Journal of Public Economics 248 (2025) 105420

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/pubec

The employment effects of a means-tested guaranteed income policy ☆

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HIGHLIGHTS

- We study the labor supply response of low-income individuals to a means-tested guaranteed income program through an RCT.
- Assignment to the program reduced labor participation by 22 %.
- Negative employment effects are larger when phase-out marginal tax rates are larger.
- Participation elasticities for a family of four range from 0.39 to 0.49.
- We find tentative evidence of substitution of labor for caregiving.

ARTICLE INFO

JEL classification:
C93
H53
I38
J64

Keywords:
Welfare
Cash transfer
Policy evaluation
RCT
Labor supply

ABSTRACT

Through a randomized controlled trial, we study the labor market effects of a generous means-tested guaranteed income policy targeting low-income families in Barcelona. Two years into the program, beneficiaries are 22 % less likely to work compared to subjects assigned to a control group. A lower phase-out of transfers attenuates negative employment effects and reduces the government cost per euro of benefit by two-thirds. Participation elasticities for a family of four range from 0.39 to 0.49. Treatment effects persisted at least 6 months after the program ended. We find indications that effects are driven by subjects with care duties, suggesting substitution of labor for care tasks.

1. Introduction

Governments worldwide rely on financial transfers to eradicate poverty and fight social exclusion. In high- and middle-income countries, such support typically takes the form of comprehensive welfare programs that provide assistance to families in need. The negative work incentives of such programs are well-described (Moffitt, 2002): Beneficiaries may choose to work less due to the income effect, while substitution effects create further work disincentives if a program is means-tested. To restore work incentives, welfare receipt is often paired with activity requirements, such as job search or training, with benefits reduced for noncompliance. The effects of these activation policies have been widely studied (Card et al., 2017, 2010; McVicar, 2020).

This study focuses on a novel antipoverty program in Barcelona (Spain). We describe the program as novel because it combines elements of both types of schemes mentioned above. The program provides a guaranteed income to households below subsistence level but regardless of

☆ This project received funding from the European Unions' Urban Innovative Actions program (UIA-01031-2016). Ramos acknowledges financial support of projects PID2022-137352NB-C41 (Ministerio de Ciencia e Innovación) and 2021SGR-0189 (Generalitat de Catalunya). Verlaat acknowledges financial support from the Dutch Research Council (406.16.538). Informed consent was obtained from subjects participating in the research.

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https://doi.org/10.1016/j.jpubeco.2025.105420
Received 27 February 2023; Received in revised form 18 May 2025; Accepted 20 May 2025
Available online 7 June 2025

beneficiaries' behavior. Importantly, households also receive the transfer when not working, which makes it different from in-work benefits. Like other welfare programs, the transfer is means-tested and phased out as earnings increase. This feature introduces a negative substitution effect on the intensive margin and reduces returns-to-entry relative to no support. We examine the program's impact on labor supply and related outcomes, including human capital formation and involvement in societal or community organizations.

The program, called B-MINCOME, was designed by the City Council of Barcelona and comprised two components: (i) a household-based cash transfer, adjusted for household income and composition, and (ii) social activation policies. Households qualified for the program if their income and assets fell below a subsistence threshold. The maximum transfer was €1297 ($2055 PPP) for a family of four and €663 ($1051 PPP) for a single-person household, about 70 %–80 % of the local poverty line.[1] Social activation policies promoted social entrepreneurship or community involvement. Some households saw transfers reduced euro-for-euro as income increased, while others faced a withdrawal rate of 25–35 cents per euro earned.

The program was tested in a field experiment in ten disadvantaged neighborhoods of Barcelona between 2017 and 2019, which allows us to leverage experimental data. There is no pre-analysis plan or preregistration.[2] We follow 1200 households from the program's start until 6 months after the last transfer. Data come from baseline and endline surveys, complemented by employment information from social security records. We focus on extensive margin responses, as data on earnings and hours worked are not available.

Our main finding is that the program had sizeable negative employment effects on average. Roughly 2 years after the program began, main recipients in treatment households are 22 % less likely to work compared to their control counterparts. Negative employment effects also occur at the household level. Job search, social participation, and investment in education appear unaffected. Notably, negative employment effects persisted for at least 6 months after the transfers ended. Our heterogeneity analysis suggests substitution of labor for caregiving tasks: Female beneficiaries seem to respond strongly to the program, as do beneficiaries with children.

The overall employment effect reflects the combined impact of different program modalities. Disentangling these effects, we find no significant differences between receiving only transfers and transfers combined with social activation plans, possibly due to limited enforcement. As expected, the withdrawal scheme matters, as negative employment effects roughly double under a 100 % phase-out. As a result, the government cost per euro of benefit is three times higher under full withdrawal—34 cents compared to 12 cents.

We relate our estimates to stylized net-of-tax rates to calculate participation elasticities. The estimated participation elasticity for a family of four ranges from 0.39 to 0.49 when accounting for part-time entries. These estimates align with participation elasticities observed in other programs. For example, Chetty et al. (2013) report participation elasticities of 0.42 for a tax holiday in Iceland (Bianchi et al., 2001) and 0.38 for an earnings subsidy for single mothers in Canada (Card and Hyslop, 2005). We also estimate income and substitution elasticities at the extensive margin. We find an income elasticity of –0.07 and a substitution elasticity of 0.15. The income elasticity parameter suggests that a €100 increase in treatment income is associated with a 1 percentage point reduction in employment probability.

Few studies have evaluated transfer programs in high- and middle-income countries that do not impose behavioral conditions on beneficiaries. Recent evidence comes from two well-known dividend programs. Jones and Marinescu (2022) find that the Alaska Permanent Fund Dividend had no effect on aggregate employment but increased part-time work, while Akee et al. (2010) report no impact on adult employment from the Eastern Band of Cherokee Indians Casino Dividend. Both programs differ from B-MINCOME in that they offer smaller payouts and lack means-testing and benefit phase-outs.

Additional evidence comes from Europe. In Finland, replacing minimum unemployment benefits with an unconditional transfer of equal size had no effect on employment outcomes (Hämäläinen et al., 2022). In Italy, a yearly cash transfer targeting low-income subjects showed no labor supply effects, except when combined with a mentoring course (Del Boca et al., 2021; Aparicio Fenoll and Quaranta, 2022). Again, these interventions differ from B-MINCOME by offering smaller transfers and lacking phase-out mechanisms.

The limited labor supply effects in these studies may stem from relatively low transfer amounts, which are unlikely to replace other income, and the absence of means-testing, which reduces substitution effects. A separate literature studies lottery winners, where larger income shocks generate stronger income effects, though these transfers also lack phase-outs. These studies consistently find that large unearned income transfers reduce hours worked but have limited effects on labor force participation (Cesarini et al., 2017; Imbens et al., 2001; Golosov et al., 2024). While valuable for understanding household responses to unearned income, lottery winners differ substantially from the low-income populations targeted by welfare programs.

Programs closer to B-MINCOME include the Assistance to Families with Dependent Children (AFDC) and, especially, the negative income tax (NIT) experiments of the 1970s, which featured guaranteed levels of 50 %–140 % of the poverty line and implicit marginal tax rates of 30 %–80 %. The results of both programs align with our findings. In the AFDC program, reducing the implicit phase-out tax rate increased labor supply on average (Chan and Moffitt, 2018).[3] Studies of the NIT experiments report extensive margin effects similar to ours: −22.5 % for wives and −3.5 % for husbands, averaged across all sites (Robins, 1985).[4] Lastly, Ananat et al. (2024) find no employment effects from eliminating the 15 % phase-in tax rate in the 2021 temporary expansion of the Child Tax Credit (CTC), which, like B-MINCOME, targeted low-income families.

We advance the literature on employment responses to transfer policies in various ways. First, we describe the effects of a recent and comparatively generous transfer program targeting low-income households. To the best of our knowledge, B-MINCOME—along with some NIT experiments—is the only cash transfer in a high- and middle-income country that offers generous support without any strings attached.[5] Randomized benefit phase-out allows us to contrast different design choices in addition to studying overall effects. By exploiting a randomized design, we also extend the body of experimental evidence in the field. Importantly, combining social security data with self-reported information from surveys, helps circumvent internal validity concerns encountered by earlier studies. Lastly, post-treatment data allow us to document the persistence of effects after the program ends.

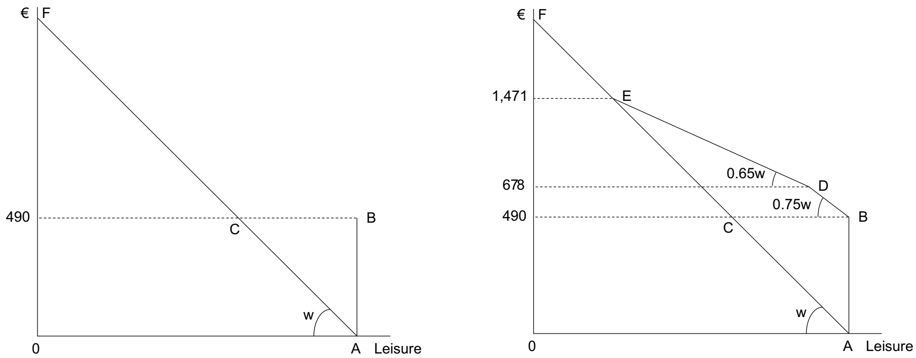
[1] We convert euros into purchasing power-adjusted U.S. dollars using the OECD purchasing power parity (PPP) exchange rate (OECD, 2021b).

[2] External researchers (Ramos and Verlaat) got involved in the evaluation after the trial began, at which point the experimental design and surveys had already been finalized.

[3] As noted by Chan and Moffitt (2018), a limitation is that changes in implicit tax rates were combined with changes in other program features, such as time limits or work requirements, which makes it difficult to isolate effects.

[4] Follow-up studies suggest potentially exaggerated effects due to misreporting and selective attrition (Ashenfelter and Plant, 1990; Burtless, 1986). Recent work suggests randomization issues at some sites and finds no labor supply effects for single mothers at sites with valid random assignment (Riddell and Riddell, 2024).

[5] B-MINCOME lacks two restrictions common in other programs. First, it does not require specific behaviors such as school attendance, health check-ups, or employment, unlike conditional cash transfers. Second, it imposes no restrictions on how recipients spend the money, unlike food stamps or housing assistance.



(a) Full Withdrawal Rate (b) Partial Withdrawal Rate

Fig. 1. Stylized budget constraints with full and partial withdrawal rates.
Note: Average transfer in the first 3 months. Amounts rounded.

2. Policy context

2.1. Treatment program

The treatment we studied is B-MINCOME, a municipal antipoverty program launched by the City Council of Barcelona in December 2017. The program aimed to combat poverty and social exclusion by improving households' socio-economic situations.[6] It comprised two components: (i) income support, the *Municipal Inclusion Support Benefit* (*SMI benefit*), and (ii) social activation. The program provided support without imposing behavioral conditions common in other welfare schemes, such as active job search.

The purpose of B-MINCOME was to supplement existing income support schemes, which often exclude individuals in low-wage or small and unstable employment due to stringent eligibility criteria. Notably, only 9 % of B-MINCOME participants received the regional social assistance benefit (RGC) at the start of the program. Before roll-out, the program was tested in a 2-year randomized controlled trial, which we describe in Section 3.

The SMI benefit provided monthly payments to lift household income to an imputed subsistence level. The benefit level varied by household income and composition, as detailed in Appendix A. Payments were made to one designated household member selected by the household (*main recipient*), with other household members treated as joint beneficiaries.

The maximum transfer was €1297 ($2055 PPP) for a family of four and €663 ($1051 PPP) for a single-person household.[7] The maximum benefit levels correspond to 70 %–80 % of the local poverty line. This makes B-MINCOME comparable in generosity to the NIT experiments, which offered guarantee levels between 50 %–140 % of the poverty level for a family of four (Robins, 1985). Other comparable programs have lower guarantee levels than B-MINCOME, ranging between €292 (Turin Cash Transfer), $325 (Alaska Permanent Fund), and €560 (Finnish Basic Income) a month.

Payments responded to changes in household post-tax income and composition.[8] For some households, additional income reduced the transfer euro-for-euro. Other households faced a 23 %–35 % withdrawal rate: Each additional euro up to €250 ($396 PPP) per month reduced the benefit by 25 cents, and each euro above €250 by 35 cents. We refer to these schemes as *full withdrawal* and *partial withdrawal* schemes. The weighted average withdrawal rate was 59 %, similar to the average tax rate of 50 % in the NIT experiments (Robins, 1985).

2.2. Work incentives

Fig. 1 shows stylized budget constraints for the control group and the two withdrawal schemes, assuming a transfer of €490 ($777 PPP), the average transfer in the first 3 months. The line ACF, with a slope equal to the wage, represents the budget constraint of a control individual. Panel (a) includes the full withdrawal group. Total income equals €490 if the individual does not work or earns less than €490, and only surpasses the transfer when labor income exceeds €490, as shown by line CF.

When the withdrawal rate is lower (25 %–35 %), as shown in Panel (b), total income exceeds the transfer as soon as labor income is positive. Now, the budget constraint is represented by line ABDEF. The change in withdrawal rate for earnings above €250 leads to a kink in the budget constraint at €678. The breakeven level, the income that reduces the transfer to zero, is €1471.[9] Under full withdrawal, the breakeven income always equals the transfer level due to the 100 % phase out.

To further illustrate work incentives we calculate participation tax rates (PTRs) for the two withdrawal schemes. For simplicity, we assume households receive no transfers beyond the average B-MINCOME transfer and transition to earning the monthly minimum wage, either full-time or part-time.[10] Stylized control households pay no income tax, due to generous tax allowances for low-income earners, resulting in a PTR of zero. Assignment to the program raises PTRs due to benefit withdrawal. Under partial withdrawal, PTRs are roughly 32 %, regardless of whether work is full-time or part-time. Under full withdrawal, the PTR rises to 47 % for full-time work and 65 % for part-time work.

[6] The trial targeted ten of the poorest neighborhoods in Barcelona, representing roughly 7 % of the city's population of 114.000 inhabitants (Ciutat Meridiana, Vallbona, Torre Baró, Roquetes, Trinitat Nova, Trinitat Vella, Baró de Viver, Bon Pastor, Verneda-La Pau, and Besòs-Maresme). Panel (a) of Fig. I.4 in Appendix I highlights these neighborhoods on a map showing the share of households earning less than €5000 ($7925 PPP) annually. Panel (b) displays the mean annual household income per capita. The maps extend into neighboring communities in the northeast, emphasizing the relative disadvantage of the target area compared to close urban areas outside Barcelona.

[7] The benefit was capped at €1676 ($2586 PPP). For comparison, the monthly minimum wage was €826 ($1309 PPP) at the start of the program and €1050 ($1664 PPP) when the program ended. The average monthly wage of a full-time private-sector employee was €2234 ($3541 PPP), according to OECD estimates (OECD, 2021a).

[8] The transfer depended on household size, providing incentives for household formation or dissolution. However, we observe no significant changes in household size as a result of treatment. Regression results are available upon request.

[9] The breakeven level is the income x that solves: $490 = 0.25 * 250 + 0.35 * (x - 250)$.

[10] Calculations use the tax code for a family of four, though tax rates low-income singles are very similar. For the part-time entry margin, we assume 57 % of the entry wage equals half the full-time minimum wage. These parameters reflect the baseline share of part-time workers and the average part-time factor.

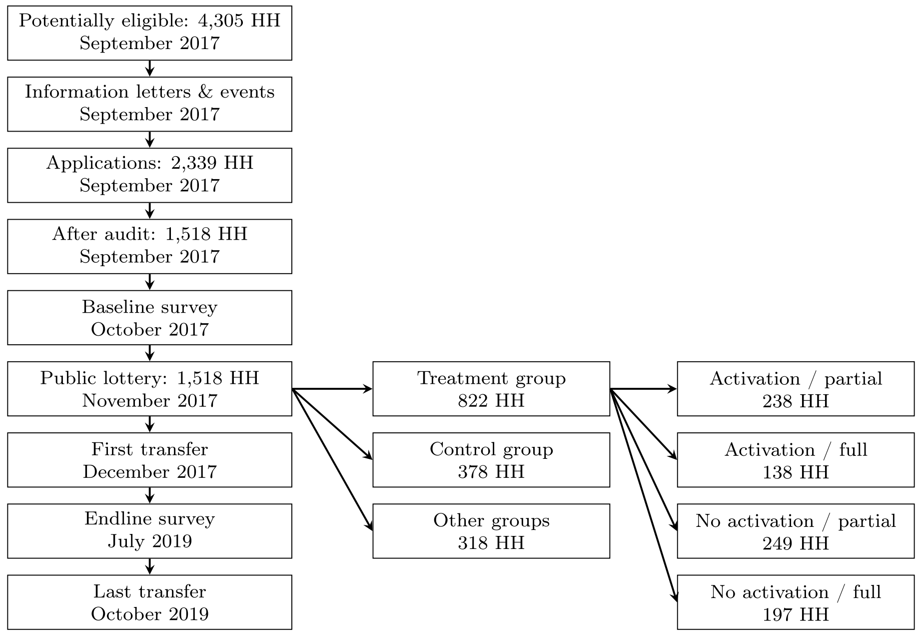


Fig. 2. Study timeline and experimental groups.

2.3. Activation plans

B-MINCOME included four activation plans, but we focus on two plans only. The first plan promoted community involvement, i.e., taking part in the social and community life of the neighborhood. Participation in this plan was voluntary and not monitored. Participants in the second plan trained to become social entrepreneurs or gained work experience in a social entrepreneurship initiative. Participation in this activity was meant to be compulsory for half of the group, but this aspect was eventually dropped.

Only one person per household could participate in the activities, and participants could be other household members than the main recipient. We describe both plans in detail in Appendix C. The other two plans, which promoted housing renovations for room rentals and offered vocational training, are excluded from our analysis due to implementation issues. Excluding these households is unproblematic due to random assignment.

2.4. Eligibility

Participation was voluntary and required an application. Households had to meet six eligibility criteria: (1) at least 2 years of registered residence in Barcelona and current residence in the target area; (2) an open file with the municipal social services office, required for legal transfer of funds; (3) at least one household member aged 25–60; (4) shared household expenses; (5) household income below a subsistence threshold; and (6) assets—excluding the primary residence—below four times the maximum annual SMI benefit.

3. Experimental design and data

3.1. Sampling

Participants were recruited from households in ten target neighborhoods (see Section 2.1). Recruitment took place over 2 months (September–October 2017) and followed three steps. First, the Municipality of Barcelona identified 4305 households likely to meet eligibility criteria using municipal social services records. This is roughly 10 % of all households in the target area. Second, these households received invitation letters with program details and information about local events, where social workers explained the program and answered questions. Around 400 events were held, and 80 % of applications were submitted on-site. As part of the application, households provided informed consent for follow-up surveys. Third, the municipality reviewed all applications to verify eligibility.

Of the 4305 households invited, 2339 (54 %) applied for the program, and 1518 (35 %) met the eligibility criteria.[11] All 1518 eligible households were enrolled in the trial and approached for the baseline survey. Fig. 2 provides a timeline illustrating the recruitment process.

Unfortunately, we are unable to investigate selection into application due to a lack of data on non-applicants. Laín and Julià (2024), who had access to such data, identified three factors associated with a higher likelihood of applying: being younger, being foreign-born, and having children. Other socioeconomic characteristics, including gender, educational attainment, household size, household income, and benefit receipt, were found to be unrelated.

3.2. Randomization

Households were assigned to experimental conditions through a public lottery conducted after the baseline survey. Participants were informed about their assignment via SMS. The lottery followed a stratified randomization design based on two variables: (i) the expected size of the SMI benefit (low, medium, or high), and (ii) whether at least one household member was employable (yes or no).[12] In Appendix B we describe the randomization mechanism in detail. Table B.11 reports the number of households per stratum.

Of the 1518 households included in the lottery, 822 were assigned to the treatment group, 378 to the control group, and 318 to groups outside the scope of this study.[13] Control households received no intervention and were only surveyed. Treatment households were randomly allocated

[11] The relatively high proportion of ineligible households is related to outdated or incomplete information in the municipal social services records.

[12] Low: up to 50 % of the maximum benefit; medium: 50 %–75 %; high: more than 75 %. A seventh stratum—households eligible for renovations—is excluded from the study (see Section 2.3).

[13] These 318 households include 24 assigned to a renovation plan, 150 assigned to vocational training, and 144 assigned to a reserve pool—all excluded from the analysis.

to different arms testing program modalities, that is, activation policies and benefit withdrawal rates.[14] Fig. 2 details the number of households per treatment combination. Treatment arms were cross-randomized except for the social entrepreneurship program, which only had a partial withdrawal option.[15]

3.3. Implementation

Treatment households participated for 23 months, receiving transfers from December 2017 to October 2019. Payments went to the main recipient's bank account. Households had to report changes in income and household composition to recalculate benefit levels, submitting bank statements as proof. Benefits were determined every quarter using post-tax income. Over- and underpayments from the previous quarter were settled in equal parts with upcoming payments. Procedures were explained via letters and information events pre-treatment (see Section 3.1).

Treatment households received an average monthly transfer of €422 ($668 PPP) during the trial, which roughly equals the average monthly household income pre-treatment.[16] Around 14 % of households received no payments due to non-take-up (discussed in Section 4.2). Conditional on take-up, the average monthly transfer was €492 ($779 PPP) (Median = €464; *SD* = €286), with a maximum of €1500 ($2376 PPP). Per capita, households received €127 ($201 PPP) per month on average. Figs. I.5 and I.6 in Appendix I illustrate the development of payments over time and the distribution of transfers, respectively.[17]

3.4. Survey data and waves

Our primary data source is surveys. The baseline survey took place in October 2017, between enrollment and randomization. At this point, respondents knew they were in the trial but had not yet been assigned to a group. The survey was administered via telephone interviews (CATI) by an independent bureau. Follow-up surveys were conducted one year in (October 2018) and 3 months before the program ended, using both CATI and personal interviews to reach harder-to-contact households. For our analysis, we only use baseline and endline data, as midline data were not accessible.

Surveys included modules on background characteristics and time use, including work, job search, social participation, and education or training. These two modules are central to this study. Additional modules covered deprivation, health, and households' financial situation, but not income. Surveys addressed two levels of observation: the household and the individual. Only main recipients were interviewed, reporting on themselves, their household, and other household members. For other household members, questions were limited to factual details, such as age or labor market status.

3.5. Administrative data sources

We complement survey data with administrative data where available. This includes household income and municipal transfers received in the 12 months before treatment, as detailed in the municipal benefits registry. Municipal transfers cover areas like education, housing, healthcare, transport, and child support. We also observe district of residency and receipt of Catalonia's guaranteed citizenship income (RGC) at the time of recruitment. Unfortunately, income reports during the trial are unavailable.

We also use individual employment data from social security records, covering June 2019–April 2020. This period captures the final five treatment months and 6 months after. About 97 % of subjects could be matched to the records, with similar rates across groups (see Table 3). Social security records only include employed individuals, not the self-employed. The data detail employment status (employed or not) on reference dates roughly 10 days apart. Unfortunately, the records lack further details like hours worked, earnings, or contract type.

3.6. Outcome variables

We construct various variables from survey data both at the main recipient level and at the household level. For a detailed description, see Table D.13 in Appendix D.

Our primary labor market outcome is a dummy indicating whether the main recipient was *working* when surveyed. To decompose treatment effects, we create three sets of dummies: (i) whether the main recipient was *employed* or *self-employed*, (ii) whether they worked *full-time* or *part-time*, and (iii) whether they held a *permanent* or *temporary* contract. Two variables measure labor market outcomes at the household level: (i) the *number of adult household members* working (ages 18–65), and (ii) a dummy indicating whether *at least one adult household member* was working.

Additional variables capture activities related to job search, social participation, and human capital formation. We include dummies indicating whether (i) the main recipient *looked for paid work* in the past 4 weeks, (ii) participated in a *civil society organization* in the past year, or (iii) followed a *study or vocational training* in the past year.[18] Household-level variables include the *number of adult household members* engaged in study or vocational training and a dummy indicating whether *at least one adult household member* followed a study or vocational training.

Lastly, we construct an additional labor market outcome using administrative data. We aggregate the 10-day observation intervals into monthly observations, which creates eleven dummy variables, each indicating whether the main recipient was *employed at least once* during the respective month (June 2019–April 2020). This administrative measure allows us to address concerns about the self-reported nature of our survey data.

3.7. Baseline balance

We assess baseline balance for three sets of variables: (i) individual and household covariates from the baseline survey, (ii) pre-treatment income and welfare dependency from municipal records, and (iii) baseline survey outcomes. Baseline survey data limit the sample to 1034 households (86 %). We compare control and treatment groups, including the four possible treatment combinations (activation/no activation × partial/full withdrawal). Specifications are detailed in Appendix E. Table 1 shows that baseline response rates and key covariates are generally balanced across groups. Minor differences, such as for household size and composition, fall within the range expected by chance. Joint orthogonality tests confirm overall balance.

The table also illustrates household demographics. Households are mostly families with children and average four members—larger than the average household at risk of poverty in Barcelona.[19] Half of all households have no education beyond compulsory schooling (primary or lower secondary). Main recipients are mostly female (73 %) and average

[14] A third modality distinguished obligatory from optional activation plans. Due to signals that mandatory participation was not enforced, we treat all plans as optional.

[15] In Section 6, we show that excluding subjects assigned to the social entrepreneurship program as part of a robustness check does not affect the main results.

[16] In the second year, 25 % of the benefit was paid in RECs—a local digital currency usable in 82 nearby businesses. Pegged to the euro and app-based, the REC was widely accepted: 89 % found the app easy to use, and 99 % knew where to spend RECs (Belmonte, 2019).

[17] Unfortunately, we lack data on household income and other transfers during treatment, limiting assessment of balance sheet effects or transfer substitution. However, pre-treatment take-up of key alternative benefits was low, suggesting minimal substitution.

[18] Civil society organizations include neighborhood organizations, school groups, non-profits, religious groups, political parties, or voluntary work.

[19] Data on the population at risk of poverty in Barcelona stems from the 2016 EU-SILC survey, which included a proprietary Barcelona sample.

Table 1
Baseline balance: covariates.

	Control mean (SD)	Treatment group	Activation		No activation		N
			Partial	Full	Partial	Full	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Baseline response	0.873	−0.006	−0.005	−0.029	0.001	0.001	1200
	(0.333)	(0.022)	(0.029)	(0.036)	(0.029)	(0.030)	
		[0.789]	[0.858]	[0.431]	[0.969]	[0.984]	
Main recipient female	0.727	−0.007	0.026	−0.028	0.004	−0.046	1034
	(0.446)	(0.031)	(0.040)	(0.050)	(0.040)	(0.044)	
		[0.818]	[0.517]	[0.580]	[0.927]	[0.294]	
Main recipient age	40.612	0.070	0.589	−0.073	−0.395	0.119	1034
	(8.534)	(0.580)	(0.777)	(0.915)	(0.773)	(0.763)	
		[0.904]	[0.449]	[0.936]	[0.609]	[0.876]	
No. members	4.112	0.210	0.290	0.045	0.207	0.230	1034
	(1.585)	(0.099)	(0.140)	(0.142)	(0.128)	(0.140)	
		[0.035]	[0.039]	[0.754]	[0.105]	[0.099]	
No. members <16	1.727	0.090	0.113	0.041	0.130	0.045	1034
	(1.210)	(0.077)	(0.108)	(0.116)	(0.100)	(0.108)	
		[0.247]	[0.297]	[0.724]	[0.194]	[0.680]	
No. members 16–64	2.315	0.107	0.161	−0.003	0.058	0.177	1034
	(1.102)	(0.074)	(0.103)	(0.110)	(0.096)	(0.103)	
		[0.152]	[0.117]	[0.981]	[0.551]	[0.088]	
Single-person hh	0.033	−0.011	−0.005	0.005	−0.026	−0.011	1034
	(0.180)	(0.012)	(0.016)	(0.021)	(0.014)	(0.017)	
		[0.348]	[0.742]	[0.819]	[0.051]	[0.514]	
Single-parent hh	0.155	−0.025	−0.013	−0.064	−0.011	−0.031	1034
	(0.362)	(0.025)	(0.033)	(0.035)	(0.033)	(0.034)	
		[0.317]	[0.686]	[0.068]	[0.741]	[0.356]	
Adults without children	0.133	0.005	0.014	−0.012	0.007	0.002	1034
	(0.340)	(0.023)	(0.032)	(0.036)	(0.030)	(0.032)	
		[0.838]	[0.669]	[0.745]	[0.814]	[0.948]	
Adults with children	0.679	0.032	0.005	0.071	0.030	0.040	1034
	(0.468)	(0.031)	(0.042)	(0.047)	(0.041)	(0.043)	
		[0.310]	[0.908]	[0.137]	[0.459]	[0.360]	
Compulsory education or less	0.491	0.010	0.008	0.011	0.026	−0.010	1034
	(0.501)	(0.034)	(0.045)	(0.055)	(0.045)	(0.048)	
		[0.779]	[0.859]	[0.842]	[0.555]	[0.832]	
Secondary education	0.421	−0.024	−0.006	−0.033	−0.042	−0.017	1034
	(0.495)	(0.034)	(0.044)	(0.053)	(0.043)	(0.047)	
		[0.479]	[0.889]	[0.539]	[0.336]	[0.718]	
Tertiary education	0.088	0.014	−0.002	0.022	0.015	0.027	1034
	(0.284)	(0.020)	(0.026)	(0.033)	(0.026)	(0.030)	
		[0.468]	[0.944]	[0.515]	[0.555]	[0.358]	
All hh members Spanish	0.455	−0.007	−0.010	−0.040	0.043	−0.042	1034
	(0.499)	(0.034)	(0.045)	(0.053)	(0.044)	(0.046)	
		[0.841]	[0.826]	[0.446]	[0.336]	[0.367]	
No hh member Spanish	0.230	−0.002	0.005	−0.016	−0.038	0.044	1034
	(0.422)	(0.029)	(0.037)	(0.045)	(0.036)	(0.041)	
		[0.945]	[0.896]	[0.715]	[0.291]	[0.284]	
Mixed nationalities	0.315	0.009	0.005	0.056	−0.005	−0.002	1034
	(0.465)	(0.031)	(0.041)	(0.052)	(0.041)	(0.044)	
		[0.782]	[0.904]	[0.274]	[0.905]	[0.958]	
Monthly hh income	419.268	26.872	41.712	15.555	24.340	19.770	1034
	(379.561)	(19.279)	(26.366)	(28.112)	(25.212)	(27.974)	
		[0.164]	[0.114]	[0.580]	[0.335]	[0.480]	
Monthly transfers	175.342	9.556	10.123	2.177	23.508	−3.514	1034
	(189.448)	(12.674)	(16.868)	(16.479)	(17.406)	(18.383)	
		[0.451]	[0.549]	[0.895]	[0.177]	[0.848]	
RGC recipient	0.058	0.019	0.046	0.030	−0.037	0.048	1034
	(0.233)	(0.018)	(0.027)	(0.032)	(0.020)	(0.028)	
		[0.293]	[0.093]	[0.349]	[0.064]	[0.090]	
Joint test (p-value)		0.951	0.720	0.893	0.503	0.819	

Note: Differences in covariates at baseline between treatment and control groups. Column 1 reports control group means with standard deviations in parentheses. Column 2 shows coefficients on the treatment dummy, estimating Eq. (E.1). Columns 3–6 report coefficients on dummies for each treatment interaction, estimating Eq. (E.2). We report robust standard errors in parentheses and p-values in brackets. The second panel includes survey data and the third panel administrative data. The sample is restricted to baseline respondents. The last row reports p-values from a joint hypothesis test. See Table D.12 in Appendix D for a description of variables.

41 years. Administrative records indicate an average monthly income of €400 ($634 PPP), about 25 % of the poverty line for a family of four. Despite this, few households received social assistance (RGC). Balance checks using administrative data on the full sample confirm baseline balance (see Table E.14).

Table 2 presents baseline survey outcomes for main recipients (upper panel) and households (lower panel). A few significant differences emerge for social participation and recent education, mainly among those assigned to activation with partial withdrawal. Joint significance is limited to that group ($p = 0.014$). Overall, the balance results suggest

Table 2
Baseline balance: survey outcomes.

	Control mean (SD)	Treatment group	Activation		No activation		N
			Partial	Full	Partial	Full	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Working	0.400	−0.025	−0.047	−0.021	0.019	−0.055	1032
	(0.491)	(0.034)	(0.044)	(0.054)	(0.044)	(0.046)	
		[0.459]	[0.285]	[0.691]	[0.668]	[0.237]	
Job search past 4 w	0.506	−0.008	0.054	−0.034	−0.011	−0.063	1031
	(0.501)	(0.033)	(0.044)	(0.053)	(0.044)	(0.047)	
		[0.809]	[0.214]	[0.525]	[0.805]	[0.180]	
Social participation	0.345	0.073	0.140	0.080	0.024	0.049	1034
	(0.476)	(0.033)	(0.044)	(0.053)	(0.043)	(0.046)	
		[0.027]	[0.002]	[0.132]	[0.576]	[0.293]	
Education past 12 m	0.203	0.047	0.098	0.051	0.001	0.038	1034
	(0.403)	(0.028)	(0.039)	(0.047)	(0.036)	(0.040)	
		[0.099]	[0.013]	[0.273]	[0.968]	[0.344]	
No. working	0.755	0.017	0.035	0.037	0.022	−0.022	1034
	(0.770)	(0.054)	(0.073)	(0.090)	(0.068)	(0.071)	
		[0.746]	[0.636]	[0.682]	[0.746]	[0.753]	
At least one working	0.579	0.005	−0.015	−0.008	0.033	0.001	1034
	(0.495)	(0.034)	(0.045)	(0.054)	(0.044)	(0.047)	
		[0.894]	[0.738]	[0.878]	[0.457]	[0.975]	
No. in education	0.576	−0.012	0.074	0.039	−0.124	−0.012	1034
	(0.852)	(0.055)	(0.072)	(0.086)	(0.067)	(0.074)	
		[0.821]	[0.306]	[0.653]	[0.064]	[0.869]	
At least one in education	0.418	−0.009	0.049	0.026	−0.080	−0.014	1034
	(0.494)	(0.034)	(0.044)	(0.055)	(0.043)	(0.047)	
		[0.797]	[0.267]	[0.630]	[0.064]	[0.772]	
Joint test (p-value)		0.134	0.014	0.867	0.287	0.362	

Note: Differences in outcomes at baseline between treatment and control groups. Column 1 reports control group means with standard deviations in parentheses. Column 2 shows coefficients on the treatment dummy, estimating Eq. (E.1). Columns 3–6 report coefficients on dummies for each treatment interaction, estimating Eq. (E.2). We report robust standard errors in parentheses and p-values in brackets. The sample is restricted to baseline respondents. The last row reports p-values from a joint hypothesis test. See Table D.13 in Appendix D for a description of variables.

the public lottery was successfully implemented and not compromised by baseline non-response. To account for residual imbalances, our preferred specification conditions on baseline outcome values and covariates. For comparison, we also report unadjusted estimates. Additionally, we assess potential bias from imbalances in unobservables using the method proposed by Oster (2019).

4. Experiment integrity

4.1. Survey attrition

Survey participation was not obligatory, raising concerns about potential attrition bias in survey outcomes. To diagnose this risk, we follow a three-step approach: (i) we test whether endline response is correlated with treatment status, (ii) compare baseline characteristics between attritors and non-attritors, and (iii) check whether endline respondents differed across treatment and control groups at baseline.

Overall, 75 % responded at endline (904 households), and 66 % completed both the baseline and endline surveys (790 households). Table 3 shows that treatment subjects were 15 percentage points more likely to respond at endline. This is plausible, as transfer receipt may increase program attachment. Despite this, additional analyses suggest attrition is unlikely to bias our results.

We find no evidence for strong selection into attrition (see Tables F.15–F.17 in Appendix F). Attrition households are more likely to be childless, male recipients, and with lower pre-treatment benefits and income. Baseline outcomes do not differ significantly between attritors and non-attritors. Balance tests for endline respondents show no major imbalances in covariates (see Tables F.18–F.19). Balance is confirmed by joint orthogonality ($p = 0.775$). Minor imbalances in baseline outcomes mirror those observed at baseline (see Table F.20). Lastly, we evaluate missingness in administrative outcome data due to unmatched records, which is minimal and quite symmetric across treatment conditions (see lower panel of Table 3).

4.2. Compliance

We assess program take-up overall and separately for the transfer and activation components. Of the 822 households assigned to treatment, 87 % (717 households) actually participated, while the rest were excluded before the program. Table H.24 in Appendix H lists reasons for exclusion, while Table H.25 shows that take-up rates are comparable across treatment arms. Non-participation was more common among households with lower pre-treatment benefits and stronger labor market attachment, suggesting limited perceived advantage in joining the program (see Tables H.26–H.28). All eligible households actually received the SMI benefit, while no control households received transfers. Roughly two-thirds of households participated in their assigned activation plan, with similar rates for community involvement (65.5 %) and social entrepreneurship (66.7 %). No other households joined an activation plan. To account for noncompliance, we estimate intent-to-treat (ITT) effects, comparing households based on their original assignment regardless of actual participation.

5. Empirical strategy

We are interested in the overall impacts of the B-MINCOME program and the effects of different program modalities, as implemented across the treatment arms. To evaluate the program's overall impact, we estimate the following specification:

$$Y_{hE} = \alpha + \beta T_h + X_h' \Theta + \Psi Y_{hB} + \Phi M_{hB} + \nu + \gamma + \epsilon_h \tag{1}$$

In this equation, Y_{hE} is the endline outcome for household h and T_h is a binary indicator for treatment assignment, with the control group as the reference category. The parameter of interest, β, captures the intent-to-treat (ITT) effect. To improve precision, we include a vector of covariates, X_h', which contains the variables listed in Table 1. Following McKenzie (2012), we also condition on the baseline value

Table 3
Observed subjects across treatment conditions.

	Control mean (SD)	Treatment group	Activation		No activation		N
			Partial	Full	Partial	Full	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Endline	0.651	0.152	0.120	0.185	0.199	0.107	1200
	(0.477)	(0.029)	(0.037)	(0.041)	(0.034)	(0.040)	
		[0.000]	[0.001]	[0.000]	[0.000]	[0.007]	
Baseline + endline	0.585	0.116	0.101	0.113	0.151	0.094	1200
	(0.493)	(0.031)	(0.040)	(0.047)	(0.039)	(0.043)	
		[0.000]	[0.013]	[0.017]	[0.000]	[0.029]	
Match admin records	0.952	0.009	0.002	0.011	0.003	0.025	1200
	(0.213)	(0.013)	(0.017)	(0.018)	(0.017)	(0.015)	
		[0.483]	[0.914]	[0.562]	[0.835]	[0.093]	

Note: Differences in survey response rates and matches with social security records between treatment and control groups. Column 1 reports control group means with standard deviations in parentheses. Column 2 shows coefficients on the treatment dummy, estimating an adapted version of Eq. (E.1). Columns 3–6 report coefficients on dummies for each treatment interaction, estimating an adapted version of Eq. (E.2). We report robust standard errors in parentheses and p-values in brackets.

of the outcome, Y_{hB}, replacing missing values with 0 and adding a missingness indicator M_{hB}. When estimating effects on administrative outcomes, Y_{hB} and M_{hB} are excluded, as no baseline data are available. We further control for the survey mode with ν, and randomization strata with γ. ϵ_h is the error term. To assess the sensitivity of our results, we also report estimates of simplified specifications, omitting X'_h, or also Y_{hB} and M_{hB}.

The overall effect of the program averages the effects of different program modalities. To disentangle these effects, we estimate a fully interacted model that includes dummy variables for each of the four treatment combinations (activation/no activation × partial/full withdrawal). In this specification, G_h^x denotes the dummy variables for the four treatment cells, with the control group serving as the reference category:

$$Y_{hE} = \alpha + \sum_{x=1}^{4} \beta_x G_h^x + X'_h\Theta + \Psi Y_{hB} + \Phi M_{hB} + \nu + \gamma + \epsilon_h \tag{2}$$

In addition to average effects, we also study effect heterogeneity by interacting the treatment dummy with a subgroup indicator:

$$Y_{hE} = \alpha + \beta_1 T_h + \beta_2 S_h + \beta_3 T_h S_h + X'_h\Theta + \gamma + \epsilon_h \tag{3}$$

In this equation, S_h represents a dummy variable identifying the subgroup of interest. All other terms remain the same as in Eq. (1). The number of controls reduces to individual covariates and randomization strata fixed effects, as we use our administrative outcome measure for reasons of statistical power. The parameter of interest is β_3, which captures the difference in ITT effects between units within and outside the subgroup.

While our tables in the main section show naive p-values, we report p-values adjusted for multiple hypothesis testing in Appendix H. We correct our p-values for testing eight hypotheses, due to eight main outcomes. We follow the free step-down methodology of Westfall and Young (1993), using 5000 bootstrap draws. The adjustment is implemented with the user-written Stata package *wyoung* (Jones et al., 2019).

6. Results

6.1. Overall impact of the program

Table 4 reports estimated treatment effects on survey outcomes at endline, three months before the final transfer. Column 1 shows control group means, while Columns 2–5 present results from four model specifications. Column 2 reports treatment effects without control variables, and Column 3 includes only the baseline value of the respective outcome as control. Column 4 shows estimates from our preferred specification, which includes a full set of controls. The last column restricts the sample to non-attriters, defined as households that responded at both baseline and endline. The first panel shows effects at the individual level, the second panel shows household-level effects.

We find that assignment to the program significantly reduced the probability of working at endline. The point estimate is −10.2 percentage points ($p = 0.003$) with all controls included. This estimate represents a 22 % reduction relative to the control group mean of 47 percent. The effect remains significant at the 5 % level after correcting for multiple inference (see Table H.29 in Appendix H). The 95 % confidence interval rules out effects smaller than −8 % relative to the control group (−0.169, −0.035). Estimates are slightly larger but very comparable when omitting sets of controls and remain unchanged when restricting the sample to non-attriters.[20]

In Table 5, we further decompose this labor supply effect. The results indicate that the effect is confined to employment rather than self-employment, which is rare among recipients anyway. Both full-time and part-time work are impacted, with reductions in full-time work accounting for two-thirds of the overall effect. Similarly, both permanent and temporary jobs are affected.

We find very similar results when analyzing administrative employment data near the endline survey (June–July 2019). The results are shown in the third panel of Table 4. Using data from June, the point estimate is −11.0 percentage points with individual covariates ($p = 0.004$).[21] The effect becomes more negative but similar when excluding controls or restricting to non-attriters, with estimates ranging from −23 % to −26 % relative to the control group. These findings make us confident that the survey results are not distorted by reporting errors or biased by attrition.

Consistent with individual-level results, we find negative labor supply effects at the household level. On average, treatment households have significantly fewer members working than control households ($p = 0.003$). The likelihood of at least one member working is 14 % lower ($p = 0.007$). Both effects stay statistically significant after multiple inference correction.

Finally, we find no evidence of treatment effects on other types of activities. Job search is uncommon overall, with only 2.4 % of control respondents having looked for work in the past 4 weeks. While the treatment effect is negative and relatively large, it is not estimated

[20] In Appendix G we perform three sensitivity analyses, which leave our interpretation of the results unchanged. We (i) evaluate potential bias due to imbalance in unobservables, (ii) assess the impact of potential differential attrition at endline, and (iii) use logistic regression for binary outcomes.

[21] The estimated coefficient is the same, when using the full sample (N = 1,157). For July 2019, the point estimate is −8.2 percentage points ($p = 0.032$). See Table H.30 in Appendix H for the results.

Table 4
Treatment effects at endline: overall impact.

	Control mean (SD) (1)	No controls (2)	Baseline value (3)	Individual controls (4)	Non-attriters (5)
Working	0.473	−0.131	−0.113	−0.102	−0.101
	(0.500)	(0.038)	(0.034)	(0.034)	(0.036)
		[0.001]	[0.001]	[0.003]	[0.005]
Job search past 4 w	0.024	−0.015	−0.015	−0.015	−0.018
	(0.155)	(0.011)	(0.011)	(0.011)	(0.012)
		[0.178]	[0.183]	[0.167]	[0.146]
Social participation	0.376	0.037	0.013	0.011	0.031
	(0.485)	(0.037)	(0.035)	(0.035)	(0.037)
		[0.324]	[0.710]	[0.761]	[0.408]
Education past 6 m	0.212	0.035	0.029	0.034	0.034
	(0.410)	(0.032)	(0.032)	(0.032)	(0.034)
		[0.273]	[0.362]	[0.279]	[0.318]
No. working	0.869	−0.181	−0.162	−0.155	−0.159
	(0.824)	(0.060)	(0.052)	(0.052)	(0.054)
		[0.003]	[0.002]	[0.003]	[0.004]
At least one working	0.637	−0.113	−0.093	−0.088	−0.082
	(0.482)	(0.038)	(0.034)	(0.033)	(0.034)
		[0.003]	[0.006]	[0.007]	[0.015]
No. in education	0.527	0.054	0.067	0.059	0.058
	(0.802)	(0.060)	(0.054)	(0.053)	(0.056)
		[0.370]	[0.217]	[0.262]	[0.300]
At least one in education	0.392	0.041	0.043	0.040	0.051
	(0.489)	(0.038)	(0.037)	(0.036)	(0.038)
		[0.285]	[0.239]	[0.271]	[0.182]
Employed (admin)	0.476	−0.125		−0.110	−0.123
	(0.501)	(0.039)		(0.038)	(0.040)
		[0.001]		[0.004]	[0.002]
Observations (survey)		900	900	900	788
Observations (admin)		869		869	760

Note: OLS estimates of treatment effects at endline. The first two panels show survey outcomes and the last panel shows employment data from social security records. Outcome variables are listed on the left and detailed in Table D.13 in Appendix D. Column 1 reports control group means with standard deviations in parentheses; other columns show coefficients on the treatment dummy under different model specifications. Column 2 excludes control variables, Column 3 only controls for the baseline value of the respective outcome. Baseline information is unavailable for administrative outcomes. Column 4 includes a full set of controls, as specified in Eq. (1). Column 5 restricts the sample to subjects that responded at both baseline and endline. All models include randomization strata fixed effects and control for the survey mode in case of survey outcomes. We report robust standard errors in parentheses and p-values in brackets.

Table 5
Treatment effects at endline: decomposition of labor supply effects.

	Employed (1)	Self-emp. (2)	Full-time (3)	Part-time (4)	Permanent (5)	Temporary (6)
Treatment effect	−0.108	0.006	−0.068	−0.037	−0.055	−0.052
	(0.034)	(0.009)	(0.029)	(0.030)	(0.024)	(0.032)
	[0.002]	[0.486]	[0.019]	[0.220]	[0.024]	[0.100]
Control mean	0.457	0.016	0.229	0.245	0.186	0.264
(SD)	(0.499)	(0.127)	(0.421)	(0.431)	(0.390)	(0.442)
Observations	900	900	900	900	894	894

Note: OLS estimates of treatment effects on survey outcomes at endline. Each column reports results for a different outcome. The upper panel shows coefficients on the treatment dummy, estimating Eq. (1). We report robust standard errors in parentheses and p-values in brackets. The second panel shows control group means with standard deviations in parentheses.

precisely enough. Participation in civil society organizations (38 % among controls) and engagement in education or vocational training (21 % among controls) are more common. For both activities, the estimated effect is positive but not statistically significant. Based on 95 % confidence intervals, we cannot rule out relative effects ranging from −15 to 21 % for civic engagement (−0.058, 0.080) and −14 to 46 % for education (−0.029, 0.097).

6.2. Effects of activation

We now examine the effects of assignment to a social activation plan versus receiving only the transfer. Columns 1–4 of Table 6 report treatment effects for each treatment cell relative to the control group. The last two columns report the average effects of activation and partial withdrawal relative to no activation and full withdrawal using linear combinations.

The results show no significant differences in labor supply responses between beneficiaries assigned to activation plans and those only receiving the transfer. The point estimates in the activation arms are somewhat larger but the average effects of activation and no activation do not differ significantly ($p = 0.252$). Limited enforcement of the plans might explain these results, as participation was not compulsory or monitored.

The results differ at the household level, where households assigned to activation are more likely to have at least one member working. The average difference between activation and no activation is 6 percentage points ($p = 0.081$). This finding may reflect activation policies targeting

Table 6
Treatment effects at endline: treatment arms.

	Activation		No activation		Average	
	Partial (1)	Full (2)	Partial (3)	Full (4)	Activation (5)	Partial (6)
Working	−0.106	−0.150	−0.067	−0.113	−0.038	0.045
	(0.042)	(0.049)	(0.042)	(0.047)	(0.033)	(0.034)
	[0.013]	[0.002]	[0.113]	[0.017]	[0.252]	[0.182]
Job search past 4 w	−0.022	−0.004	−0.012	−0.020	0.000	−0.004
	(0.011)	(0.015)	(0.013)	(0.012)	(0.009)	(0.008)
	[0.052]	[0.787]	[0.381]	[0.109]	[0.981]	[0.648]
Social participation	0.034	0.072	−0.027	−0.008	0.068	−0.027
	(0.047)	(0.054)	(0.044)	(0.049)	(0.038)	(0.037)
	[0.469]	[0.187]	[0.543]	[0.873]	[0.069]	[0.462]
Education past 6 m	0.100	0.051	0.001	−0.007	0.083	0.026
	(0.044)	(0.048)	(0.038)	(0.042)	(0.034)	(0.032)
	[0.023]	[0.293]	[0.988]	[0.869]	[0.014]	[0.415]
No. working	−0.202	−0.186	−0.117	−0.131	−0.073	0.001
	(0.066)	(0.075)	(0.064)	(0.072)	(0.052)	(0.053)
	[0.002]	[0.013]	[0.068]	[0.072]	[0.156]	[0.991]
At least one working	−0.129	−0.109	−0.056	−0.069	−0.060	−0.002
	(0.042)	(0.047)	(0.042)	(0.046)	(0.034)	(0.035)
	[0.002]	[0.021]	[0.176]	[0.139]	[0.081]	[0.943]
No. in education	0.079	0.114	0.024	0.045	0.061	−0.027
	(0.071)	(0.079)	(0.063)	(0.074)	(0.055)	(0.054)
	[0.264]	[0.152]	[0.704]	[0.539]	[0.273]	[0.612]
At least one in education	0.050	0.081	0.024	0.020	0.040	−0.011
	(0.048)	(0.054)	(0.045)	(0.050)	(0.038)	(0.037)
	[0.294]	[0.133]	[0.586]	[0.693]	[0.290]	[0.767]
Employed (admin)	−0.086	−0.149	−0.067	−0.168	−0.004	0.084
	(0.048)	(0.055)	(0.047)	(0.050)	(0.038)	(0.037)
	[0.076]	[0.007]	[0.158]	[0.001]	[0.920]	[0.023]
Observations per cell	183	114	209	149		

Note: OLS estimates of treatment effects on outcomes at endline. Outcome variables are listed on the left and described in detail in Table D.13 in Appendix D. Columns 1–4 report treatment effects for each treatment cell relative to the control group, estimating Eq. (2). Columns 5 and 6 report the average effects of activation relative to no activation, and partial relative to full withdrawal, using linear combinations. We report robust standard errors in parentheses and *p*-values in brackets. The sample for administrative outcomes is restricted to endline respondents for comparability.

other household members. However, it should be noted that the effect is only significant at 10 % level and does not survive correction for multiple comparisons.

For other activities, results suggest that beneficiaries assigned to activation are more likely to engage in social participation and education than those receiving only the transfer. However, these effects could be mechanical, as respondents may have interpreted participation in activation plans as social or educational activities. Supporting this interpretation, the differential effect on education disappears when excluding the training-intensive social entrepreneurship plan (see Table H.31 in Appendix H).

6.3. *Effects of full withdrawal and elasticities*

Results suggest a stronger labor supply response of main recipients when the benefit is phased out euro-for-euro. The point estimates in the full withdrawal arms are larger when using our administrative employment measure: –14.9 and –16.8 compared to –8.6 and –6.7 percentage points. On average, effects differ by 8.4 percentage points ($p = 0.023$).[22] The findings remain consistent when using the full sample instead of endline respondents with administrative data, with an average difference of 7.7 percentage points ($p = 0.018$). When using survey data, the average difference is smaller (4.3 percentage points) and not statistically significant, but the 95 % confidence interval includes the previous estimate (–2.4, 11.0).

These findings suggest that the full withdrawal regime provided stronger work disincentives, aligning with predictions of standard labor supply models and the PTRs presented in Section 2.2. Using these results, we calculate participation elasticities, which we define as the log change in employment rates divided by the log change in net-of-tax rates (Chetty et al., 2013). Table 7 summarizes the key parameters and results. Based on our stylized calculations, the partial withdrawal regime reduced the net-of-tax rate by about 30 % compared to the control group. The full withdrawal regime lowered the net-of-tax rate by roughly 50 % when the participation margin is the full-time minimum wage, and 65 % when accounting for part-time work. Employment rates are estimated for the full sample.

Under partial withdrawal, the entry margin is less relevant, which leads to comparable participation elasticities of 0.47 and 0.49. Under full withdrawal, entry below the minimum wage is less beneficial in net terms, resulting in a lower elasticity of 0.39 compared to 0.66. Notably, the elasticities are not estimated precisely enough to reject the equality of estimates between the partial and full withdrawal schemes.

Imposing further assumptions, we estimate income and substitution elasticities at the extensive margin. We take baseline income as given and assume no substitution of other income sources or changes in household characteristics. Treatment income is the average transfer in the first 3 months. To obtain wage rates we subtract the withdrawal rate from 1, that is 0 for the control group, 1 for full withdrawal, and 0.3 for partial withdrawal. We obtain elasticities by estimating Eq. (1), but replacing the treatment dummy with variables for treatment income and the wage rate.

We find an income elasticity of –0.07 and a substitution elasticity of 0.15 when using administrative employment data and the full sample. The results for survey data (limited to endline respondents) are

[22] Remember that the social entrepreneurship plan was not cross-randomized and only faced a partial withdrawal rate. Excluding this group from the sample does not alter the results (see Table H.31 in Appendix H).

Table 7
Participation elasticities.

	Full-time			Part-time		
	Control	Partial	Full	Control	Partial	Full
Net-of-tax rate	100.0	67.4	53.3	100.0	68.3	34.7
Employment rate	45.8	38.0	30.3	45.8	38.0	30.3
Elasticity		0.47	0.66		0.49	0.39
		(0.19)	(0.13)		(0.19)	(0.08)

Note: Elasticities of employment rates with respect to net-of-tax rates. Net-of-tax rates are calculated as 1 minus participation tax rates. Full-time refers to households earning the full-time minimum wage upon entry. Part-time accounts for the baseline fraction of part-time workers. Employment rates are estimated using the full sample and include individual controls. Elasticities are defined as the log change in employment rates divided by the log change in net-of-tax rates, following Chetty et al. (2013). Standard errors are estimated using the delta method and reported in parentheses.

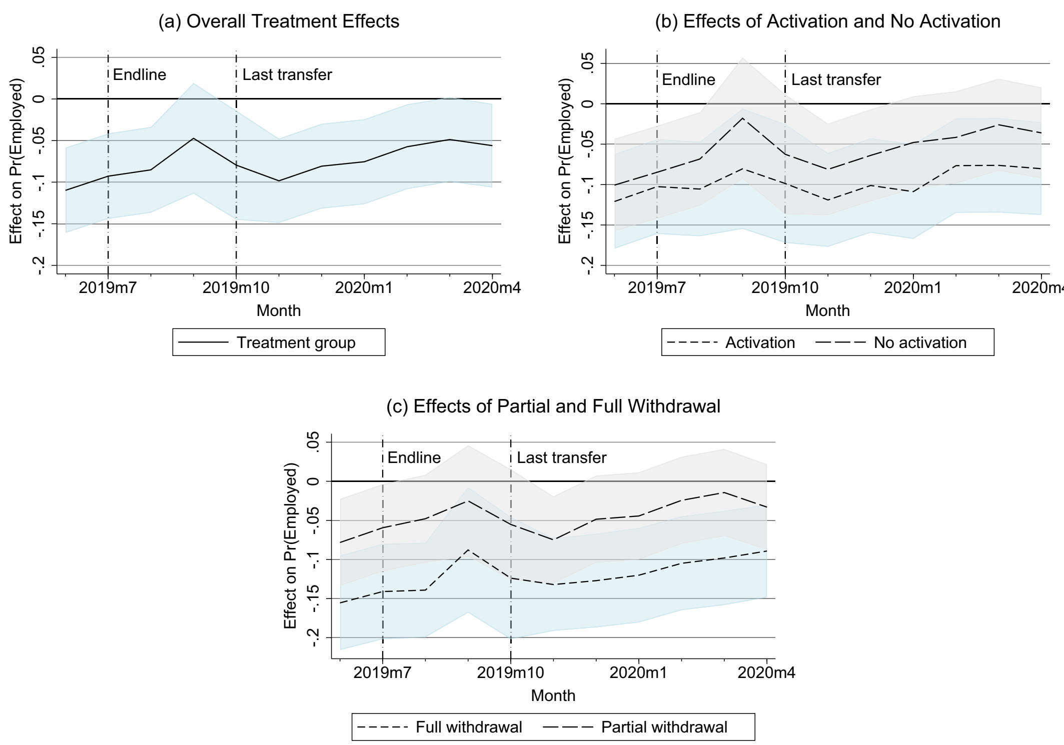


Fig. 3. Treatment effects on employment probabilities using administrative data.
Note: Grey and colored areas are 95 % confidence intervals. Graphs show ITT effects, which are estimated using separate regressions for each month. Models include individual controls and randomization strata fixed effects. Sample is full sample.

very similar with –0.07 and 0.11, respectively. Expressed as a per-euro elasticity, the income elasticity parameter suggests that a €100 increase in treatment income is associated with a 1 percentage point reduction in employment probability. Per $100 PPP, the reduction is 0.6 percentage points.

6.4. Persistence of effects post-treatment

We now examine the persistence of post-treatment effects. Households may offset income loss after the program ends, reducing negative labor supply effects, or effects may persist due to human capital depreciation and other scarring mechanisms. Fig. 3 plots monthly effect estimates from June 2019 to April 2020.[23] Unfortunately, data beyond this period are unavailable. We rely on administrative data, as survey data end 3 months before the trial. Results are based on the full sample but similar when limited to endline respondents (see Fig. I.7 in Appendix I). Panel (a) shows overall effects, Panel (b) and (c) show effects by arm.

[23] Note that Spain imposed a full COVID-19 lockdown in late March 2020.

We find that employment effects remain negative throughout the entire post-treatment period and only reduce toward the end of the observation window. This effect persistence could hint at scarring effects. Another explanation, despite supportive evidence, involves compensatory efforts by authorities, such as social workers promoting other support programs. The effects for treatment arms follow the same pattern as overall effects. Particularly, differences observed at endline between the withdrawal modalities persist until the end of the observation period.

6.5. Heterogeneous treatment effects

We now assess whether treatment effects vary across different types of beneficiaries and households. Specifically, we compare effects by recipient gender (male vs. female), household composition (without vs. with children), and the interaction between these two factors.

We focus on the program's overall impact and interact the treatment dummy with subgroup indicators, as in Eq. (3). To increase power, we use administrative employment data, which includes subjects regardless of endline response. We define subgroups using baseline survey data,

Table 8
Heterogeneous treatment effects: employment.

	Gender (1)	Children (2)	Gender X Children (3)
Treatment	−0.038	0.004	0.161
	(0.062)	(0.079)	(0.115)
	[0.544]	[0.962]	[0.161]
Interaction effect			
Female	−0.116		
	(0.073)		
	[0.111]		
Any child 0–16		−0.152	
		(0.086)	
		[0.080]	
Male w/ children			−0.273
			(0.135)
			[0.044]
Female wo/ children			−0.283
			(0.155)
			[0.068]
Female w/ children			−0.322
			(0.122)
			[0.008]
Observations	998	998	998

Note: OLS estimates of treatment and interaction effects on employment probabilities in June 2019. Employment data were obtained from social security records. Column 1 interacts the treatment dummy with a dummy indicating the gender of the main recipient. Column 2 interacts the treatment dummy with a dummy indicating children in the household. Column 3 interacts the treatment dummy with the previous two dummies. The first row reports coefficients on the treatment dummy for the reference group. We report robust standard errors in parentheses and *p*-values in brackets. All models include randomization strata fixed effects and individual covariates. The sample is restricted to baseline respondents.

which reduces the estimation sample to baseline respondents. Table 8 reports the results.

We find suggestive evidence that labor supply effects vary by gender (see Column 1). The point estimate for male recipients is small (–3.8 percentage points), while the interaction effect is sizeable but marginally insignificant despite using the largest available sample (–11.6 percentage points, $p = 0.111$). Results are similar using July data (see Table H.32 in Appendix H). Using survey outcomes, the interaction effect is larger and significant at the 5 % level (–15.2 percentage points, $p = 0.047$; see Table H.33).

Next, we find tentative evidence that recipients with childcare responsibilities respond more strongly to the program (see Column 2). The point estimate is close to zero for households without children or with children older than 16. The interaction effect is large and statistically significant at the 10 % level (–15.2 percentage points, $p = 0.080$).[24] When using June data, the interaction effect decreases and loses statistical significance (–9.4 percentage points; see Table H.32). On a cautionary note, we cannot confirm these results with survey data. The point estimate for the reference group is now negative (–8.0 percentage points) and the interaction effect not significant (see Table H.33).

When interacting both factors, we find that female recipients respond more strongly to the program regardless of childcare status, while male recipients with children also show a stronger response (see Column 3). All heterogeneity results remain consistent when running separate regressions for each subgroup (see Table H.34) or interacting all controls with the subgroup dummies (see Table H.35).

[24] We also distinguish households according to the age of the youngest child in four bins: 0–2 years, 3–5 years, 6–11 years, and 12–16 years. The interaction effects are negative but not statistically significantly different from each other (see Table H.36 in Appendix H).

In sum, the results suggest that negative labor supply effects may stem from substituting paid work with domestic duties. More detailed time-use data would be needed to investigate this channel directly. Nonetheless, it is plausible that beneficiaries reduce work mainly due to the domestic duties they face. This pattern was also observed in the NIT experiments, where wives showed stronger employment responses (Robins, 1985; Burtless, 1986).

7. Discussion and conclusion

We evaluated an antipoverty program in Barcelona (Spain) that provided a guaranteed means-tested income to households in financial need without behavioral conditions. The program consisted of a monthly cash transfer to a designated household member (*main recipient*) and social activation plans. The maximum transfer was €1297 ($2055 PPP) for a family of four and €663 ($1051 PPP) for a single-person household, equivalent to 70 %–80 % of the local poverty line. The benefit was phased out with additional income. Some households faced a euro-for-euro phase-out, while others saw their benefit reduced by 25–35 cents for every euro earned.

We studied the program's impact on employment, investment in human capital (following training or education) and community engagement. Our analysis draws on survey data and social security records. For identification, we exploit the fact that the program was trialed in a 2-year RCT with roughly 1200 households across ten target neighborhoods.

The overall impact of the program can be summarized in four parts. First, we find strong evidence of sizeable negative labor supply effects: after 2 years, treated households were 14 % less likely to have at least one member working, and main recipients were 22 % less likely to work. Second, employment rates remained lower 6 months post-treatment, suggesting labor supply decisions may be difficult to reverse. Third, there is no evidence of substantial effects on social participation or education-related activities. Fourth, there is tentative evidence of labor substitution for caregiving.

The employment effects we find are larger compared to recent European policies and U.S. dividend programs, which offer smaller benefits and lack phase-outs. However, they are on par with evidence from the NIT experiments, where transfer amounts are similar and phase-outs are also significant.

Our final finding results from subgroup analyses suggesting that female beneficiaries and beneficiaries with children respond strongly to the program. On a cautionary note, these results are less robust across data sources than overall effects. If labor supply reductions are related to caregiving, they could generate positive externalities, like improvements in children's education and health outcomes or reductions in adolescent delinquency. Follow-up research is needed to examine program effects in these areas and reach conclusions about broader welfare effects.

In addition to overall impacts, we compared two randomized program modalities: (i) assignment to a social activation plan versus transfers alone, and (ii) a 100 % versus a 25 %–35 % withdrawal rate. While there is no strong evidence that activation matters, labor supply responses differ significantly between withdrawal schemes. Negative effects are nearly twice as large under 100 % phase-out compared to lower phase-out: –34 % versus –16 % relative to the control group.

We used these estimates to calculate participation elasticities, which range from 0.39 to 0.49 for a family of four, accounting for part-time work. While evidence on participation elasticities from similar programs is limited, policies that temporarily alter work incentives yield comparable results. For instance, Chetty et al. (2013) report elasticities of 0.42 for a tax holiday in Iceland (Bianchi et al., 2001), and 0.38 for the Canadian Self-Sufficiency Project, an earnings subsidy for single mothers (Card and Hyslop, 2005). In contrast, the Finnish basic income experiment finds lower elasticities, ruling out values above 0.16 (Hämäläinen et al., 2022). The authors attribute this to minimal incentive changes for single mothers and larger families, groups targeted by welfare programs studied before. Imposing stricter assumptions, we find a substitution elasticity of 0.15 and an income elasticity of -0.07 at the extensive margin. This implies that a €100 increase in treatment income is associated with a 1 percentage point reduction in employment probability.

The two withdrawal schemes also differ in their impact on government revenues. We use a back-of-the-envelope calculation to quantify this impact. Assuming entry at the minimum wage there is no foregone tax revenue due to generous allowances. But the government saves on transfer payments when people work. Under partial withdrawal, the savings are €343 for a family of four, assuming an average transfer of €490. Under full withdrawal, the entire transfer is saved due to the 100 % phase-out. As mentioned, employment probabilities decline by 16 % under partial withdrawal and 34 % under full withdrawal. Taken together, the government cost per euro of benefit due to forgone savings is 12 cents under partial withdrawal and 34 cents under full withdrawal.

Our study has several limitations. First, since the program lasted only 2 years, estimated behavioral responses may differ from those under a permanent policy. Theory suggests that temporary programs generate smaller income effects but larger substitution effects due to limited consumption adjustments and time-varying net wages (Metcalf, 1973). Both mechanisms may be relevant in our setting. Empirical evidence from the Seattle/Denver NIT experiment finds weaker labor supply responses in short-term treatment arms, particularly under low taxes (Robins and West, 1978; Burtless and Greenberg, 1982). Second, data constraints prevent us from studying intensive margin responses, long-term effects, impacts on other transfers or total disposable income, and potential substitution of work for other activities. While underreporting of self-employment or informal labor is a concern, we consider it unlikely.[25] Lastly, given the scale of the trial, our estimates may not capture general equilibrium effects that could emerge under full-scale rollout, such as aggregate demand effects.[26]

Our findings provide some interesting directions for future research. First, the program may have achieved policy objectives beyond employment, such as improving health and well-being, reducing financial hardship, or preventing evictions—impacts hinted at in local reports (Todeschini and Sabes-Figuera, 2019). A broader evaluation could clarify trade-offs between objectives. Second, exploring effects on intra-household bargaining could be insightful. Lastly, more research is needed to understand community-level effects. Transfers with no spending restrictions may have distinct effects on the local economy, crime, or neighborhood quality.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgements

We would like to thank the journal editor, Damon Jones, and two anonymous referees for their comments, which helped improve the paper. We are grateful to all study participants, the Catalan Institute of Public Policy Evaluation (Ivàlua), and Barcelona City Council, particularly, Lluís Torrens and Lluís Batlle. This article would not have been possible without the initial research work conducted principally by Ramon Sabes, Jaume Garcia, and Laura Kirchner. Timo Verlaat would like to thank Ivàlua for hosting him as a visiting researcher and for the guidance and support received. Federico Todeschini worked at Ivàlua while the program was designed and rolled out.

Table A.9
Example calculation for SMI benefit.

Member	Income	Subsistence level		
		Living costs	Housing costs	
		Imputed	Imputed	Actual
Adult 1	€450.00	€402.60	€260.00	
Adult 2	€450.00	€148.00	€110.00	€650.00 (rent) + €50.00 (taxes)
Child 1	–	€148.00	€40.00	
Child 2	–	€148.00	€40.00	
Sum	€900.00	€846.60	€450.00	€700.00
		€846.60 (living costs) + €450.00 (housing costs; lower value) = €1296.60 (imputed subsistence level)		
Total SMI	€1296.60 (imputed subsistence level) – €900.00 (household income) = €396.60 (monthly benefit)			

Appendix A. Determining the SMI benefit level

The SMI benefit level equals the difference between a household's imputed subsistence level and monthly income. We will now describe both items in more detail.

The imputed subsistence level is the sum of a household's imputed living and housing costs. Living costs include costs for energy and water utilities. The fixed values to impute a household's living costs are €402.60 ($638 PPP) per month for the first adult and €148.00 ($235 PPP) for every additional household member. Housing costs comprise rent, mortgage payments, municipal taxes, and property taxes. The fixed values to impute a household's housing costs are €260.00 ($412 PPP) per month for the first adult, €110.00 ($174 PPP) for a second household member, and €40.00 ($63 PPP) for every additional household member. If imputed housing costs exceed actual housing costs, the latter are considered.

Household income is the sum of the incomes of all household members in a given month. This includes income from work, homeownership, and financial investments. Household income cannot fall below zero.

For illustration, Table A.9 provides an example calculation for a four-person household consisting of two adults and two children. The example household would receive a monthly transfer of €396.60 ($586 PPP). The table also shows the maximum transfer, which is equal to the imputed subsistence level of €1296.60 ($2055 PPP). The maximum transfer is also the eligibility threshold for the household under consideration.

Appendix B. Randomization mechanism

Households were assigned to experimental conditions per stratum. The randomization mechanism was modeled after a lottery that assigns places in the city's public nurseries. The mechanism works as follows:

1. Each household at random receives a unique administration number between 1 and the total number of households in the stratum.
2. From a bag containing ten balls with the numbers 0 to 9, nine balls are taken with replacement to obtain a nine-digit number.
3. Dividing this number by the number of households in the respective stratum, one obtains a quotient and a remainder.
4. Households are sorted consecutively according to their administration number. The sorted list starts with the household whose administration number is the one next to the remainder. For instance, if the remainder is 6, the first position on the list goes to the household with administration number 7, the second position to the household with number 8, etc.

[25] Our finding that self-employment is rare (1.6 % in the control group) aligns with Catalonian administrative data (Generalitat de Catalunya, 2018). Based on these records, we estimate a 2.7 % self-employment rate for a sample comparable to B-MINCOME. Underreporting of informal labor also seems unlikely, as surveys asked no question about income, were not linked to income reports, and administered by an independent bureau.

[26] See Egger et al. (2022) and Jones and Marinescu (2022) for two different analyses of macroeconomic or general equilibrium effects of cash transfers.

Table B.10
Assignment probabilities per stratum.

No.	Strata		No activation		Community involvement		Social entrepreneurship	Control group	Other groups
	Expected SMI	Employable	Full	Partial	Full	Partial	Partial		
1	High	Yes	9 %	11 %	6 %	6 %	4 %	37 %	26 %
2	High	No	15 %	17 %	10 %	10 %	8 %	41 %	–
3	Medium	Yes	10 %	13 %	7 %	7 %	6 %	42 %	16 %
4	Medium	No	14 %	16 %	10 %	10 %	8 %	43 %	–
5	Low	Yes	18 %	22 %	12 %	12 %	8 %	23 %	4 %
6	Low	No	17 %	23 %	13 %	13 %	8 %	26 %	–

Note: Percentages do not add up to 100 % due to rounding. Other groups comprise an activation plan offering vocational training. This experimental condition is excluded from the study. The table omits stratum no. 7 (see Table B.11), which is excluded from the study, too.

Table B.11
Number and share of households per randomization strata.

No.	Strata		Households	
	Expected SMI	Employable	No.	Percent
1	High	Yes	274	18.0
2	High	No	81	5.3
3	Medium	Yes	379	25.0
4	Medium	No	164	10.8
5	Low	Yes	419	27.6
6	Low	No	165	10.9
7	Other		36	2.4
Total			1518	100.0

Note: Households in stratum no. 7 are excluded from the study and only listed for completeness. The stratum comprises households eligible for a housing renovation program.

5. Households are assigned to an experimental condition going through the ordered list from top to bottom, allocating the first x number of households to the first condition, the second x number of households to the second condition, etc. Although conditions are assigned in the same order in each stratum, the number of available places in each condition differs between strata. Consequently, assignment probabilities in the different strata are different. Table B.10 lists the assignment probabilities per stratum.

Appendix C. Description of social activation plans

The implementation of the social activation plans was outsourced to different local implementers. We describe the content and scope of the two plans of interest for our study.

The community involvement plan consisted of a series of workshops organized by two NGOs in each of the ten target neighborhoods. The workshops aimed to promote and facilitate micro-projects of participants that would benefit their neighborhood's community. For example, participants worked on developing a neighborhood campaign, collecting community stories, organizing photo and video exhibitions, or developing a neighborhood tour.

The social entrepreneurship plan consisted of three phases, an intake phase, and two training phases. During the intake phase, households assigned to the plan were invited to interview sessions with program implementers at local social facilities. The goal of the interviews was to provide information about the plan, assess the capabilities of different household members, and select a household member who would participate in the following phases. At the end of the intake phase, groups were formed according to individual profiles and interests. During the first training phase, participants followed two courses of 1 month each, covering basic entrepreneurial skills, such as financial planning. Classes took place three times a week. During the second training phase, participants could choose between two training tracks. In the first track, participants developed a business plan, supported by coaching (200 hours) and further skills training (235 hours). In the second track, participants joined existing local social entrepreneurship initiatives to gain work experience (at least 6 hours per week for 3–6 months).

Appendix D. Lists of variables

See Tables D.12 and D.13.

Table D.12
List of covariates with description.

Variable	Description	Source
Monthly hh income	Average monthly household income in the period April 2016 to July 2017; data is retrieved from tax income statements	Municipal benefit registry
Monthly transfers	Average monthly municipal transfers received in the 12 months before the start of treatment. Municipal transfers may include schooling, housing, and healthcare allowances, transport subsidies, and child benefits.	Municipal benefit registry
RGC recipient	1 if household received Catalonia's guaranteed citizenship income (*renta garantizada de ciudadanía*, or RGC) at the time of recruitment and 0 otherwise.	Municipal benefit registry
Main recipient female	1 if main recipient is female and 0 otherwise.	Survey
Main recipient age	Age in years.	Survey
No. of members	Number of household members (with different age cutoffs).	Survey
Single-person hh	1 if household has one adult member and 0 otherwise. Adult members are members of age 16 or older.	Survey
Single-parent hh	1 if household has one adult member living with a child under age 16 and 0 otherwise.	Survey
Adults without children	1 if household has more than one adult member and 0 otherwise.	Survey
Adults with children	1 if household has more than one adult member living with at least one child under age 16 and 0 otherwise.	Survey
Compulsory education or less	1 if no household member completed compulsory education or at least one household member completed compulsory education and 0 otherwise. Compulsory education comprises primary education and lower secondary education.	Survey
Secondary education	1 if at least one household member completed secondary education and 0 otherwise. Secondary education comprises higher secondary education and vocational education.	Survey
Tertiary education	1 if at least one household member completed tertiary education and 0 otherwise. Tertiary education comprises university education.	Survey
All hh members Spanish	1 if all household members are Spanish citizens and 0 otherwise.	Survey
No hh members Spanish	1 if no household member is a Spanish citizen and 0 otherwise.	Survey
Mixed nationalities	1 if at least one household member is a Spanish citizen and 0 otherwise.	Survey

Table D.13
List of outcome variables with description.

Variable	Description	Source
Working	1 if main recipient indicated to currently work in paid employment or to be self-employed and 0 otherwise.	Survey
Employed	1 if main recipient indicated to currently work in paid employment and 0 otherwise.	Survey
Self-employed	1 if main recipient indicated to currently be self-employed and 0 otherwise.	Survey
Working full-time	1 if main recipient indicated to work full-time (employed or self-employed) and 0 otherwise.	Survey
Working part-time	1 if main recipient indicated to work part-time (employed or self-employed) and 0 otherwise.	Survey
Permanent contract	1 if main recipient indicated to work under an indefinite contract and 0 otherwise.	Survey
Temporary contract	1 if main recipient indicated to work under a fixed-term contract and 0 otherwise.	Survey
Job search past 4 w	1 if main recipient answered *yes* to the question: "In the past 4 weeks, have you tried to find paid employment (including work of any type and even if it was just for a few hours)?", and 0 if main recipient answered *no*.	Survey
Social participation	1 if main recipient indicated to have taken active part in at least one of the following groups, organizations, or initiatives in the past 12 months and 0 otherwise: neighborhood organization, school organization, parents' association, non-profit organization, religious group, political party, any other organization offering volunteer opportunities.	Survey
Education past 6 m	1 if main recipient indicated to have followed a study (vocational or tertiary education) or non-school education (e.g., a private course) in the past 6 months and 0 otherwise.	Survey
No. working	Number of household members aged between 18 and 65 in paid employment or self-employed.	Survey
At least one working	1 if at least one household member aged between 18 and 65 is in paid employment or self-employed and 0 otherwise.	Survey
No. in education	Number of household members aged between 18 and 65 that followed a study (vocational or tertiary education) or non-school education (e.g., a private course) in the past 6 months.	Survey
At least one in education	1 if at least one household member aged between 18 and 65 has followed a study (vocational or tertiary education) or non-school education (e.g., a private course) in the past 6 months and 0 otherwise.	Survey
Employed (admin.)	1 if main recipient is listed as employed in social security records at least once in a given month and 0 otherwise.	Social security records

Appendix E. Analyzing baseline balance

To test for differences in baseline observables between treatment and control households, we estimate the following specification:

$$Y_{hB} = \alpha + \beta T_h + \gamma + \epsilon_h \tag{E.1}$$

In that equation, Y_{hB} denotes the variable of interest for household h measured at baseline. T_h is a treatment dummy indicating assignment to treatment. γ denotes randomization strata fixed effects and ϵ_h is the error term.

In a second step, we include a dummy variable for each of the four treatment combinations (activation/no activation × partial/full withdrawal). In this specification, G_h^x are dummies for each of the four treatment cells and the reference category is the control group:

$$Y_{hB} = \alpha + \sum_{x=1}^{4} \beta_x G_h^x + \gamma + \epsilon_h \tag{E.2}$$

In both cases, we report the parameters denoted by β.

Table E.14
Baseline balance: admin covariates (full sample).

	Control mean (SD)	Treatment group	Activation		No activation		N
			Partial	Full	Partial	Full	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Monthly hh income	424.650	23.598	39.531	0.459	20.683	24.165	1200
	(381.618)	(18.041)	(24.726)	(27.250)	(23.510)	(26.076)	
		[0.191]	[0.110]	[0.987]	[0.379]	[0.354]	
Monthly transfers	173.043	4.451	6.429	−8.361	20.685	−9.396	1200
	(184.509)	(11.433)	(15.289)	(14.796)	(15.712)	(16.645)	
		[0.697]	[0.674]	[0.572]	[0.188]	[0.573]	
RGC recipient	0.056	0.025	0.067	0.028	−0.029	0.041	1200
	(0.229)	(0.016)	(0.026)	(0.029)	(0.019)	(0.025)	
		[0.116]	[0.010]	[0.330]	[0.123]	[0.104]	
Joint test (p-value)		0.267	0.032	0.718	0.154	0.210	

Note: Differences in covariates obtained from administrative sources between treatment and control groups. Column 1 reports control group means with standard deviations in parentheses. Column 2 shows coefficients on the treatment dummy, estimating Eq. (E.1). Columns 3–6 report coefficients on dummies for each treatment interaction, estimating Eq. (E.2). We report robust standard errors in parentheses and p-values in brackets. The last two rows show p-values from a joint hypothesis test. The table reports results for the full sample. See Table D.12 in Appendix D for a description of variables.

Appendix F. Analyzing attrition

To test for differences in baseline observables between attrition and non-attrition households, we estimate the following specification:

$$Y_{hB} = \alpha + \beta_1 attrition_h + \gamma + \epsilon_h \tag{F.1}$$

Here, Y_{hB} describes the variable of interest for household h at baseline. The variable $attrition_h$ is a dummy taking the value 1 if a household was surveyed at baseline, but not at endline, and 0 otherwise. For administrative data, the attrition dummy may also take the value 1 if a household is missing at endline, irrespective of baseline response. γ denotes randomization strata fixed effects and ϵ_h is the error term. We report the results of this analysis in Tables F.15–F.17 below.

Table F.15
Attrition versus non-attrition households: baseline survey covariates.

	Non-attrition mean (SD) (1)	Attrition (2)	N (3)
Main recipient female	0.737 (0.441)	−0.073 (0.034) [0.033]	1034
Main recipient age	40.690 (8.336)	0.598 (0.642) [0.352]	1034
No. members	4.152 (1.526)	0.015 (0.108) [0.893]	1034
No. members <16	1.759 (1.159)	−0.114 (0.087) [0.191]	1034
No. members 16–64	2.314 (1.090)	0.117 (0.080) [0.146]	1034
Single-person hh	0.029 (0.168)	0.003 (0.013) [0.790]	1034
Single-parent hh	0.147 (0.354)	−0.024 (0.025) [0.334]	1034
Adults without children	0.124 (0.330)	0.062 (0.027) [0.022]	1034
Adults with children	0.700 (0.459)	−0.041 (0.034) [0.220]	1034
Compulsory education or less	0.496 (0.500)	0.041 (0.037) [0.258]	1034
Secondary education	0.404 (0.491)	−0.040 (0.035) [0.251]	1034
Tertiary education	0.100 (0.300)	−0.001 (0.022) [0.964]	1034
All hh members Spanish	0.461 (0.499)	0.050 (0.037) [0.177]	1034
No hh member Spanish	0.219 (0.414)	−0.004 (0.030) [0.898]	1034
Mixed nationalities	0.320 (0.467)	−0.046 (0.033) [0.167]	1034
Joint test (*p*-value)		0.247	

Note: Differences in baseline survey covariates between attrition and non-attrition households. Attrition households are households that filled in the baseline survey but not the endline survey. Column 1 reports means and standard deviations for non-attrition households. Column 2 shows the coefficient on the attrition dummy, estimating Eq. (F.1). We report robust standard errors in parentheses and *p*-values in brackets. The last row reports *p*-values from a joint hypothesis test. The sample does not comprise 1200 observations due to baseline non-response. See Table D.12 in Appendix D for a description of variables.

Table F.16
Attrition versus non-attrition households: baseline admin covariates.

	Non-attrition mean (SD) (1)	Attrition (2)	N (3)
Monthly hh income	544.729 (418.980)	−30.478 (17.993) [0.091]	1200
Monthly transfers	185.495 (191.889)	−35.321 (10.505) [0.001]	1200
RGC recipient	0.084 (0.277)	0.019 (0.018) [0.280]	1200
Joint test (*p*-value)		0.002	

Note: Differences in covariates obtained from administrative sources between attrition and non-attrition households. Attrition households are households that did not fill in the endline survey. Column 1 reports means and standard deviations for non-attrition households. Column 2 shows the coefficient on the attrition dummy, estimating Eq. (F.1). We report robust standard errors in parentheses and *p*-values in brackets. The last row reports *p*-values from a joint hypothesis test. See Table D.12 in Appendix D for a description of variables.

Table F.17
Attrition versus non-attrition households: baseline survey outcomes.

	Non-attrition mean (SD) (1)	Attrition (2)	N (3)
Working	0.392 (0.489)	−0.054 (0.035) [0.125]	1032
Job search past 4 w	0.500 (0.500)	−0.034 (0.036) [0.347]	1031
Social participation	0.389 (0.488)	−0.001 (0.036) [0.969]	1034
Education past 12 m	0.242 (0.428)	−0.038 (0.030) [0.208]	1034
No. working	0.749 (0.761)	−0.016 (0.059) [0.779]	1034
At least one working	0.582 (0.493)	−0.047 (0.036) [0.195]	1034
No. in education	0.576 (0.797)	−0.090 (0.055) [0.105]	1034
At least one in education	0.423 (0.494)	−0.051 (0.035) [0.148]	1034
Joint test (*p*-value)		0.373	

Note: Differences in outcomes at baseline between attrition and non-attrition households. Attrition households are households that filled in the baseline survey but not the endline survey. Column 1 reports means and standard deviations for non-attrition households. Column 2 shows the coefficient on the attrition dummy, estimating Eq. (F.1). We report robust standard errors in parentheses and *p*-values in brackets. The last row reports *p*-values from a joint hypothesis test. The sample does not comprise 1200 observations due to baseline non-response. See Table D.13 in Appendix D for a description of variables.

We use Eq. (E.1) and (E.2) to test for differences in baseline observables at endline between households assigned to treatment and control groups and different treatment cells. We restrict the sample to households that filled in both the baseline and the endline survey. For administrative data, we also report results for our endline sample unconditional on baseline response. Tables F.18–F.20 below report the results of this second analysis.

Table F.18
Balance of endline sample: baseline survey covariates.

	Control mean (SD)	Treatment group	Activation		No activation		N
			Partial	Full	Partial	Full	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Main recipient female	0.733	0.001	0.039	−0.011	−0.002	−0.033	790
	(0.443)	(0.037)	(0.045)	(0.055)	(0.046)	(0.051)	
		[0.987]	[0.394]	[0.848]	[0.960]	[0.516]	
Main recipient age	40.353	0.257	1.146	−0.434	0.042	−0.017	790
	(8.150)	(0.672)	(0.885)	(0.969)	(0.863)	(0.904)	
		[0.703]	[0.196]	[0.655]	[0.962]	[0.985]	
No. members	4.127	0.177	0.342	0.141	0.113	0.090	790
	(1.529)	(0.115)	(0.165)	(0.156)	(0.141)	(0.165)	
		[0.126]	[0.039]	[0.365]	[0.424]	[0.585]	
No. members <16	1.719	0.136	0.157	0.168	0.101	0.133	790
	(1.121)	(0.089)	(0.122)	(0.128)	(0.112)	(0.125)	
		[0.126]	[0.197]	[0.192]	[0.368]	[0.288]	
No. members 16–64	2.317	0.069	0.206	−0.022	0.042	0.006	790
	(1.136)	(0.089)	(0.124)	(0.119)	(0.112)	(0.115)	
		[0.442]	[0.096]	[0.853]	[0.708]	[0.956]	
Single-person hh	0.027	−0.003	0.010	−0.000	−0.022	0.005	790
	(0.163)	(0.013)	(0.019)	(0.020)	(0.014)	(0.020)	
		[0.809]	[0.615]	[0.982]	[0.123]	[0.810]	
Single-parent hh	0.167	−0.036	−0.038	−0.089	−0.019	−0.016	790
	(0.374)	(0.031)	(0.039)	(0.039)	(0.039)	(0.042)	
		[0.249]	[0.322]	[0.025]	[0.618]	[0.708]	
Adults without children	0.131	−0.017	−0.021	−0.052	0.011	−0.025	790
	(0.338)	(0.027)	(0.036)	(0.037)	(0.035)	(0.036)	
		[0.532]	[0.548]	[0.162]	[0.747]	[0.492]	
Adults with children	0.674	0.056	0.050	0.141	0.030	0.036	790
	(0.470)	(0.037)	(0.049)	(0.051)	(0.047)	(0.051)	
		[0.131]	[0.303]	[0.006]	[0.519]	[0.490]	
Compulsory education or less	0.498	−0.006	−0.010	0.005	0.012	−0.034	790
	(0.501)	(0.041)	(0.053)	(0.062)	(0.051)	(0.056)	
		[0.882]	[0.852]	[0.933]	[0.820]	[0.549]	
Secondary education	0.398	0.017	0.031	0.030	−0.007	0.021	790
	(0.491)	(0.040)	(0.052)	(0.061)	(0.050)	(0.055)	
		[0.682]	[0.552]	[0.624]	[0.892]	[0.709]	
Tertiary education	0.104	−0.010	−0.021	−0.035	−0.005	0.013	790
	(0.306)	(0.024)	(0.030)	(0.033)	(0.031)	(0.035)	
		[0.666]	[0.487]	[0.292]	[0.876]	[0.709]	
All hh members Spanish	0.407	0.037	0.028	−0.017	0.106	−0.008	790
	(0.492)	(0.039)	(0.052)	(0.058)	(0.050)	(0.054)	
		[0.353]	[0.591]	[0.769]	[0.034]	[0.889]	
No hh member Spanish	0.249	−0.027	−0.023	−0.027	−0.074	0.031	790
	(0.433)	(0.034)	(0.044)	(0.051)	(0.041)	(0.048)	
		[0.431]	[0.602]	[0.603]	[0.071]	[0.520]	
Mixed nationalities	0.344	−0.010	−0.005	0.044	−0.032	−0.024	790
	(0.476)	(0.038)	(0.049)	(0.059)	(0.047)	(0.052)	
		[0.801]	[0.914]	[0.456]	[0.491]	[0.649]	
Joint test (*p*-value)		0.775	0.253	0.316	0.393	0.739	

Note: Differences in baseline survey covariates between treatment and control groups at endline. Column 1 reports means and standard deviations for the control group. Column 2 shows the coefficient on the treatment dummy, estimating Eq. (E.1). Columns 3–6 report coefficients on dummies for each treatment interaction, estimating Eq. (E.2). We report robust standard errors in parentheses and *p*-values in brackets. The last row reports *p*-values from a joint hypothesis test. The sample comprises of subjects responding at both baseline and endline. See Table D.12 in Appendix D for a description of variables.

Table F.19
Balance of endline sample: baseline admin covariates.

	Control mean (SD)	Treatment group	Activation		No activation		N
			Partial	Full	Partial	Full	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Monthly hh income	411.846	28.998	48.234	25.042	13.976	29.985	904
	(390.486)	(21.576)	(29.555)	(30.528)	(26.660)	(30.759)	
		[0.179]	[0.103]	[0.412]	[0.600]	[0.330]	
Monthly transfers	172.129	13.770	12.682	−0.034	29.518	3.816	904
	(189.831)	(14.482)	(17.752)	(17.707)	(19.136)	(20.835)	
		[0.342]	[0.475]	[0.998]	[0.123]	[0.855]	
RGC recipient	0.041	0.039	0.080	0.050	−0.020	0.061	904
	(0.198)	(0.018)	(0.030)	(0.032)	(0.020)	(0.029)	
		[0.030]	[0.007]	[0.116]	[0.330]	[0.036]	
Monthly hh income	401.559	37.065	59.632	50.928	20.833	21.482	790
	(390.835)	(22.557)	(30.987)	(30.464)	(28.109)	(32.208)	
		[0.101]	[0.055]	[0.095]	[0.459]	[0.505]	
Monthly transfers	172.603	20.347	18.349	14.328	32.452	10.856	790
	(194.723)	(15.797)	(19.299)	(19.494)	(21.182)	(22.630)	
		[0.198]	[0.342]	[0.463]	[0.126]	[0.632]	
RGC recipient	0.041	0.034	0.063	0.053	−0.023	0.064	790
	(0.198)	(0.019)	(0.031)	(0.035)	(0.022)	(0.031)	
		[0.068]	[0.038]	[0.127]	[0.282]	[0.041]	
Joint test (*p*-value)		0.081	0.018	0.360	0.308	0.180	
Joint test (*p*-value)		0.090	0.047	0.208	0.225	0.236	

Note: Differences in covariates from administrative sources between treatment and control groups and treatment arms at endline. Column 1 reports means and standard deviations for households in the control group. Column 2 shows the coefficient on the treatment dummy, estimating Eq. (E.1). Columns 3–6 report coefficients on dummies for each treatment interaction, estimating Eq. (E.2). We report robust standard errors in parentheses and *p*-values in brackets. The last two rows report *p*-values from a joint hypothesis test. The sample in the upper panel comprises of endline respondents. The sample in the lower panel comprises of subjects responding at both baseline and endline. See Table D.12 in Appendix D for a description of variables.

Table F.20
Balance of endline sample: baseline survey outcomes.

	Control mean (SD)	Treatment group	Activation		No activation		N
			Partial	Full	Partial	Full	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Working	0.421	−0.036	−0.062	−0.027	−0.003	−0.054	788
	(0.495)	(0.040)	(0.051)	(0.061)	(0.051)	(0.054)	
		[0.370]	[0.228]	[0.653]	[0.948]	[0.318]	
Job search past 4 w	0.516	−0.011	0.063	−0.085	−0.008	−0.051	788
	(0.501)	(0.039)	(0.051)	(0.060)	(0.050)	(0.054)	
		[0.772]	[0.217]	[0.155]	[0.875]	[0.343]	
Social participation	0.344	0.072	0.124	0.054	0.026	0.084	790
	(0.476)	(0.039)	(0.052)	(0.060)	(0.049)	(0.055)	
		[0.067]	[0.017]	[0.362]	[0.588]	[0.123]	
Education past 12 m	0.204	0.057	0.109	0.052	0.028	0.038	790
	(0.404)	(0.034)	(0.046)	(0.053)	(0.043)	(0.046)	
		[0.088]	[0.018]	[0.323]	[0.505]	[0.412]	
No. working	0.792	−0.034	0.005	−0.002	−0.031	−0.107	790
	(0.746)	(0.061)	(0.084)	(0.100)	(0.076)	(0.078)	
		[0.582]	[0.953]	[0.983]	[0.683]	[0.168]	
At least one working	0.620	−0.042	−0.056	−0.054	−0.019	−0.050	790
	(0.487)	(0.040)	(0.053)	(0.061)	(0.051)	(0.055)	
		[0.290]	[0.286]	[0.382]	[0.712]	[0.365]	
No. in education	0.615	−0.034	0.060	0.043	−0.146	−0.054	790
	(0.895)	(0.068)	(0.086)	(0.102)	(0.080)	(0.089)	
		[0.618]	[0.486]	[0.674]	[0.067]	[0.548]	
At least one in education	0.430	−0.006	0.057	0.036	−0.072	−0.023	790
	(0.496)	(0.040)	(0.052)	(0.062)	(0.050)	(0.055)	
		[0.886]	[0.279]	[0.561]	[0.152]	[0.679]	
Joint test (*p*-value)		0.173	0.031	0.817	0.086	0.580	

Note: Differences in baseline survey outcomes between treatment and control groups at endline. Column 1 reports means and standard deviations for households in the control group. Column 2 shows the coefficient on the treatment dummy, estimating Eq. (E.1). Columns 3–6 report coefficients on dummies for each treatment interaction, estimating Eq. (E.2). We report robust standard errors in parentheses and *p*-values in brackets. The last row reports *p*-values from a joint hypothesis test. The sample comprises of subjects responding at both baseline and endline. See Table D.13 in Appendix D for a description of variables.

Appendix G. Sensitivity analyses

We perform three additional analyses to assess the sensitivity of our results. First, we evaluate potential bias due to imbalance in unobservables relying on Oster (2019). This method provides bias-adjusted effect estimates under two assumptions: (i) The relative degree of selection on observables and unobservables (δ), and (ii) the explained variation in a hypothetical regression with both observed and unobserved controls (R_{max}).

Following the author, we assume equal importance of observables and unobservables ($\delta = 1$), and set R_{max} to 1.3 times the R-squared of our fully controlled model, a value deemed adequate based on prior randomized controlled trials. Table G.21 shows the results. The first

Table G.21
Treatment effects at endline: coefficient stability (Oster, 2019).

	Baseline		Adjusted	
	Uncontrolled coefficient ($\tilde{\beta}$) (SE) [$\tilde{R}$] (1)	Controlled coefficient ($\dot{\beta}$) (SE) [$\dot{R}$] (2)	β^* [Rmax] (3)	δ for $\beta = 0$ given Rmax (4)
Working	−0.131 (0.038) [0.017]	−0.102 (0.034) [0.277]	−0.097 [0.360]	11.5
Job search past 4 w	−0.015 (0.011) [0.008]	−0.015 (0.011) [0.036]	−0.016 [0.047]	−1061.0
Social participation	0.037 (0.037) [0.024]	0.011 (0.035) [0.167]	−0.000 [0.217]	1.0
Education past 6 m	0.035 (0.032) [0.021]	0.034 (0.032) [0.094]	0.037 [0.122]	−26.2
No. working	−0.181 (0.060) [0.022]	−0.155 (0.052) [0.328]	−0.146 [0.426]	11.5
At least one working	−0.113 (0.038) [0.019]	−0.088 (0.033) [0.293]	−0.082 [0.381]	10.5
No. in education	0.054 (0.060) [0.015]	0.059 (0.053) [0.255]	0.066 [0.332]	−11.1
At least one in education	0.041 (0.038) [0.014]	0.040 (0.036) [0.142]	0.041 [0.185]	−121.7

Note: OLS estimates of treatment effects and bias-adjusted treatment effects on survey outcomes at endline follow the method of Oster (2019). Outcome variables are listed on the left and described in detail in Table D.13 in Appendix D. Column 1 reports coefficients on the treatment dummy from an uncontrolled model, and Column 2 from a controlled model, estimating Eq. (1). We report robust standard errors in parentheses and R-squared in brackets. Column 3 shows bias-adjusted coefficients, assuming equal importance of observables and unobservables ($\delta = 1$), and setting R_{max} equal to 1.3 times the R-squared of the fully controlled model. The last column reports values for δ at which treatment effects are zero given R_{max}.

Table G.22
Treatment effects at endline: Lee bounds.

	Lee bound lower (1)	Lee bound upper (2)	N (3)	N selected (4)
Working	−0.285 (0.050) [0.000]	−0.057 (0.046) [0.215]	1200	900
Job search past 4 w	−0.024 (0.010) [0.013]	−0.010 (0.012) [0.397]	1200	900
Social participation	−0.083 (0.052) [0.109]	0.147 (0.048) [0.002]	1200	900
Education past 6 m	−0.112 (0.049) [0.022]	0.107 (0.039) [0.007]	1200	900
No. working	−0.416 (0.074) [0.000]	−0.049 (0.076) [0.520]	1200	900
At least one working	−0.229 (0.049) [0.000]	0.001 (0.050) [0.985]	1200	900
No. in education	−0.161 (0.074) [0.030]	0.208 (0.075) [0.006]	1200	900
At least one in education	−0.071 (0.052) [0.172]	0.160 (0.049) [0.001]	1200	900
Employed (admin)	−0.297 (0.058) [0.000]	−0.036 (0.047) [0.454]	1200	869

Note: Estimates of treatment effect intervals follow the bounding method of Lee (2009). We report standard errors in parentheses and p-values in brackets. Column 1 reports upper Lee bounds and column 2 reports lower Lee bounds. Column 3 shows the number of observations in the full sample and column 4 shows the sample size observed at endline. We estimate Lee bounds with the user-written Stata command *leebounds* and use randomization strata as a tightening variable (Tauchmann, 2014).

Table G.23
Treatment effects at endline: logistic regression.

	Control mean (SD) (1)	No controls (2)	Baseline value (3)	Individual controls (4)	Non-attriters (5)
Working	0.473	0.579	0.547	0.559	0.544
	(0.500)	(0.158)	(0.182)	(0.188)	(0.204)
		[0.001]	[0.001]	[0.002]	[0.003]
At least one working	0.637	0.624	0.613	0.622	0.632
	(0.482)	(0.160)	(0.180)	(0.184)	(0.196)
		[0.003]	[0.007]	[0.010]	[0.020]
Employed (admin)	0.476	0.595		0.598	0.559
	(0.501)	(0.161)		(0.173)	(0.185)
		[0.001]		[0.003]	[0.002]
Observations (survey)		900	900	899	788
Observations (admin)		869		868	760

Note: Logistic regression estimates of treatment effects on survey outcomes at endline. Outcome variables are listed on the left and described in detail in Table D.13 in Appendix D. Column 1 reports control group means with standard deviations in parentheses. Columns 2–5 show coefficients on the treatment dummy, using different specifications and sample restrictions. We report coefficients as Odds Ratios. Robust standard errors are shown in parentheses and *p*-values are provided in brackets.

two columns report estimates from our uncontrolled and fully controlled models, while the third column shows bias-adjusted coefficients (β^*).

We find that coefficients are stable, alleviating concerns about unobservable selection. The labor supply response of the main recipient shifts slightly from –10.2 to –9.7 percentage points. The effect on the number of working household members adjusts from –0.155 to –0.146. Bias-adjusted coefficients remain well within the 95 % confidence intervals of baseline results.

The method also determines the values of δ required for treatment effects to become zero. For most outcomes, δ is large, indicating that unobservables would need to be far more important than observables to produce a zero effect.[27] The only exception is social participation, where the effect is already zero under equal importance of observables and unobservables.

Second, we estimate intervals for the true treatment effects under potential differential attrition at endline, following the method proposed by Lee (2009). We employ the user-written Stata command *leebounds* and use randomization strata as a tightening variable (Tauchmann, 2014). Table G.22 reports the results. The estimated intervals are fairly wide. For work-related outcomes, intervals include only negative values, although the upper bounds are not statistically significantly different from zero. For outcomes related to social participation and education, the intervals span both positive and negative values.

We are hesitant to put too much weight on these results. Selection into endline attrition and differential attrition was minimal (see Section 4.1). Moreover, results for administrative data are very similar when comparing endline respondents with the full sample. The extremity of the bounds is illustrated by estimating intervals for our administrative outcome where we have nearly complete data. The interval ranges from –29.7 to –3.6 percentage points, while the true uncontrolled estimate is –12.2 percentage points.

Lastly, we use logistic regression instead of OLS to estimate effects when the dependent variable is binary. Table G.23 reports the results, which do not change significantly.

Appendix H. Additional tables

See Tables H.24–H.36.

Table H.24
Number and share of households excluded from the program per reason.

Reason	No. of Households	Share of households (%)
Not eligible due to income or assets	38	36.2
No show	29	27.6
Refusal	22	21.0
Residency outside target area	16	15.2
Total	105	100.0

Table H.25
Participation rates per treatment arm.

		Activation		No activation	Total
		Social entrepreneurship	Community involvement		
Withdrawal	Full	–	92.0 %	85.2 %	88.1 %
	Partial	90.0 %	86.2 %	85.5 %	86.7 %
Total		90.0 %	89.1 %	85.4 %	87.2 %

Note: Number of households actually participating in the B-MINCOME program in each treatment arm is divided by the number of households assigned to each treatment arm.

[27] For example, unobservables would need to be 11.5 times more important than observables to eliminate the labor supply effect of the main recipient.

Table H.26
Participants versus non-participants: baseline survey covariates.

	Participation mean (SD) (1)	Non-participation (2)	N (3)
Main recipient female	0.719 (0.450)	−0.026 (0.055) [0.642]	704
Main recipient age	40.802 (8.508)	0.734 (1.027) [0.475]	704
No. members	4.182 (1.494)	0.112 (0.207) [0.590]	704
No. members <16	1.762 (1.186)	−0.129 (0.142) [0.363]	704
No. members 16–64	2.343 (1.068)	0.156 (0.150) [0.298]	704
Single-person hh	0.027 (0.163)	0.001 (0.022) [0.979]	704
Single-parent hh	0.139 (0.346)	−0.035 (0.038) [0.351]	704
Adults without children	0.134 (0.341)	0.055 (0.048) [0.247]	704
Adults with children	0.700 (0.459)	−0.020 (0.057) [0.723]	704
Compulsory education or less	0.513 (0.500)	−0.023 (0.060) [0.699]	704
Secondary education	0.380 (0.486)	0.043 (0.059) [0.471]	704
Tertiary education	0.107 (0.309)	−0.019 (0.035) [0.579]	704
All hh members Spanish	0.468 (0.499)	0.076 (0.061) [0.208]	704
No hh member Spanish	0.214 (0.410)	0.006 (0.049) [0.900]	704
Mixed nationalities	0.318 (0.466)	−0.083 (0.051) [0.104]	704
Joint test (*p*-value)		0.661	

Note: Differences in baseline survey covariates between households participating and not participating in the B-MINCOME program. Non-participating households are households assigned to treatment, but not actually included in the program for various reasons (see Table H.24). Participating households took up treatment. Column 1 reports means and standard deviations for participating households. Column 2 shows the coefficient on a non-participation dummy, estimating an adapted version of Eq. (F.1). We report robust standard errors in parentheses and *p*-values in brackets. The last row reports *p*-values from a joint hypothesis test. The sample does not comprise 822 observations due to baseline non-response. See Table D.12 in Appendix D for a description of variables.

Table H.27
Participants versus non-participants: baseline admin covariates.

	Participation mean (SD) (1)	Non-participation (2)	N (3)
Monthly hh income	576.754 (421.840)	−17.617 (32.658) [0.590]	822
Monthly transfers	180.801 (187.915)	−53.229 (15.903) [0.001]	822
RGC recipient	0.105 (0.306)	0.000 (0.033) [0.992]	822
Joint test (*p*-value)		0.019	

Note: Differences in covariates obtained from administrative sources between households participating and not participating in the B-MINCOME program. Non-participating households are households assigned to treatment, but not actually included in the program for various reasons (see Table H.24). Participating households took up treatment. Column 1 reports means and standard deviations for participating households. Column 2 shows the coefficient on a non-participation dummy, estimating an adapted version of Eq. (F.1). We report robust standard errors in parentheses and *p*-values in brackets. The last row reports *p*-values from a joint hypothesis test. See Table D.12 in Appendix D for a description of variables.

Table H.28
Participants versus non-participants: baseline survey outcomes.

	Participation mean (SD) (1)	Non-participation (2)	N (3)
Working	0.351 (0.478)	0.180 (0.060) [0.003]	702
Job search past 4 w	0.502 (0.500)	−0.140 (0.057) [0.015]	701
Social participation	0.414 (0.493)	−0.033 (0.059) [0.580]	704
Education past 12 m	0.246 (0.431)	0.013 (0.053) [0.800]	704
No. working	0.722 (0.756)	0.213 (0.106) [0.045]	704
At least one working	0.562 (0.497)	0.067 (0.059) [0.256]	704
No. in education	0.545 (0.750)	0.029 (0.092) [0.750]	704
At least one in education	0.409 (0.492)	−0.012 (0.059) [0.844]	704
Joint test (*p*-value)		0.010	

Note: Differences in baseline survey outcomes between households participating and not participating in the B-MINCOME program. Non-participating households are households assigned to treatment, but not actually included in the program for various reasons (see Table H.24). Participating households took up treatment. Column 1 reports means and standard deviations for participating households. Column 2 shows the coefficient on a non-participation dummy, estimating an adapted version of Eq. (F.1). We report robust standard errors in parentheses and *p*-values in brackets. The last row reports *p*-values from a joint hypothesis test. The sample does not comprise 822 observations due to baseline non-response. See Table D.13 in Appendix D for a description of variables.

Table H.29
Treatment effects at endline: adjusted *p*-values.

	Control mean (SD)	Treatment group	Activation		No activation	
			Partial	Full	Partial	Full
	(1)	(2)	(3)	(4)	(5)	(6)
Working	0.473	−0.102	−0.106	−0.150	−0.067	−0.113
	(0.500)	(0.034)	(0.042)	(0.049)	(0.042)	(0.047)
		[0.034]	[0.262]	[0.060]	[0.832]	[0.304]
Job search past 4 w	0.024	−0.015	−0.022	−0.004	−0.012	−0.020
	(0.155)	(0.011)	(0.011)	(0.015)	(0.013)	(0.012)
		[0.538]	[0.607]	[0.999]	[0.982]	[0.832]
Social participation	0.376	0.011	0.034	0.072	−0.027	−0.008
	(0.485)	(0.035)	(0.047)	(0.054)	(0.044)	(0.049)
		[0.761]	[0.993]	[0.907]	[0.997]	[0.999]
Education past 6 m	0.212	0.034	0.100	0.051	0.001	−0.007
	(0.410)	(0.032)	(0.044)	(0.048)	(0.038)	(0.042)
		[0.597]	[0.360]	[0.970]	[0.999]	[0.999]
No. working	0.869	−0.155	−0.202	−0.186	−0.117	−0.131
	(0.824)	(0.052)	(0.066)	(0.075)	(0.064)	(0.072)
		[0.034]	[0.062]	[0.262]	[0.691]	[0.706]
At least one working	0.637	−0.088	−0.129	−0.109	−0.056	−0.069
	(0.482)	(0.033)	(0.042)	(0.047)	(0.042)	(0.046)
		[0.056]	[0.062]	[0.353]	[0.907]	[0.867]
No. in education	0.527	0.059	0.079	0.114	0.024	0.045
	(0.802)	(0.053)	(0.071)	(0.079)	(0.063)	(0.074)
		[0.597]	[0.959]	[0.879]	[0.999]	[0.997]
At least one in education	0.392	0.040	0.050	0.081	0.024	0.020
	(0.489)	(0.036)	(0.048)	(0.054)	(0.045)	(0.050)
		[0.597]	[0.970]	[0.863]	[0.997]	[0.999]

Note: OLS estimates of treatment effects on survey outcomes at endline. Outcome variables are listed on the left and described in detail in Table D.13 in Appendix D. Column 1 reports control group means with standard deviations in parentheses. Column 2 shows coefficients on the treatment dummy, estimating Eq. (1). Columns 3–6 report treatment effects for each treatment cell relative to the control group, estimating Eq. (2). We report robust standard errors in parentheses and adjusted *p*-values using the Westfall and Young (1993) methodology and 5000 bootstrap draws in brackets. All models include randomization strata fixed effects and control for the survey mode, the respective baseline value, and the covariates listed in Table 1.

Table H.30
Treatment effects at endline: administrative data (2).

	Control mean (SD)	No controls	Individual controls	Non-attriters	Full sample
	(1)	(2)	(3)	(4)	(5)
Employed (admin)	0.472	−0.100	−0.082	−0.096	−0.093
	(0.500)	(0.039)	(0.038)	(0.040)	(0.031)
		[0.011]	[0.032]	[0.017]	[0.003]
Observations		869	869	760	1157
Individual controls			✓	✓	✓

Note: OLS estimates of treatment effects on employment probabilities in July 2019. Employment data were obtained from social security records. Column 1 reports control group means with standard deviations in parentheses; other columns show coefficients on the treatment dummy from different models. Column 2 excludes control variables, while Column 3 includes individual covariates. In both columns, the sample is restricted to endline survey respondents. Column 4 restricts the sample to subjects responding at both baseline and endline. Column 5 reports results for the full sample. We report robust standard errors in parentheses and *p*-values in brackets. All models include randomization strata fixed effects.

Table H.31
Treatment effects at endline: excluding the social entrepreneurship arm.

	Activation		No activation		Average	
	Partial	Full	Partial	Full	Activation	Partial
	(1)	(2)	(3)	(4)	(5)	(6)
Working	−0.122	−0.152	−0.072	−0.116	−0.044	0.038
	(0.048)	(0.049)	(0.042)	(0.047)	(0.036)	(0.035)
	[0.011]	[0.002]	[0.091]	[0.015]	[0.215]	[0.280]
Job search past 4 w	−0.019	−0.004	−0.012	−0.020	0.003	−0.001
	(0.013)	(0.015)	(0.014)	(0.012)	(0.010)	(0.008)
	[0.137]	[0.796]	[0.370]	[0.108]	[0.728]	[0.895]
Social participation	0.085	0.076	−0.022	−0.002	0.094	−0.009
	(0.055)	(0.055)	(0.044)	(0.049)	(0.041)	(0.039)
	[0.122]	[0.164]	[0.617]	[0.973]	[0.022]	[0.815]
Education past 6 m	0.014	0.049	0.004	−0.007	0.031	−0.007
	(0.047)	(0.048)	(0.038)	(0.043)	(0.035)	(0.033)
	[0.773]	[0.311]	[0.918]	[0.861]	[0.380]	[0.837]

(*continued on next page*)

Table H.31 *(continued)*

	Activation		No activation		Average	
	Partial (1)	Full (2)	Partial (3)	Full (4)	Activation (5)	Partial (6)
No. working	−0.241	−0.182	−0.115	−0.130	−0.093	−0.014
	(0.076)	(0.075)	(0.064)	(0.073)	(0.056)	(0.055)
	[0.002]	[0.016]	[0.072]	[0.075]	[0.100]	[0.804]
At least one working	−0.145	−0.107	−0.056	−0.068	−0.067	−0.007
	(0.051)	(0.047)	(0.042)	(0.046)	(0.038)	(0.037)
	[0.004]	[0.023]	[0.179]	[0.143]	[0.079]	[0.842]
No. in education	0.008	0.112	0.027	0.046	0.019	−0.052
	(0.084)	(0.080)	(0.063)	(0.074)	(0.061)	(0.057)
	[0.921]	[0.161]	[0.666]	[0.533]	[0.757]	[0.363]
At least one in education	−0.021	0.080	0.028	0.021	−0.000	−0.035
	(0.054)	(0.053)	(0.045)	(0.051)	(0.041)	(0.039)
	[0.701]	[0.135]	[0.530]	[0.678]	[0.992]	[0.377]
Employed (admin)	−0.123	−0.151	−0.071	−0.170	−0.020	0.071
	(0.056)	(0.056)	(0.048)	(0.050)	(0.041)	(0.039)
	[0.028]	[0.007]	[0.138]	[0.001]	[0.619]	[0.065]
Observations per cell	112	114	209	149		

Note: OLS estimates of treatment effects on outcomes at endline but excluding units assigned to the social entrepreneurship arm. Outcome variables are listed on the left and described in detail in Table D.13 in Appendix D. Columns 1–4 report treatment effects for each treatment cell relative to the control group, estimating Eq. (2). Column 5 and 6 report the average effects of activation relative to no activation, and partial relative to full withdrawal, using linear combinations. We report robust standard errors in parentheses and *p*-values in brackets. The sample for administrative outcomes is restricted to endline respondents for comparability.

Table H.32
Heterogeneous treatment effects: employment (2).

	Gender (1)	Children (2)	Gender X Children (3)
Treatment	−0.032	−0.028	0.153
	(0.063)	(0.080)	(0.115)
	[0.614]	[0.725]	[0.184]
Interaction effect			
Female	−0.100		
	(0.073)		
	[0.173]		
Any child 0–16		−0.094	
		(0.087)	
		[0.283]	
Male w/ children			−0.260
			(0.135)
			[0.055]
Female wo/ children			−0.331
			(0.154)
			[0.032]
Female w/ children			−0.281
			(0.122)
			[0.022]
Observations	998	998	998

Note: OLS estimates of treatment and interaction effects on employment probabilities in July 2019. Employment data were obtained from social security records. Column 1 interacts the treatment dummy with a dummy indicating the gender of the main recipient. Column 2 interacts the treatment dummy with a dummy indicating children in the household. Column 3 interacts the treatment dummy with the previous two dummies. The first row reports coefficients on the treatment dummy for the reference group. We report robust standard errors in parentheses and *p*-values in brackets. All models include randomization strata fixed effects and individual covariates. The sample is restricted to baseline respondents.

Table H.33
Heterogeneous treatment effects: survey data.

	Gender (1)	Children (2)	Gender X Children (3)
Treatment	0.009	−0.080	0.117
	(0.065)	(0.097)	(0.124)
	[0.885]	[0.406]	[0.344]
Interaction effect			
Female	−0.152		
	(0.076)		
	[0.047]		
Any child 0–16		−0.027	
		(0.103)	
		[0.790]	
Male w/ children			−0.159
			(0.144)
			[0.270]
Female wo/ children			−0.378
			(0.181)
			[0.037]
Female w/ children			−0.247
			(0.130)
			[0.058]
Observations	788	788	788

Note: OLS estimates of treatment and interaction effects on the probability that the main recipient is working. Employment data were obtained from survey data. Column 1 interacts the treatment dummy with a dummy indicating the gender of the main recipient. Column 2 interacts the treatment dummy with a dummy indicating households larger than four members (median = 4). Column 3 interacts the treatment dummy with four dummy variables, indicating households in which the youngest child is 0–2, 3–5, 6–1, or 12–16 years old, respectively. Column 4 interacts the treatment dummy with a dummy indicating children in the household. The first row reports coefficients on the treatment dummy for the reference group. We report robust standard errors in parentheses and *p*-values in brackets. The sample is restricted to baseline and endline respondents.

Table H.34
Treatment effects per subgroup: employment.

	Gender		Any child 0–16		Male		Female	
	Male (1)	Female (2)	No (3)	Yes (4)	No child (5)	Child (6)	No child (7)	Child (8)
Treatment	−0.031	−0.152	−0.018	−0.147	0.163	−0.073	−0.144	−0.158
	(0.065)	(0.040)	(0.085)	(0.036)	(0.152)	(0.078)	(0.123)	(0.042)
	[0.632]	[0.000]	[0.831]	[0.000]	[0.290]	[0.350]	[0.247]	[0.000]
Control mean	0.471	0.469	0.313	0.498	0.227	0.554	0.385	0.480
(SD)	(0.502)	(0.500)	(0.468)	(0.501)	(0.429)	(0.501)	(0.496)	(0.501)
Observations	283	715	148	850	61	222	87	628

Note: OLS estimates of treatment effects on employment probabilities per subgroup. Employment data were obtained from social security records (June 2019). Each column shows the coefficient on the treatment dummy, restricting the sample to a different subgroup. We report robust standard errors in parentheses and *p*-values in brackets. All models include randomization strata fixed effects and control for individual covariates. The sample is restricted to baseline respondents.

Table H.35
Heterogeneous treatment effects: fully interacted.

	Gender (1)	Children (2)	Gender X Children (3)
Treatment	−0.067	0.013	0.204
	(0.067)	(0.083)	(0.089)
	[0.319]	[0.878]	[0.021]
Interaction effect			
Female	−0.089		
	(0.079)		
	[0.260]		
Any child 0–16		−0.168	
		(0.091)	
		[0.064]	
Male w/ children			−0.339
			(0.119)
			[0.004]
Female wo/ children			−0.232
			(0.135)
			[0.085]
Female w/ children			−0.372
			(0.100)
			[0.000]
Observations	998	998	998

Note: OLS estimates of treatment and interaction effects on employment probabilities in June 2019. Employment data were obtained from social security records. Column 1 interacts the treatment dummy and all controls with a dummy indicating the gender of the main recipient. Column 2 interacts the treatment dummy and all controls with a dummy indicating children in the household. Column 3 interacts the treatment dummy and all controls with the previous two dummies. The first row reports coefficients on the treatment dummy for the reference group. We report robust standard errors in parentheses and *p*-values in brackets. All models include randomization strata fixed effects and control for individual covariates. The sample is restricted to baseline respondents.

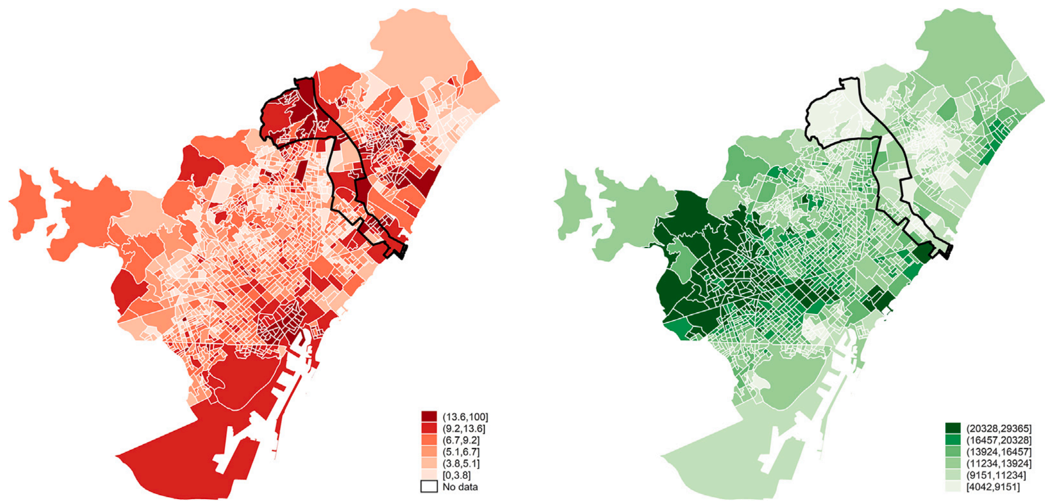
Table H.36
Heterogeneous treatment effects: other subgroups.

	HH size (1)	Age children (2)
Treatment	−0.131	0.006
	(0.042)	(0.079)
	[0.002]	[0.944]
>4 members	0.018	
	(0.068)	
	[0.796]	
Youngest child 0–2		−0.155
		(0.107)
		[0.148]
Youngest child 3–5		−0.130
		(0.104)
		[0.212]
Youngest child 6–11		−0.147
		(0.100)
		[0.144]
Youngest child 12–16		−0.234
		(0.127)
		[0.065]
Observations	998	998

Note: OLS estimates of treatment and interaction effects on employment probabilities in July 2019. Employment data were obtained from social security records. Column 1 interacts the treatment dummy with a dummy indicating a household with more than four members. Column 2 interacts the treatment dummy with dummies indicating the age of the youngest child in the household, using four age bins. The first row reports coefficients on the treatment dummy for the reference group. We report robust standard errors in parentheses and *p*-values in brackets. All models include randomization strata fixed effects and individual covariates. The sample is restricted to baseline respondents.

Appendix I. Additional figures

See Figs. I.4–I.7.



(a) Share of Households with Annual Earnings below €5,000 in 2016.

(b) Mean Annual Household Income per Capita in 2016.

Fig. I.4. Maps of Barcelona showing household income.
Note: Both maps display neighborhoods of Barcelona and neighboring communities to the North-East (Badalona, Sant Adrià del Besòs, and Santa Coloma de Gramanet). The target area of the trial is circled in black. Breaks of intervals are the 10th, 25th, 50th, 75th and 90th percentiles of the distribution of the respective variable. *Source*: Own calculations based on data from the National Statistics Institute's experimental statistics (*INE Estadística Experimental*). The data can be accessed at https://www.ine.es/experimental/experimental.htm.

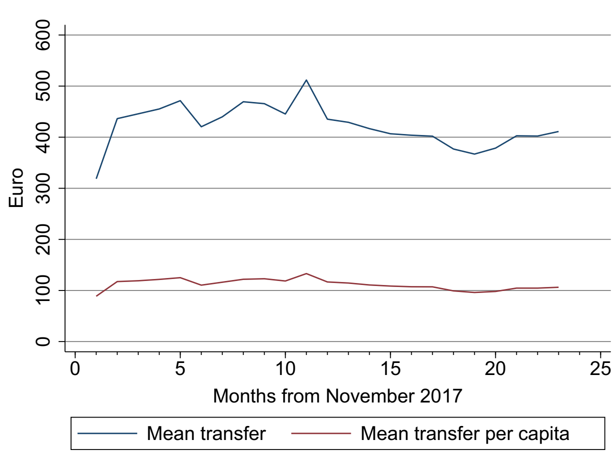


Fig. I.5. Mean transfer and mean transfer per capita per treatment month.
Note: Zero payments included.

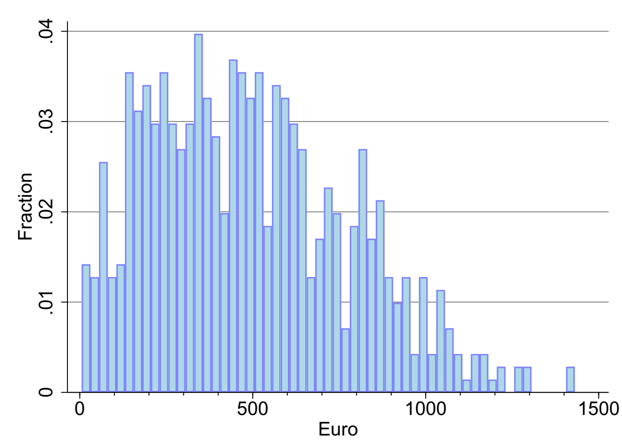


Fig. I.6. Distribution of mean monthly transfers.
Note: Zero payments excluded.

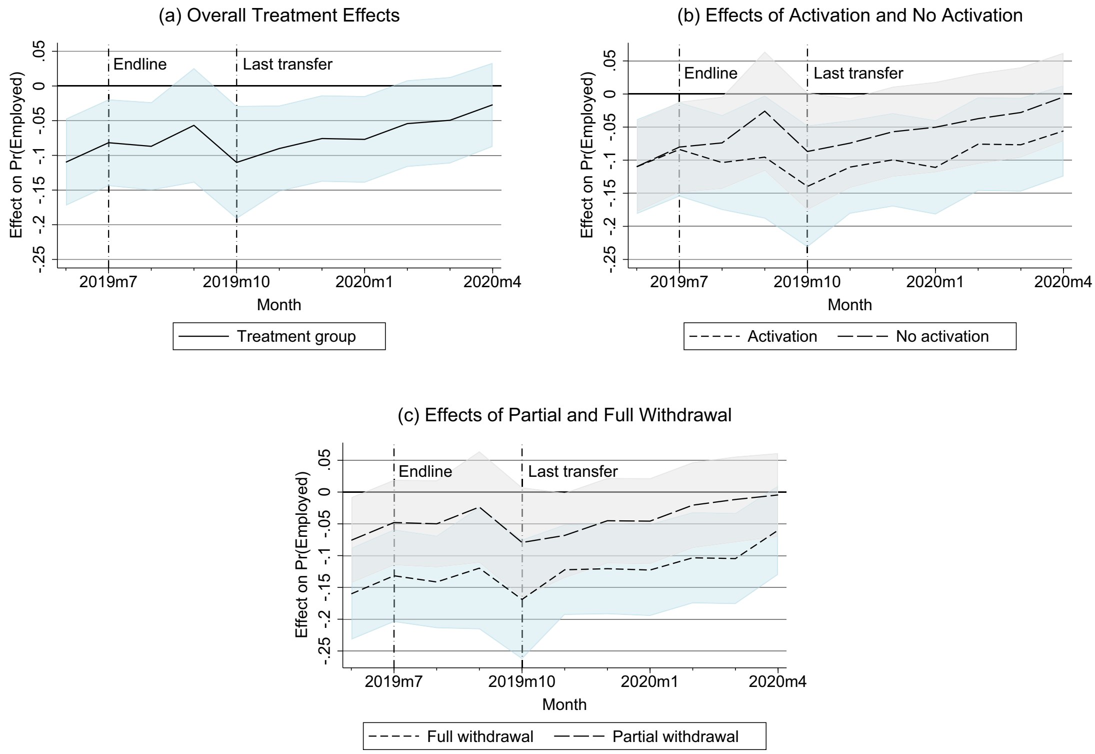


Fig. I.7. Treatment effects on employment probabilities using administrative data.

Note: Grey and colored areas are 95 % confidence intervals. Graphs show ITT effects, which are estimated using separate regressions for each month. Models include individual controls and randomization strata fixed effects. Sample is restricted to endline survey respondents.

Data availability

The authors do not have permission to share data.

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