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IZA DP No. 15976

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ABSTRACT

The Employment Effects of Generous and Unconditional Cash Support*

While unconditional cash transfers have been studied extensively in developing countries, little is known about their effects in a wealthier context. Through a randomized controlled trial, we study the employment effects of a generous and unconditional transfer targeting low-income families in Spain. Two years into the program, subjects assigned to treatment are 20 percent less likely to work than subjects assigned to a control group. Assignment to an activation plan does not attenuate adverse effects; a more lenient transfer withdrawal rate does. It appears that effects are driven by subjects with children, suggesting substitution of labour for care tasks.

JEL Classification: C93, H53, I38, J64

Keywords: welfare reform, cash transfer, basic income, policy evaluation, RCT

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1 Introduction

Governments worldwide rely on financial transfers to eradicate poverty and fight social exclusion. In developing countries, an increasing share of such transfer programs has taken the form of unconditional support, removing any conditions on beneficiaries' actions. Arguments such as lower program costs and the psychological benefits of self-determined spending frequently motivate the implementation of unconditional support. Evaluations have shown these programs to improve health (Pega et al., 2017), education outcomes (Baird et al., 2014), and psychological well-being (Haushofer and Shapiro, 2016), while labor supply effects are small or absent (Baird et al., 2018; Banerjee et al., 2017; Bastagli et al., 2016).

While the impacts of unconditional transfer programs in developing countries are well documented, little is known about their effectiveness in higher-income countries. Effects could differ for several reasons. Labor markets and other economic institutions are structured differently, leading to different constraints. Moreover, transfers might complement existing, potentially extensive safety nets. With this study, we aim to advance the literature on unconditional transfer programs by describing their employment effects in the context of an advanced welfare state. Our analysis uses data from a field experiment in Barcelona (Spain), trialing a generous and unconditional municipal cash transfer program.

We address the following main research question: What is the effect of generous and unconditional cash support on adult labor force participation? The negative work incentives of welfare programs are well described (Moffitt, 2002): Beneficiaries may decide to work less due to the income effect, while substitution effects provide further disincentives if a program is means-tested. With unconditional support, effort requirements that could restore work incentives are absent. However, a transfer without strings attached may allow for human capital or other investments that result in higher wages and thus increase work incentives. Also, fear of scarring effects or human capital deterioration could prevent beneficiaries from working less or leaving the workforce altogether.

In addition to studying overall employment effects, we seek to answer three sub-questions. First, we are interested in the impact on choices between salaried work versus self-employment, temporary versus permanent contracts, and full-time versus part-time work. An unconditional transfer may affect these choices by alleviating credit constraints (self-employment), providing liquidity while searching for a better job (permanent employment), or funding leisure (part-time work). Our second sub-question is: What is the effect on job search, human capital formation, and social participation? It is interesting to study these outcomes, as an unconditional transfer may allow for different time allocations between work-related and other activities. Our third and last sub-question is: Do different program modalities help maintain work incentives? We focus on two modalities: a social activation component and a generous transfer withdrawal rate. In addition to average effects, we also study effect heterogeneity, focusing on households with and without care responsibilities.

We use data from a two-year randomized controlled trial (RCT) that tested a new municipal antipoverty program targeting economically vulnerable households in disadvantaged neighborhoods of Barcelona. The program consisted of two components: (i) a household-based cash transfer, depending on household income, size, and composition, and (ii) different social activation policies. The cash transfer averaged roughly €500 ($792 PPP) per month, which is about half the monthly statutory minimum wage and approximately 90 percent of households' monthly income before the start of the program. Payments were made to a designated adult household member, the *main recipient*.[1] Activation policies targeted social entrepreneurship and community involvement.

We apply three comparisons. First, we compare households randomly chosen to become program beneficiaries with households assigned to a control group. Second, we contrast treatment households randomly assigned to a social activation plan with transfer-only households. Third, we compare treatment households assigned to a 100 percent transfer withdrawal rate with those assigned a 25–35 percent withdrawal rate. Our primary data sources are two waves of surveys; conducted at baseline and endline. We complement this data with employment information from administrative records.

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

Our main findings can be summarized in three parts. First, we find that the program had sizeable and adverse employment effects on average. Roughly two years after the start of the program, main recipients in treatment households are 20 percent less likely to work compared to their counterparts in control households. We find confirmation of negative employment effects when pooling outcomes at the household level. Probabilities of job search, social participation, and participation in education activities appear unaffected. Notably, adverse employment effects persist six months after the end of transfers.

Second, assignment to a social activation plan does not prevent negative labor supply effects. If anything, we find tentative evidence that some employment outcomes may worsen under activation. Implementing a more lenient benefit reduction rate attenuates but does not eliminate negative labor supply effects.

Third, the results of a heterogeneity analysis suggest that adverse employment effects are driven by households with care responsibilities. While employment effects are largely absent among households without children, they are sizeable and negative among households with children living at home. Hence, a potential mechanism explaining our results may be substituting labor for care tasks.

So far, only a few studies have evaluated the working of unconditional transfer programs in developed countries. Their findings largely contrast with the sizeable negative employment effects that we find. Among the earliest are a handful of studies that analyzed negative income tax (NIT) experiments conducted at five study sites in the United States and Canada in the 1970s. The NIT experiments randomly assigned households to a monthly guaranteed income without any work requirements but subject to a withdrawal rate (see Burtless, 1986, for a detailed description of the experiments). Evaluations find moderate declines in employment and hours worked, which are probably exaggerated due to misreporting and selective attrition (Ashenfelter and Plant, 1990; Burtless, 1986; Robins, 1985).

More recent evidence relates to dividend programs. These programs differ from traditional transfer schemes by providing cash assistance without a means test, i.e., irrespective of household income. Jones and Marinescu (2022) study the labor market effects of the Alaska Permanent Fund Dividend, which distributes oil-production revenues to every Alaska resident in the form of a yearly transfer ($1,600 in 2019). Using a synthetic control group method, the authors find no impact on aggregate employment but increases in part-time work. Akee et al. (2010) evaluate the Eastern Band of Cherokee Indians Casino Dividend, a household-based transfer of $4,000–$6,000 a year. Using a differences-in-difference method, the authors find no effects on adult employment outcomes.

Evidence from a European context comes from Finland and Italy. Studying the Finish basic income experiment, which replaced minimum unemployment benefits with an unconditional transfer of equal size, Hämäläinen et al. (2022) find no employment effects. Evidence on a program similar to ours comes from Del Boca et al. (2021) and Aparicio Fenoll and Quaranta (2022). The authors evaluate a cash transfer of €2,500–€3,500 a year targeting low-income individuals in Turin (Italy). Results show positive labor supply effects for male recipients when receiving the transfer conditional on a labor-market-oriented mentoring course and no effects on labor market outcomes if the transfer is provided unconditionally.

We add to this literature by describing the effects of a comparatively generous transfer program. To the best of our knowledge, the program we study is the first cash transfer in a developed country that provides subsistence-level assistance without any strings attached. Moreover, by exploiting a randomized design and collecting social security data next to self-reported information from surveys, we can circumvent internal validity concerns encountered by earlier studies. Lastly, studying a temporary program and collecting data post-treatment allows us to document the persistence of effects after program termination.

The remainder of this paper is organized as follows. Section 2 describes the program studied. Section 3, discusses the policy and local context. In Section 4, we set out the experimental design and method, while Section 5 covers data collection and outcome variables. Section 6 discusses experiment integrity and Section 7 our empirical strategy. In Section 8, we present our results, while Section 9 concludes.

2 Treatment Program

The treatment we study is a municipal antipoverty program introduced by the City Council of Barcelona. The program, named B-MINCOME, aimed to improve households' socio-economic situation to combat poverty and social exclusion in disadvantaged neighborhoods. Before roll-out, the program was tested in a two-year randomized controlled trial, which we describe in Section 4. B-MINCOME consisted of two main components: (i) an income support component, called *Municipal Inclusion Support Benefit* (henceforth: *SMI benefit*), and (ii) an activation component. We now describe both components in more detail.

SMI benefit. The SMI benefit was a monthly payment to stock up household income to an imputed subsistence level. Accordingly, the benefit level depended on household income, size, and composition. Appendix A provides a detailed account of how the city council determined the benefit level. Transfers could vary between €100 ($154 PPP) and €1,676 ($2,586 PPP) per month. The maximum level corresponds with twice the 2016 at-risk-of-poverty threshold for single-person households in the area. For comparison, the national monthly minimum wage was €826 ($1,309 PPP) when implementing the program. Although the program targeted households, payments were made to one designated household member (henceforth: *main recipient*) selected by the household. Hence, only one person per household could register for the program. Other (potential) household members were treated as joint beneficiaries. The benefit level responded to changes in household income, size, and composition. For some households, additional income would reduce the transfer one-on-one (withdrawal rate of 100 percent). Other households faced a withdrawal rate of 23–35 percent. We will describe these modalities in more detail in Section 4.3.

Activation policies. The B-MINCOME program included two social activation plans promoting social entrepreneurship and community involvement, respectively.[2] The first plan encouraged participation in

[2] B-MINCOME included two additional plans, promoting housing renovations for

the social and community life of the neighborhood. Under the second plan, participants trained to become social entrepreneurs or gained work experience in a social entrepreneurship initiative. A maximum of one person per household could take part in the activities. Participants could also be household members other than the main recipient.

Participation in the B-MINCOME program was voluntary and subject to application. Main recipients and their households had to meet six criteria to be eligible for the program. First, household members had to be registered as Barcelona residents for at least two years and live in the target area of the trial. Second, eligibility required an open file at the municipal social service office for legal reasons.[3] Third, at least one household member had to be aged between 25 and 60. Fourth, the household members had to share (not divide) household expenses. Fifth, household income at the start of the program had to lie below an eligibility threshold, such that the household would receive a monthly transfer. Lastly, excluding the household's primary residence, household assets could not exceed four times the maximum annual SMI benefit.

3 Background

3.1 Socio-Economic Context

The B-MINCOME program was implemented in Barcelona, the capital of the autonomous community of Catalonia and Spain's second-most populated city. In 2017, roughly 1.6m people lived within Barcelona city limits, while the Barcelona urban area counted 5.0m inhabitants (Eurostat, 2021).

Despite being an economic driving force, accounting for one-fifth of the Spanish GDP (National Statistics Institute, 2021), significant socio-economic disparities exist within the city. Among the most disadvantaged

room rental and offering vocational training. Due to implementation problems, we exclude households assigned to these two plans from our analysis. Excluding households is unproblematic due to random assignment.

[3]The municipal social service office provides information, assistance, and financial aid to vulnerable citizens. The services offered are diverse and may include, e.g., financial emergency aid, access to soup kitchens, temporary housing, child allowances, or counseling.

districts are ten neighborhoods located at the North-Eastern city limits known as the *Eix Besòs* area (Besòs Axis).[4] The city council chose this area, which comprises roughly 7 percent of Barcelona's total population (114.000 inhabitants), as a target area for trialing the program.

The area's socio-economic vulnerability shows in several indicators.[5] *Eix Besòs* has some of the highest unemployment rates in Barcelona. While roughly 7 percent of Barcelona's total working-age population was registered unemployed at the start of the trial, unemployment rates were almost twice as high in some of the target neighborhoods. In 2016, the mean household income per capita in the target area was roughly 50 percent of an average Barcelona household. The vulnerability of inhabitants also shows in education indicators. In most target neighborhoods, approximately 40 percent of the adult population have either no degree or completed no more than primary education—a rate almost twice the city's average.

For illustration, Panel A of Figure 1 shows a map on the neighborhood-level with the share of households earning less than €5,000 ($7,925 PPP) per year, circling the ten target neighborhoods in black. Panel B shows a map with the mean annual household income per capita. The maps extend into neighboring communities in the north-east, revealing that the target area also stands out compared to close urban areas outside Barcelona city limits.

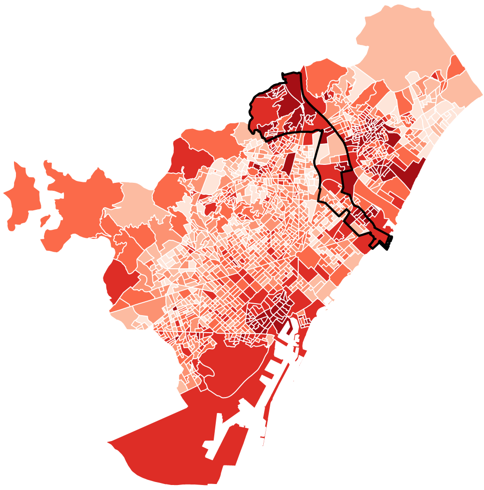
3.2 Institutional Context

The B-MINCOME program complements existing income support schemes available in the trial area. Due to Spain's decentralized political structure, some schemes are region-specific, while others are centralized.

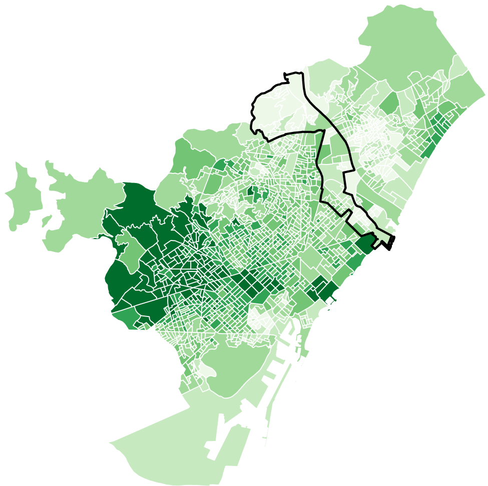
A region-specific scheme is Catalonia's guaranteed citizenship income (*renta garantizada de ciudadanía*, or RGC), a household-based social assistance benefit. The RGC is unlimited in time, means-tested, and condi-

[4]The ten neighborhoods are Ciutat Meridiana, Vallbona, Torre Baró, Roquetes, Trinitat Nova, Trinitat Vella, Baró de Viver, Bon Pastor, Verneda-La Pau, and Besòs-Maresme.

[5]Data on all indicators come from the Statistical Office of the Municipality of Barcelona. The data can be accessed at `https://ajuntament.barcelona.cat/estadistica/angles/index.htm`.



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



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

Figure 1: 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 percentile 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`.

tional on household members not working.[6] The benefit starts with €564 ($894 PPP) per month for single-person households and may reach €1,062 ($1,683 PPP) per month for households with five or more members. These amounts correspond to roughly 70 and 130 percent of the national monthly minimum wage.[7] To receive the benefit, claimants must remain registered with the Public Employment Services of Catalonia and accept suitable job offers. A tranche of €150 ($238 PPP) is conditional on complying with an employment plan.

Centralized schemes are UI benefits and family allowances. UI benefits are means-tested, time-limited (between 6–24 months), and either pay

[6]Only single parents working part-time are eligible despite having a job.

[7]In 2017, the minimum wage was €826 ($1,309 PPP), taking 12 payments per year into account. For comparison, the average monthly wage of a full-time private-sector employee was €2,234 ($3,541 PPP), according to OECD estimates (OECD, 2021a).

for 50–70 percent of reference earnings (contributory benefits) or roughly 60 percent of the national monthly minimum wage (non-contributory benefits). The national family allowance is a non-contributory and means-tested transfer of €24 ($38 PPP) per month per child below 18 years of age.

Although various income support schemes are in place, they only cover some in need of financial support. For example, people that rely on low-wage jobs and small or unstable employment often fail to meet the eligibility requirements of existing schemes, such as extensive formal employment records or complete withdrawal from the labor force. By design, the B-MINCOME program extends subsistence financial support to previously uncovered households.

4 Design and Methods

4.1 Sampling

Participants for the B-MINCOME trial were recruited among households living in ten target neighborhoods (see Section 3.1 for more information). Recruitment for the trial took two months (September–October 2017) and included three steps. First, the Municipality of Barcelona identified 4,305 households expected to meet the eligibility criteria based on information collected from municipal social services records. That is roughly 10 percent of all households living in the target area. Second, the municipality sent letters to the selected households informing them about the program and inviting them to apply. Households could also join one of 400 information events in the target area. Applicant households signed an informed consent sheet to approve being followed through surveys and administrative records during and after the trial. Third, the municipality screened the received applications and selected all households that were actually eligible.

Of the 4,305 households invited, 2,339 (54 percent) had applied for the program, of which 1,518 (35 percent) met all criteria.[8] All 1,518 eligible households were enrolled in the trial and approached for a baseline survey. Figure 2 shows a study timeline including the different recruitment steps.

[8]The high share of eventually ineligible households has to do with the quality and up-to-dateness of information in the municipal social services records.

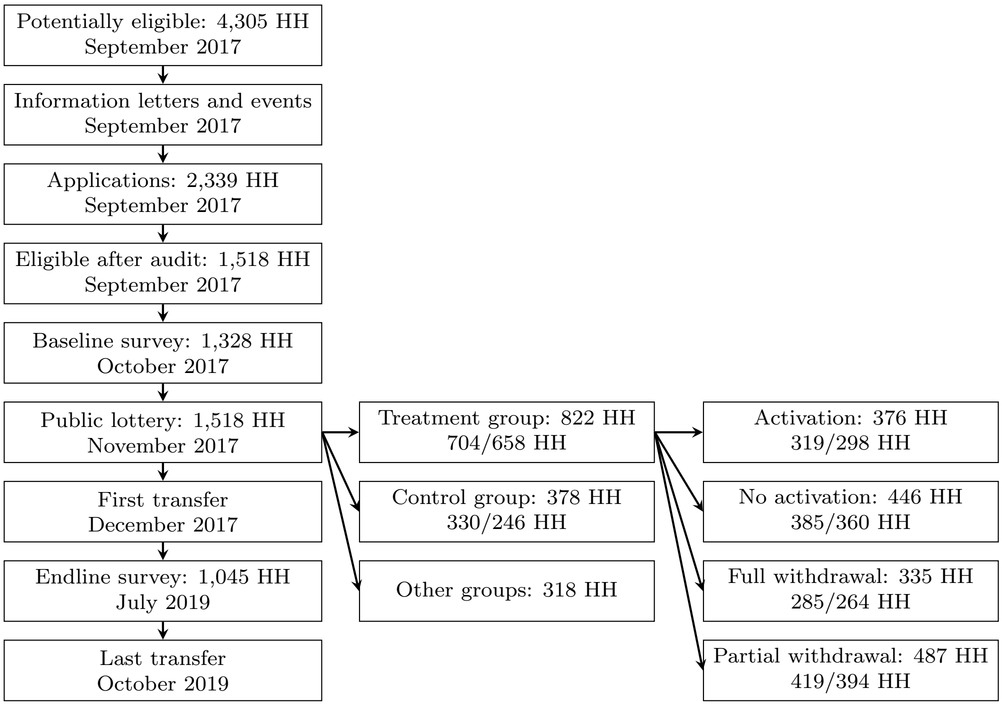


Figure 2: Study Timeline and Treatment Arms.

Note: Numbers separated by a slash indicate survey response at baseline and endline, respectively.

4.2 Randomization

Households were assigned to different experimental conditions through a public lottery. Randomization took place after the baseline survey and participants were informed about their assignment via SMS. The lottery followed a stratified randomization design. Two variables were used to form randomization strata: (i) the expected size of the SMI benefit that a household would receive (three categories: high, medium, low), and (ii) a dummy variable indicating the employability of at least one household member (yes, no).[9] In Appendix B we describe the randomization mechanism in detail. Table F.1 in Appendix F reports the number of households per stratum.

[9]Low: up to 50 percent of the maximum benefit; Medium: between 50 and 75 percent of the maximum benefit; High: more than 75 percent of the maximum benefit. Employability was included as a stratum due to a treatment arm offering vocational training. As mentioned before, we exclude this arm from our analysis due to implementation issues. A seventh stratum comprised households eligible for household renovations. These households are also excluded from the study and only mentioned for completeness.

Table 1: Number of Households per Treatment Arm.

		Activation		No activation	Total
		Soc. entrepreneurship	Comm. involvement		
Withdrawal	Full	–	138	197	335
	Partial	100	138	249	487
Total		100	276	446	822

4.3 Treatment Arms

Of the 1,518 households included in the lottery, 378 were assigned to the control group and 822 to the treatment group. The remaining 318 households were assigned to groups outside the scope of this study.[10] Control households did not receive any intervention and were only approached for surveys. Treatment households were randomly allocated to different treatment arms testing program modalities. The program modalities concerned activation policies and benefit withdrawal rates.[11] Table 1 cross-tabulates the number of households assigned to each treatment arm. As is shown, treatment arms were cross-randomized, except for the social entrepreneurship arm.[12] The treatment arms were set up as follows:

Activation versus no activation. All treatment households were randomly assigned to one of four social activation plans or no activation plan. As mentioned before, we only include households assigned to the plans targeting social entrepreneurship and community involvement next to households assigned to no plan. We describe the two plans in detail in Appendix .

Full versus partial withdrawal. Remember that increases in income reduced the SMI benefit. All treatment households were randomly assigned to two different withdrawal rates—full or partial withdrawal.

[10]The 318 households comprise 24 households assigned to an activation plan targeting household renovations, 150 households assigned to an activation plan offering vocational training, and 144 households forming a reserve pool. All three groups are excluded from our analysis.

[11]A third modality concerned the obligation to participate in an activation plan (obligatory versus optional). Due to signals that the municipality did not enforce mandatory participation, we disregard these two treatment arms and treat all activation plans as optional.

[12]In Section 8, we show that excluding this arm from our analyses as part of a robustness check leaves our main results unchanged.

> Households subject to the full withdrawal rate saw their transfer decrease one-on-one with any additional income (100 percent withdrawal rate). Households assigned to the partial withdrawal arm faced a 25–35 percent withdrawal rate, depending on their extra income. Each additional euro up to €250 ($396 PPP) per month would reduce the benefit by 25 cents, and each euro above €250 per month would reduce the benefit by 35 cents.

4.4 Implementation

Treatment households participated in the B-MINCOME program for 23 months in total. The first transfer of the SMI benefit took place in December 2017; the last transfer occurred in October 2019. All payments were transferred to a private bank account of the main recipient. Treatment households were obliged to report changes in household income, size, and composition every quarter to recalculate the benefit level. If applicable, benefit adjustments came into effect with the next payment. Potential overpayment or underpayment in the preceding quarter was settled with payments in the upcoming quarter in equal parts.

Households assigned to treatment received an average monthly transfer of €422 ($668 PPP). Roughly 14 percent of households received no payments, which can be explained by non-take-up (we discuss non-take-up in Section 6.3). Conditional on positive transfers, the average monthly transfer was €492 ($779 PPP) (Median = €463; SD = €286). Transfers did not exceed €1,500 ($2,376 PPP). Per capita, households received €166 ($263 PPP) per month on average (conditional on positive transfers). Figure G.2 in Appendix G shows the distribution of average monthly transfers.

In the second year of the trial, 25 percent of the monthly benefit was paid out in a local digital currency called REC (Real Economy Currency). Participants could use this currency for payment in designated shops and organizations within Barcelona. The REC was at parity with the euro and could be used with a mobile app or a payment card.

5 Data Collection and Outcomes

5.1 Administrative Data Sources

We use administrative data sources to collect information on participant and household characteristics, households' welfare histories, and labor market participation. Data on background characteristics come from the municipal civil registry. We observe the main recipient's age and gender, household size, composition, and residency (city district) at the time of recruitment. Further, we collect data on household income, and municipal transfers received in the 12 months pre-treatment from the municipal benefit registry. Municipal transfers are household-based and include, e.g., schooling, housing, and healthcare allowances, transport subsidies, and child benefits. Lastly, we observe whether households received Catalonia's guaranteed citizenship income (RGC) at the time of recruitment.

Information on labor market participation comes from social security records. These records contain individual-level employment information. We have access to records covering June 2019 to April 2020. Hence, this data is only available for the last five treatment months and six months post-treatment. Note that the records only include employed individuals; we do not have access to administrative data on self-employed individuals. The records detail an individual's labor market status (employed versus not in the records) on fixed reference dates separated by windows of usually ten days. Unfortunately, the records do not include further employment information, such as hours worked, earnings, or the type of contract.

5.2 Survey Data and Waves

We complement the information obtained from administrative data sources with survey data. The surveys included a module on background information with questions on, e.g., socio-demographics and household characteristics. Another module asked about time use, including work, job search, social participation, and education and training. These two modules are of interest to this study. Other modules collected information on, e.g., deprivation, health and well-being, and household finances. All surveys covered two levels of observation—the household and the individual. Only main recipients filled in surveys. Hence, main recipients provided information on themselves, their household, and other household members. Questions about other household members only concerned factual information, e.g., age or labor market status.

Participants were surveyed three times. The first wave (baseline) took place in the four weeks between enrollment and the public lottery (October 2017). Thus, respondents knew about their participation in the trial but had yet to be assigned to a group. A survey bureau administered the baseline questionnaire through computer-assisted telephone interviewing (CATI). The second wave (midline) took place about one year into the pilot (October 2018). The third and last wave (endline) took place three months before the end of the pilot (July 2019). In contrast to the baseline survey, follow-up rounds allowed for computer-assisted personal interviewing (CAPI). CAPI was meant to facilitate surveying households with language difficulties and households not answering the phone. We only use data from the baseline and the endline survey in our analysis.

5.3 Outcome Variables

We construct fourteen outcome variables based on survey data. Ten of these variables measure outcomes at the level of the main recipient, and four variables pool outcomes at the household level. All variables are based on information reported by the main recipient. For a list and detailed description of all outcome variables, see Table D.2 in Appendix D.

Labor market outcomes. Our primary outcome variable is a dummy indicating whether the main recipient was *working* (either employed or self-employed) when surveyed. We create three sets of additional dummy variables to decompose treatment effects on the probability of having work. The first two dummies indicate whether the main recipient was *employed* or *self-employed*, respectively. The second set of dummies indicates *full-time* or *part-time work*, respectively. As we do not observe hours worked, these two variables will serve as proxies for labor supply decisions at the intensive margin. The third set of dummies indicates work under a *permanent* or a *temporary* contract, respectively. These two dummies serve as proxies for the quality of employment.

Two additional variables pool labor market outcomes at the household level. The first variable counts the *number of adult household members* working (employed and self-employed).[13] The second variable is a dummy taking the value 1 if *at least one adult household member* is working and 0 otherwise.

Other activities. The remaining variables measure activities related to job search, social participation, and human capital formation. First, we include a dummy indicating whether the main recipient tried to *find paid employment* in the past four weeks. Second, we construct a dummy taking the value 1 if the main recipient was active in any *civil society organization or initiative* in the past year.[14] Another dummy indicates whether the main recipient followed any *study or vocational training* in the past year.

Two additional variables measure human capital formation at the household level. The first variable counts the *number of adult household members* that followed any study or vocational training in the past year. The second variable is a dummy indicating whether *at least one adult household member* participated in a study or vocational training.

The self-reported nature of our survey data may raise concerns about data accuracy. Therefore, we construct an additional variable measuring the main recipient's labor market status using administrative data obtained from social security records. We consolidate the 10-day observation intervals into monthly observations. This operation leaves us with eleven dummy variables, one for each month between June 2019 and April 2020. Each dummy takes the value 1 if the main recipient was *employed at least once in the respective month* and 0 otherwise.

[13] Adult household members are members between 18 and 65 years of age.

[14] Civil society organizations and initiatives include neighborhood organizations, school organizations or parents' associations, non-profit organizations, religious groups or organizations, political parties, and any voluntary work.

Table 2: Sample Descriptive Statistics.

	Mean (1)	SD (2)	Min. (3)	Max. (4)	N (5)
Panel A: Administrative data					
No. of hh members	3.442	1.529	1	11	1,200
No. of hh members 25-65	1.712	0.668	0	6	1,200
No. of children (cond.)	1.753	0.828	1	5	741
Monthly hh income	535.620	416.512	0	1,768	1,200
Monthly transfers	172.948	184.301	0	2,084	1,200
Main recipient female	0.734	0.442	0	1	1,150
Main recipient age	42.967	9.907	9	91	1,192
Panel B: Baseline survey data					
No hh member working	0.397	0.490	0	1	1,034
Single-person hh	0.030	0.171	0	1	1,034
Single-parent hh	0.141	0.348	0	1	1,034
Adults without children	0.139	0.346	0	1	1,034
Adults with children	0.690	0.463	0	1	1,034
Compulsory education or less	0.506	0.500	0	1	1,034
Secondary education	0.395	0.489	0	1	1,034
Tertiary education	0.100	0.300	0	1	1,034
All hh members Spanish	0.472	0.499	0	1	1,034
No hh member Spanish	0.219	0.413	0	1	1,034
Mixed nationalities	0.309	0.463	0	1	1,034
Owner-occupied house	0.249	0.433	0	1	1,029

Note: See Table D.1 in Appendix D for a description of variables. Data on recipient age may be erroneous, which explains the odd minimum and maximum values.

5.4 Sample Characteristics

Table 2 reports summary statistics for the 1,200 households included in our sample. We show information from administrative sources (Panel A) and the baseline survey (Panel B). As we will explain in Section 6.1, we encounter missing data in the surveys and some administrative records. Therefore, some summary statistics do not include the full sample.

Our sample descriptive statistics illustrate the economic and social vulnerability of households included in the trial. In sum, participating households are relatively large on average and, for the most part, families with children. Household income and labor market attachment are low, while most households receive financial support from the municipality. Figures on educational attainment suggest low levels of human capital formation on average. We now discuss sample characteristics in more detail.

Most households in our sample are adults with children (69 percent). The remaining third are single-parent households (13 percent), adults without children (14 percent), and single-person households (3 percent). Households have 3.5 members on average (SD = 1.5), which makes them somewhat larger than an average household at risk of poverty in Barcelona (2.5 members).[15] Households with children have roughly two children (members younger than 16) living at home on average. Three out of four households rent their domicile, while one quarter owns their house (25 percent).

Households' economic vulnerability shows in low incomes and high dependency on municipal transfers. The average monthly net household income in the year before the trial is €536 ($850 PPP; $SD =$ €417). Hence, the average household in our sample lives off an income that equals 30 percent of the 2016 at-risk-of-poverty threshold for households with two adults and two children in the area. Roughly 80 percent of households in our sample claimed municipal transfers at some point in the year before the trial.[16] Municipal transfers usually do not cover basic needs; on average, households received €173 ($274 PPP) in monthly transfers. Only 10 percent of households received the regional social assistance benefit (RGC). The baseline survey shows that in 40 percent of responding households, all members are out of work. For comparison, that rate is 17 percent among Barcelona's population at risk of poverty.

Survey data on educational attainment indicates low levels of education in most households. In every second household, no member has a degree that exceeds compulsory education (primary and lower secondary education). In 40 percent of households, the highest level attained by any member is secondary education (higher secondary education or vocational training). Only in 10 percent of households does at least one member hold a tertiary degree. For comparison, among Barcelona's population at risk of poverty, 40 percent of households fall into the first category, 26 percent into the second, and 29 percent into the third. Regarding nationalities, all members hold a Spanish nationality in every second household. In 22 percent of households, no member is a Spanish citizen. In the remaining fraction, both Spanish and non-Spanish nationalities occur. Lastly, data on main recipients shows that a large majority is female (73 percent). On average, main recipients are 43 years old ($SD = 10$).

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

[16] Food subsidies, safety-net benefits, and family assistance are the most common transfers.

6 Experiment Integrity

6.1 Attrition

Participation in the B-MINCOME trial did not depend on filling in surveys, which introduces a risk of attrition-related bias for outcomes based on survey data. We follow a three-step procedure to diagnose this risk. First, we assess whether survey response is correlated with treatment status. Second, we test for differences in baseline outcomes between attrition and non-attrition households at endline. Lastly, we assess whether attrition households in the control group differ from those in the treatment group.

We test for differences in survey response rates between the treatment and the control group using the following specification:

$$response_{ht} = \alpha + \beta_{1t} T_h + \gamma + \epsilon_{ht} \tag{1}$$

In this equation, the variable $response_{ht}$ is a dummy taking the value 1 if household h was surveyed during wave t and 0 otherwise. t may denote the baseline or endline survey. T_h is a treatment dummy indicating the assignment of household h to the treatment group. γ denotes randomization strata fixed effects. ϵ_{ht} is the error term.

For comparisons between different treatment arms, we use a slightly adapted specification:

$$response_{ht} = \alpha + \beta_{1t} T_h^x + \beta_{2t} C_h + \gamma + \epsilon_{ht} \tag{2}$$

Here, the dummy variable T_h^x indicates assignment to treatment arm x. x may denote the activation policy or partial withdrawal arm. The dummy variable C_h indicates assignment to the control group. Hence, the reference category is households assigned to the treatment arm without activation policy or full benefit withdrawal, respectively. All other features remain the same as in Eq.(1).

In both cases, we are interested in the parameters denoted by β_{1t}, which describe differences in response rates between groups or treatment arms of interest at wave t. Table 3 reports differences in response rates at baseline, endline, and endline conditional on baseline. Column (1) shows response rates in the control group. Column (2) reports estimated differences in

Table 3: Attrition: Differences in Survey Response Rates Across Treatment Conditions.

	Control mean (SD) (1)	Treatment group (2)	Activation policy (3)	Partial withdrawal (4)	N (5)
Baseline	0.873	-0.006	-0.015	0.010	1,200
	(0.333)	(0.022)	(0.025)	(0.025)	
		[0.789]	[0.551]	[0.704]	
Endline	0.651	0.152	-0.015	0.021	1,200
	(0.477)	(0.029)	(0.028)	(0.029)	
		[0.000]	[0.605]	[0.457]	
Baseline and endline	0.585	0.116	-0.021	0.025	1,200
	(0.493)	(0.031)	(0.032)	(0.033)	
		[0.000]	[0.524]	[0.452]	

Note: Differences in survey response rates between treatment and control groups and treatment arms. Column (1) reports control group means with standard deviations in parentheses. Column (2) shows coefficients on the treatment dummy, estimating Eq.(1). Column (3) and (4) report coefficients on the treatment dummy, estimating Eq.(2). We report robust standard errors in parentheses and p-values in brackets.

response rates between the treatment and the control group. Column (3) and (4) compare response rates in the activation versus no activation arm and the partial withdrawal versus full withdrawal arm, respectively.

At baseline, we find no significant differences in response rates between the treatment and the control group and different treatment arms, respectively. Roughly 87 percent of households filled in the baseline survey. Expectedly, response rates are lower at endline. In the control group, 65 percent of households filled the endline survey; 59 percent were surveyed at both baseline and endline. Response rates at follow-up do not differ significantly between treatment arms. However, treatment households have a significantly higher probability of responding at follow-up compared to control households. The difference in endline response rates is an estimated 15 percentage points (23 percent) and slightly lower but still statistically significant when conditioning on baseline response.

Hence, while baseline response shows no relation with treatment status, the response at follow-up correlates with treatment assignment. This finding seems plausible, assuming that transfer receipt increases the attachment to the program. Our two additional analyses help us to diagnose attrition in more detail. The results of these analyses make us confident that, even though response rates at follow-up differ between treatment and control groups, attrition is unlikely to bias our results.

First, regressing our baseline outcome variables on a dummy indicating attrition at follow-up, we find no significant differences between the two groups of households. Table E.1 in Appendix E reports detailed results. Second, regressing our baseline outcome variables on a treatment dummy, restricting our sample to attrition households at endline, we find no significant differences, except for one outcome variable. Attrition households assigned to the treatment group are 20 percent more likely to have at least one member working at baseline ($p = 0.089$). We report detailed results in Table E.2 in Appendix E.

We repeat these analyses, comparing households in the activation treatment arms (results shown in Table E.3) and withdrawal treatment arms (results shown in Table E.4). We find no significant differences except for social participation in the activation arms. Attrition households assigned to activation are 70 percent more likely to have their main recipient show civic engagement at baseline ($p = 0.010$).

6.2 Baseline Balance

We perform two tests of baseline balance. First, we assess baseline balance in terms of covariates. We include variables measuring household size and income, and dependency on welfare transfers pre-treatment. Second, we test for differences in survey outcomes at baseline. While the first test builds on administrative data and includes our full sample, the second test restricts the sample to baseline respondents.[17] Both tests aim to assess the integrity of the public lottery mechanism used for randomization.

We compare households assigned to control and treatment groups and households assigned to different treatment arms. For the former comparison, we use the following specification:

$$Y_{hB} = \alpha + \beta_1 T_h + \gamma + \epsilon_h \tag{3}$$

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

[17] Remember that we miss outcome information at baseline for roughly 13 percent of our sample.

Comparing different treatment arms, we use a slightly adapted specification:

$$Y_{hB} = \alpha + \beta_1 T_h^x + \beta_2 C_h + \gamma + \epsilon_h \tag{4}$$

Here, T_h^x is a dummy variable indicating assignment to a treatment arm x. As before, x may denote the activation policy or partial withdrawal arm. C_h is a dummy variable indicating assignment to the control group. Hence, the reference category is households assigned to the treatment arm without activation policy or full benefit withdrawal, respectively. All other terms remain the same as in Eq.(3).

Assessing baseline balance, we are interested in the parameters denoted by β_1, which either describe the differences at baseline between treatment and control households or households assigned to different treatment arms. Table 4 reports the results for balance in terms of covariates and Table 5 for baseline outcomes. In both tables, Column (1) shows control group means and standard deviations. Column (2) reports differences between the treatment and the control condition. Column (3) presents differences between households assigned to an activation policy and households that are not; Column (4) shows differences between households assigned to partial and full withdrawal.

Regarding covariates, households assigned to the treatment group do not differ significantly from those assigned to the control group. The same holds for households assigned to different treatment arms, with two exceptions. First, households assigned to an activation policy are 5 percentage points (90 percent) more likely to receive Catalonia's guaranteed citizenship income (RGC) at the time of recruitment ($p = 0.019$). Second, on average, households assigned to the partial withdrawal arm received roughly €23 (13 percent) more in monthly transfers pre-treatment. However, this difference is only significant at the 10 percent level ($p = 0.077$). In conclusion, the results of the first balancing test strongly suggest that the public lottery was executed correctly.

For baseline outcomes, a few significant differences appear. Main recipients assigned to treatment are 7 percentage points (20 percent) more likely to show civil engagement ($p = 0.027$). They are also more likely to have

Table 4: Baseline Balance: Covariates.

	Control mean (SD) (1)	Treatment group (2)	Activation policy (3)	Partial withdrawal (4)	N (5)
No. of hh members	3.463	0.088	-0.004	-0.028	1,200
	(1.596)	(0.094)	(0.101)	(0.101)	
		[0.352]	[0.972]	[0.778]	
No. of children	1.101	0.039	-0.001	-0.005	1,200
	(1.076)	(0.065)	(0.073)	(0.074)	
		[0.551]	[0.992]	[0.948]	
No. of hh members 25-65	1.704	0.042	0.001	-0.030	1,200
	(0.719)	(0.043)	(0.045)	(0.045)	
		[0.332]	[0.990]	[0.502]	
Monthly hh income	424.650	23.598	2.970	15.498	1,200
	(381.618)	(18.041)	(20.480)	(20.616)	
		[0.191]	[0.885]	[0.452]	
Monthly transfers	173.043	4.451	-6.396	22.686	1,200
	(184.509)	(11.433)	(12.584)	(12.822)	
		[0.697]	[0.611]	[0.077]	
RGC recipient	0.056	0.025	0.051	-0.018	1,200
	(0.229)	(0.016)	(0.022)	(0.022)	
		[0.116]	[0.019]	[0.409]	

Note: Differences in covariates between treatment and control groups and treatment arms. Column (1) reports control group means with standard deviations in parentheses. Column (2) shows coefficients on the treatment dummy, estimating Eq.(3). Column (3) and (4) report coefficients on the treatment dummy, estimating Eq.(4). We report robust standard errors in parentheses and p-values in brackets. See Table D.1 in Appendix D for a description of variables.

followed education in the past six months, although this difference is barely significant at the 10 percent level. Main recipients in the activation arm are also more likely to be involved in civil ($p = 0.025$) and educational activities ($p = 0.051$). Moreover, households in the activation arm have more members studying on average ($p = 0.018$) and a higher chance of at least one member following education ($p = 0.014$). For the partial withdrawal arm, the only significant difference appears for the probability of job search in the past four weeks—main recipients assigned to activation are more likely to have looked for work ($p = 0.056$). In conclusion, the number of imbalances lies slightly higher than expected by chance, given the number of hypotheses tested (three comparisons, eight outcomes). Potentially, some selectiveness is introduced by baseline non-response. We will condition on the baseline value of the respective outcome when estimating treatment effects, which should control for any baseline imbalances encountered. As part of a sensitivity analysis, we will also report unadjusted estimates.

Table 5: Baseline Balance: Survey Outcomes.

	Control mean (SD) (1)	Treatment effect (2)	Activation policy (3)	Partial withdrawal (4)	N (5)
Working	0.400	-0.025	-0.024	0.028	1,032
	(0.491)	(0.034)	(0.037)	(0.037)	
		[0.459]	[0.515]	[0.452]	
Job search past 4w	0.506	-0.008	0.056	0.072	1,031
	(0.501)	(0.033)	(0.037)	(0.038)	
		[0.809]	[0.130]	[0.056]	
Social participation	0.345	0.073	0.083	0.019	1,034
	(0.476)	(0.033)	(0.037)	(0.038)	
		[0.027]	[0.025]	[0.616]	
Education in past 12m	0.203	0.047	0.064	0.006	1,034
	(0.403)	(0.028)	(0.033)	(0.033)	
		[0.099]	[0.051]	[0.867]	
No. of members working	0.755	0.017	0.033	0.027	1,034
	(0.770)	(0.054)	(0.060)	(0.060)	
		[0.746]	[0.581]	[0.655]	
At least one member working	0.579	0.005	-0.032	0.012	1,034
	(0.495)	(0.034)	(0.038)	(0.038)	
		[0.894]	[0.400]	[0.752]	
No. of members in education	0.576	-0.012	0.136	-0.036	1,034
	(0.852)	(0.055)	(0.057)	(0.058)	
		[0.821]	[0.018]	[0.535]	
At least one member in education	0.418	-0.009	0.092	-0.019	1,034
	(0.494)	(0.034)	(0.037)	(0.038)	
		[0.797]	[0.014]	[0.606]	

Note: Differences in baseline outcomes between treatment and control groups and treatment arms. Column (1) reports control group means with standard deviations in parentheses. Column (2) shows coefficients on the treatment dummy, estimating Eq.(3). Column (3) and (4) report coefficients on the treatment dummy, estimating Eq.(4). We report robust standard errors in parentheses and p-values in brackets. The sample is restricted to baseline respondents. See Table D.2 in Appendix D for a description of variables.

6.3 Compliance

We determine compliance (or take-up) for the program as a whole and separately for the income support and activation component. Of the 822 households assigned to the treatment group, 717 (87 percent) actually participated in the program. The remaining 105 households were excluded before the start of the program due to various reasons. Those reasons include refusal, no-show, residency outside the target area, and ineligibility due to income or assets. Table F.2 in Appendix F lists the share of households per reason. Table F.3 in Appendix F shows that participation rates are comparable across treatment arms.

All households eligible for the SMI benefit actually received the transfer (both in euro and in the local digital currency). At the same time, none of the households assigned to the control group received any payment associated with the B-MINCOME program. In the activation arm, roughly two-thirds of households took up their assigned treatment (conditional on joining B-MINCOME). Take-up rates are similar for both activation plans, with 65.5 percent in the community involvement arm and 66.7 percent in the social entrepreneurship arm. None of the households assigned to the control group or the treatment arm without activation participated in an activation plan.

We account for noncompliance by estimating intent-to-treat (ITT) effects. Following this approach, we compare households according to their original group assignment, regardless of actual participation in the program or an activation plan. We elaborate on this strategy in more detail in the following section.

7 Empirical Strategy

We are interested in the overall impacts of the B-MINCOME program and the effects of different program modalities, as implemented in the treatment arms. To assess the overall impact of the program on survey outcomes, we estimate the following specification:

$$Y_{hE} = \alpha + \beta T_h + X_h'\Theta + \Psi Y_{hB} + \Phi M_{hB} + \nu + \gamma + \epsilon_h \tag{5}$$

In that equation, Y_{hE} describes the outcome of interest for household h at endline. T_h is a dummy indicating the assignment of household h to the treatment group. Hence, our reference category is households assigned to the control group. X_h' is a vector of covariates, which we include to increase the precision of our estimates. The vector contains the variables listed in Table 4. To further increase precision, we follow McKenzie (2012), and condition on the baseline value of the respective outcome, denoted by Y_{hB}. As mentioned in Section 6.1, we encounter survey non-response at baseline. To avoid losing observations at follow-up due to missing baseline data, we replace missing baseline outcomes with 0 and include a dummy variable, denoted by M_{hB}, indicating missingness at baseline. To control for survey mode, we include ν, a dummy indicating the CAPI method. γ denotes randomization strata fixed effects and ϵ_h is the error term.

Our parameter of interest is β, which describes the intent-to-treat (ITT) effect of the B-MINCOME program. We consider this limitation unproblematic as we can also expect non-take-up of the program as a whole or certain program features under program roll-out. Viewed in this light, the impacts of program implementation are the main parameters of interest.

To estimate the relative effects of different program modalities, we use a slightly modified specification:

$$Y_{hE} = \alpha + \beta_1 T_h^x + \beta_2 C_h + X_h'\Theta + \Psi Y_{hB} + \Phi M_{hB} + \nu + \gamma + \epsilon_h \quad (6)$$

As in previous specifications, T_h^x is a dummy variable indicating assignment to a treatment arm x, while C_h indicates assignment to the control group. All other features remain the same as in Eq.(5). As before, x may denote the activation policy or partial withdrawal arm. Our parameter of interest is β_1, which describes the estimated effect of assignment to an activation policy versus no activation, or assignment to a partial versus a full withdrawal rate.

When estimating treatment effects on administrative outcomes, we omit outcomes at baseline and controls for survey mode. Accordingly, Eq.(5) simplifies to:

$$Y_{ht} = \alpha + \beta_t T_h + X_h'\Theta + \gamma + \epsilon_{ht} \quad (7)$$

and Eq.(6) changes to:

$$Y_{ht} = \alpha + \beta_{1t} T_h^x + \beta_{2t} C_h + X_h'\Theta + \gamma + \epsilon_{ht} \quad (8)$$

In addition to average effects, we also study effect heterogeneity. For this analysis, we focus on the overall impact of the program and survey outcomes. We examine effect heterogeneity by interacting the treatment dummy with a dummy indicating a subgroup of interest. Accordingly, Eq.(5) changes into:

$$\begin{aligned} Y_{hE} = \alpha + &\beta_1 T_h + \beta_2 S_h + \beta_3 T_h S_h + \\ &X_h'\Theta + \Psi Y_{hB} + \Phi M_{hB} + \nu + \gamma + \epsilon_h \end{aligned} \quad (9)$$

In that equation, S_h denotes a dummy variable indicating a subgroup of interest. All other terms remain the same as in Eq.(5). The parameter of interest is β_3, which describes the difference in ITT effects between the units inside and outside the respective subgroup.

While our tables in the main section show naive p-values, we report p-values adjusted for multiple hypothesis testing in Appendix F. To adjust p-values, we follow the free step-down methodology of Westfall and Young (1993) and base our adjustment on 10,000 bootstrap draws.[18]

8 Results

We are interested in the overall impact of the B-MINCOME program but also in the effects of different treatment modalities. While Section 8.1 addresses the overall impact, Section 8.2 and 8.3 contrast the effects of activation versus no activation, and partial versus full transfer withdrawal, respectively. We discuss program impacts in four steps. First, we present and decompose labor market effects at the individual level using survey data. Second, we confirm our survey results using administrative data. Third, we discuss labor market effects at the household level. Lastly, we report effects on adjacent outcomes: job search, social participation, and education.

8.1 Overall Impact of the Program

Table 6 presents estimated treatment effects on survey outcomes at endline, three months before the last transfer. The table only includes outcomes measured at the level of the main recipient. Further below, we discuss results for outcomes pooled at the household level. Column (1) shows control group means and standard deviations. Column (2) reports coefficients on the treatment dummy, estimating Eq.(5). We report robust standard errors in parentheses and p-values in brackets.

[18]We adjust our p-values for testing hypotheses on eight outcomes. We exclude six outcome variables, which are meant to decompose effects on labor participation. We calculate adjusted p-values using the user-written Stata package *wyoung* (Jones et al., 2019).

Table 6: Treatment Effects at Endline: Main Recipient.

	Control mean (SD) (1)	Treatment effect (2)	Activation policy (3)	Partial withdrawal (4)	N (5)
Working	0.473	-0.095	-0.043	0.031	901
	(0.500)	(0.034)	(0.033)	(0.033)	
		[0.005]	[0.190]	[0.343]	
Job search past 4w	0.024	-0.015	-0.002	-0.002	904
	(0.155)	(0.011)	(0.008)	(0.009)	
		[0.157]	[0.785]	[0.835]	
Social participation	0.378	0.008	0.084	-0.021	904
	(0.486)	(0.035)	(0.037)	(0.037)	
		[0.818]	[0.023]	[0.572]	
Education past 6m	0.212	0.032	0.090	0.030	900
	(0.410)	(0.032)	(0.033)	(0.033)	
		[0.321]	[0.007]	[0.356]	

Note: OLS estimates of treatment effects on survey outcomes at endline (individual level). Outcome variables are listed on the left and described in detail in Table D.2 in Appendix D. Column (1) reports control group means with standard deviations in parentheses. Column (2) shows coefficients on the treatment dummy, estimating Eq.(5). Column (3) and (4) present coefficients on the treatment dummy, estimating Eq.(6). We report robust standard errors in parentheses and p-values 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 4.

We find that assignment to the program has a significant negative effect on the probability of working at endline. The point estimate is –9.5 percentage points ($p = 0.005$), which corresponds with a negative effect of 20 percent relative to the control group mean of 47 percent. The effect remains significant at the 10 percent level after correction for multiple inference (see Table F.4 in Appendix F). In Table 7, we further decompose this labor force participation effect. For comparison, the first row again reports the non-decomposed effect.

The results show that negative labor supply effects are confined to paid employment rather than self-employment. Moreover, the results suggest reductions in both full-time and part-time work, though full-time work appears more affected in relative terms. While chances to work full-time are 25 percent lower relative to the control group ($p = 0.045$), the relative effect is 16 percent for part-time work ($p = 0.184$). Lastly, both permanent and temporary work is affected, though the effect is larger for permanent contracts in relative terms. While chances to have a permanent contract are 27 percent lower relative to the control group ($p = 0.040$), the relative effect is 18 percent for temporary contracts ($p = 0.131$).

Estimating treatment effects on our administrative measure of labor force participation confirms our finding. Panel A of Figure 3 plots point

Table 7: Treatment Effects at Endline: Decomposition of Labor Supply Effects.

	Control mean (SD) (1)	Treatment effect (2)	Activation policy (3)	Partial withdrawal (4)	N (5)
Working	0.473	-0.095	-0.043	0.031	901
	(0.500)	(0.034)	(0.033)	(0.033)	
		[0.005]	[0.190]	[0.343]	
Employed	0.457	-0.101	-0.046	0.017	901
	(0.499)	(0.034)	(0.033)	(0.033)	
		[0.003]	[0.159]	[0.615]	
Self-employed	0.016	0.007	0.003	0.015	901
	(0.127)	(0.009)	(0.011)	(0.011)	
		[0.474]	[0.817]	[0.191]	
Working full-time	0.229	-0.058	0.015	0.023	901
	(0.421)	(0.029)	(0.027)	(0.027)	
		[0.043]	[0.590]	[0.390]	
Working part-time	0.245	-0.039	-0.058	0.010	901
	(0.431)	(0.030)	(0.029)	(0.030)	
		[0.197]	[0.045]	[0.743]	
Permanent contract	0.186	-0.050	-0.011	0.023	895
	(0.390)	(0.024)	(0.023)	(0.024)	
		[0.040]	[0.626]	[0.318]	
Temporary contract	0.264	-0.048	-0.031	0.003	895
	(0.442)	(0.032)	(0.030)	(0.030)	
		[0.131]	[0.294]	[0.916]	

Note: OLS estimates of treatment effects on survey outcomes at endline (individual level). Outcome variables are listed on the left and described in detail in Table D.2 in Appendix D. Column (1) reports control group means with standard deviations in parentheses. Column (2) shows coefficients on the treatment dummy, estimating Eq.(5). Column (3) and (4) present coefficients on the treatment dummy, estimating Eq.(6). We report robust standard errors in parentheses and p-values 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 4.

estimates and 95 percent confidence intervals for treatment effects in several months. For now, we direct our attention to the estimate in the month of the endline survey (indicated by a black dashed line and labeled accordingly). The point estimate of –9.0 percentage points ($p = 0.004$) is very similar to the coefficient reported in Table 6. This result makes us confident that our finding is not distorted by inaccurate reporting or biased due to survey attrition.

Concordant with the results for main recipients, we find negative labor supply effects when pooling outcomes at the household level. We report these results in Table 8. We find that—on average and controlling for household size—treatment households have significantly fewer members working than control households ($p = 0.003$). Likewise, chances of at least one member working are significantly lower among households assigned to treatment ($p = 0.007$). Both effects survive the correction for multiple inference (see Table F.4 in Appendix F).

Table 8: Treatment Effects at Endline: Household.

	Control mean (SD) (1)	Treatment effect (2)	Activation policy (3)	Partial withdrawal (4)	N (5)
No. of members working	0.870	-0.154	-0.066	0.000	904
	(0.823)	(0.051)	(0.052)	(0.052)	
		[0.003]	[0.201]	[0.994]	
At least one member working	0.638	-0.089	-0.060	-0.013	904
	(0.481)	(0.033)	(0.034)	(0.034)	
		[0.007]	[0.075]	[0.709]	
No. of members in education	0.533	0.053	0.075	0.011	904
	(0.806)	(0.054)	(0.054)	(0.054)	
		[0.328]	[0.164]	[0.836]	
At least one member in education	0.394	0.044	0.051	0.004	904
	(0.490)	(0.036)	(0.037)	(0.037)	
		[0.222]	[0.169]	[0.908]	

Note: OLS estimates of treatment effects on survey outcomes at endline (household level). Outcome variables are listed on the left and described in detail in Table D.2 in Appendix D. Column (1) reports control group means with standard deviations in parentheses. Column (2) shows coefficients on the treatment dummy, estimating Eq.(5). Column (3) and (4) present coefficients on the treatment dummy, estimating Eq.(6). We report robust standard errors in parentheses and p-values 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 4.

Lastly, we find no evidence of effects on outcomes measuring other types of activities (see again Table 6). It appears that, in general, job search is not a very common activity. Merely 2.4 percent of control respondents looked for work in the past four weeks. The point estimate on the treatment dummy is negative and sizable in relative terms (roughly 60 percent lower chances compared to the control group) but not estimated precisely enough. Participation in civil society organizations and following a study or vocational training are more common activities. Among control respondents, 38 percent report civic engagement, while 21 percent indicate having followed education in the past six months. For both outcomes, the point estimate on the treatment effect is positive but relatively small and not statistically significant. When measuring education-related activities at the household level (see again Table 8), we find the same result: point estimates are positive but not statistically significant.

8.2 Effects of Activation Versus No Activation

We now consider the effects of being assigned to a social activation plan versus receiving the SMI benefit without activation. Column (3) of Table 6 reports results for outcomes measured at the level of the main recipient.

We find no evidence that recipients assigned to activation have different chances of working at endline compared to their counterparts receiving nothing but the benefit. The point estimate on the treatment dummy is negative but not statistically significant. For comparison, Panel B of Figure 3 shows treatment effects when using our administrative measure of labor force participation. The results are consistent with those obtained using survey data.

When decomposing the effect, results suggest that activation does harm employment chances, but only in the domain of part-time work. We find a significant negative effect for part-time work ($p = 0.045$), which is compensated by a small (and insignificant) increase in the likelihood of working full-time (see Column (3) of Table 7). We find no evidence of effects for other decompositions.

We find confirmation for impaired employment outcomes under activation when pooling outcomes at the household level. We find that households assigned to the activation arm are less likely to have at least one member working compared to their counterparts receiving only the benefit (see Column (3) of Table 8). Finding employment effects at the household level rather than at the individual level is consistent with activation policies potentially targeting household members other than the main recipient (see Section 2). On a cautionary note, the effect is only statistically significant at the 10 percent level and does not survive the correction for multiple inference. There is no evidence of effects on other pooled outcomes.

For other activities, results suggest that recipients assigned to activation are more likely to spend time on social participation and education than their benefit-only counterparts (see Column (3) of Table 6). The effects are statistically significant at the 5 and 1 percent level, respectively. However, only the effect on education survives the correction for multiple inference.

We interpret these results with caution for two reasons. First, both outcomes already differed significantly at baseline (see Table 5). When estimating effects without controlling for baseline values, coefficients only slightly increase in size (see Section 8.6). We believe that this finding provides some reassurance but that the imbalances at baseline warrant caution nonetheless. Second, respondents may have interpreted their participation in an activation plan or components thereof as social participation or education activities. Therefore, results on both outcomes may partly reflect program participation rather than outcomes realized outside the program. Consistent with that reasoning, we find that the point estimate for the effect on education roughly halves when excluding the training-heavy social entrepreneurship arm from the sample (see Table F.5 in Appendix F).

8.3 Effects of Partial Versus Full Withdrawal

This subsection presents the effects of being assigned to a partial withdrawal rate versus a full withdrawal rate. Column (4) of Table 6 reports results for outcomes measured at the level of the main recipient.

We find no evidence for differences in effects between the two treatment arms—neither for employment outcomes nor for outcomes measuring other activities. The same holds when pooling outcomes at the household level (see Column (4) of Table 8). Remember that the social entrepreneurship arm was not cross-randomized and only faced a partial withdrawal rate. As a robustness check, we exclude this arm from our sample, which leaves results unchanged (see Table F.5 in Appendix F).

We obtain different results when estimating effects using administrative data (see Panel C of Figure 3). We find that in the month of the endline survey, recipients under the full withdrawal regime were 6.0 percentage points less likely to be employed than their counterparts under the partial withdrawal regime ($p = 0.033$). This result suggests that the full withdrawal regime provided stronger work disincentives, which fits the predictions provided by standard labor supply models.

It remains an open question why administrative data leads to different results than survey data when studying the effects of withdrawal modalities. A potential explanation is differential attrition at endline. Point estimates indicate that attrition recipients in the partial withdrawal arm were more likely to work at baseline ($p = 0.300$; see Table E.4 in Appendix E). Hence, survey results may be downward biased due to higher chances of missing outcomes of working recipients in the partial withdrawal arm. Given that we do not encounter attrition in administrative data sources, the effects estimated on administrative data may prove more reliable when comparing partial and full withdrawal.

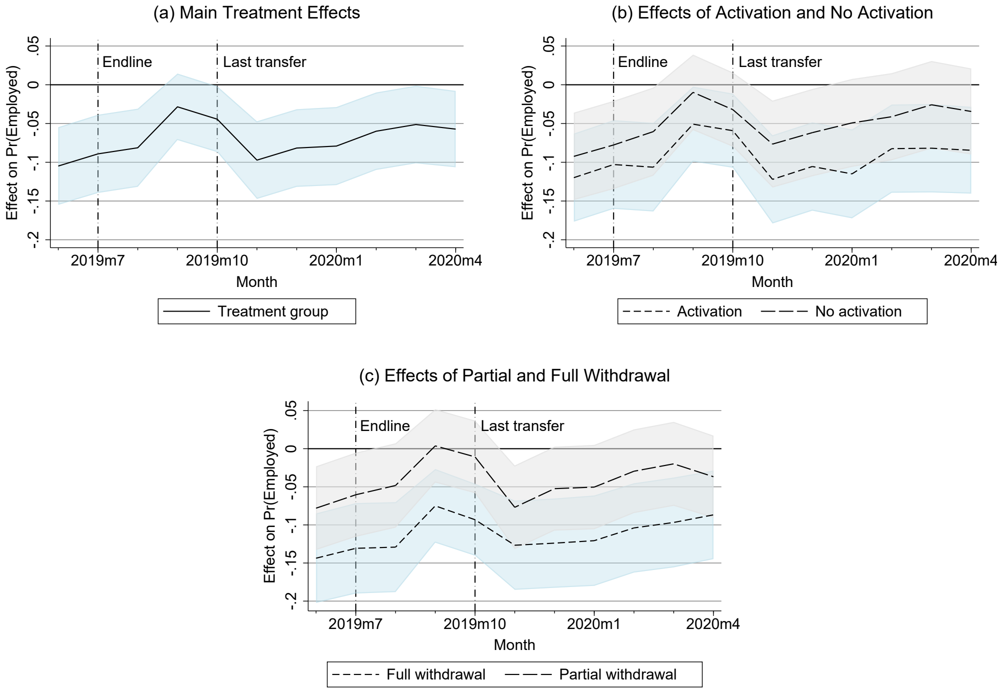


Figure 3: Treatment Effects on Employment Probabilities Using Administrative Data.

Note: Grey and colored areas are 95 percent confidence intervals. Graphs show ITT effects, which are estimated using separate regressions for each month. Panel A estimates Eq.(7), while Panel B and C estimate Eq.(8).

8.4 Persistence of Effects Post-Treatment

We now assess the persistence of effects post-treatment. On the one hand, we would expect that households will try to compensate for their loss in income once the program ends. This behavior may attenuate negative labor supply effects in the last months of the trial or after that. On the other hand, negative effects may persist, e.g., if being out of work resulted in human capital depreciation or had other scarring effects.

We rely on administrative data to examine effect persistence, as our survey data only reaches as far as three months before the end of the trial. With administrative data, we can follow subjects up to six months after the last transfer. Figure 3 plots monthly effect estimates from June 2019—the month before the endline survey—until April 2020. Note that Spain imposed a full lockdown due to the unfolding COVID-19 pandemic at the end of March 2020.

We find that negative employment effects briefly diminish toward the end of the trial before quickly reverting to previous levels (see Panel A of Figure 3). In the longer term, the effects diminish in size but remain negative throughout. This finding may suggest that the adverse effect of the program on labor supply is persistent. On the other hand, the pattern of returning negative effects after the program's termination could indicate the presence of compensatory efforts by authorities. For example, social workers may have advertised other support programs among treated households.

The effects for treatment arms follow the same dynamic as overall treatment effects (see Panel B and C of Figure 3). The difference in effects between the respective arms remains essentially constant over time. This is to say that—also in the longer term—there is no evidence that assignment to activation leads to significantly different effects than receiving no more than the benefit. For the treatment arms testing different withdrawal rates, the difference in effects observed at endline persists until the end of our observation window.

8.5 Heterogeneous Treatment Effects

We now assess whether treatment effects differ for households with and without care responsibilities. Understanding to what extent effects are driven by households with care responsibilities may help to uncover potential treatment mechanisms. For our analysis, we compare effects of households with and without children.

Table 9 reports the results of three models. In each model, we interact the treatment dummy with a dummy indicating children, following Eq.(9). For the first model, we set the cutoff age for children at 16 years (65 percent of households have children of 16 years or younger). For the second and third model, we lower the cutoff age to 15 and 14 years, respectively (62 and 59 percent of households have children of 15 or 14 years or younger, respectively).[19]

[19] In Table F.6 in Appendix F we further vary the cutoff age to 19, 12, and 5 years or younger, respectively. The results still suggest that households with children decrease their labor supply more, although not all interaction effects are statistically significant.

Table 9: Heterogeneous Treatment Effects at Endline.

	Main recipient working			At least one member working		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.023 (0.056) [0.677]	-0.012 (0.054) [0.818]	-0.003 (0.052) [0.959]	-0.015 (0.062) [0.808]	-0.032 (0.060) [0.590]	-0.009 (0.057) [0.877]
Interaction terms w/ treatment dummy						
HH w/ children (16 years or younger)	-0.108 (0.069) [0.121]			-0.123 (0.074) [0.097]		
HH w/ children (15 years or younger)		-0.128 (0.068) [0.060]			-0.100 (0.072) [0.164]	
HH w/ children (14 years or younger)			-0.150 (0.067) [0.025]			-0.143 (0.070) [0.043]
N	901	901	901	904	904	904

Note: OLS estimates of treatment and interaction effects on survey outcomes at endline. Column (1)–(3) report effects on the probability that the main recipient is working. Column (4)–(6) report effects on the probability of any household member working. Outcome variables are described in detail in Table D.2 in Appendix D. The model in Column (1) and (4) includes a term interacting the treatment dummy with a dummy indicating that there are children of 16 years or younger in the household, the model in Column (2) and (5) a term interacting the treatment dummy with a dummy indicating that there are children of 15 years or younger in the household, and Column (3) and (6) a term interacting the treatment dummy with a dummy indicating that there are children of 14 years or younger in the household. We report robust standard errors in parentheses and p-values 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 4.

We choose these cutoff points as compulsory secondary education in Catalonia lasts until the age of 16. In contrast to primary schools (for students between 6 and 12 years of age), which offer full-day care, secondary schools usually finish the day at lunchtime. For reasons of brevity, we focus on the overall impact of the program and on two work-related outcome variables: (i) the probability that the main recipient is working and (ii) the probability that any household member is working.

We find tentative evidence that adverse labor supply effects are larger among main recipients with care responsibilities. Remember that assignment to the program reduced the probability that the main recipient is working by 9.5 percentage points (see Table 6). We find much smaller point estimates of –2.3 to –0.3 percentage points for recipients without children, while the coefficients on the interaction terms are sizeable, with –10.8 to –15.0 percentage points. The two largest negative coefficients are significant at the 10 and 5 percent level, respectively. When pooling work probabilities at the household level, effect heterogeneity shows the same pattern as for individual outcomes.

In sum, the results are consistent with the idea that the negative labor supply effects of the program stem from the substitution of labor for domestic and care work among recipients with children. We would need more detailed survey data on time use to investigate this channel directly. Still, it seems plausible that main recipients with children work less mainly because of the care duties they face. The finding is also consistent with evidence from comparable programs tested in the past. For example, Robins (1985) and Burtless (1986) report stronger reductions in work effort among single female heads in some of the 1970s U.S. negative income tax experiments.

8.6 Sensitivity Analyses

We assess the sensitivity of our results in three steps. First, we estimate effects on survey outcomes excluding all control variables except strata fixed effects and a dummy for the survey mode from our main models specified in Eq.(5) and (6). We find that unadjusted effect estimates are somewhat larger but do not differ much from those obtained when including control variables. This result makes us confident that the few imbalances observed at baseline (see Table 4 and 5) are not concerning.

Second, we estimate effects on survey outcomes, including additional covariates. These covariates measure individual or household background characteristics and are constructed using baseline survey data.[20] We include a dummy indicating the gender of the main recipient, dummies for the neighborhood in which the household is located (ten neighborhoods), dummies for the type of household (four types), dummies for household composition regarding nationalities (three types), and dummies for the highest education level reached by any household member (three levels). Table F.8 in Appendix F reports results, which hardly change.

Third, we use logistic regression instead of OLS to estimate effects when the dependent variable is binary. Table F.9 in Appendix F reports the results, which do not change materially.

[20] To account for missingness in these covariates due to baseline non-response, we code missing values as zero and include an additional dummy variable indicating non-response at baseline.

9 Discussion and Conclusion

Concerned by potentially negative work incentives, antipoverty programs usually provide monetary support in return for fulfilling activity-related criteria, e.g., efforts directed at human capital formation or labor market insertion. In this paper, we studied the employment effects of an antipoverty program that does not include any such conditions. The two-year program, implemented in Barcelona (Spain), consisted of a monthly cash transfer to households with income below the subsistence level. The benefit level depended on the household income, size, and composition. On average, households received roughly €500 ($792 PPP) per month, equivalent to nearly 50 percent of the national minimum wage. Although the benefit was household-based, transfers were made to the account of a designated household member, the main recipient.

We studied the impacts of the program on outcomes related to employment and activities that indicate investment in human capital (following training or education) and the community (social participation). Our analysis uses data from social security records and survey data. For identification, we exploit the fact that the program got trialed in an RCT including roughly 1,500 households recruited in ten target neighborhoods.

Our findings for overall impacts can be summarized in four parts. First, we find strong evidence for sizeable negative labor supply effects. After two years, households assigned to the cash transfer were 14 percent less likely to have at least one member working compared to households assigned to the control group; main recipients were 20 percent less likely to work. Second, negative employment effects persisted until at least six months after the last payment. Third, we find tentative evidence that effects are mainly driven by households with care responsibilities. Fourth, there is no evidence of effects on social participation and education-related activities.

In addition to studying overall impacts, we contrasted different program modalities implemented in treatment arms. These modalities were: assignment to an activation plan (directed at community involvement or social entrepreneurship) versus pure benefit receipt and a 100 percent transfer withdrawal rate versus a 25–35 percent withdrawal rate. We find tentative evidence that activation matters. While some employment-related outcomes worsen under activation, there could be a positive impact on social participation and education-related activities. However, whether this result merely reflects participation in an activation plan remains unclear. Expectedly, the transfer withdrawal rate matters. A more lenient withdrawal rate attenuates negative labor supply effects.

In sum, our results suggest that the adverse labor supply effects of unconditional transfers should not be underestimated. While the negative effects reported in previous studies are usually neglectable or moderate, our findings suggest sizeable effects. What may explain our results? First, the B-MINCOME transfers were rather generous compared to comparable interventions. Possibly, the income effect was large enough to affect labor supply decisions at the extensive margin. Second, B-MINCOME transfers were subject to a withdrawal scheme, which may amplify substitution effects. Our findings for treatment arms with different withdrawal rates suggest that such effects indeed played a role.

In line with existing evidence, we find stronger labor supply responses among recipients with care responsibilities. Our results suggest that effects are almost entirely be driven by this group of participants. If reductions in labor supply are related to care duties, we may expect improvements in children's outcomes. For instance, children's education outcomes or health could improve. Adolescents may be less likely to commit (minor) crimes. Follow-up research will be needed to examine program effects in such domains and come to conclusions about broader welfare effects. Another important finding concerns the persistence of effects. Employment rates in the treatment group remain lower even six months after the last transfer, indicating that households' labor supply decisions may be hard to reverse.

Naturally, our study is subject to some limitations, one of which concerns data availability. The social security records we could access only contain binary information on an individual's employment status. Other important employment-related outcomes, such as hours worked, earnings or occupations remain unobserved. Access to such information would allow for a more comprehensive investigation of labor supply effects, including, e.g., decisions at the intensive margin. Equally unobserved remain effects later than six months after the program, which leaves open the question of how long-lasting impacts are. Moreover, lacking data on household income more broadly, we cannot examine whether the program affected receipt of other public transfers or total disposable household income. Lastly, lacking more comprehensive data on time use, we cannot assess whether beneficiaries substituted work for other tasks. Another obvious limitation has to do with external validity. The same program may affect households in less disadvantaged areas differently. Likewise, effects in rural areas may differ from effects observed in urban places. Future research will be needed to confirm our findings in such settings.

Lastly, our findings provide some interesting directions for future research. First of all, the program may have achieved other potential policy objectives. Such objectives may include improving health and psychological well-being, alleviating financial hardship, promoting home improvements, or preventing evictions. Local project reports already provide evidence pointing at broader effects (Todeschini and Sabes-Figuera, 2019). Evaluating the program in broader terms will allow for a more comprehensive understanding of "positive" and "negative" program effects and potential trade-offs between the different goals. Studying effects on household composition, marital status, and intra-household bargaining may also be worthwhile. Furthermore, it will be interesting for future research to examine effect heterogeneity more comprehensively. For instance, labor supply responses may differ between occupations or baseline income levels. Lastly, more research is needed to understand the community effects of unconditional antipoverty efforts. Cash transfers that can be spent or invested with no strings attached may have distinct effects on the local economy, crime rates, or other neighborhood quality indicators.

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[Appendices for online publication]

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.

Imputed subsistence level. 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 is considered.

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

For illustration, Table A.1 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). Table A.1 also shows the eligibility threshold for the household under consideration. To be eligible for the program, the household's income cannot exceed the imputed subsistence level of €1,296.60 ($2,055 PPP).

Table A.1: 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) +
Child 1	–	€148.00	€40.00	€50.00 (taxes)
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) = €1,296.60 (imputed subsistence level)		
Total SMI	€1,296.60 (imputed subsistence level) – €900.00 (household income) = €396.60 (monthly benefit)			

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.
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.1 lists the assignment probabilities per stratum.

Table B.1: 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 percent 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 F.1 in Appendix F), which is excluded from the study, too.

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 in further detail below.

Community involvement. 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.

Social entrepreneurship. 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 that 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 one 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).

D Lists of Variables

Table D.1: List of Covariates With Description.

Variable	Description	Source
Monthly household 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.	Municipal civil registry
Main recipient age	Age in years.	Municipal civil registry
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
Owner-occupied house	1 if the household lives in owned property and 0 otherwise.	Survey

Table D.2: 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
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
Job search past 4w	1 if main recipient answered *yes* to the question: "In the past four 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 6m	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 six months and 0 otherwise.	Survey
No. of members working	Number of household members aged between 18 and 65 in paid employment or self-employed.	Survey
At least one member 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. of members 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 six months.	Survey
At least one member 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 six months and 0 otherwise.	Survey

E Attrition Analyses

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

$$Y_{hB} = \alpha + \beta_1 attrition_h + \gamma + \epsilon_h \tag{10}$$

Here, Y_{hB} describes the outcome 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. γ denotes randomization strata fixed effects and ϵ_h is the error term. We report the results of this analysis in Table E.1 below. Column (1) shows the means and standard deviations for the group of non-attrition households. Column (2) reports coefficients on the attrition dummy.

We use the following specification to test for differences in baseline outcomes between attrition households assigned to treatment and control groups:

$$Y_{hB} = \alpha + \beta_1 T_h + \gamma + \epsilon_h \tag{11}$$

In this equation, all features are the same as in Eq.(10), except for the dummy variable T_h, which indicates assignment to the treatment group. Estimating Eq.(11), we restrict the sample to households that filled in the baseline survey but not the endline survey. Table E.2 below reports the results of this second attrition analysis. Column (1) shows control group means and standard deviations. Column (2) presents coefficients on the treatment dummy.

Lastly, we test for differences in baseline outcomes between attrition households assigned to different treatment arms using a slightly adapted specification:

$$Y_{hB} = \alpha + \beta_1 T_h^x + \beta_2 C_h + \gamma + \epsilon_h \tag{12}$$

Here, T_h^x indicates assignment to a treatment arm x. As previously, x may denote the activation policy arm or the partial withdrawal arm. C_h indicates assignment to the control group. All other terms remain unchanged compared to Eq.(11). Again, we restrict the sample to households that filled in the baseline survey but not the endline survey. Table E.3 below shows the results of this analysis for the activation arms and Table E.4 for the withdrawal arms.

Table E.1: Attrition: Differences Between Attrition and Non-Attrition Households.

	Non-attrition mean (SD) (1)	Attrition (2)	N (3)
Working	0.387	-0.054	1,032
	(0.487)	(0.035)	
		[0.125]	
Job search past 4w	0.504	-0.034	1,031
	(0.500)	(0.036)	
		[0.347]	
Social participation	0.401	-0.001	1,034
	(0.490)	(0.036)	
		[0.969]	
Education in past 12m	0.249	-0.038	1,034
	(0.433)	(0.030)	
		[0.208]	
No. of members working	0.732	-0.016	1,034
	(0.762)	(0.059)	
		[0.779]	
At least one member working	0.568	-0.047	1,034
	(0.496)	(0.036)	
		[0.195]	
No. of members in education	0.586	-0.090	1,034
	(0.811)	(0.055)	
		[0.105]	
At least one member in education	0.424	-0.051	1,034
	(0.494)	(0.035)	
		[0.148]	

Note: Differences in baseline outcomes 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.(10). We report robust standard errors in parentheses and p-values in brackets. The sample does not comprise 1,200 observations due to baseline non-response. See Table D.2 in Appendix D for a description of variables.

Table E.2: Attrition: Differences Between Attrition Households in Treatment and Control Groups.

	Control mean (SD) (1)	Treatment (2)	N (3)
Working	0.358	-0.027	244
	(0.482)	(0.064)	
		[0.674]	
Job search past 4w	0.486	-0.026	243
	(0.502)	(0.065)	
		[0.690]	
Social participation	0.349	0.066	244
	(0.479)	(0.064)	
		[0.306]	
Education in past 12m	0.202	-0.000	244
	(0.403)	(0.054)	
		[0.998]	
No. of members working	0.679	0.159	244
	(0.815)	(0.114)	
		[0.167]	
At least one member working	0.495	0.110	244
	(0.502)	(0.065)	
		[0.089]	
No. of members in education	0.495	-0.003	244
	(0.753)	(0.098)	
		[0.978]	
At least one member in education	0.394	-0.041	244
	(0.491)	(0.065)	
		[0.531]	

Note: Differences in baseline outcomes between attrition households in the treatment and control groups. Attrition households are households that filled in the baseline survey but not the endline survey. Column (1) reports means and standard deviations for attrition households in the control group. Column (2) shows the coefficient on the treatment dummy, estimating Eq.(11). We report robust standard errors in parentheses and p-values in brackets. See Table D.2 in Appendix D for a description of variables.

Table E.3: Attrition: Differences Between Attrition Households in the Activation and No Activation Arm.

	No activation mean (SD) (1)	Activation (2)	N (3)
Working	0.333	-0.032	244
	(0.475)	(0.082)	
		[0.701]	
Job search past 4w	0.366	0.140	243
	(0.485)	(0.086)	
		[0.105]	
Social participation	0.306	0.223	244
	(0.464)	(0.085)	
		[0.010]	
Education in past 12m	0.153	0.080	244
	(0.362)	(0.070)	
		[0.251]	
No. of members working	0.819	-0.122	244
	(0.845)	(0.137)	
		[0.372]	
At least one member working	0.597	-0.079	244
	(0.494)	(0.083)	
		[0.339]	
No. of members in education	0.458	0.005	244
	(0.768)	(0.131)	
		[0.971]	
At least one member in education	0.319	0.041	244
	(0.470)	(0.083)	
		[0.616]	

Note: Differences in baseline outcomes between attrition households in the activation and no activation treatment arm. Attrition households are households that filled in the baseline survey but not the endline survey. Column (1) reports means and standard deviations for attrition households in the no activation arm. Column (2) shows the coefficient on the treatment dummy, estimating Eq.(12). We report robust standard errors in parentheses and p-values in brackets. See Table D.2 in Appendix D for a description of variables.

Table E.4: Attrition: Differences Between Attrition Households in the Partial and Full Withdrawal Arm.

	Full withdrawal mean (SD)	Partial withdrawal	N
	(1)	(2)	(3)
Working	0.276	0.085	244
	(0.451)	(0.082)	
		[0.300]	
Job search past 4w	0.448	-0.007	243
	(0.502)	(0.087)	
		[0.937]	
Social participation	0.362	0.089	244
	(0.485)	(0.086)	
		[0.299]	
Education in past 12m	0.241	-0.064	244
	(0.432)	(0.071)	
		[0.365]	
No. of members working	0.810	-0.062	244
	(0.868)	(0.139)	
		[0.657]	
At least one member working	0.569	-0.004	244
	(0.500)	(0.084)	
		[0.960]	
No. of members in education	0.500	-0.039	244
	(0.731)	(0.129)	
		[0.760]	
At least one member in education	0.379	-0.046	244
	(0.489)	(0.083)	
		[0.585]	

Note: Differences in baseline outcomes between attrition households in the partial and full withdrawal treatment arm. Attrition households are households that filled in the baseline survey but not the endline survey. Column (1) reports means and standard deviations for attrition households in the full withdrawal arm. Column (2) shows the coefficient on the treatment dummy, estimating Eq.(12). We report robust standard errors in parentheses and p-values in brackets. See Table D.2 in Appendix D for a description of variables.

F Additional Tables

Table F.1: 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			1,518	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.

Table F.2: 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 F.3: 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 divided by the number of households assigned to each treatment arm.

Table F.4: Treatment Effects at Endline: Adjusted p-values.

	Control mean (SD) (1)	Treatment effect (2)	Activation policy (3)	Partial withdrawal (4)	N (5)
Working	0.473	-0.095	-0.043	0.031	901
	(0.500)	(0.034)	(0.033)	(0.033)	
		[0.076]	[0.534]	[0.928]	
Job search past 4w	0.024	-0.015	-0.002	-0.002	904
	(0.155)	(0.011)	(0.008)	(0.009)	
		[0.504]	[0.754]	[1.000]	
Social participation	0.378	0.008	0.084	-0.021	904
	(0.486)	(0.035)	(0.037)	(0.037)	
		[0.832]	[0.158]	[0.984]	
Education past 6m	0.212	0.032	0.090	0.031	900
	(0.410)	(0.032)	(0.033)	(0.033)	
		[0.664]	[0.050]	[0.928]	
No. of members working	0.870	-0.155	-0.066	-0.000	904
	(0.823)	(0.051)	(0.052)	(0.052)	
		[0.048]	[0.534]	[1.000]	
At least one member working	0.638	-0.089	-0.060	-0.013	904
	(0.481)	(0.033)	(0.034)	(0.034)	
		[0.076]	[0.348]	[0.996]	
No. of members in education	0.533	0.053	0.076	0.008	904
	(0.806)	(0.054)	(0.054)	(0.054)	
		[0.664]	[0.534]	[1.000]	
At least one member in education	0.394	0.044	0.051	0.003	904
	(0.490)	(0.036)	(0.037)	(0.037)	
		[0.544]	[0.534]	[1.000]	

Note: OLS estimates of treatment effects on survey outcomes at endline (individual and household level). Outcome variables are listed on the left and described in detail in Table D.2 in Appendix D. Column (1) reports control group means with standard deviations in parentheses. Column (2) shows coefficients on the treatment dummy, estimating Eq.(5). Column (3) and (4) present coefficients on the treatment dummy, estimating Eq.(6). We report robust standard errors in parentheses and adjusted p-values using the Westfall and Young (1993) methodology and 10,000 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 4.

Table F.5: Treatment Effects at Endline: Excluding the Social Entrepreneurship Arm.

	Control mean (SD) (1)	Treatment effect (2)	Activation policy (3)	Partial withdrawal (4)	N (5)
Working	0.473	-0.097	-0.052	0.032	830
	(0.500)	(0.034)	(0.035)	(0.035)	
		[0.005]	[0.137]	[0.351]	
Job search past 4w	0.024	-0.014	0.001	0.000	833
	(0.155)	(0.011)	(0.009)	(0.009)	
		[0.204]	[0.917]	[0.967]	
Social participation	0.378	0.014	0.114	-0.012	833
	(0.486)	(0.036)	(0.040)	(0.039)	
		[0.689]	[0.005]	[0.760]	
Education past 6m	0.212	0.006	0.044	-0.009	829
	(0.410)	(0.032)	(0.035)	(0.033)	
		[0.857]	[0.208]	[0.789]	
No. of members working	0.870	-0.155	-0.085	-0.005	833
	(0.823)	(0.052)	(0.056)	(0.055)	
		[0.003]	[0.133]	[0.924]	
At least one member working	0.638	-0.086	-0.065	-0.010	833
	(0.481)	(0.034)	(0.037)	(0.036)	
		[0.011]	[0.076]	[0.783]	
No. of members in education	0.533	0.035	0.046	-0.020	833
	(0.806)	(0.055)	(0.059)	(0.056)	
		[0.516]	[0.430]	[0.719]	
At least one member in education	0.394	0.029	0.022	-0.022	833
	(0.490)	(0.037)	(0.040)	(0.038)	
		[0.435]	[0.580]	[0.563]	

Note: OLS estimates of treatment effects on survey outcomes at endline (individual and household level) excluding units assigned to the social entrepreneurship treatment arm. Outcome variables are listed on the left and described in detail in Table D.2 in Appendix C. Column (1) reports control group means with standard deviations in parentheses. Column (2) shows coefficients on the treatment dummy, estimating Eq.(5). Column (3) and (4) present coefficients on the treatment dummy, estimating Eq.(6). We report robust standard errors in parentheses and p-values 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 4.

Table F.6: Heterogeneous Treatment Effects at Endline (Varying the Age of Children).

	Main recipient working			At least one member working		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.057	-0.040	-0.074	-0.006	-0.026	-0.092
	(0.063)	(0.049)	(0.037)	(0.069)	(0.053)	(0.038)
	[0.371]	[0.416]	[0.048]	[0.934]	[0.622]	[0.016]
Interaction terms with treatment dummy						
HH with children (19 years or younger)	-0.060			-0.127		
	(0.074)			(0.080)		
	[0.421]			[0.110]		
HH with children (12 years or younger)		-0.097			-0.122	
		(0.066)			(0.068)	
		[0.144]			[0.073]	
HH with children (5 years or younger)			-0.111			-0.022
			(0.087)			(0.086)
			[0.204]			[0.802]
N	895	901	895	898	904	898

Note: OLS estimates of treatment and interaction effects on survey outcomes at endline. Column (1)–(3) report effects on the probability that the main recipient is working. Column (4)–(6) report effects on the probability that any household member is working. Outcome variables are described in detail in Table D.2 in Appendix D. The model in Column (1) and (4) includes a term interacting the treatment dummy with a dummy indicating that there are children of 19 years or younger in the household, the model in Column (2) and (5) a term interacting the treatment dummy with a dummy indicating that there are children of 12 years or younger in the household, and Column (3) and (6) a term interacting the treatment dummy with a dummy indicating that there are children of 5 years or younger in the household. We report robust standard errors in parentheses and p-values 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 4.

Table F.7: Unadjusted Treatment Effects at Endline.

	Control mean (SD) (1)	Treatment effect (2)	Activation policy (3)	Partial withdrawal (4)	N (5)
Working	0.473	-0.130	-0.066	0.044	901
	(0.500)	(0.038)	(0.037)	(0.038)	
		[0.001]	[0.076]	[0.242]	
Employed	0.457	-0.139	-0.068	0.031	901
	(0.499)	(0.038)	(0.037)	(0.037)	
		[0.000]	[0.064]	[0.413]	
Self-employed	0.016	0.008	0.002	0.014	901
	(0.127)	(0.010)	(0.012)	(0.011)	
		[0.394]	[0.870]	[0.229]	
Working full-time	0.229	-0.069	0.006	0.020	901
	(0.421)	(0.031)	(0.029)	(0.029)	
		[0.027]	[0.838]	[0.495]	
Working part-time	0.245	-0.061	-0.072	0.025	901
	(0.431)	(0.032)	(0.031)	(0.031)	
		[0.059]	[0.019]	[0.433]	
Permanent contract	0.186	-0.067	-0.027	0.011	895
	(0.390)	(0.029)	(0.027)	(0.027)	
		[0.022]	[0.321]	[0.686]	
Temporary contract	0.264	-0.069	-0.044	0.019	895
	(0.442)	(0.033)	(0.031)	(0.031)	
		[0.036]	[0.150]	[0.544]	
Job search past 4w	0.024	-0.015	-0.002	-0.001	904
	(0.155)	(0.011)	(0.008)	(0.008)	
		[0.180]	[0.824]	[0.927]	
Social participation	0.378	0.035	0.100	-0.024	904
	(0.486)	(0.037)	(0.038)	(0.039)	
		[0.340]	[0.009]	[0.538]	
Education past 6m	0.212	0.035	0.101	0.028	900
	(0.410)	(0.032)	(0.034)	(0.034)	
		[0.273]	[0.003]	[0.397]	
No. of members working	0.870	-0.183	-0.051	0.028	904
	(0.823)	(0.060)	(0.061)	(0.060)	
		[0.002]	[0.397]	[0.646]	
At least one member working	0.638	-0.115	-0.081	-0.002	904
	(0.481)	(0.037)	(0.039)	(0.040)	
		[0.002]	[0.038]	[0.960]	
No. of members in education	0.533	0.047	0.133	-0.006	904
	(0.806)	(0.060)	(0.060)	(0.061)	
		[0.432]	[0.027]	[0.926]	
At least one member in education	0.394	0.038	0.078	-0.002	904
	(0.490)	(0.038)	(0.039)	(0.039)	
		[0.324]	[0.043]	[0.968]	

Note: OLS estimates of treatment effects on survey outcomes at endline (individual and household level). Outcome variables are listed on the left and described in detail in Table D.2 in Appendix D. Column (1) reports control group means with standard deviations in parentheses. Column (2) shows coefficients on the treatment dummy, estimating Eq.(5). Column (3) and (4) present coefficients on the treatment dummy, estimating Eq.(6). We report robust standard errors in parentheses and p-values in brackets. All models include randomization strata fixed effects and control for the survey mode.

Table F.8: Treatment Effects at Endline With Additional Controls.

	Control mean (SD) (1)	Treatment effect (2)	Activation policy (3)	Partial withdrawal (4)	N (5)
Working	0.473	-0.092	-0.044	0.035	901
	(0.500)	(0.034)	(0.033)	(0.034)	
		[0.008]	[0.176]	[0.297]	
Employed	0.457	-0.097	-0.050	0.023	901
	(0.499)	(0.034)	(0.033)	(0.034)	
		[0.004]	[0.130]	[0.499]	
Self-employed	0.016	0.007	0.005	0.012	901
	(0.127)	(0.009)	(0.011)	(0.011)	
		[0.445]	[0.649]	[0.257]	
Working full-time	0.229	-0.059	0.003	0.019	901
	(0.421)	(0.029)	(0.027)	(0.027)	
		[0.043]	[0.906]	[0.476]	
Working part-time	0.245	-0.035	-0.050	0.017	901
	(0.431)	(0.031)	(0.030)	(0.030)	
		[0.258]	[0.095]	[0.570]	
Permanent contract	0.186	-0.056	-0.011	0.023	895
	(0.390)	(0.025)	(0.024)	(0.024)	
		[0.025]	[0.650]	[0.335]	
Temporary contract	0.264	-0.041	-0.034	0.009	895
	(0.442)	(0.032)	(0.030)	(0.031)	
		[0.198]	[0.264]	[0.781]	
Job search past 4w	0.024	-0.016	0.000	-0.002	904
	(0.155)	(0.011)	(0.009)	(0.008)	
		[0.131]	[1.000]	[0.807]	
Social participation	0.378	0.010	0.073	-0.027	904
	(0.486)	(0.035)	(0.037)	(0.037)	
		[0.771]	[0.052]	[0.467]	
Education past 6m	0.212	0.034	0.091	0.032	900
	(0.410)	(0.032)	(0.034)	(0.033)	
		[0.283]	[0.007]	[0.323]	
No. of members working	0.870	-0.155	-0.080	-0.002	904
	(0.823)	(0.051)	(0.052)	(0.052)	
		[0.003]	[0.122]	[0.976]	
At least one member working	0.638	-0.089	-0.066	-0.009	904
	(0.481)	(0.033)	(0.034)	(0.035)	
		[0.006]	[0.056]	[0.800]	
No. of members in education	0.533	0.058	0.069	0.006	904
	(0.806)	(0.054)	(0.054)	(0.053)	
		[0.282]	[0.203]	[0.917]	
At least one member in education	0.394	0.045	0.044	0.002	904
	(0.490)	(0.036)	(0.037)	(0.037)	
		[0.214]	[0.232]	[0.947]	

Note: OLS estimates of treatment effects on survey outcomes at endline (individual and household level). Outcome variables are listed on the left and described in detail in Table D.2 in Appendix D. Column (1) reports control group means with standard deviations in parentheses. Column (2) shows coefficients on the treatment dummy, estimating Eq.(5). Column (3) and (4) present coefficients on the treatment dummy, estimating Eq.(6). We report robust standard errors in parentheses and p-values 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 4. Additional controls are a dummy variable indicating the gender of the main recipient, dummies for the neighborhood in which the household is located (ten neighborhoods), dummies for the type of household (four types), dummies for household composition regarding nationalities (three types), and dummies for the highest education level reached by any household member (three levels).

Table F.9: Treatment Effects at Endline Using Logistic Regression.

	Control mean (SD) (1)	Treatment effect (2)	Activation policy (3)	Partial withdrawal (4)	N (5)
Working	0.473	0.589	0.758	1.203	901
	(0.500)	(0.184)	(0.191)	(0.196)	
		[0.004]	[0.146]	[0.346]	
Job search past 4w	0.024	0.390	0.823	0.824	694
	(0.155)	(0.575)	(0.752)	(0.820)	
		[0.102]	[0.796]	[0.814]	
Social participation	0.378	1.043	1.492	0.902	904
	(0.486)	(0.171)	(0.174)	(0.176)	
		[0.805]	[0.021]	[0.557]	
Education past 6m	0.212	1.221	1.671	1.224	900
	(0.410)	(0.197)	(0.191)	(0.194)	
		[0.310]	[0.007]	[0.299]	
At least one member working	0.638	0.618	0.720	0.939	904
	(0.481)	(0.181)	(0.185)	(0.188)	
		[0.008]	[0.076]	[0.738]	
At least one member in education	0.394	1.235	1.259	1.025	904
	(0.490)	(0.172)	(0.169)	(0.169)	
		[0.220]	[0.173]	[0.882]	

Note: Logistic regression estimates of treatment effects on survey outcomes at endline (individual and household level). Outcome variables are listed on the left and described in detail in Table D.2 in Appendix D. Column (1) reports control group means with standard deviations in parentheses. Column (2) shows coefficients on the treatment dummy and Column (3) and (4) coefficients on dummies indicating the respective treatment arm. We report coefficients in Odds Ratios. Robust standard errors are shown in parentheses and p-values 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 4.

G Additional Figures

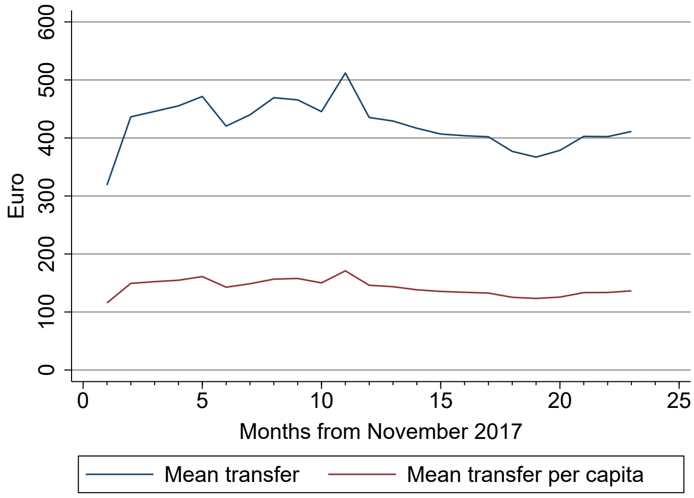

Figure G.1: Mean Transfer and Mean Transfer per Capita per Treatment Month.

Note: Zero payments included.

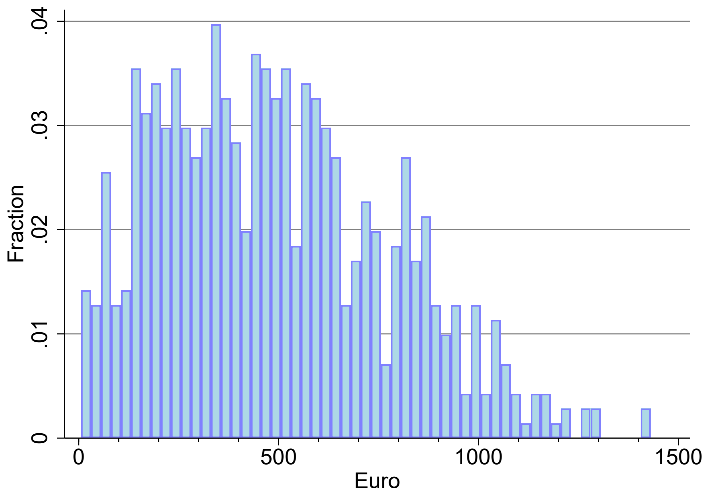

Figure G.2: Distribution of Mean Monthly Transfers.

Note: Zero payments excluded.