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Short-Term and Long-Term Effects of Cash for Work: Evidence from a Randomized Controlled Trial in Tunisia

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ABSTRACT *While a growing literature analyzes the economic effects of cash for work programs in developing countries, there remains little evidence about the longer-term effects of these interventions. This paper presents findings from a randomized controlled trial evaluating a three month intervention providing public works employment in rural Tunisia. The evaluation design incorporates two dimensions of randomization – community-level randomization to treatment and control, and individual-level randomization among eligible individuals – and a sample of 2,718 individuals was tracked over five years. The findings suggest that cash for work leads to significant increases in labor market engagement, assets, consumption, financial inclusion, psychological well being, and women's empowerment one-year post-treatment; however, these effects have largely attenuated to zero five years post-treatment, with the exception of a positive effect on assets. There is also evidence of large positive spillover effects within treatment communities, but these effects similarly attenuate over time.*

KEYWORDS: Cash for work; randomized controlled trial; North Africa; spillover effects

JEL CLASIFFICATION: O12; O15

1. Introduction

Workfare or public works employment programs have long been prominent in many developing countries, and form an important part of the social safety net in contexts such as India (the National Rural Employment Guarantee Scheme) and Ethiopia (the Productive Safety Net Program). These programs typically have some of the same advantages as large-scale cash transfers – namely, they are a simple strategy to directly boost consumption and savings, enable investment, and reduce poverty among the poorest households – but the additional requirement for employment could in principle have additional positive effects: it may facilitate self-targeting toward individuals without higher-return opportunities, or provide an opportunity for individuals to build skills. In practice, however, the latter channels have not generally been substantiated, particularly for workfare programs that center around low-skilled manual labor (Murgai, Ravallion, & Van de Walle, 2016). Recent reviews of primarily quasi-experimental evidence have suggested that public works programs generally do not seem to boost employability or enhance

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skills, and as such, it may be challenging to justify the higher costs of implementing such programs vis-a-vis simpler direct cash transfer interventions (Card, Kluve, & Weber, 2010; Gehrke & Hartwig, 2018).

This paper presents findings from a randomized controlled trial in Tunisia designed to evaluate the short- and long-term effects of the Community Works and Local Participation (CWLP) pilot, a program that provided short-term paid employment to the long-term unemployed for approximately three months. Compensation provided was around $200 per month or $610 in total, relative to average monthly consumption per capita in the control arm of $370; thus this was a proportionally large transfer, providing around 1.6 months allocation of consumption expenditure.[1] (The compensation of $200 monthly was also above the minimum wage, at that time around $178 per month; while it would likely not suffice to meet monthly household consumption requirements, it is roughly equal to half a typical household's monthly consumption budget and thus might be sufficient if there was a second income earner.)[2] While the project was implemented in communities defined as rural according to Tunisia's national classification, the population in the sample communities is majority non-agricultural, and individuals were eligible if they were aged 18 to 60, had been unemployed for at least 12 months (and were not actively engaged in agriculture), and were not enrolled in secondary or tertiary education.

The evaluation employs a novel design in order to estimate treatment effects along multiple dimensions: first, a community-level randomization assigned 80 villages (imadas) to either treatment or control status. Second, in each community, non-governmental organization leaders or local leaders identified individuals who were eligible for the program based on the stated criteria, and in treatment communities, a random subset of these eligible individuals were offered employment.[3] There are thus three samples of interest that are observed: the eligible and treated individuals in treatment communities; the eligible and untreated individuals in treatment communities; and the eligible and untreated individuals in control communities. This design allows us to generate high-quality estimates of both the direct and the spillover effects of the intervention by comparing eligible individuals in treatment communities to untreated counterparts in control communities.

The primary sample includes 2,718 individuals who were sampled from the list of eligible individuals constructed at the community level; no baseline survey was conducted, though pre-treatment data at the locality level from the 2014 census is available and is used to verify balance in the village-level randomization. The intervention and associated public works activities were rolled out between April and September 2015, and the first follow-up survey was implemented approximately one year later between April 2016 and January 2017. This was followed by a second, long-term follow-up conducted between December 2020 and April 2021, approximately five and half years following program implementation.[4] The measured outcomes include a range of variables capturing wage employment and self-employment by both the target beneficiary and other household members, economic welfare, investment in human capital, social cohesion, psychosocial well-being, and women's empowerment. All outcomes of interest were pre-specified, and treatment effects for outcome families were estimated following Kling, Liebman, and Katz (2007). (This method generates a summary index that is the equally weighted average of Z-scores of each component variable.) We also report p-values based on both traditional statistical inference and corrected for multiple hypothesis testing.

The primary findings based on the cross-village comparison of treated and untreated individuals suggest that the intervention had significant and large short-term effects on both primary economic outcomes and secondary psychosocial outcomes. We observe significant increases of between .2 and .4 standard deviations in indices of labor market participation, assets, consumption, and financial inclusion, as well as increases of comparable magnitude in psychosocial well-being and women's empowerment. (Null effects are observed for outcomes linked to human capital investment, coping mechanisms conditional on shocks, and social cohesion.) These effects are driven by a large increase in the probability that the respondent as well as other household members report any work over the past month, leading to an increase in expenditure particularly on housing costs, an increased stock of assets including livestock and consumer durables, and a substantial (proportional) increase in savings relative to a base of essentially zero.

By the five-year follow-up, however, the short-term positive effects have substantially attenuated. For economic outcomes, the positive effects on assets and consumption remain of comparable magnitude and are weakly statistically significant, but only the increase in assets remains significant when corrected for multiple hypothesis testing. The effects on the other indices are uniformly statistically insignificant, and the hypothesis that the effects in rounds one and two are consistent can be rejected for the labor market variables, financial inclusion, psychological well-being, and women's empowerment and agency. The estimated treatment effects for primary (economic) outcomes are also robust to any corrections for bias introduced by selection into the survey sample or attrition over time in both survey rounds, but the treatment effects for secondary outcomes are not robust to this correction.

The findings based on the cross-village comparison of untreated individuals in treatment and control communities – allowing for estimates of local spillover effects of the intervention – suggest a largely similar pattern. In the short run, there are positive effects on the primary and secondary outcomes of similar magnitude (again around .3 standard deviations), other than financial inclusion. The effects on secondary outcomes are somewhat reduced, though the increase in psychosocial well-being remains statistically significant. In the long run, none of the estimated effects for the spillover sample remain statistically significant. (In addition, the estimated spillover effects in the short run are not robust to corrections for selection into the evaluation sample.)

Finally, we can evaluate the within-village comparison between individuals who are eligible and offered employment and eligible but not offered employment. This analysis shows a statistically insignificant difference in both the short and the long run, a finding that is unsurprising given that the effects on direct beneficiaries and eligible non-targeted individuals in treatment communities are largely parallel.

Our paper makes several contributions to the existing literature. First, we provide new evidence about the long-term effects of workfare or public works employment programs. We particularly contribute by tracking a wide range of both economic and psychosocial outcomes over a much longer time horizon (up to five years). Much of the existing literature analyzing the effects of public works employment has centered around two large, government-run programs, NREGA in India and the PSNP in Ethiopia, that can provide seasonal employment over a number of years. An extremely large literature has analyzed the effects of NREGA on labor market outcomes (Berg, Bhattacharyya, Rajasekhar, & Manjula, 2018; Imbert & Papp, 2015), poverty (Muralidharan, Niehaus, & Sukhtankar, 2017; Ravi & Engler, 2015), migration (Imbert & Papp, 2020), conflict (Fetzer, 2020; Khanna & Zimmermann, 2017), education, child cognition, and child labor (Afridi, Mukhopadhyay, & Sahoo, 2016; Li & Sekhri, 2019; Mani et al., 2020; Shah & Steinberg, 2021), and infant health (Chari, Glick, Okeke, & Srinivasan, 2019). For the PSNP, evidence suggests impacts are limited on average due to low transfer levels, though there are some effects on food security and livestock assets (Berhane, Gilligan, Hoddinott, Kumar, & Taffesse, 2014; Gilligan, Hoddinott, & Taffesse, 2009) as well as child nutrition (Porter & Goyal, 2016). Both the NREGA and the (urban) PSNP have been shown to have large positive spillover effects when general equilibrium effects are evaluated (Franklin, Imbert, Abebe, & Mejia-Mantilla, 2024; Muralidharan, Niehaus, & Sukhtankar, 2023).

Beyond these two large recurring programs, other papers have analyzed similar short-term public works employment programs. In Côte d'Ivoire, a randomized trial suggested that seven months of temporary employment in road maintenance had no persistent effects fifteen months later, other than higher savings (Bertrand, Crépon, Marguerie, & Premand, 2021); similarly, in the Democratic Republic of Congo, there were minimal persistent effects of a four-month job offer 18 months later, other than modest effects on employment and savings (Brandily-Snyers, Mvukiyehe, Smets, van der Windt, & Verpoorten, 2022). A randomized trial of workfare in

Colombia found positive effects on consumption and labor supply that persisted up to a year (Alik-Lagrange, Attanasio, Meghir, Polanía-Reyes, & Vera-Hernández, 2017). In Argentina, a public works employment project implemented in response to the 2002 economic crisis reduced unemployment and poverty (Galasso & Ravallion, 2004). In Comoros, another randomized trial of a public works employment program found evidence of a significant increase in international migration (Gazeaud, Mvukiyehe, & Sterck, 2023). In Yemen, public works employment increased labor supply and seemed to have a protective effect vis-a-vis adverse coping mechanisms during an economic downturn Christian, de Janvry, Egel, and Sadoulet (2015). In Malawi, however, a public works employment program had no effects on food security or use of fertilizer (Beegle, Galasso, & Goldberg, 2017).

Importantly, very few of the papers in the existing public works employment literature analyze effects on psychosocial or non-economic outcomes; and none, to our knowledge, report treatment effects for a horizon longer than about two years. We thus contribute by providing novel evidence around the effects of public works employment on social cohesion, psychological well being, and women's empowerment, and analyzing the effects on both economic and non-economic outcomes for a much longer follow-up period of five years.

Second, we contribute to a growing literature analyzing spillover effects of cash transfers or other cash benefit programs. Here, we benefit from a robust double-randomized design that allows us to rigorously estimate spillover effects on individuals who are eligible for the program and who report similar observable characteristics, but who are randomly not offered the program, and we find spillovers that are large, positive, and of equal magnitude to the effects for direct beneficiaries. (This design has previously been used by only two other papers to our knowledge, Egger, Haushofer, Miguel, Niehaus, and Walker (2019) and Beegle et al. (2017).) In the existing literature, Angelucci and De Giorgi (2009) analyzes the effects of Progresa on local non-eligible households and finds evidence of significant positive spillovers in terms of consumption, and similar positive spillovers are reported in a large evaluation of unconditional cash transfers in Kenya (Egger et al., 2019). Two papers have shown positive spillover effects of government public works programs (Franklin et al., 2024; Muralidharan et al., 2023), and large positive spillovers of BRAC's Targeting the Ultrapoor – a multifaceted graduation program that also encompasses cash transfers – are also documented for consumption and economic outcomes, as well as for nutritional outcomes (Bandiera et al., 2017; Raza, Van de Poel, & Van Ourti, 2018). There is also evidence of positive spillovers from economic aid provided to refugees (Taylor et al., 2016). By contrast, there has also been evidence of negative spillovers of cash transfers in some contexts, particularly in more remote communities (Beegle et al., 2017; Filmer, Friedman, Kandpal, & Onishi, 2021; Haushofer & Shapiro, 2016).

Third, we contribute to a literature analyzing longer-term effects of cash transfers or cash grants (though we analyze cash for work, as opposed to unconditional cash). Our findings are generally consistent with a literature suggesting the positive effects of cash decay in the medium-term and often converge to zero (Baird, McIntosh, & Özler, 2019; Blattman, Fiala, & Martinez, 2020; Blattman & Dercon, 2018; Haushofer & Shapiro, 2018), though the pattern is not fully uniform and some transfers (particularly in conjunction with complementary programming) can have persistent effects (Ahmed et al., 2023). Overall, the body of evidence for long-term effects of any form of cash programming is not large: a recent meta-analysis notes that roughly half of evaluations report on programs still ongoing at the point of data collection, and of those reporting post-program data, the time elapsed is only around a year. By contrast, multifaceted graduation model interventions that layer cash with a broader set of interventions seem to be more effective in generating longer-term poverty exit, though again the body of evidence is not large (Balboni, Bandiera, Burgess, Ghatak, & Heil, 2022; Banerjee, Duflo, & Sharma, 2021).

This paper proceeds as follows. Section 2 describes the context, the experimental design and the data collection. Section 3 presents the analytical strategy, the outcomes of interest, and the main results. Section 4 concludes.

2. Methodology

2.1. Setting and intervention

In the decade prior to the 2011 Jasmine revolution, Tunisia's economy showed consistent growth and was among the leading performers in the Middle East and North Africa (MENA) region, with average annual growth in gross domestic product of 4.2 percent (World Bank, 2011). In subsequent years however, Tunisia's economic growth slowed. Higher food prices exacerbated economic woes and in January 2014 culminated in political unrest and the toppling of Zine Ben Ali, the country's long-time ruler (Campante & Chor, 2012). Average annual GDP growth between 2011 and 2015, the year in which this project was launched, was only 1.7%.

In addition, the revolution substantially impacted access to basic services. In some localities, critical facilities such as clinics and hospitals were closed, while food supply routes were disrupted, thus making disadvantaged populations even more vulnerable. Existing plans to expand or improve health and education services especially in disadvantaged areas stalled (World Bank, 2011).

Against this backdrop, the World Bank and the Tunisian Ministry of Vocational Training and Employment launched the Community Works and Local Participation (CWLP) project in Jendouba, a rural and underserved governorate. Importantly, rural in the context of Tunisia does not necessarily imply the population is majority agricultural; census data from 2014 indicates that in Jendouba, the only a quarter of the population in rural imadas or villages (targeted for this program) was reported to be primarily engaged in agriculture. Another 8% were engaged in manufacturing, 20% in commerce and transportation, 15% in public employment, and 22% in education, health and administration.[5] Within this non-urban and also largely non-agricultural population, CWLP targeted individuals characterized by long-term unemployment with an offer of temporary employment. The objective was to provide immediate income support to smooth consumption and strengthen individuals' future earning capacity (via the provision of skills development or work experience), while also improving productive infrastructure.

CWLP financed approximately 40 public works projects in Jendouba Province, for which workers were required to be between 18 and 60 years old, and to have been out of work for at least 12 months. Households who were actively engaged in agricultural production were identified as participating in employment and thus were ineligible, and individuals who had participated in previous public works employment programs were similarly ineligible. These local public works projects were chosen by local non-governmental organizations in conjunction with community leaders. Eligible projects all included the upgrading of local infrastructures and services, and a minimum of 70% of the budget was required to be devoted to labor costs.

The first round of CWLP was implemented between 2012 and 2014; our study focuses on the second round, launched between April and September 2015 with an average duration of around three months.[6] In identifying eligible individuals, priority was given to the poorest households, women, at-risk youth, and heads of households. Those who completed the program received a wage of around \$10.18 (in purchasing power parity-adjusted dollars) daily, for an estimated total of 825 Tunisian dinars or approximately \$610 over three months. The wage provided was thus above the prevailing minimum daily wage at the time, \$8.88. Reports from collaborating organizations suggests that attendance at the work site was generally enforced for those participating, and this is broadly consistent with the self-reports from participants.[7]

2.2. Experimental design

The second round of the CWLP evaluated in this paper was rolled out as a randomized controlled trial in 80 imadas, or villages, in the Jendouba governorate, the lowest level administrative unit in Tunisia.[8] We implemented the randomization in two steps in order to capture both direct and spillover effects of the CWLP. In the first step, the village-level randomization, we first stratified the 80 sample villages into three groups by population – less populated,

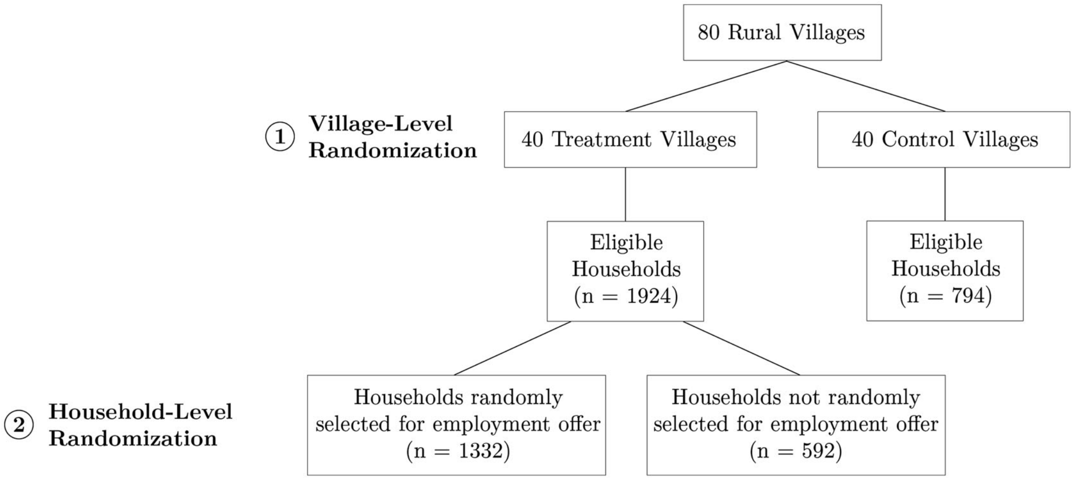

Figure 1. Study design.
Notes: This figure illustrates the experimental design. The 80 rural villages were first randomized into 40 treatment and control villages. Following the identification of eligible households, assignment to the cash-for-work programs was randomized in treatment villages.

moderately populated, and more populated.[9] Randomization was conducted within these three strata, assigning 40 villages to treatment and 40 villages to control.

In the second stage, the individual-level randomization, local NGOs in treatment villages compiled lists of around 60–65 poor unemployed residents eligible for employment in public work projects. We randomly selected around 42 of these eligible workers to participate in each village; the others were not offered employment. Among those who were offered employment, take-up of employment was around 73%, yielding around 31 participating workers in each village. Note that all individuals offered employment will be identified as treated individuals in treated communities for the purposes of the intent-to-treat analysis conducted here.[10]

In addition, local leaders in the control villages also compiled lists of 40 village residents who would have been eligible for the program if their villages had been assigned to treatment, thus constituting the control sample. This selection of eligible individuals in control communities was conducted approximately a year following the selection of individuals in treatment communities, but leaders were advised to refer retrospectively to individuals' outcomes in selecting beneficiaries. Figure 1 depicts the experimental design, and Figure A1 shows a map of the project locations. All randomization procedures were conducted by the research team using Stata.

2.3. Data collection

The evaluation did not include a baseline survey, though baseline administrative data at the imada level is available drawing on the Tunisia population census from 2014. Instead, two rounds of follow-up surveys were conducted. The sampling frame is constituted by the beneficiary lists constructed in both treatment and control communities as described above. In treatment villages, the beneficiary list included an average of around 60–65 beneficiaries per village, for a total of 2,537 total respondents, and the entire beneficiary list was targeted for the survey. In the control villages, the survey firm randomly sampled 20 individuals out of the 40 individuals included on the constructed list of eligible individuals.

The total target sample was thus 2,537 individuals in the treatment villages and 800 individuals in the control villages; the realized evaluation sample, as captured in Figure 1, was 1924 individuals in the treatment villages (76% of the target sample), and 794 in the control villages, for a total of 2,718 individuals.[11]

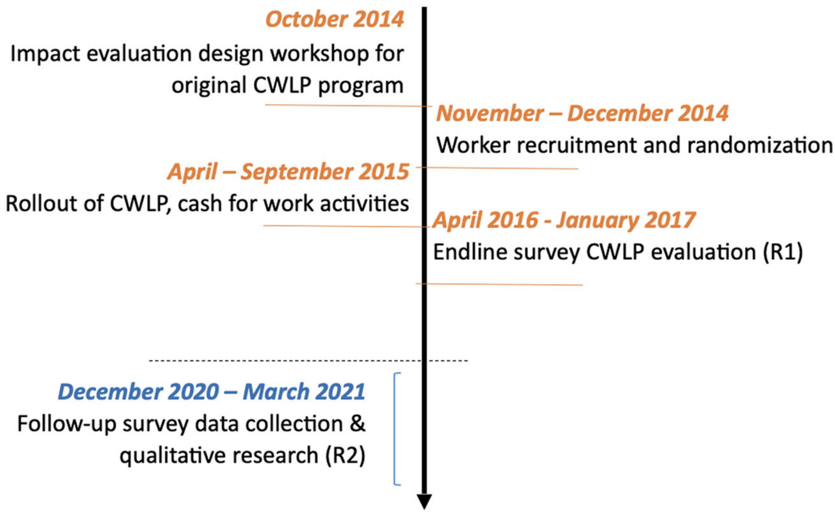

Figure 2. Timeline.
Notes: This figure captures the evaluation timeline.

We conducted the first round of data collection between April 2016 and January 2017, on average roughly 12 months after the end of the intervention in each treatment village (paid employment generally ended around August 2015).[12] The second round of data collection was conducted between December 2020 and March 2021, approximately five and a half years post-treatment, as depicted in Figure 2. From the evaluation sample of 2,718 individuals surveyed in the first follow-up survey, the second follow-up survey included 2,185 individuals for an attrition rate of 19.6 percent (22.5% in treatment villages and 12.6% in control villages); this difference is not statistically significant. Again, more details about potential bias induced by attrition between the first and second follow-up survey rounds will be provided in Section 3.5.

There was also a second trial with a separate cross-randomization nested within this sample that is analyzed in a separate paper (Gazeaud, Khan, Mvukiyehe, & Sterck, 2023). This second trial included (virtually) all households from the CWLP sample in which the sampled individual was a woman; households from the CWLP sample in which the sampled individual was a man did not enter the second trial.[13] In the second trial, randomization was conducted at the household / individual level to assign the woman in the household to receive a cash grant (with or without a couples' training) that was rolled out following the first follow-up survey analyzed here (but prior to the second follow-up survey). Table A1 in the Appendix summarizes the structure of cross-randomization; within the CWLP sample, 48% of households do not enter the second trial (primarily because the respondent is a man), and 26% each enter the second trial assigned to treatment and control. We will present findings in which we also demonstrate our primary findings are robust to this cross-randomization.

The survey instruments that were used in both survey rounds consisted of a questionnaire administered to the individual who was identified as eligible for cash for work.[14] The survey collected information on the composition of the household, the economic activities of its members, assets, consumption, the economic shocks faced by the household, social cohesion, civic participation, women's empowerment, and psychological well-being.

3. Empirical analysis

3.1. Outcomes of interest and conceptual framework

The outcomes of interest were pre-specified at the launch of the experiment and include six primary outcome families: labor market outcomes for the primary respondent, labor market outcomes for other household members, consumption, assets, financial inclusion, and human capital. The four secondary outcome families include coping mechanisms (vis-a-vis shocks experienced by the household), psychological well-being, social cohesion, and women's empowerment and agency.

Tables A2 and A3 summarize the variables included. The summary outcome measures are constructed following Kling et al. (2007) and are generally defined identically for the first and second rounds of follow-up. Details about the cases in which the outcome families were not defined identically, and about any deviations from the pre-analysis plan, are provided in Appendix A.[15]

Given a large existing literature analyzing the effect of public works employment on the outcomes of interest, we do not provide a full conceptual framework, but simply a brief overview of potentially relevant channels through which the intervention may shape these outcomes. For the economic outcomes of interest, there are two primary channels for effects. The first channel is the direct effect generated by the infusion of cash into the household that could be used to amass assets, invest in training, purchase goods, generate savings, or potentially invest in a business (or fund a search for employment), either for the main respondent or another household member. The second channel is the indirect effect if respondents develop skills or amass experience in their period of public works employment that leads to an increased probability of employment post-intervention; or, if respondents use the cash earned to fund the start-up or search costs associated with identifying new employment. This increased level of economic activity and income may in turn may lead to positive effects on other economic variables.

For secondary outcomes of interest, we hypothesize that enhanced economic status through the direct and indirect channels may lead to a shift away from adverse coping mechanisms in the face of shocks (e.g. households who have more resources will not be required to disinvest in assets in response to a negative shock). We also hypothesize that enhanced economic status could lead to increased engagement in the community and enhanced psychological well-being. There is also the potential for enhanced female empowerment in the form of increased economic engagement or decision-making, though there could also be a backlash effect in response to the intervention that would lead to decreased economic empowerment for women.

3.2. Baseline balance

To assess balance across villages assigned to the treatment and control arms, Panel A of Table 1 reports results summarizing covariate balance at the village level using data from the 2014 population census. The villages in the sample are characterized by an average population size of around 1000 households or 4000 individuals; around 73% of the population is constituted by adults. Unemployment rates are high, averaging above 25% for individuals aged fifteen and above, and education rates are also notably low compared to national averages: more than half of heads of household report no education. We uniformly observe that there are no statistically different differences in these covariates comparing across the treatment and control villages. In addition, the magnitude of the differences is generally low in absolute terms: for example, the mean unemployment rate differs by only .9 percentage points comparing across the treatment and control arms, and the percentage of household heads reporting different levels of education varies by between two and four percentage points.[16]

Panel B of Table 1 then reports balance using time-invariant covariates at the household level; these covariates were measured in the first follow-up survey, but are presumptively unchanged vis-a-vis baseline.[17] We report the mean for households randomly selected for an offer of employment in treatment villages in Column (1); for households randomly selected for a non-offer of employment in treatment villages in Column (2); and for households in control villages in Column (3). We then report the p-value corresponding to the pair-wise comparisons across each of these three samples (conditional on strata fixed effects).

Table 1. Balance across treatment and control arms

Panel A: Balance in village-level covariates (2014 census)	(1) Control mean	(2) Treatment mean	(3) Difference	(4) P-value	(5) N	(6)	(7)
Number of households	1081.475	926.847	−154.628	0.211	80		
	(893.523)	(547.547)	(165.695)				
Average household size	3.864	3.913	0.049	0.734	80		
	(0.294)	(0.279)	(0.064)				
Percentage of males aged 18 years old and above	0.355	0.349	−0.006	0.337	80		
	(0.024)	(0.023)	(0.005)				
Percentage of females aged 18 years old and above	0.384	0.385	0.002	0.549	80		
	(0.020)	(0.020)	(0.004)				
Percentage of males aged 14–30 years old	0.129	0.131	0.001	0.831	80		
	(0.014)	(0.014)	(0.003)				
Percentage of females aged 14–30 years old	0.132	0.135	0.003	0.639	80		
	(0.019)	(0.020)	(0.004)				
Unemployment rate (age 15 and above)	0.266	0.275	0.009	0.734	80		
	(0.119)	(0.098)	(0.024)				
Illiteracy rate (age 10 and above)	0.397	0.409	0.011	0.936	80		
	(0.139)	(0.112)	(0.028)				
Percentage of household heads: no education	0.530	0.554	0.024	0.723	80		
	(0.192)	(0.155)	(0.039)				
Percentage of household heads: primary educ.	0.292	0.314	0.022	0.258	80		
	(0.095)	(0.090)	(0.021)				
Percentage of household heads: secondary or higher educ.	0.178	0.132	−0.047	0.143	80		
	(0.143)	(0.101)	(0.028)				
Previous PWP	0.350	0.475	0.125	0.280	80		
	(0.483)	(0.506)	(0.111)				

(*Continued*)

Table 1. Continued.

Panel B: Balance in time-invariant household covariates (reported in round one, 2016)

	Treatment villages Treated Mean	Treatment villages Untreated Mean	Control villages Mean	Within: Offered emp. vs. spillovers p-value	Between: Offered emp. vs. control p-value	Spillovers: Spillovers vs. control p-value	N
Age	41.472	41.522	40.772	0.904	0.293	0.273	2718
	(10.671)	(11.419)	(11.767)				
Female	0.550	0.497	0.557	0.064 *	0.824	0.103	2718
	(0.498)	(0.500)	(0.497)				
Married	0.703	0.691	0.647	0.569	0.122	0.264	2718
	(0.457)	(0.463)	(0.478)				
No primary education	0.617	0.600	0.592	0.534	0.684	0.798	2718
	(0.486)	(0.490)	(0.492)				
Worked more than three months in 2013	0.084	0.057	0.072	0.055 *	0.292	0.561	2718
	(0.278)	(0.233)	(0.258)				
Born in the village	0.858	0.861	0.826	0.856	0.204	0.255	2718
	(0.349)	(0.346)	(0.379)				
Born outside the governorate	0.113	0.111	0.142	0.932	0.185	0.212	2718
	(0.316)	(0.315)	(0.350)				

Notes: In Panel A, each row reports the mean of the stated variable as measured in the 2014 census in villages in the control and treatment arms and the difference, and the p-value from a regression in which the covariate is regressed on a binary treatment variable. In Panel B, each row reports the mean of a time-invariant household covariate measured in the first follow-up survey for households in the specified arm; the p-values reported correspond to pairwise tests comparing across the three subsamples of interest (treated individuals in treated villages, untreated individuals in treated villages, and untreated individuals in control communities). All specifications use strata fixed effects and standard errors are clustered at the village level.
***$p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Again, we generally observe that there are no statistically significant differences in covariates comparing across these three sets of households, though two covariates differ at the ten percent level comparing across the treated and spillover samples within treatment communities. The average respondent identified as eligible for public works employment is around 40 years old; 55% are female, and 70% are married. A majority (around 60%) report no education. Engagement in economic activities is extremely low (fewer than 10% report working in either wage employment or self-employment for more than three months in 2013), consistent with the programmatic criteria targeting the long-term unemployed. The overwhelming majority were born locally. Note that here, the definition of work includes engagement in self-employment (as well as agriculture), and is not necessarily identical to the definitions of employment defined by the census as reported in Panel A.

A more detailed assessment of balance at the household level is unfortunately infeasible in this evaluation given the absence of a baseline survey (the only household-level covariates available are the time-invariant covariates described in the previous paragraph). This renders it challenging to assess whether the selection of eligible respondents in the treatment and control arms generated samples that are fully comparable. However, the available evidence at both the village and household levels does suggest that observable characteristics are generally parallel across arms. We can also characterize the sample vis-a-vis the national poverty line by drawing on the estimated level of consumption subsequently measured for households in the control arm. The estimated level of consumption in the control arm in the follow-up survey conducted in 2016 was \$370 per month for the household, or around \$92.5 monthly per capita given a household size of four. This is about 16% above an estimated national poverty line of around \$80 monthly (\$2.60 daily) as of 2010 (Molini, 2019); given that the household data was collected six years later, this suggests the sample that is proximate to or slightly above the poverty line.

3.3. Results

Given the randomized design, the primary empirical specification is simple. Outcome variables of interest reported at the individual level Y_{iv} for individual i in village v are regressed on a binary variable for treatment assignment T_v. (Some, though not all of those variables are in fact reported by the individual for the whole household: i.e. household-level consumption and household ownership of assets.) In the first specification, we restrict the sample to those individuals sampled in treatment communities and offered treatment as well as those individuals sampled in control communities in order to measure direct intervention effects. We also include binary variables for the randomization strata, λ_v, and standard errors are clustered at the village level.

$$Y_{iv} = \beta_1 T_v + \lambda_v + \epsilon_{iv} \tag{1}$$

We also estimate a parallel specification identical to Equation (1) including only those individuals in the treatment arm who were eligible for treatment but not offered treatment (as well as individuals in the control communities) in order to estimate local spillover effects. (Note that in these initial specifications we do not control for the subsequent cross-randomization within the female-only sample to the cash grant, given that is orthogonal by construction; however, we will subsequently demonstrate our findings are virtually unchanged when this additional control is included.) The final specification of interest exploits the within-village randomization, using data from treatment villages only; outcomes are regressed on the individual-level variable for an offer of work $WorkOffer_{vi}$.

$$Y_{iv} = \beta_2 WorkOffer_{iv} + \epsilon_{iv} \tag{2}$$

We report both conventional p-values and q-values corrected for multiple inference, setting the rate of false discovery at q = 0.05 (Benjamini & Hochberg, 1995). For a detailed description of calculation of q-values, see Anderson (2008).

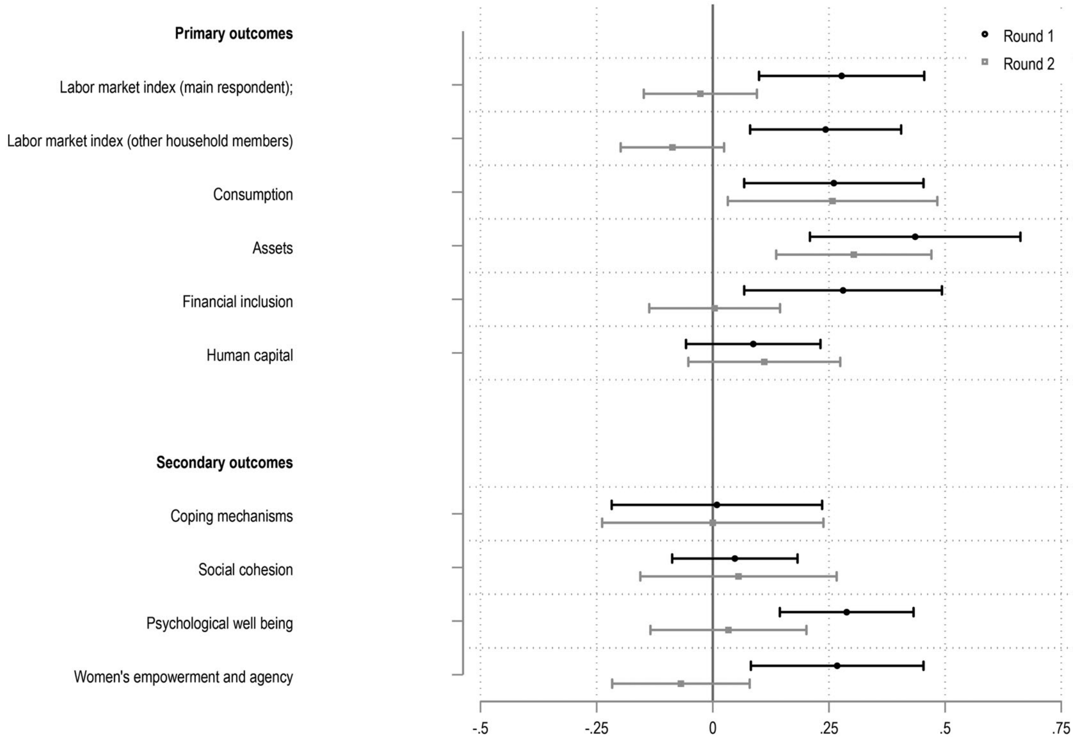

Figure 3. Main results.

Notes: This figure reports the treatment effects for the primary and secondary outcome families in the first follow-up survey round (one year post-treatment) and second follow-up survey round (five years post-treatment). The outcome families are described in Table A2 for primary outcomes and Table A3 for secondary outcomes. We report the primary treatment effect estimate comparing across treated individuals offered employment in treatment villages and untreated individuals in control villages, corresponding to specification (1); the corresponding coefficients are reported in Columns (1) through (4) of Tables 2 and 3. The bars capture 95% confidence intervals; these intervals are constructed using q-values that are corrected for multiple hypothesis testing.

Figure 3 and Tables 2 and 3 capture the main results. For concision, the figure presents the primary estimated treatment effects for outcome families comparing across treated and eligible individuals in treatment and control villages respectively, with estimated coefficients for round one and round two. The confidence intervals are constructed using q-values that are robust to corrections for multiple hypothesis testing. Tables 2 and 3 present results from rounds one and two, respectively. In both tables, the first set of columns captures the main treatment effect estimated comparing across treatment and control villages using specification (1), analogous to the figure; the second set of column captures the estimated spillover effects comparing across treatment and control villages, using specification (1) for the sample of eligible individuals not offered treatment; and the third set of columns captures the within-village treatment effect, using specification (2).

Figure 3 shows that there are significant and positive effects of cash-for-work comparing across eligible individuals in treatment and control communities in the first follow-up round: this includes an increase in the index of the respondent's labor market outcomes of .28 standard deviations, an increase in household labor market activity of .24 standard deviations, an increase in the consumption and assets indices of between .2 and .4 standard deviations, and an increase in the financial inclusion index of .28 standard deviations. There is no significant increase in human capital. For secondary outcomes, there are similarly positive effects on psychological well-being and women's empowerment and agency, both around .3 standard

Table 2. Estimated treatment effects: round one

	Across villages				Spillovers				Within villages			
	(1) T-C	(2) SE	(3) p-value (FDR-adj)	(4) N	(5) T-C	(6) SE	(7) p-value (FDR-adj)	(8) N	(9) T-C	(10) SE	(11) p-value (FDR-adj)	(12) N
Primary Outcomes												
Labor market of the main respondent	0.277***	0.089	0.003 (0.009)	2126	0.319***	0.080	0.000 (0.001)	1386	–0.013	0.049	0.789 (0.790)	1924
Labor market of the household	0.243***	0.082	0.004 (0.010)	2124	0.308***	0.075	0.000 (0.001)	1385	–0.037	0.048	0.447 (0.519)	1921
Consumption expenditures	0.261***	0.097	0.009 (0.015)	2126	0.226**	0.111	0.044 (0.089)	1386	0.043	0.050	0.388 (0.519)	1924
Assets owning	0.436***	0.114	0.000 (0.002)	2126	0.368***	0.123	0.004 (0.009)	1386	0.037	0.050	0.467 (0.519)	1924
Financial inclusion	0.280**	0.107	0.010 (0.015)	2126	0.084	0.095	0.376 (0.614)	1386	0.116**	0.050	0.020 (0.201)	1924
Human capital	0.087	0.073	0.235 (0.294)	2126	0.022	0.086	0.799 (0.888)	1386	0.061	0.050	0.227 (0.519)	1924
Secondary Outcomes												
Coping mechanisms	0.009	0.114	0.938 (0.939)	467	–0.072	0.122	0.556 (0.696)	251	0.113	0.106	0.288 (0.519)	462
Social cohesion	0.047	0.068	0.486 (0.540)	2126	0.003	0.067	0.960 (0.960)	1386	0.055	0.056	0.322 (0.519)	1924
Psychological well being	0.288***	0.072	0.000 (0.002)	2125	0.327***	0.081	0.000 (0.001)	1386	–0.046	0.050	0.359 (0.519)	1923
Women's empowerment and agency	0.268***	0.094	0.005 (0.011)	1162	0.100	0.126	0.429 (0.614)	723	0.125*	0.069	0.071 (0.356)	1015

Notes: The table presents the primary treatment effects for the outcomes of interest in the first follow-up survey, for the between-village comparison of eligible treated individuals vis-a-vis control village individuals; the spillover comparison of eligible untreated individuals vis-a-vis control arm individuals; and the within-village comparison of eligible treated and untreated individuals. All specifications include strata fixed effects and standard errors clustered at the village level. FRD q-values are calculated according to Anderson (2008).
***$p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3. Estimated treatment effects: round two

	Across villages					Spillovers					Within villages				
	(1) T-C	(2) SE	(3) p-value (FDR-adj)	(4) N	(5) R2 = R1 (FDR-adj)	(6) T-C	(7) SE	(8) p-value (FDR-adj)	(9) N	(10) R2 = R1 (FDR-adj)	(11) T-C	(12) SE	(13) p-value (FDR-adj)	(14) N	(15) R2 = R1 (FDR-adj)
Primary Outcomes															
Labor market of the main respondent	−0.027	0.061	0.662	1748	(0.001)	0.009	0.066	0.896	1131	(0.001)	−0.041	0.056	0.464	1491	(0.682)
			(0.929)					(0.896)					(0.936)		
Labor market of the household	−0.087	0.056	0.124	1748	(0.000)	−0.054	0.059	0.366	1131	(0.000)	−0.026	0.057	0.648	1491	(0.878)
			(0.419)					(0.896)					(0.936)		
Consumption expenditures	0.258**	0.113	0.026	1748	(0.985)	0.290**	0.125	0.023	1131	(0.713)	−0.014	0.056	0.797	1491	(0.427)
			(0.167)					(0.150)					(0.985)		
Assets owning	0.303***	0.084	0.001	1748	(0.210)	0.271***	0.087	0.003	1131	(0.429)	0.001	0.057	0.985	1491	(0.556)
			(0.007)					(0.034)					(0.985)		
Financial inclusion	0.004	0.071	0.956	1748	(0.021)	0.014	0.075	0.852	1131	(0.570)	0.006	0.058	0.915	1491	(0.188)
			(1.000)					(0.896)					(0.985)		
Human capital	0.111	0.082	0.181	1748	(0.826)	0.048	0.085	0.572	1131	(0.820)	0.049	0.058	0.393	1491	(0.846)
			(0.472)					(0.896)					(0.936)		
Secondary outcomes															
Coping mechanisms	0.000	0.120	1.000	438	(0.957)	−0.054	0.142	0.705	253	(0.926)	0.053	0.113	0.638	397	(0.703)
			(1.000)					(0.896)					(0.936)		
Social cohesion	0.055	0.106	0.604	1748	(0.943)	0.032	0.102	0.754	1131	(0.815)	0.036	0.055	0.508	1491	(0.829)
			(0.929)					(0.896)					(0.936)		
Psychological well being	0.034	0.084	0.691	1748	(0.007)	0.069	0.090	0.442	1131	(0.022)	−0.035	0.057	0.539	1491	(0.887)
			(0.929)					(0.896)					(0.936)		
Women's empowerment and agency	−0.069	0.074	0.359	1074	(0.001)	−0.017	0.083	0.834	655	(0.415)	−0.041	0.072	0.565	951	(0.047)
			(0.778)					(0.896)					(0.936)		

Notes: The table presents the primary treatment effects for the outcomes of interest in the second follow-up survey, for the between-village comparison of eligible treated individuals vis-a-vis control village individuals; the spillover comparison of eligible untreated individuals vis-a-vis control arm individuals; and the within-village comparison of eligible treated and untreated individuals. All specifications include strata fixed effects and standard errors clustered at the village level. FRD q-values are calculated according to Anderson (2008). The columns denoted R2 = R1 reports the p-values corresponding to tests of equality for the estimated coefficients for the same outcomes comparing across rounds.
***$p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

deviations; however, there is no evidence of any effects on coping mechanisms or social cohesion. (As described in more detail below, the variables related to women's empowerment and agency relate to the level of economic empowerment of the principal woman in the household: either the individual sampled for treatment, if the treated individual is female, or his spouse, if the treated individual is male.)

It is clear in the figure, however, that these effects have substantially attenuated in the second round and are no longer statistically different from zero. We can see in Table 3 that in the second round, we observe an increase in consumption of .26 standard deviations that is not statistically significant when corrected for multiple hypothesis testing; and an increase in assets of .30 standard deviations that remains statistically significant. None of the other coefficients in round two are significant. Though in some cases the width of the confidence intervals does not allow us to reject the hypothesis that the effects are equal in magnitude across the two follow-up rounds, this hypothesis can be rejected for labor market outcomes, financial inclusion, psychological well-being, and women's empowerment and agency.

Returning to Table 2, Columns (6) through (10) capture the spillover effects estimated comparing eligible but untreated individuals in treatment and control communities. Here, the coefficients for the primary and secondary outcome families are comparable in magnitude vis-a-vis the direct treatment effects (and in some cases, slightly larger). This suggests that these individuals also benefit significantly via informal social support from treated individuals, and/or local economic spillovers of the cash payments. There is, however, no effect on financial inclusion, or women's empowerment. Unsurprisingly, given that the direct and spillover treatment effects are similar, the final set of columns capturing the within-village difference between eligible individuals who are and are not offered treatment shows coefficients that are generally small in magnitude and statistically insignificant when corrected for multiple hypothesis testing. These effects also show the same pattern of attenuation across rounds: the only statistically significant spillover effect in the second round is an increase in the asset index, and there are no significant within-village treatment effects in the second round.

We also report the same set of findings using alternate specifications. Importantly, in Table S1 in the supplementary material we re-estimate the primary results controlling for the cross-randomization status in the second, cash grant experimented reported in Gazeaud, Khan et al. (2023) and conducted following the first follow-up survey; we restrict these findings to the sample of CWLP households in which the tespondent is a female, given that only women entered the second experiment. For concision, this table focuses on only the between-village and spillover specifications and reports the coefficients on both the CLWP (the public works intervention of interest here) and the cash grant. Note that the cash grant was disbursed only following the round one survey, and thus the effects of the cash grant assignment in round one can be interpreted only as evidence of ex ante statistical balance, while the effects of cash grant assignment in round two correspond to causal effects of cash. The estimated effects of the CLWP are largely unchanged vis-a-vis the previous specification, and the effects of the cash grant are consistent with the findings discussed at some length in Gazeaud, Khan et al. (2023).

We report further specifications including additional controls in Tables S2 and S3; in both cases, we compile the findings from rounds one and two into a single table for concision. In Table S2, we include additional control variables for all time-invariant individual-level demographic characteristics reported in the balance tests in Panel B of Table 1, and in Table S3, we add additional controls for survey period.[18] In both tables, the main findings are unchanged.

Moving beyond the aggregate indices, Tables S11–S30 report the estimated treatment effects for the individual variables in each index to unpack the mechanisms for the underlying effect. For labor market outcomes, we can observe in Table S11 that engagement in short-term public works employment leads to a near-doubling of the probability that treated individuals report any work over the past month in round one (an increase of eight percentage points, relative to a control arm mean of nine percentage points), and an increase in the number of days worked over the last month (again, a near doubling). This includes both wage employment and self-employment: among the subsample reporting recent work, 31% report that their work is in agriculture, 36% construction, 13% food industry, 6% trade and commerce, 3% government employment, and 10% other. The low base rates of any employment in the control arm are consistent with the targeting of the intervention to an extremely economically inactive population.

In round two, however, these effects have in fact reversed in sign, with treated individuals reporting a significant five percentage point decline in the probability of any work, relative to a mean of 18%; conversely, treated individuals are significantly more likely to report that they have looked for paid work (an increase of 10 percentage points, relative to a mean in the control arm of 22%). This constitutes suggestive evidence that the medium-term effects of the intervention may in fact be slightly negative for labor market outcomes, though there seems to be a positive effect on search behavior. (One hypothesis is that respondents have raised their wage expectations suboptimally high after exposure to the wage offered by the public works program, though we cannot substantiate this directly.)

A similar pattern is evident for household-level labor market outcomes, capturing whether the household head (who is the same as the respondent in about 50% of cases) or any other household member reports any work over the past month; we observe a large and positive effect in the first round that is weakly negative in the second round. In the first round, there is an increase in the probability of any work for the household head of 14 percentage points relative to the mean in the control arm of 20%, but in the second round, the estimated effect is a decline of seven percentage points, relative to a mean in the control arm of 26%.

The detailed findings on consumption and assets reported in Tables S15–S18 suggest that the increase in consumption in the short run is driven primarily by increased consumption of staple foods (an increase of $22 over the past month, relative to a mean in the control arm of $55, for a proportional increase of 40%). There are also increases in discretionary categories of expenditure such as communications, rent and/or housing repairs, and other services. In the second round, many of the coefficients remain positive but are somewhat more noisily estimated, though the increase in staple food consumption remains significant. For assets, there is a significant increase in reported ownership of livestock, furniture, and electronic equipment, and these coefficients are largely consistent across both survey rounds (with the exception of livestock). The pattern of effects over time is particularly notable for financial inclusion outcomes, where in round one we observe a very large proportional increase in savings in the treatment arm: the current stock of savings increases by $0.93 relative to a mean of just seven cents in the control arm, increasing nearly fourteen-fold. By the second round, however, savings is precisely zero in both arms, reducing the treatment effect to a null.[19] There are no significant effects on debt or reported ownership of a bank account in either survey round.

For secondary outcomes, we can see that the substantial increase in psychological well-being in the first round is driven by a reduction in adverse feelings of loss of control and uselessness, and a substantial increase in feelings of social connectedness, as proxied by the number of people with whom the respondent would share a decision to depart the village. The increase in women's empowerment is substantially driven by a large increase in the probability that the female member of the household reports any income generating activity: the probability increases by five percentage points relative to a base probability of two, nearly quadrupling. Again, all of these effects have attenuated to zero by the second round, often inverting in sign.

We can also compare the magnitudes of the estimated effects on economic outcomes in the short run to the actual magnitude of the transfer (again, $610 over three months). The total positive effect on monthly consumption (summing across all categories enumerated) is around $36. If this pattern of increased consumption was consistent over the 12 months since the conclusion of the program, that would suggest around $432 or an amount equivalent to 71% of the value of the original transfer was directed toward increased consumption. (Of course, households have presumably also experienced a positive treatment effect on income earned during this period.) The value of additional assets is challenging to assess, as no data on asset prices was collected; and the value of additional savings (though proportionally large relative to the mean of virtually zero) is minimal, with no evidence of any shift in debt. In general, it seems plausible to conclude that the transfer was substantially directed toward increased consumption in the post-transfer year. This pattern would also be consistent with the attenuation of effects in the longer-term as funds are depleted (and as the initial positive effect on the probability of reporting any work disappears).

To sum up, there is very little evidence that a short-term cash-for-work program generated persistent economic or non-economic effects in this context. The short-term effects were large for both economic and non-economic outcomes and for both direct beneficiaries and eligible individuals who were not offered work but benefited from spillover effects. Unfortunately, we do not have data that would allow us to identify the channel for the large spillovers; channels could include informal transfers from beneficiaries to non-beneficiaries as in Angelucci and De Giorgi (2009) or positive effects on local wages as in Franklin et al. (2024), but we do not have data on transfers or on local wages.

Contextualizing our findings in the literature, the short-term effects seem to be as large or somewhat larger than the positive effects of other cash transfer programs documented in other contexts. A recent meta-analysis of cash transfer effects suggests effects of around $2 in monthly consumption per $100 in total amount transferred (Leight, Hirvonen, & Zafar, 2024); that would imply an increase of around $12 in monthly consumption in this sample while the actual short-term effect observed is considerably larger (more than $30). The magnitude of the positive effects on consumption, assets, and other variables are broadly similar to those reported for large-scale GiveDirectly transfers in Kenya averaging around $900 in value (Egger et al., 2019; Haushofer & Shapiro, 2016), that similarly show positive effects in the range of .2–.4 standard deviations, or 20–50% relative to the mean in the control arm. Again, this is arguably consistent with this transfer program in Tunisia (providing only around $600, considerably less than the GiveDirectly transfers in Kenya) having unusually large effects in the short-term. As previously noted, the fact that the effects decay in the long-term is also consistent with a range of evidence about minimal longer-term effects of cash transfers (Baird et al., 2019; Blattman et al., 2020; Blattman & Dercon, 2018; Haushofer & Shapiro, 2018), and the null effect on employment in the longer-term is similarly consistent with existing evidence around the generally minimal impacts of cash on work (Baird, McKenzie, & Özler, 2018).

3.4. Heterogeneous effects

The pre-analysis plan specified a number of dimensions of heterogeneity analysis.[20] Here, we report heterogeneity with respect to participant gender, the village-level unemployment rate (as reported in the 2014 census), and the mean incidence of recent shocks in the village (as reported in the first follow-up survey). The first community-level variable captures local economic conditions more broadly; the second community-level variable captures whether the area has recently experienced a high volume of adverse shocks.

The results presented in Tables S4–S6 generally suggest there is no meaningful variation in the estimated treatment effects. For heterogeneity with respect to local economic conditions, there is some weak evidence the treatment-induced increase in consumption in round one may be smaller, but this interaction effect is noisily estimated and the pattern is not observed consistently. For heterogeneity with respect to the incidence of local shocks, there may be some positive interaction with financial inclusion (and negative interaction for social participation), but again the pattern is not consistent across outcomes.

3.5. Selection into the evaluation sample

There are two forms of potential selection into the evaluation sample that are relevant for this analysis. First, as previously noted only 76% of individuals included on the eligibility list for the employment intervention in treatment communities, and thus targeted for inclusion in the evaluation, were in fact surveyed in the first round of follow-up and entered the evaluation sample. This includes 69% of those who were eligible and not offered employment, and 79% of those who were eligible and offered employment.[21] In addition, an attrition rate of 19.6% was observed between the first and second follow-up surveys.

To explore potential selection into the sample, we estimate treatment effects in the first round using Lee bounds (for a detailed description of the methodology, see (Lee, 2009)). As in the main estimation, we include randomization strata indicators as a baseline covariate in the analysis to tighten the bounds, and employ bootstrapped standard errors.[22] The results for the main outcome families are reported in Table S7. The evidence suggests that the positive treatment effects for the primary economic outcomes for individuals directly randomized into treatment are generally robust, particularly for labor market outcomes and assets. However, the estimated treatment effects for secondary outcomes for treated individuals (psychological well-being and women's empowerment) have bounds that cross zero, as do the estimated treatment effects capturing spillovers for individuals in treatment communities who were not offered employment.

Also, as previously noted, attrition was non-trivial in this sample given the long-follow-up period: 20% of respondents attrited between the first and second surveys. To analyze the predictors of attrition, Table S8 presents evidence using the following specification. A binary variable for attrited is regressed on a (time-invariant) household covariate as measured in the first survey round, a treatment indicator, and the interaction between the two; the same covariates presented in the balance tests in Table 1 are employed here.

$$Attrit_{iv} = \beta_1 X_{iv} + \beta_2 T_v + \beta_3 X_{iv} \times T_v + \epsilon_{iv} \quad (3)$$

The findings suggest that respondents who are older, female, married, have some education and are from outside the governorate are significantly less likely to attrite, suggestive of lower levels of mobility for these subpopulations, and the coefficients are large: women are 16 percentage points more likely to be successfully surveyed in the second round, and married individuals are 10 percentage points more likely to be surveyed. There is also some evidence of heterogeneity by treatment status in these patterns: treated women are even less likely to attrite, possibly because of the positive effects of the intervention (at least in the short-term). Respondents who have some labor market experience and who were born locally are also somewhat less likely to attrite in treatment communities. Table S9 replicates the balance tests reported in Panel B of Table 1 for the subsample of households observed in the long-term follow-up, and though the evidence of imbalance is still limited, there may be some risk of bias. In order to explore the robustness of the estimated treatment coefficients in the long-run to the observed pattern of attrition, we also estimate Lee bounds for the second-round treatment effect estimates using a parallel strategy. The results reported in Table S10 are generally consistent with those previously reported, in that only the small positive effects on consumption and assets are positive and significant, suggesting that attrition is not a meaningful source of bias in these results.[23]

4. Conclusion

This paper provides new evidence from a multilevel randomized controlled trial around the short- and long-term effects of a three-month cash for work project targeting the long-term unemployed in rural Tunisia. The program provided a wage stipend equivalent to around

1.6 months of household consumption expenditure, and it led to substantial short-term positive effects (around one year post-intervention) on a range of economic and social outcomes including consumption, assets, financial inclusion, psychological well-being, and women's empowerment. Importantly, these effects are observed not only for individuals in treatment villages who were randomly selected for an offer of employment, but also for eligible individuals in treatment villages who were randomly selected not to receive an offer of employment, suggestive of meaningful and large intravillage spillover effects.

However, there is very little evidence that these effects persist five years post-program. Other than a weakly positive effect on assets, treatment and control individuals show little evidence of differential outcomes at this point, suggesting that engagement in short-term public works labor had no meaningful effects on shifting economic trajectories in the medium-term. In particular, there is no evidence that the intervention led to any shift in labor market integration or skill acquisition five years post-treatment. Given that the public works labor was largely unskilled, it is arguably somewhat unsurprising that three months of experience in this form of unskilled work did not have any substantial effect on access to longer-term labor market opportunities.

These findings add to a growing evidence base suggestive of very limited persistent effects of short-term public works employment, though these programs could still be a useful mechanism to provide a short-term buffer against adverse shocks or to smooth consumption. Further research may usefully explore the relative effectiveness and particularly cost-effectiveness of cash for work vis-a-vis cash transfers; given the absence of evidence that public works programs are effective in building skills or increasing employability, the relative advantage of these interventions vis-a-vis simpler social safety net programs remains an open question.

Notes

1. Note that consumption in the control arm was measured around one year following the implementation of the intervention itself.
2. Minimum wage data for 2015 is retrieved from https://tradingeconomics.com/tunisia/minimum-wages and converted to purchasing power-adjusted US dollars using exchange rates reported by the World Bank.
3. The timing of the identification of eligibility did, however, differ across treatment and control communities; the implications of this difference will be discussed further below in the empirical design.
4. While this survey was conducted, broadly, during the pandemic period, stringent lockdowns had largely been lifted in Tunisia by late 2020 (International Labor Organization / Economic Research Forum, 2022). There may of course still be a general negative effect of pandemic-related disruptions on economic outcomes measured in this survey round, but we do not expect this to be a meaningful source of bias in the estimated treatment effects.
5. In Tunisia, criteria for establishing rurality include population size and density, the salience of agriculture as a major economic activity, geographic isolation, and political considerations; accordingly, it is unsurprising that even a rural area could be substantially non-agricultural.
6. Figure A1 in the Appendix shows the timeline for the launch and conclusion of each project.
7. Within those households randomly selected to receive an employment offer, only 10% of the sample do not self-report working despite the fact that they were recorded to have accepted the offer of employment and entered the program; it is possible that in some cases these individuals did not in fact work. Only 3% of control households (19 households) report having worked; it is possible that they were allowed to participate in error, but it is also possible they are inaccurately reporting participation in another form of employment.
8. The Jendouba governorate comprises 95 imadas in total, 15 of which are classified as urban. These urban imadas were excluded from the evaluation, leading to a sample of 80 rural imadas.
9. Jendouba's rural villages range in size from 1,000 to 7,000 residents. We classified villages with fewer than 2,090 residents as less populated; between 2,095 and 4,156 residents as moderately populated; and more than 4,156 residents as more populated.
10. The randomization of individuals into an offer of employment was conducted in two phases; following the first round of employment offers and the response, in which around 20% of individuals declined to participate, additional replacements were randomly selected and offered employment in order to meet the target participant numbers. Again, all individual who received an offer are coded as treated.
11. In addition, data was collected from a separate random sample of individuals in both treatment and control communities. This data is not employed in this analysis.

12. Data collection was contracted to a local professional team, under close supervision by the research team. The research team also developed all survey instruments and other research protocols as well as the training of field staff (i.e. data enumerators and supervisors).
13. In addition, 62 CWLP households in which the sampled individual was a woman also did not enter the second trial if they were lost to follow-up at the point of cash grant disbursement.
14. The two survey rounds were almost, but not entirely identical. The second round included some minor modifications to portions of survey questions used in the first round as well as a novel module on the COVID-19 pandemic, which did not exist at the time of the first endline survey round.
15. The pre-analysis plan was originally registered with EGAP and can be found on-line, https://osf.io/nd53a, EGAP registration ID 20170520AA.
16. We also report the mean probability that the locality was included in a previous round of public works programming; this probability is between 35% and 48%. The difference is 12 percentage points, but this gap is not statistically significant.
17. Given the absence of a baseline survey, no other data on pre-treatment covariates is available. Marital status is not strictly time-invariant, but given low rates of divorce in rural Tunisia, it is unlikely to show meaningful shifts over time for a predominantly middle-aged population.
18. The first follow-up survey spanned a period of nine months. We simply divide this period into two halves, and control for early and late survey timing.
19. A small number of observations reporting positive savings are winsorized.
20. The specified dimensions were gender, pre-existing levels of wealth/affluence, geographic isolation, project type, and community-level shocks. There is insufficient variation in project type and geographic isolation to pursue this analysis.
21. 99% of those who were identified as eligible in control communities were sampled, a rate that may substantially reflect the fact that the list of eligible individuals in control communities was generated only shortly prior to the follow-up survey.
22. The only available information for those eligible respondents who were not included in the first survey round is the village and the sex of the respondent. Covariates that may explain attrition are used to split the sample into cells. Bounds are calculated separately for each randomization strata. The results are also robust to the inclusion of a binary variable for gender to tighten the bounds.
23. These results are robust when including a female indicator to tighten the bounds.

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Disclosure statement

No potential conflict of interest was reported by the author(s).

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Appendix A. Outcome families reported

In this section, we provide a brief overview of any differences in the outcome families analyzed vis-a-vis the pre-analysis plan that was pre-registered, and as relevant, also note any differences in outcome variables comparing across the first and second follow-up survey rounds. It is important to note that the original PAP also specified that any outcome variable for which 95% or more of the sample provided the same response would be excluded from the analysis, a rule that has been consistently employed here.

Two outcome families included in the originally registered PAP were completely dropped as it was subsequently identified that there was no plausible channel for the intervention to target those outcomes: shocks, and access to basic services. Provision of public works employment would not alter households' exposure to economic and non-economic shocks (though it might alter their response to those shocks, as captured in the variables linked to coping mechanisms), and similarly would not alter their access to health or education services. In addition, one outcome family (intimate partner violence) is omitted from this analysis as it will be reported in a separate paper.

For labor market outcomes, there were five potentially relevant outcome families originally described in the pre-analysis plan (wage employment, other employment, non-agricultural enterprise, other farming activities, and employment and income by other household members). Two of these pre-specified outcome families had insufficient variation to be analyzed (other employment, and non-agricultural enterprise), and the detailed module on farming activities was collected only in the second follow-up survey round. Accordingly, for concision we have collapsed these to two outcome families: respondent labor market outcomes and household labor market outcomes. We also report only the binary variables, rather than the binary and continuous variables.

For consumption, food and non-food consumption have been combined, and there are no other differences vis-a-vis the pre-analysis plan. For assets, there are no difference vis-a-vis the pre-analysis plan. For financial inclusion, this outcome family was originally named debt and savings index. Two of the pre-specified variables (saved money in the last three months, and total amount of savings) were combined into a slightly different question (total money saved over the last 12 months). Two additional questions around debt were added (contracted any debt over the last 12 months, and amount of debt over the last two months).

For human capital, two variables from the originally specified set of outcomes were omitted because there was no plausible channel for the intervention to target these outcomes: literacy of the main respondent, and education level of the main respondent. Given the age of the respondents, they had plausibly completed their educational trajectory considerably prior to the intervention launch.

Table A1. Cross-randomization: female cash grant

	Male	Female: Control	Female: Treatment	Female: Do not enter	Total
Treated	600	361	349	22	1332
Spillover	298	151	131	12	592
Control	352	204	228	10	794
Total	1,250	716	708	44	2718

Notes: This table summarizes the cross-randomization within the sample analyzed in this evaluation and the randomized trial reported in Gazeaud, Khan, et al. (2023).

Table A2. Primary families of outcomes

Outcome family		Indicators
Labor market: main respondent	Primary	Any work (wage or self-employment) (past 4 weeks) Number of days worked in main employment (past 4 weeks) Active employment search (past 4 weeks)
Labor market: other household members	Primary	Any work (wage or self-employment) for household head (past 4 weeks) Any work (wage or self-employment) for any other household member (past 4 weeks)
Consumption	Primary	Value of past-month household consumption in categories: meat and fish; fruit and legumes / vegetables; eggs and milk; oil and fat; beverages; cigarettes and alcohol; other food; healthcare; education; leisure; transportation; electricity / gas / water; communications; cleaning and hygiene items; rent/small repairs; other services
Assets	Primary	Count variables for household ownership of any mode of transportation / vehicle; livestock; furniture; electronic equipment; binary variables for cement or brick wall; cement or tile roof; reports title to home; owns land; also, self-reports three or higher on poverty scale
Financial inclusion	Primary	Amount of savings (past year) Binary variable for any debt (past year) Current debt balance Reports any bank account
Human capital	Primary	Received formal training in a trade Reports skills would like to use in the future

Notes: This table summarizes the variables included in each primary outcome family. For consumption, the survey collected data about food consumption over the past week and non-food consumption over the past month, but both are converted to monthly aggregates for the purposes of analysis. For the financial inclusion index, the variable capturing access to a bank account was reported only in the second follow-up round.

For coping mechanisms, there are no differences vis-a-vis the pre-analysis plan other than that some mechanisms were aggregated up to generate seven variables instead of five. For social cohesion, the only difference vis-a-vis the pre-analysis plan is that migration has been excluded, to be reported in a separate paper; inter-personal trust has also been more appropriately re-named violent conflict.

For psychological well-being, there are no differences vis-a-vis the pre-analysis plan in round one. In round two, four variables are not reported: fear of being exploited, a feeling of

Table A3. Secondary families of outcomes

Outcome family		Indicators
Coping mechanisms (in response to shock)	Secondary	Reduced food consumption Borrowed money from friends, neighbors or cooperatives Received assistance from friends, community, NGO, or government Drew down assets or savings
Social cohesion	Secondary	Community participation and cohesion Collective action Violent conflict inside/outside imada (inverted)
Psychological well being	Secondary	Fear of losing control (inverted) Fear of being exploited (inverted) Feeling of uselessness for others (inverted) Positive relationships between household members Would share with others decision to leave the village Feels accepted within family Feels accepted by other households Feels in control Feels that goals can be accomplished
Women's empowerment and agency	Secondary	Woman reports any earned income over past six months Woman decides how income will be used Man decides alone how income will be used (inverted) Woman reports income generating activity

Notes: This table summarizes the variables included in each secondary outcome family. For the psychological well being index, there were some minor differences in the variables reported across the two follow-up survey rounds. In round two, four variables are not reported: fear of being exploited, a feeling of uselessness for others, a feeling of acceptance within the household, and a feeling of acceptance by other households. The first two variables linked to self-esteem / depression are replaced by two others: a binary variable for feeling depressed, and a binary variable for loss of interest in activities.

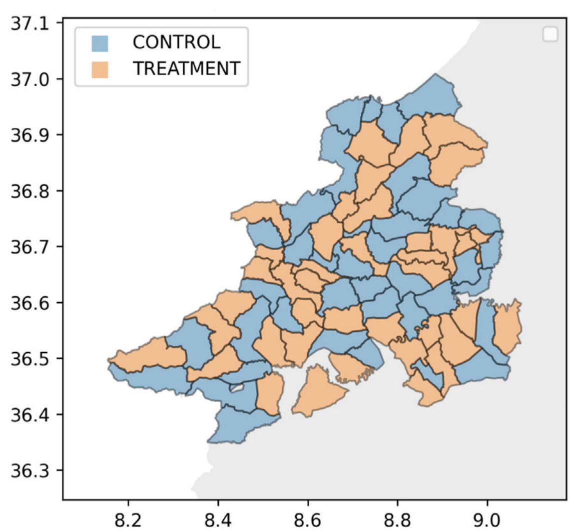

Figure A1. Study locations.

uselessness for others, a feeling of acceptance within the household, and a feeling of acceptance by other households. The first two variables linked to self-esteem / depression are replaced by two others: a binary variable for feeling depressed, and a binary variable for loss of interest in activities.

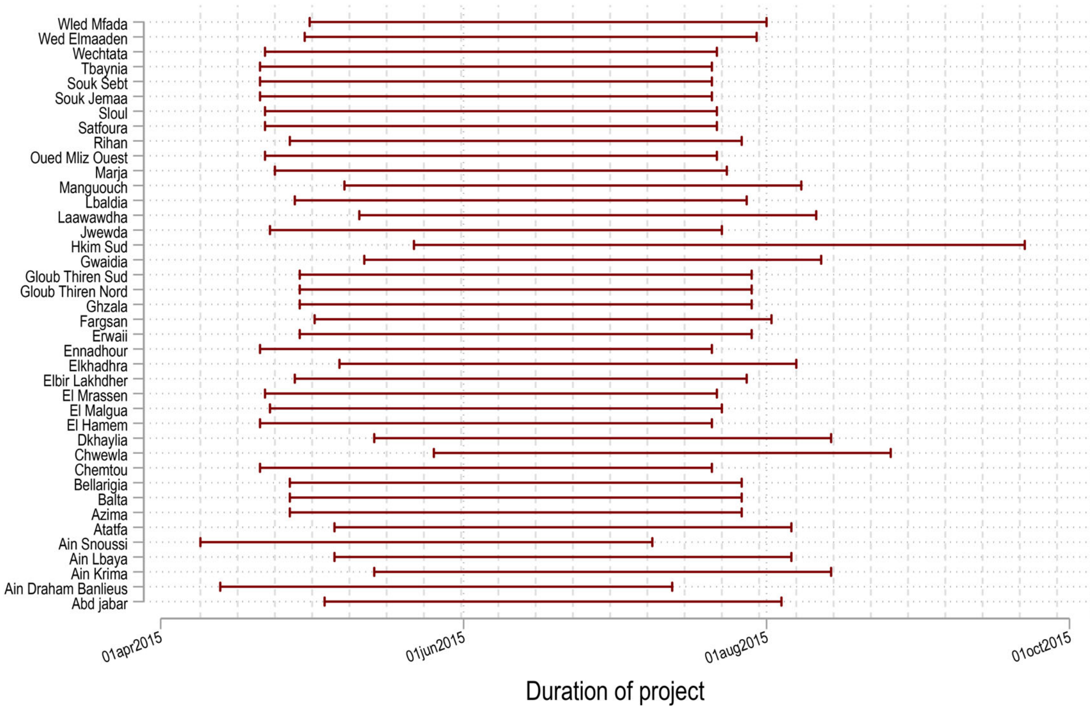

Figure A2. Timeline of the projects.

For women's empowerment and agency, four of the seven variables are reported; the remaining three had an insufficient level of variation.

The designation of primary and secondary outcome families is consistent with the PAP, except that the coping mechanisms outcomes are re-designated as secondary.