlikely to drive our conclusions (Appendix Table A.5). Some results, e.g. the estimated impacts on labor market outcomes and agriculture, become less salient with the more extreme KL bounds (± 0.25 SD).

Multiple testing. In the main analysis, we estimate 148 parameters of interest (37 outcomes and four t-tests per outcome). To address multiple testing concerns, we report sharpened q -values that control the false discovery rate (Anderson, 2008). Overall, p -values and q -values are similar, suggesting that our inferences are robust to corrections for multiple testing.

4. Results

We study impacts on IGAs in Table 1, on women's empowerment and well-being in Table 2, and on household living standards, shocks, and migration in Table 3. Results with disaggregated indices are shown in Appendix E.

IGAs. Results show that the cash grant and training program had a positive effect on female IGAs (H1.1), but only for women who could not invite their male partners to the training. Compared to the control group, women who received the cash grant and the training without their partner were 3.3 percentage points more likely to have an IGA at endline. This effect is larger – 3.9 percentage points – if we consider a dummy variable identifying women who had an IGA before the COVID-19 pandemic (in March 2020, i.e. 1.5 years after the intervention).

However, we find no effect on these outcomes if male partners are invited to the training. The gender dialogue intervention seems to have backfired: women who received the cash grant and the joint training with their partner were 4.1 percentage points less likely to have an IGA than women who could participate to the training alone (q -value = 0.050). The effect is larger – 5.7 percentage points – for IGAs before the COVID-19 pandemic (q -value = 0.017). These effects remain statistically significant if we consider wage- and self-employment separately.⁹ Compared to women who had to attend the training alone, women in the joint training treatment group have lower total income (q -value = 0.027) and work fewer hours on average (q -value = 0.037).

Overall, the impact of the cash grant and training program on women's IGAs seems limited and far from being transformative. In fact, one of the most salient results of Table 1 is the very low percentage of women having an IGA in the control group (7.5%).

The effect on the IGAs of household heads is positive but too small to be statistically significant at conventional levels, even in the joint training arm. However, we find evidence that the cash grants were used to promote the IGAs of other household members (H2.1). The cash grant and training program significantly increased the number of other household members with an IGA (+28%), especially when the partner was not involved in the program (+49%), which is consistent with H2.2. This suggests that women might be able to optimally invest in the activities of other household members, provided their partners are not involved in the training.¹⁰

The effect of participation in the cash grant and training program on total household income is positive, with no significant difference between the two treatment groups. While women's income is significantly higher when they are invited to participate in the training alone, the income of household heads is higher with the joint training (this latter effect is not statistically significant at conventional level). The two

effects partly balance each other out. This suggests that participating in the training alone versus the joint training generates some substitution effect between women's and men's IGAs, which we only imperfectly capture for men's IGAs (perhaps because the data on men's IGAs are collected from women).

These results suggest that women had more leeway to invest the grant in their own IGAs – be it wage- or self-employment – if their partner was not involved in the training. Men's involvement may have reduced women's privacy over the cash grant.¹¹ Men involved in the training sessions may have also felt more part of the program, giving them more legitimacy to influence how cash grants were spent. Due to data limitations, we are unable to test these mechanisms. However, given strong gender norms and the difficulty for women to hide income in this context, we conjecture that the second explanation is more plausible. This interpretation is in line with the model of Lowe and McKelway (2021), which assumes that intra-household decision rights are not clearly delineated, and that interventions that make household decision-making more joint can give “veto power” to the spouse. Our findings are also consistent with a model with information asymmetries in the couple, in which the household can either invest in an IGA with returns known to the men, or invest in the woman's IGA, but only her knows the expected returns. If the husband is involved in the training, he chooses to invest in the activity with known returns. If the husband is not involved, she selects the best investment opportunity. Formally testing the relevance of these mechanisms is a fruitful area for future research.

The effects of the cash grant and training program on agricultural and livestock farming – two activities traditionally done by women – are salient (H1.1).¹² Households that received the cash grant and training program are 2.5 percentage points (+40%) more likely to report agriculture as an IGA. While we find some evidence of change in agricultural practices, agricultural techniques remain traditional, with almost no use of fertilizers and pesticide. Agriculture is mostly small-scale: for households reporting an agricultural IGA, the average production in the 2019 agricultural season was 61 kg, and the value of the production was TND 192 (USD 219 in PPP terms). Households who own land have an average area of 0.85 hectares. The impacts on the quantity and value of agricultural production are positive and highly significant.

Households that received a cash grant are also 4.7 percentage points (+25%) more likely to report livestock farming as an IGA. Households in the treatment groups have more goats, and are significantly more likely to have purchased goats, chickens, and mules since January 2019 (Appendix Table A.7). The joint participation into the training had no significant marginal effect on agriculture and livestock farming.

Women's empowerment and well-being. We find no significant effect on an index of women's agency. Coefficients are negative but insignificant at conventional levels. We assess impacts on each of the 16 questions used to construct the women's agency index in Appendix Table A.8. A first set of 11 questions captures whether women are involved in decision-making related to household finances (e.g., expenditure on food, livestock, or land). A large majority of women report being involved in these decisions, even in the control group, suggesting that the margin for improvement is limited. The five other questions examine

⁹ In contrast to Bauchet et al. (2015) and Bandiera et al. (2017), we find no clear evidence of substitution effect between different types of IGAs for women in the two treatment groups.

¹⁰ We only collected data on hours worked for women participants.

¹¹ Another version of this mechanism is that women attending the training alone may have been able to describe the requirements of the cash grants to their partners in a way that would favor their activities (e.g., presenting the transfers as conditional on investments in their own business or in a female-operated business).

¹² We note that only 19% of households in the control group reported agriculture or livestock as an IGA. Many more households have a few animals or cultivate a small plot of land but do not consider their involvement in subsistence agriculture as an IGA. For example, 39% of households in the control group have goats and 53% of households have chickens.

Table 1
Treatment effects on income generating activities (IGAs).

	(1)	(2)	Eq. (1)	Eq. (2)			
	Control	Control	(3)	(4)	(5)	(6)	(7)
	mean	SD	Treatment	T1: Cash & Training alone	T2: Cash & Joint training	p-value T1 = T2	N
Women's IGAs							
Has an IGA [†]	0.075	0.264	0.013 (0.012) [0.340]	0.033** (0.016) [0.072]	−0.008 (0.013) [0.617]	0.017 [0.050]	1,824
Had an IGA before COVID	0.091	0.288	0.011 (0.013) [0.483]	0.039** (0.017) [0.059]	−0.018 (0.014) [0.285]	0.002 [0.017]	1,824
Business profit (in Dinars) [†]	9.505	66.500	0.358 (2.860) [0.748]	3.033 (3.905) [0.508]	−2.337 (2.985) [0.508]	0.175 [0.243]	1,824
Business employees [†]	0.010	0.271	0.003 (0.012) [0.731]	0.000 (0.011) [0.776]	0.005 (0.017) [0.716]	0.769 [0.716]	1,824
Total income (in Dinars) [†]	35.893	149.039	7.932 (6.866) [0.302]	21.637** (9.691) [0.059]	−5.885 (6.993) [0.483]	0.006 [0.027]	1,824
Total hours worked (last 7 days) [†]	2.785	10.130	−0.118 (0.433) [0.726]	0.659 (0.574) [0.302]	−0.841* (0.457) [0.106]	0.009 [0.037]	1,824
IGAs of other household members							
Household head has an IGA	0.341	0.474	0.025 (0.020) [0.285]	0.021 (0.025) [0.483]	0.029 (0.025) [0.302]	0.796 [0.726]	1,824
Number of other HH members with an IGA	0.140	0.400	0.039* (0.020) [0.095]	0.069** (0.028) [0.045]	0.008 (0.022) [0.716]	0.043 [0.082]	1,824
Household head income (in Dinars)	133.822	230.264	9.389 (10.103) [0.440]	3.771 (12.148) [0.716]	15.040 (12.447) [0.289]	0.422 [0.508]	1,824
Other HH members income (in Dinars)	64.950	198.728	6.671 (8.672) [0.508]	11.155 (10.818) [0.360]	2.133 (10.417) [0.731]	0.462 [0.536]	1,824
Agriculture							
Household has an agricultural IGA	0.062	0.241	0.025** (0.011) [0.059]	0.027* (0.014) [0.092]	0.022* (0.013) [0.127]	0.745 [0.716]	1,824
Used chemicals	0.018	0.156	0.021** (0.009) [0.059]	0.019* (0.011) [0.130]	0.023* (0.013) [0.100]	0.742 [0.716]	1,824
Quantity produced (in Kg)	1.666	20.371	6.077*** (1.859) [0.012]	7.115*** (2.639) [0.031]	5.032** (2.259) [0.059]	0.517 [0.582]	1,824
Value of the production (in Dinars)	8.736	76.197	12.383*** (4.604) [0.031]	15.454** (6.305) [0.045]	9.289* (5.402) [0.128]	0.398 [0.483]	1,824
Livestock							
HH has a livestock IGA [†]	0.187	0.390	0.047*** (0.016) [0.019]	0.047** (0.020) [0.050]	0.047** (0.019) [0.045]	0.989 [0.776]	1,824
Total stock (in Dinars) [†]	463.197	656.976	38.218 (29.484) [0.264]	38.743 (36.050) [0.340]	37.688 (36.017) [0.354]	0.980 [0.776]	1,824
Bought since January 2019 (in Dinars)	32.235	196.140	29.751*** (9.275) [0.013]	24.685** (10.868) [0.059]	34.855*** (11.775) [0.019]	0.435 [0.508]	1,824

Notes: This table reports the intent-to-treat effects of the interventions on women's IGAs, other household members' IGAs, agriculture, and livestock. Columns 1 and 2 report the mean and standard deviation of the outcome variables in the control group. Column 3 reports the effect of the cash grant and training program (β_1 in Eq. (1)). Columns 4 and 5 report the effect of the cash grant and training program with and without the male partner (β_1 and β_2 in Eq. (2)). Column 6 reports the p-value for the null hypothesis that the effect of the cash grant and training program is the same with and without the male partner ($\beta_1 = \beta_2$ in Eq. (2)). Outcomes denoted with [†] are available at baseline and used as control. Regressions also include enumerator fixed effects and a vector of control variables selected using the double LASSO method of Chernozhukov et al. (2017). As our impact evaluation builds on the sample of a previous experiment, we control for women's treatment status in that prior experiment in all regressions. Robust standard errors are reported in parentheses. FDR q-values are reported in brackets.

*** $p < 0.01$.

** $p < 0.05$.

* $p < 0.1$.

Table 2

Treatment effects on women's empowerment and well-being.

			Eq. (1)	Eq. (2)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Control	Control	Treatment	T1: Cash &	T2: Cash &	p-value	N
	mean	SD		Training	Joint training	T1 = T2	
				alone			
Women's empowerment							
Women's agency index	−0.000	1.000	−0.044 (0.038) [0.302]	−0.033 (0.046) [0.549]	−0.056 (0.046) [0.285]	0.646 [0.657]	1,824
Partners' views index	−0.001	0.999	.	.	−0.030 (0.054) [0.617]	.	830
Women's financial index	0.000	1.000	0.408*** (0.087) [0.001]	0.408*** (0.133) [0.018]	0.408*** (0.088) [0.001]	0.998 [0.776]	1,824
Women's wellbeing							
Cantrill's ladder							
Current life satisfaction	2.356	1.470	0.269*** (0.062) [0.001]	0.276*** (0.075) [0.007]	0.261*** (0.078) [0.011]	0.867 [0.746]	1,824
Life satisfaction one year ago	2.411	1.494	0.239*** (0.060) [0.003]	0.268*** (0.075) [0.008]	0.209*** (0.073) [0.023]	0.508 [0.576]	1,824
Predicted life satisfaction in three years	3.411	2.125	0.221*** (0.081) [0.029]	0.255*** (0.098) [0.037]	0.187* (0.099) [0.098]	0.544 [0.590]	1,824
Relative wealth	2.821	1.656	0.244*** (0.067) [0.007]	0.269*** (0.083) [0.012]	0.218*** (0.082) [0.033]	0.588 [0.622]	1,824
Mental health (MHI-5 score)	43.154	19.500	1.405* (0.783) [0.115]	2.119** (0.940) [0.059]	0.686 (0.952) [0.545]	0.178 [0.243]	1,824

Notes: This table reports the intent-to-treat effects of the interventions on women's empowerment and well-being. Columns 1 and 2 report the mean and standard deviation of the outcome variables in the control group. Column 3 reports the effect of the cash grant and training program (β_1 in Eq. (1)). Columns 4 and 5 report the effect of the cash grant and training program with and without the male partner (β_1 and β_2 in Eq. (2)). Column 6 reports the p-value for the null hypothesis that the effect of the cash grant and training program is the same with and without the male partner ($\beta_1 = \beta_2$ in Eq. (2)). *Women's agency index*: index constructed from the answers of the woman to 16 questions about her role in the decision-making of personal and household decisions (standardized average). *Partner views index*: index constructed from the answers of the partner to 73 questions on women's agency, spouses communication, and his perceptions of gender roles, women abilities for economic activities, gender-based violence, reproductive health, and women autonomy (standardized average). *Woman financial index*: standardized average of nine questions on woman financial access and financial situation. *Cantrill's ladder* ranges from 1 to 10 (where 10 is the top of the ladder). *Mental health*: the MHI-5 score is based on responses across five categories (happiness, peacefulness, nervousness, downheartedness, depression) with higher values indicating better mental health (0-100 point scale). Outcomes denoted with * are available at baseline and used as control. Regressions also include enumerator fixed effects and a vector of control variables selected using the double LASSO method of Chernozhukov et al. (2017). As our impact evaluation builds on the sample of a previous experiment, we control for women's treatment status in that prior experiment in all regressions. Robust standard errors are reported in parentheses. FDR q-values are reported in brackets.

*** $p < 0.01$.** $p < 0.05$.* $p < 0.1$.

whether women have agency to take personal decisions related to personal purchases, occupation, and social participation. A majority of women do not take these personal decisions alone, regardless of their treatment status. Estimated effects are insignificant for all but one of the 16 questions and the only significant coefficient is small and negative. Overall, these results suggest that hypotheses H1.2 and H1.3 are not verified: the interventions did not affect women's agency. Consequently, hypotheses H1.4 is irrelevant in our context. These results also suggest that the mechanism through which partners' involvement in the training backfired is not related to a persistent negative effect of the joint training on women's agency.

We find strong evidence that the cash grant and training program improved women's access to finance. The treatment effect on a standardized index is 0.41 standard deviations. Women in the treatment group are not only more likely to have a bank account (which was a requirement to get the cash grant), they also have higher levels of savings, borrowed money, and repaid their debt (Appendix Table A.9).

Women who received the cash grant and training program also report higher life satisfaction. The positive effect is identified not only for current life satisfaction (+0.18 SD), but also for life satisfaction one year before the endline survey (+0.16 SD), and expected life satisfaction three years after the survey (+0.10 SD). It is worth noting

that self-reported life satisfaction is very low: only 2.4 on average in the control group on a Cantrill's ladder ranging from 1 to 10. The recipients of the cash grant and training program characterize their relative wealth as higher than that of other households of their community (+0.15 SD). The impacts on women's agency, access to finance, and well-being, are similar in the two treatment arms, suggesting that partners' involvement in the training had no marginal effect on these outcomes.

We also find a positive and significant effect of the cash grants on women's mental health, as measured using the MHI-5 scale (+0.07 SD). We find suggestive evidence that this positive impact is mainly driven by women who attended the training alone, but the difference between the two treatment arms is statistically insignificant at conventional levels.

Household living standards, shocks, and migration. The cash grant and training intervention had a positive impact on living standards (H2.1). The effect on total consumption per capita is positive and statistically significant (+7.6%). The positive effect on consumption is largely driven by the positive effect on food consumption per capita (+10.7%).

Households that received the cash grants also have more assets. The estimated effect on an asset index is +0.13 SD. Positive effects are observed on a wide range of assets, including ownership of motorcycles,

Table 3
Treatment effects on living standards, shocks, and migration.

	(1)	(2)	Eq. (1)	Eq. (2)			
	Control	Control	(3)	(4)	(5)	(6)	(7)
	mean	SD	Treatment	T1: Cash & Training alone	T2: Cash & Joint training	p-value T1 = T2	N
Household living standards							
Total consumption per capita (in Dinars per day) [†]	8.428	6.472	0.638** (0.266) [0.050]	0.745** (0.335) [0.059]	0.544* (0.321) [0.130]	0.601 [0.627]	1,824
Food consumption per capita (in Dinars per day) [†]	3.582	3.004	0.385*** (0.116) [0.011]	0.420*** (0.144) [0.020]	0.349** (0.140) [0.045]	0.666 [0.667]	1,824
Non-food consumption per capita (in Dinars per day) [†]	3.757	2.750	0.080 (0.120) [0.576]	0.080 (0.149) [0.622]	0.080 (0.144) [0.617]	1.000 [0.776]	1,824
Asset index (stock) [†]	0.000	1.000	0.126*** (0.038) [0.011]	0.148*** (0.049) [0.018]	0.105** (0.043) [0.047]	0.425 [0.508]	1,824
Asset index (bought since 2019)	−0.000	1.000	0.009 (0.042) [0.731]	0.026 (0.052) [0.632]	−0.009 (0.049) [0.746]	0.539 [0.590]	1,824
Shocks and coping mechanisms							
Economic shock [†]	0.016	0.125	0.025*** (0.007) [0.011]	0.020** (0.009) [0.059]	0.030*** (0.010) [0.019]	0.401 [0.483]	1,824
Other shock [†]	0.250	0.433	−0.002 (0.019) [0.768]	−0.019 (0.022) [0.483]	0.016 (0.023) [0.576]	0.189 [0.258]	1,824
Extreme coping strategy [†]	0.123	0.328	−0.028** (0.013) [0.060]	−0.018 (0.015) [0.294]	−0.038** (0.015) [0.045]	0.236 [0.295]	1,824
Other coping strategy [†]	0.152	0.359	0.009 (0.015) [0.616]	−0.023 (0.018) [0.270]	0.041** (0.020) [0.082]	0.005 [0.023]	1,824
Migration							
Household migration (since baseline)	0.094	0.295	−0.030** (0.012) [0.045]	−0.030** (0.014) [0.075]	−0.029** (0.014) [0.082]	0.953 [0.776]	2,000
Individual migration (since 2019) [†]	0.190	0.393	0.002 (0.018) [0.748]	−0.001 (0.022) [0.776]	0.006 (0.022) [0.726]	0.797 [0.726]	1,824
Expected likelihood of future migration	0.054	0.226	0.033*** (0.011) [0.018]	0.016 (0.012) [0.272]	0.050*** (0.014) [0.008]	0.033 [0.069]	1,824

Notes: This table reports the intent-to-treat effects of the interventions on household living standards, shocks, and migration. Columns 1 and 2 report the mean and standard deviation of the outcome variables in the control group. Column 3 reports the effect of the cash grant and training program (β_1 in Eq. (1)). Columns 4 and 5 report the effect of the cash grant and training program with and without the male partner (β_1 and β_2 in Eq. (2)). Column 6 reports the p-value for the null hypothesis that the effect of the cash grant and training program is the same with and without the male partner ($\beta_1 = \beta_2$ in Eq. (2)). *Asset index*: standardized index using the method of Filmer and Pritchett (2001) on a set of 21 assets. *Economic shocks*: dummy equal to one in case of job loss, failed business or loss of livelihood in the last 24 months. *Other shocks*: dummy equal to one in case of death/disease of a household member or theft in the last 24 months. *Extreme coping strategy*: dummy equal to one if the household reduced the number of meals, took children out of school or fostered children to friends to face a shock. *Other coping strategy*: dummy equal to one if the household took debts, received help, sold assets, or used savings to face a shock. *Household migration*: dummy equal to one if the household has moved to a different imada (value of one is imputed for attrited households who could not be surveyed because they migrated). *Individual migration*: dummy equal to one if at least one household member has migrated since 2019. *Expected likelihood of future migration*: dummy equal to one if at least one household member is likely or very likely to migrate to another city or governorate in the next 12 months. Outcomes denoted with [†] are available at baseline and used as control. Regressions also include enumerator fixed effects and a vector of control variables selected using the double LASSO method of Chernozhukov et al. (2017). As our impact evaluation builds on the sample of a previous experiment, we control for women's treatment status in that prior experiment in all regressions. For individual migration (since 2019), the equivalent variable in the baseline is "individual migration (since 2015)". Robust standard errors are reported in parentheses. FDR q-values are reported in brackets.

*** $p < 0.01$.

** $p < 0.05$.

* $p < 0.1$.

televisions, generators, ventilators, tables, mattresses, and the number of rooms in the house (Appendix Table A.10).¹³ The effects on measures

¹³ We find no effect on the variable "Asset index (bought since 2019)". Two main reasons may explain this discrepancy. First, the cash grant was delivered between September 2018 and January 2019, implying that asset purchases done immediately after receiving the cash might be excluded from this variable. Second, recall issues leading to attenuation bias are more likely

of living standards are very similar in the two treatment arms, suggesting that partners' involvement in the training had no marginal effect on living standards.

These effects on women's well-being and mental health and on household consumption and assets are broadly consistent with the

with this variable, because respondents might not accurately remember when they bought the assets.

effects on total household income. In general, effects are positive and the magnitudes of impacts are higher when women are invited to the training alone, but the differences between the two treatment arms are not statistically significant.

The recipients of cash grants are 2.5 percentage points more likely to report having suffered from an economic shock in the two years preceding the survey (job loss, bad business, or loss of livelihood). This effect is quite large (+156%) given that this type of negative shock is extremely rare in the control group. This result suggests that some women used part of the grant to invest in an IGA, but these activities did not survive until the endline survey. We also find suggestive evidence that the grants help households cope with shocks without having to take extreme decisions such as skipping meals or taking children out of school.

Finally, we find that households in the treatment groups are 3 percentage points less likely to have moved to a different Imada at endline (H2.3). This result suggests that the opportunity cost of migrating may have increased thanks to the cash grants and the better livelihood opportunities and higher living standards that resulted from the program. We also examine a variable indicating whether some household members had left their household and migrated individually since January 2019, that is, about two years before the endline survey. We find small and non-significant effects. However, the program positively impacted migration intentions.

Cost-benefit analysis. We use two approaches to assess the cost-effectiveness of the cash grant and training program (Appendix Table A.6). First, we follow Banerjee et al. (2015) and compare the cost of the program with the net present value of the ITT on consumption and assets, assuming that effects on consumption are permanent. Using a social discount rate of 5%, we find that the material benefits of the cash grant and training program are 17 times higher than its cost. While the confidence interval is large [2.5 – 31.3], we note that even its lower bound is above 1.

Recognizing that the approach of Banerjee et al. (2015) is based on a rather optimistic assumption – that the ITT effect on consumption is permanent – we also estimate how long the ITT effect on consumption should last to fully cover the program costs. We find that only 1.2 years of benefits are needed to obtain a positive benefit–cost ratio, which shows that the program is highly cost-effective, even in the short-run.

5. Conclusion

Our study assessed the potential of a program of capital injections and gender-sensitive financial training to empower women and address gender-specific barriers in the labor markets.

Our results are mixed. On the one hand, we find that households as a whole massively benefited from the cash grant and gender-sensitive training program, which is consistent with the literature on the impact of large one-off grants in developing economies (see e.g., Haushofer and Shapiro 2016, Crépon et al. 2020, Kondylis et al. 2021). We identified positive effects on a broad range of outcomes, including food consumption, asset ownership, investments in agriculture and livestock farming, and employment of other household members. We also find that women in the treatment groups are more satisfied with their lives, have better mental health, and have greater access to finance. Our estimates suggest that the program is highly cost-effective. These results demonstrate the potential of cash transfer and training programs to stimulate economic activity and reduce household poverty.

On the other hand, the program had only limited impacts on women's employment and no impact on women's agency. Involving male partners in the training backfired and reduced women's participation in IGAs, women's income, and women's hours worked. This latter result contrasts with the findings of Bulte et al. (2017, 2018), who found limited marginal impact of involving men in a business training program. Further research is needed to explore both the theoretical

underpinnings driving this effect, and how generalizable the effect may be.

Overall, our paper confirms the importance of redistribution mechanisms within households (Bernhardt et al., 2019). Our study also highlights the difficulty of challenging gender roles and stimulating women's employment in traditional societies, and that attempting to do so can backfire. This highlights the need to carefully develop and test unproven interventions that directly target gender norms.

CRedit authorship contribution statement

Jules Gazeaud: Pre-analysis plan, Data curation, Formal analysis, Writing – original draft, Writing – review & editing. **Nausheen Khan:** Study design, Project implementation, Pre-analysis plan, Writing – review & editing. **Eric Mvukiyehe:** Study design, Funding acquisition, Project implementation, Pre-analysis plan, Writing – review & editing. **Olivier Sterck:** Pre-analysis plan, Formal analysis, Writing – original draft, Writing – review & editing.

Data availability

Data will be shared upon publication on the websites of the authors (computational reproducibility has been checked by the World Bank DIME Analytics team)

Online Appendix. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.jdeveco.2023.103169>.

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