A Randomized Controlled Trial on the Provision of Financial and Social Capital to Low-Income Households in the United States

*Jaroszewicz, A.[1], *Jachimowicz, J.M.[2], *Hauser, O.P.[3],
Ashraf, N.[4], Bianchi, E.[5], Meier, S.[6] & Haushofer, J.[7]

[1] Institute for Quantitative Social Science, Harvard University, Cambridge, MA 02138
[2] Organizational Behavior Unit, Harvard Business School, Boston, MA 02163
[3] Department of Economics, University of Exeter Business School, Exeter, EX4 4JR
[4] Department of Economics, London School of Economics, London WC2A 2AE
[5] Organization & Management Department, Goizueta Business School, Emory University
[6] Management Division, Columbia Business School, Columbia University
[7] Department of Economics, Stockholm University; MPI Bonn; IFN;
Busara Center for Behavioral Economics

* The first three authors contributed equally. Please address correspondence to:
ajaroszewicz@hbs.edu, jjachimowicz@hbs.edu, & o.hauser@exeter.ac.uk

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Appendices: The survey instrument and power analysis code are available on the AEA RCT Registry.

Abstract

Prior research, conducted predominantly in low-income countries, has found that unconditional cash transfers can be an effective means of lifting people out of poverty. We propose that financial capital alone may be insufficient to fully address the challenges posed by poverty, which include a lack of belongingness, information, and goal-setting accountability—challenges that may be more adequately addressed through social capital. We hypothesize that the effects of financial and social capital are additive or potentially even multiplicative, such that each form of capital adds to or even enhances the effectiveness of the other. In collaboration with a US-based non-profit organization, we will test these predictions in an 18-month randomized controlled trial. We will randomize approximately 1,800 low-income households in Cambridge and Boston, MA, into one of four treatment arms: financial capital (unconditional cash transfers of $500 per month for 18 months), social capital (encouragement to develop and strengthen social ties), the combination of both the financial and social capital treatments, or no treatment. Using in-depth panel data composed of surveys, administrative bank account data, and administrative welfare receipt data, we not only seek to understand whether these forms of capital can improve financial, psychological, health, and family well-being outcomes, but also why and for whom they may improve outcomes. Taken together, these data will offer insights into how financial and social capital can support low-income US households in sustainably moving out of poverty.

Keywords: Poverty, Welfare, Well-Being, Behavioral

Summary: An RCT with 1,800 households tests the effects of combining financial and social resources on lifting US households out of poverty.

One in three Americans report struggling to make ends meet [1], and one in eight Americans live below the poverty line [2]. Poverty is also an isolating and lonely experience, as many people feel ashamed of their inability to support themselves and their families [3]. Yet, with many people living in financial difficulty, a large number share the same obstacles and challenges. Thus, it is possible that social capital—the extent to which one can rely on friends and family for support, information, and company [4]—could provide buffers against, or potentially even solutions to, these hardships.

In this large-scale, longitudinal randomized controlled trial (RCT), we aim to understand the extent to which the negative effects of poverty can be ameliorated through financial and social capital support. Households will be randomly assigned to either (a) receive a "no-strings-attached" unconditional cash transfer of $500 per month (totaling $9,000 over 18 months), (b) be encouraged to develop their social capital by creating and engaging with a "peer group" of four to eight participating households, (c) receive both the financial assistance and social capital interventions, or (d) not receive any intervention. We predict that the provision of financial and social capital alone will each have a significant and positive effect on outcomes, and that the effect of combining the two will be larger than the effect of providing either alone.

We study the effects of these treatments on four sets of outcomes: financial, psychological, health, and family well-being. Moreover, we investigate the role of intrapersonal and interpersonal mechanisms to shed light on what might drive these outcomes. Our trial will involve approximately 1,800 low-income households in Boston and Cambridge, MA, and track their outcomes regularly over the course of 18 months. The trial will be conducted in close collaboration with a national non-profit organization, which will deliver the interventions, and a state governmental agency, which will provide critical administrative data and aid in recruiting households. Through these partnerships, we will collect granular data on households using surveys, transaction-level administrative banking data, and administrative welfare receipt data.[a] Our study design will help us better understand the extent to which financial and social capital—both independently and together—might help low-income households escape poverty.

Summary of Past Work and Our Contribution

A large body of research has examined the effects of providing financial resources to poor individuals in low-income countries. Studies in Kenya, South Africa, Zambia, Zimbabwe, and many other countries (e.g., [5–10]) have found that unconditional cash transfers (UCTs), which provide no-strings-attached cash to households in need for any expenses of their choice (including medication, education, large household purchases, business investments, or leisure), can have positive effects on a broad range of outcomes. Given that poverty is often accompanied by income variability [11,12], depression [13], social isolation [14], and a lack of information [15], an additional potential pathway out of poverty may be through social capital. Providing households in poverty with the opportunity to access a wider network, which can give financial assistance, share childcare responsibilities, or give personal, professional or financial advice, may allow them to weather hardships more effectively [16,17]. Peers can also provide emotional support and hold a person accountable to reach his or her goals [16,18]. As a result, the provision of either financial or social capital may be an effective strategy of lifting low-income households out of poverty.

Unique to our study design is that we test both unconditional cash transfers and a social capital intervention, separately and in combination. Households will be randomized into one of four groups: Cash-only, Social-only, Cash+Social, and No Treatment Control. We predict that

[a] Although our outcomes are measured at the individual level, we view our treatment as having the potential to affect entire households. Our surveys therefore aim to capture both individual and household-level outcomes.

the financial assistance and social capital opportunities will each individually improve households' outcomes. We further hypothesize that the combination of the two will be most effective. While to some extent the two forms of capital may address the same problems through different means, we believe they will also have additive effects, addressing different sets of problems households may have. Moreover, there may even be multiplicative effects, such that each form of capital enhances the other and increases its treatment intensity. We outline our hypotheses, including why we believe that the combination of cash and social capital will be most effective at improving outcomes, in more detail in the *Hypothesis Development* section below.

To be eligible for the trial, households must reside in Cambridge or Boston, MA, and have a total earned household income between 0% and 200% of the federal poverty line. Participants in the Cash-only (hereafter, "C") treatment arm will receive $9,000 over 18 months in fully unconditional payments, paid out as $500 per month. For a household of four living at the poverty line, these payments will raise participants' annual earned income by 22%. That is, participants in this arm who earn near the poverty line will receive unconditional payments equal to almost three months of income per year. The Social-only ("S") arm will be placed into peer groups with three to seven other participants in this treatment arm and will be encouraged to interact with their peer groupmates through monthly meetings. They will also be given access to an online platform that aims to help people develop and strengthen social ties, in part by decreasing barriers to help-seeking and -giving. Finally, they will be given a small unconditional payment of $20 per month.[b] Those in the Cash+Social ("CS") arm will receive the same financial

[b] This payment will be used to compensate participants for their time, minimize attrition, and keep constant the communications and relationship building the UCTs generate between the participant and the non-profit organization. Importantly, the amount is small enough that we do not expect it to act as proper UCT payment in

benefits as those in the Cash treatment arm and the same social capital benefits as those in the Social treatment arm. Finally, those in the No Treatment Control ("NTC") arm will receive only the small unconditional payment ($20 per month) and will not be given any opportunity to participate in any of the social interventions. Each treatment arm is described in further detail below.

We will examine the effects of these treatments on a wide range of outcomes, leveraging panel data and a rich and complementary array of datasets. All participants will be asked to complete quarterly surveys, which will ask about financial, psychological, health, and family well-being outcomes. We will also observe transaction-level financial outcomes from checking and savings accounts, including savings stocks, direct deposits of earned income, and late fees charged. Finally, for participants who are recipients of any welfare program administered by our partner state agency,[c] or who have received benefits in the five years before the trial begins, we will also observe administrative data from that agency. This data will include a comprehensive past and present financial profile, welfare applications and receipts, Supplemental Nutrition Assistance Program benefit usage, and employment outcomes, enabling us to verify and complement many survey measures.

The first and most fundamental aim of this study is to quantify the effects of the treatment arms (C, S, and CS), relative to the NTC arm, on our pre-registered outcomes. Specifically, we seek to measure the effects of our treatments on financial (e.g., subjective financial well-being,

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itself. The $20 per month payments increase the annual earned income for a household of four at the poverty line by 0.9%. Participants in the cash arms receive 25 times more.

[c] This agency administers three programs: the Supplemental Nutrition Assistance Program (SNAP, formerly known as food stamps), Transitional Aid for Families with Dependent Children (TAFDC, which provides cash assistance to families with children or women in the final stages of pregnancy), and Emergency Aid to the Elderly, Disabled and Children (EAEDC, which provides cash assistance to people who are elderly, unable to work due to disability, and/or caring for dependents with certain types of characteristics). Only participants who consent to giving us their state agency identification number will have their survey responses connected to the state agency's data.

number of bills left unpaid), psychological (e.g., sense of agency, depression), physical health (e.g., sleep quality, nutritional quality), and family well-being (e.g., parenting quality, quality of relations with one's partner) outcomes.

Second, we will aim to identify the underlying mechanisms that may drive our treatment effects. To this end, we will examine the roles of both intrapersonal mechanisms (e.g., cognitive capacity, time preferences, risk preferences), as well as interpersonal mechanisms (e.g., sense of support from others, comfort with asking for help, extent to which others are helping one achieve one's goals).

Finally, our study design gives us the opportunity to test whether the treatments work particularly well for specific subpopulations. We outline some of these tests in the *Exploratory Analyses* section below.

Hypothesis Development

The Role of Financial Capital

Prior work suggests that providing low-income households with financial capital can have beneficial effects. Unconditional cash transfers, which provide money without imposing any conditions (in contrast to conditional cash transfers, which require recipients to meet a condition, such as enrolling their children in school, before receiving the money; [19,20]), ease liquidity constraints while providing recipients with maximum flexibility on how to use their funds. This, in turn, can improve economic [9,21,22], educational [23–25], and health outcomes [21]. As a result, UCTs have been regarded as an important tool to boost low-income households' progress out of poverty [26].

Although most UCT research has been conducted in low-income countries, several studies have examined the effects of providing financial capital to low-income households in the

United States—the context of our study—and found similar effects (e.g., [21,23,27]). However, a few challenges remain in the application of UCT in the US. First, some opponents of UCT—and the related but distinct concept of universal basic income—argue that it is unreasonably expensive [28–30].[d] Second, it is unclear whether UCTs are associated with negative externalities. While some studies have found evidence for modest declines in labor supply as a result of such transfers, particularly among women [31,32], others do not [26].

Third, providing financial capital may not be sufficient on its own for lifting households out of poverty. While relatively little is known about the mechanisms by which UCTs function, prior literature suggests that lifting financial constraints can have positive consequences for an individual's sense of agency and cognitive capacity [33–35]. Poverty has also been associated with other "intrapersonal" shifts, such as risk preferences and time (discounting) preferences [17,36,37]. Together, these data suggest that UCTs may be adept at addressing *intra*personal mechanisms. However, poverty is also associated with a host of other factors, which UCTs may not be best poised to address. For example, poor individuals often lack social belonging and consequently experience reduced emotional, professional and financial support [3,14,17,38]. UCTs may not be sufficient to address these equally important *inter*personal mechanisms, and may not alone resolve the barriers that low-income households face when trying to advance out of poverty.

The Role of Social Capital

While financial capital may help address intrapersonal mechanisms of poverty, we propose that social capital may be better suited to address the interpersonal mechanisms of poverty. In the absence of a financial safety net, low-income individuals require other strategies

[d] The primary distinction between these two concepts is that universal basic income provides every household, regardless of its financial status, a guaranteed income for an indefinite period of time. UCTs, on the other hand, are often provided only to low-income households and may only be provided for a limited period.

to cope with financial hardship. One option is to develop a network to call on in emergencies. Indeed, past research has found that low-income individuals benefit from their social resources when they help them meet challenges associated with limited financial resources [39–43]. Consider that low-income households may choose to develop a system of rotating child care arrangements, or borrow and lend money to people within their informal networks—networks which are likely a critical part of their safety net [44]. For instance, some work has found that being randomized to meet more (rather than less) frequently with one's microfinance institution group led to more risk pooling and better economic outcomes [45]. Similarly, among Americans who could not cover a $400 emergency expense, almost 30% report that they would weather a financial emergency by borrowing money from friends and family [46]. Taken together, this research suggests that greater social capital might boost low-income households' ability to address challenges that arise from lacking financial capital.

Social capital may also address challenges endemic in poverty that go beyond just these material or labor-sharing benefits. Lower socioeconomic status groups often experience not just a poverty of financial resources, but also a "poverty of information" [15,47]. Knowing where to find good value-for-money bargains is important when finances are tight. Financially-constrained individuals are more likely to avoid pricing surcharges [48], notice hidden taxes [49], and avoid fraudulent charges [50] relative to people who are higher income, suggesting that low-income individuals may be a valuable source of information for each other. Indeed, stronger social capital can be a source of new and economically-important information and advice, and such information sharing among low-income groups has been shown to improve financial outcomes [18,51,52].

In addition to material or labor-sharing benefits and information, a social safety net can provide psychological reassurance for individuals who fear financial hardship in the future. That is, simply believing that one *could* receive help from others—e.g., that one could ask one's community to provide material or informational assistance if needed—may be sufficient to bolster confidence and change behavior, even if one does not actually take advantage of that help [17,35,53]. Furthermore, feelings of belonging, emotional support, and sharing experiences with peers who can sympathize with one's situation are associated with better health, well-being, and even employment outcomes [3,54–56].

A final channel through which our social capital intervention can improve outcomes is by encouraging support groups in which the members set goals and hold one another accountable to complete those goals. Effective goal-setting, alongside regular small self-help support group meetings, has been found to improve individual and business outcomes of low-income microentrepreneurs in Latin America [16]. Such results may extend beyond actual goal-setting accountability to simply knowing that individuals are accountable to other peer group members, as one field experiment suggests [18].[e]

Given this body of research, we hypothesize that encouraging low-income households to develop and maintain their social capital will have a beneficial effect on their outcomes.

Combining Financial and Social Capital

The research described above indicates that both financial and social capital are likely to be independently effective, and may thus have a positive effect on low-income households relative to not having those types of capital.

[e] In addition to the beneficial effects of receiving help, we note that participants may also benefit from helping others in their peer groups. Helping others could, for instance, increase self-esteem and a sense of agency.

What might happen when the two forms of capital are combined? One possibility is that our treatments will address the same problems through different means, and thus that combining the two will not be substantively better than either alone. There is some reason to believe that this may be true. For instance, both forms of capital can likely be used to ease liquidity constraints. The financial capital treatment can do so directly, while the social capital treatment can do so indirectly by giving people a network of peers on whom they can rely on for financial capital in case of need.

Such effects notwithstanding, however, we believe that the two forms of capital will also address somewhat different problems that low-income households may face, and thus that the combination of the two forms of capital will be more effective than either alone. For instance, financial capital affords its recipients independence and agency, which prior work has argued has both intrinsic value [57,58] and a range of positive downstream consequences [35,59,60]. Social capital, on the other hand, may provide households with a sense of belonging, useful information, and better support in reaching one's goals. We term this ability of the two forms of capital to address different problems as additive effects.

It is possible that in addition to these additive effects, financial and social capital will also have multiplicative effects—i.e., that each form of capital will enhance the effectiveness of the other. The combination of financial and social capital could be synergistic in at least four ways. First, if each person in a community or peer group experiences not perfectly positively correlated shocks, financial capital can be shared across households to help them smooth consumption over those shocks [61]. In the context of our study, consider that while each household in the C arm will have access to $500 per month, each household in the CS arm—through their peer group—will effectively have a potential safety net of $2,000 to $4,000 per month (and up to $36,000 to $72,000 when considering the full 18-

month trial duration), depending on peer group size. That is, for those in the CS treatment arm, each individual household will potentially have access to greater financial resources than those in the C arm, resources which they could choose to pool to address any individual household's financial emergency (for similar reasoning, see research on microfinance groups, rotating savings and credit associations, and self-help groups; [62–64]).

Second, the mere knowledge that these increased funds may be available for each household in the CS arm may provide psychological benefits, including reduced stress and improved subjective financial well-being (e.g., [17,35,53,65,66]), even if the funds are not used.

Third, households in the peer groups can provide each other with information or advice that can allow a household to make better use of newly available funds. That is, while information or advice on how to use money is likely useful even when one has limited funds, and money alone is likely useful even when one does not have advice on how to use it, having both simultaneously is likely best.

Finally, the combination of financial and social capital means that a household in a peer group has more money available with which to set new financial goals to which the peer group can hold that member accountable. Setting and being held accountable to one's financial goals likely produces better outcomes when one has more money to use; and having more money likely produces better outcomes when one can set and achieve goals that one believes are best for one's future self (e.g., [16,18]).[f]

Given the possibility of additive and/or multiplicative effects, we hypothesize that households receiving both forms of capital will experience better outcomes than households receiving just one form of capital.

We next describe the methods of the study, our pre-analysis plan, hypothesized results, and what we hope to learn from them. Importantly, we also discuss what we learn from the trial

[f] Note that while the first and second multiplicative pathways outlined here rely on a person being connected to a peer group *that also has financial capital*, the third and fourth do not.

should we find mixed evidence for our hypotheses, or fail to find support for our hypotheses altogether.

Methods

Ethics

This study was approved by the Harvard University Institutional Review Board (IRB19-1341). All participants will provide informed consent.

Population and Recruitment

Population. The participants will be residents of Boston or Cambridge, MA who are at least 16 years old and have a total earned household income below 200% of the federal poverty line (i.e., up to $53,000 for a household of four in 2021[g]; median household income in Boston 2015-2019 in 2019 dollars: $71,115[h]). Participants must have at least one dependent under 18 and must not be past or present recipients of any other program administered by the partner non-profit organization.[i] No more than one person per residence can be enrolled. These criteria will be verified by the non-profit organization and, where possible, cross-checked with the welfare benefit agency's administrative data ex-post. Applicants will also be asked to confirm that they are willing and able to commit to the full 18 months, speak English and/or Spanish, and have reliable internet access through a computer or smart phone.

The non-profit with which we partner has been administering a program similar to that of the CS arm in several cities around the US.[j] This program has been active in Cambridge and

[g] https://aspe.hhs.gov/2021-poverty-guidelines; accessed on April 14, 2021.

[h] https://www.census.gov/quickfacts/fact/table/bostoncitymassachusetts/INC110219#INC110219; accessed on April 14, 2021.

[i] There is one exception to this rule: households that received a one-time COVID-19 emergency relief payment from the non-profit organization are allowed to enroll.

[j] Historically, the program the non-profit organization administered was a two-year program that involved giving households financial capital (up to $2,400 over the two years) and requiring them to meet monthly with their peer groups. Thus, the core components of the non-profit's original program are similar to the ones that we test in this

Boston in the past but has not been serving households in the area for several years. Historically, the population served in Cambridge and Boston has had an annual household earned income between $5,000 and $50,000. About 43% of participants have been African-American, 35% have been Hispanic (non-White), 2% have been White, 1% have been Asian or Pacific Islander, and 1% have been Native American, with the remaining 17% not reporting their race. About 41% of household members have been female. Roughly 45% of household members have been under the age of 18, 48% have been between 18 and 65, and 2% have been above 65 (the remaining 5% did not report their age). The vast majority of households (86%) have included children, and the average household size has been three people. We anticipate that our recruited sample will have similar characteristics.

Recruitment and Retention. Recruitment will be conducted through a combination of text messages from the partner government agency, advertisements on the partner government agency's website, information sessions, mailed pamphlets, community meetings organized by the partner non-profit and other similar non-profit organizations, and a social media campaign, in part supported by trusted community partners. The non-profit organization, along with support from the research team, will be responsible for the recruitment, treatment administration, and data collection.

Prior to enrollment, households will be informed that their participation is part of an 18-month research project that aims to (a) better understand the benefits of different support

RCT. However, there are some differences: historically the financial capital has been conditional rather than unconditional, and the peer groups have been created by the program participants themselves rather than by the non-profit (see the *Peer Group Creation and Randomization* section below). Moreover, while the non-profit organization has data suggesting that their program is effective at improving financial outcomes, these data are based on pre-post measures rather than an RCT with a control group. In addition, the organization has not tested the social and cash components separately. Thus, to the extent that the combination of financial and social capital improves households' outcomes, it is not clear which component(s) of the equation are driving results. The present RCT helps to address these questions.

programs, and (b) inform the future of the non-profit's and state government agency's offerings to low-income households. It will be disclosed that not all households will receive the same benefits during this study period and that these benefits will be assigned to each household based on randomization, so that the research team can evaluate and compare different aspects of the program. Households will not be informed of the specific differences between the treatment arms or our hypotheses.

Participants will be enrolled across multiple "waves," such that participants start the study at different times but—once enrolled—experience the same temporal gaps between surveys, payments, and other interventions. The first wave will begin in June 2021, with subsequent waves spaced approximately one month apart. Enrolling across month-long intervals rather than at smaller or longer intervals allows us to ensure we have a sufficiently large pool from which to create peer groups (see the *Peer Group Creation & Randomization* section below) while also limiting the amount of time households must wait before beginning the trial to no more than one month.

Although we plan to recruit for approximately six months, the scale of this project requires some flexibility. We will closely monitor recruitment and attrition rates throughout the trial. If the rates indicate that we are not on track to have the target number of participants in each treatment arm by the end of the eighteenth month, we will increase advertisement and outreach, consider expanding to a broader geographic area, consider increasing payment, and consider adding non-financial incentives, such as allowing participation in the trial to count towards welfare recipients' work requirements.

Treatment Arms

Our experimental design has been informed and shaped through qualitative interviews with households in the target population. Each household will be enrolled in the trial for 18 months. Households will be randomized into one of four treatment arms, which will orthogonally manipulate providing financial capital and encouraging people to develop and maintain their existing social capital.

Figure 1A provides an overview of the experimental conditions in our trial and Table 1 summarizes the financial incentives and social capital development opportunities. After participants have been verified to be eligible for the trial and provided informed consent, they will be required to complete three tasks to fully onboard. First, they must create an account on an online platform the non-profit has created. Second, they will be asked to choose how they want to be paid in the trial: they can provide their bank account information for direct bank account deposits, connect their checking or savings accounts to the platform to receive their money through the platform, or choose to receive their payments through a physical payment card. Any checking or savings account participants connect will be tracked, with the participant's permission. Finally, all participants will need to complete the baseline (Month 0) survey, the first of our surveys (see Appendix for the complete materials). Participants will be paid $60 for completing these tasks at sign-up. Participants who are unable to receive the sign-up funds due to providing incorrect banking information or information for an expired account, as well as participants who choose to receive their funds on the online platform but do not claim their funds, will receive four notices asking them to address the problem. Participants who fail to address the problem within 60 days of payment will be removed from the trial.

Table 1. Procedures and payments for the four treatment arms.

	NTC (No Treatment Control)	**C** (Cash-only)	**S** (Social-only)	**CS** (Cash+ Social)
Sign on bonus ($60)	x	x	x	x
Quarterly surveys ($40 * 6 = $240)	x	x	x	x
Completion bonus ($100)	x	x	x	x
Small unconditional cash transfer ($20 * 18 = $360)	x		x	
Large unconditional cash transfer ($500 * 18 = $9,000)		x		x
Access to basic online platform	x	x	x	x
Access to social components of online platform			x	x
Matched into peer group			x	x
Monthly peer group meetings			x	x

All participants who remain actively enrolled in the trial (see the *Attrition, Non-Compliance, Contamination, Spillovers, and Power Analysis* section for details on how we define active enrollment) will receive at least a $20 per month stipend, with the cash arms receiving $500 instead of $20. To incentivize completion, participants who are actively enrolled by the end of the 18 months will further receive a bonus payment of $100. Those who complete all the surveys will be eligible for a chance to receive an additional $1,000 bonus.

In return, participants will be asked to complete surveys each quarter for the 18 months. That is, after they have completed the Month 0 baseline survey and formally enrolled, they will be asked to complete "endline" surveys in Months 3, 6, 9, 12, 15, and 18. Figure 1C outlines the timeline. All surveys, including the Month 0 survey, will be sent by email, administered online, and mobile-device-friendly. Participants will have the option to take the surveys in English or Spanish. Each survey will take approximately 40 minutes, although participants will be able to stop the survey and return to it later if they choose. The surveys will be sent on approximately the same day of the month each quarter, and participants will be given 14 days to complete the survey from the time that it is sent to them. They will receive $40 for completing each endline survey. If all surveys are completed, a participant in a non-cash arm will receive $760 over the course of 18 months.

All payments (i.e., the sign-on bonus, monthly unconditional cash transfers, survey payments, and completion bonus(es)) will be made according to the participant's preferred mode of payment (bank account deposit, online platform account, or physical payment card). Monthly payments will always be made on the same day each month, and survey payments will be made within a few weeks of the participant completing the survey. There will be no restrictions or requirements on when the funds can be used or what they can be used for.

The No Treatment Control ("NTC") arm will not receive any additional intervention beyond what is described above. We will next describe how each of the other three treatment arms differs from the NTC arm.[k]

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[k] In addition, we will also have an "Out of Sample Control" group—a group of participants who do not actively enroll in the study but for whom we have some administrative data from the state governmental agency with whom we partner. Although these participants will differ from our main sample in some ways (e.g., they will not self-select into being in the study), comparing our main sample's outcomes to the outcomes of this group will help us to control for the possibility that simply being enrolled in the study and answering surveys shifts outcomes. See the *Out of Sample Control Group* section below.

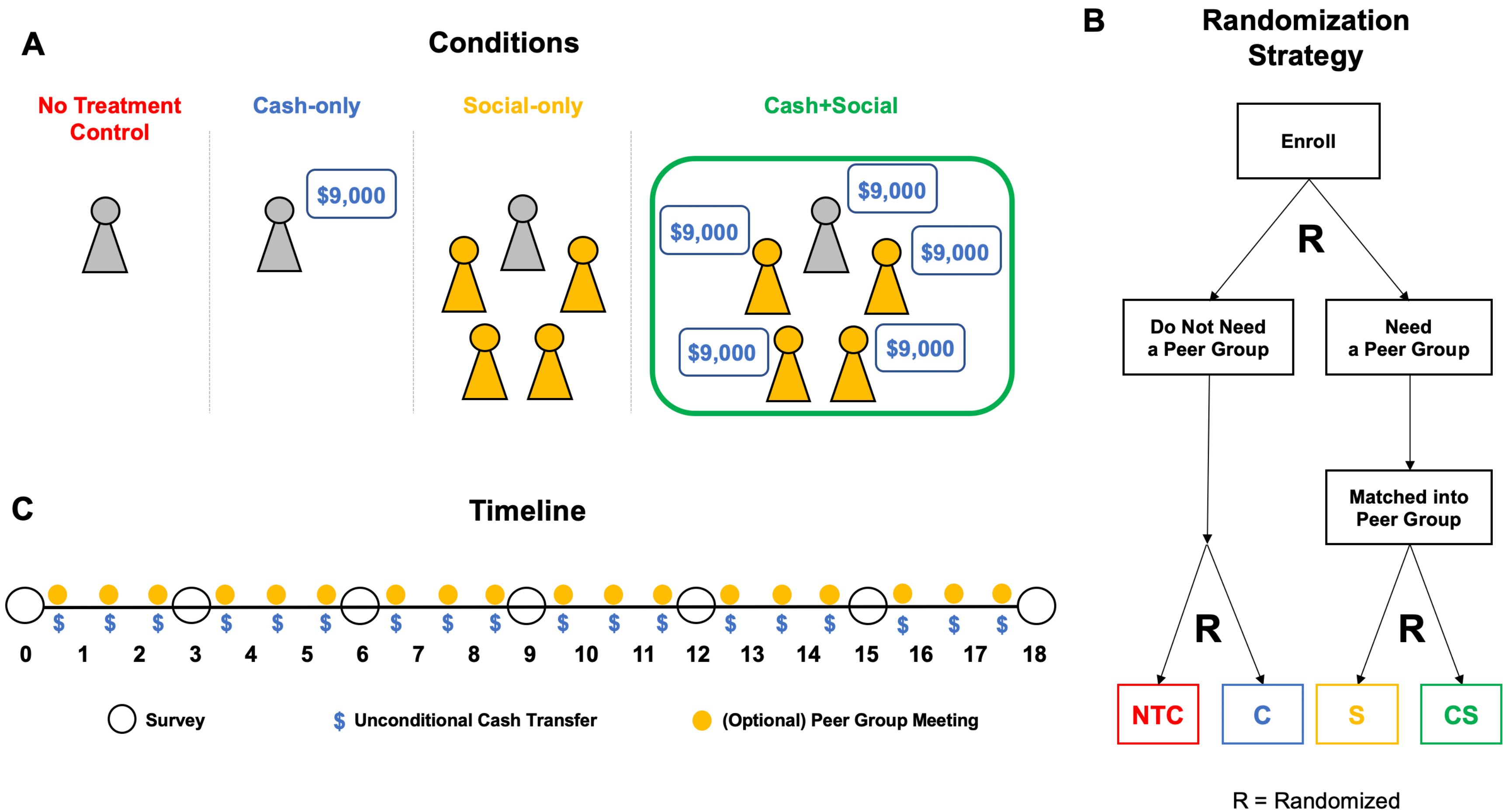


Figure 1. Experimental conditions, randomization strategy, and timeline. A Compared to the control group (NTC – red), the experimental treatments vary whether a participant receives a large unconditional cash transfer only (C – blue), the opportunity to develop social capital (S – orange), or both the cash and social capital assistance (CS – green). **B** Our two-stage randomization approach enables us to optimize the peer group matches by ensuring there is a relatively large pool of participants to draw from during the matching process, while also ensuring causal inferences from the experimental design. **C** The 18-month timeline illustrates when we will administer the quarterly surveys, when participants will receive cash payments, and when participants in the S and CS treatment arms will be expected to meet with their peer group.

The Cash-only arm ("C") will complete the same survey procedures as the NTC arm, but receive larger unconditional cash payments. Instead of receiving $20 per month, households in this treatment arm will receive $500 per month—a 25-fold difference. That is, they will receive $9,000 in unconditional cash payments over the course of 18 months, in addition to the $400 they can earn for remaining enrolled and completing surveys, for a total of $9,400 over the course of the 18-month trial. Our monthly payment schedule takes the universal basic income approach of providing a steady stream of liquid funds [65] to create financial slack and help people smooth over shocks [67].

One common concern with unconditional cash transfer studies is that participants may receive fewer benefits from means-tested government programs if they are receiving significant financial support from a research study. That is, a household that was previously eligible to receive certain benefits may lose some or all of those benefits if they start to receive significant income from a research study. This is a problem not only for recruitment and retention, but also for being able to precisely say what treatment participants received in the study (e.g., was it $500 per month, or $500 per month minus the lost benefits from other programs?). However, this is not a concern in our trial: to address this, the non-profit organization worked with the state governmental agency to *not* count the unconditional cash transfers, the bonuses, or the survey payments received in this study towards participants' income for any benefit program they operate, including the widely used Supplemental Nutrition Assistance Program.[1]

We next turn to the Social-only ("S") arm. Like those in the NTC arm, participants randomized into the S arm will receive only the smaller monthly unconditional cash transfers

[1] We are very grateful to our state governmental agency partners for their hard work in making this possible. Note that funds received in the study may affect benefits received for programs not funded by the state, and prospective participants will be encouraged to speak with their benefits administrators about this possibility.

($20 per month), the sign on bonus, survey payments, and completion bonus (up to $760 over the course of 18 months). Critically, however, they will also be encouraged to develop and maintain their social capital. At onboarding, they will be matched into "peer groups" of three to seven other participants with whom they will interact for the remainder of the study. Details on how these peer groups will be created are below in the *Peer Group Creation and Randomization* section.

Participants in the S arm will be encouraged to meet monthly with their peer groups.[m] They will receive guidance before they start the study encouraging them to discuss and decide on a peer group structure (e.g., rotating leadership or rotating responsibilities), peer group purpose (e.g., creating individual goals and holding one another accountable, or creating peer group goals), and meeting and communication norms (e.g., checking in with one another regularly, developing a plan for holding regular meetings that complement the peer group structure and purpose). This guidance will first be shared through a presentation at an orientation (what we call "Welcome Days"; see the *Peer Group Creation and Randomization* section below), which will be led by non-profit staff and former members of the non-profit's program. In addition, participants will receive handouts to help them decide what would work best for them and their peer group. These handouts will also be shared at the Welcome Days and the staff will provide support in filling them out. Participants will be encouraged to refer back to and update these handouts as they progress in the trial. Halfway through the trial, peer groups will be encouraged to do a "Team Relaunch" in which they reevaluate their current system, reaffirm their commitment to the group and any goals, and make any needed changes.

[m] Given the ongoing COVID-19 pandemic, peer groups will initially be encouraged to meet virtually only. However, if and when it becomes safe to do so, we will begin to encourage peer groups to meet in person.

Importantly, the Welcome Days and the Team Relaunch will be designed to incorporate best practices from the team and group launch [68] and relationship building [69] literatures. For instance, the staff will emphasize the importance of common purpose [70] and encourage peer groups to build up psychological safety [71] to help ensure that all peer group members feel included and supported. We believe that these practices will strengthen the ties between peer group members and their willingness to support one another throughout the trial, while also balancing the need for group-led autonomy.

Peer groups will receive “sample agendas” for their meetings, which will include a mix of bond-formation exercises and conversation starters. However, groups will not be required to use these agendas; there will be no restrictions for where or when the monthly peer group meetings take place or what is to occur at the meetings. The meetings will not be supervised by the non-profit organization or the research team, giving participants full control over how to structure and conduct these meetings. To obtain insights into what occurs at the meetings, we will ask participants to report on the frequency of these meetings and the topics that are discussed.

Finally, participants in the S arm will have access to the social components of the organization’s online platform. The core of the social components of the online platform is giving users the opportunity to connect with other members on the platform in groups of up to eight individuals. Once in a group, the group members can communicate with one another and post announcements, questions, and comments. In addition to providing the users with a channel to facilitate communication, the online platform is structured around helping the groups accomplish two main things. First, participants are encouraged to develop shared and/or individual goals and support one another in achieving them. For instance, the platform prompts users to describe their goals to the rest of the group and how the group can help them reach their goals (see Figure 2A). Second, they are encouraged to ask for and offer one another help, whether that help comes in the form of material goods, time, transportation, skills, connections, advice, or information. For instance, to make people more comfortable with asking for help, the platform reminds users that everyone needs help sometimes and that needing help is not a personal failure, but rather an opportunity to help others feel closer to them (see Figure 2B).

All S arm participants will automatically be connected with the other members of their peer group on the online platform. In addition, they will have the option to connect with as many other individuals on the platform as they choose, creating groups based on interests, goals, or simply friendship. Participants will not be able to create groups with anyone in the C or NTC arms, and will need to have a participant's email address to invite them to their peer group.

Participants in the Cash+Social ("CS") arm will receive both the financial and social capital interventions, exactly as they are described above. That is, participants in the CS arm will receive $9,000 in unconditional cash payments and an additional $400 if they complete all surveys; be placed into peer groups of four to eight households; participate in a Welcome Day and Team Relaunch; be encouraged to meet monthly with their peer group; and be given access to the social components of the online platform.

Peer Group Creation and Randomization

Aims. As mentioned above, two of the treatment arms (the S and CS arms) will complete the study procedures as part of a peer group. The peer group-creation process will involve matching randomly-chosen participants in a way that we believe will maximize the functionality of the peer groups.

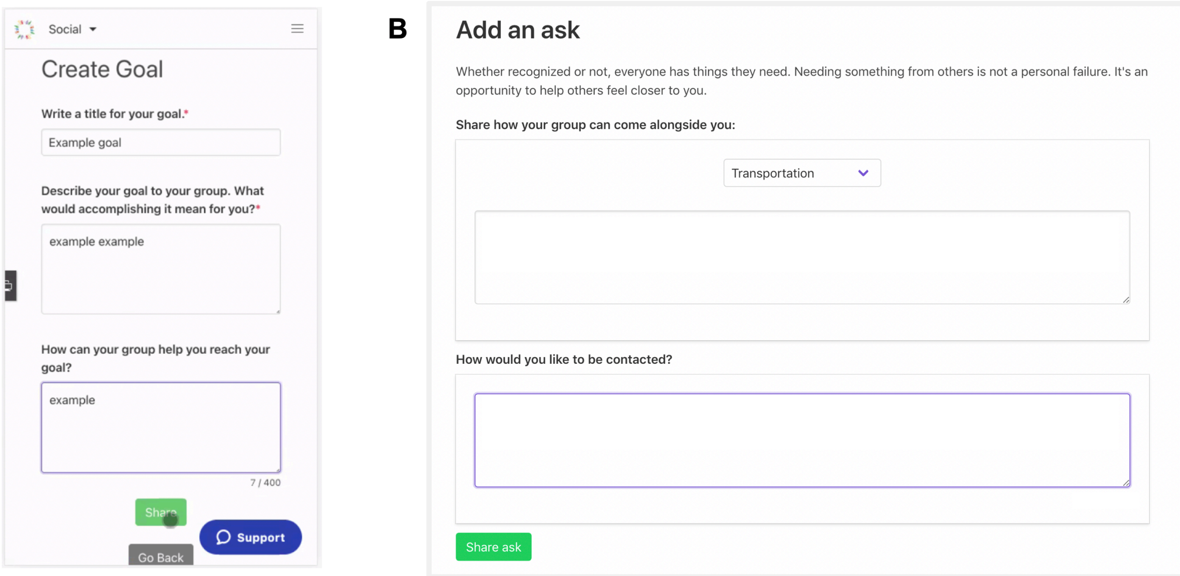
A B


Figure 2. Online platform images. A Example of goal creation and sharing prompt. **B** Example of help-requesting prompt. The platform interface may change with time based on user feedback and the organization's evolving priorities.

There are several advantages to creating peer groups from randomly-chosen participants as opposed to allowing participants to create their own peer groups from existing ties. First, this strategy may grow each participant's network, allowing them to build up social capital. Second, it is plausible that the marginal impact of encouraging people to interact with their peer groupmates will be larger if people are not already interacting with them extensively. Third, creating a peer group with new ties allows for the possibility of new group norms to develop—norms that may allow people to be more open with discussing financial issues or asking for help.

At the same time, matching peer groups on variables that are likely critical to the success of the peer group (language and location) will likely lead to stronger social connections. This could improve the effectiveness of the manipulation and decrease attrition. In addition, matching on these variables is likely to be most externally valid, as most social networks share a common language and geographic location [72].

Randomization Strategy. Figure 1B summarizes the randomization strategy. To accommodate the fact that two treatment arms (S and CS) have peer groups while the other two arms (C and NTC) do not, we will use a two-stage randomization process. In the first stage of randomization, we will randomize the enrolled households into either the “need a peer group” meta-treatment (which will ultimately become the S and CS arms) or the “do not need a peer group” meta-treatment (which will ultimately become the NTC and C arms). Here, “enrolled” households are those that have signed the consent form, completed their baseline survey, created an account on the online platform, and chosen their preferred method of payment. Participants will not be informed which meta-treatment they have been assigned to.

We will then match participants in the “need a peer group” meta-treatment into peer groups of four to eight households based on the language(s) they speak and their geographic location. In particular, we will first match households based on their preferred language, grouping them into either English- or Spanish-speaking pools. Next, within each language pool, we will use a set of prespecified rules that consider public transit maps, natural barriers (e.g., rivers), and cultural differences to generate eight distinct “geographic regions.” Finally, within each language pool and geographic region, we will match participants in such a way as to minimize the geographic distance between the home addresses of each member of the peer group.[n] In situations in which participants do not provide usable address data, we will use the center of the zip code they provide (which will be required and vetted to be a valid Boston or Cambridge zip code). This matching code will be made publicly available.

Once participants have been matched into peer groups, the peer groups will be randomized to the S or CS treatment arm. At the same time, participants who were randomized

[n] For privacy reasons, participants will be allowed to provide an address or landmark that is near their home rather than their home address.

into the “do not need a peer group” meta-treatment will be randomized to the NTC or C treatment arm. All participants will receive an email informing them of their treatment arm assignment, with an emphasis on the fact that the assignment was random.

Participants in the S and CS arms will then be invited to “Welcome Days,” events created by the partner non-profit.[o] Attendance at these Welcome Days will be voluntary but strongly encouraged. As mentioned above, the Welcome Days will include presentations and hands-on activities to help peer groups develop a system or plan for their peer group structure and monthly meetings. Each Welcome Day will include multiple peer groups. However, to minimize the possibility of contamination, peer groups in the S and CS treatment arms will be invited to separate Welcome Days.

Attrition, Non-Compliance, Contamination, Spillovers, and Power Analysis

In this section, we discuss attrition, non-compliance, contamination and spillover. We will also discuss our power analysis, which informs our recruitment targets.

Attrition. We define “attrition” as participants failing to complete two surveys in a row within the allowed time frame (14 days since receiving a survey). Repeatedly failing to complete surveys is, for study purposes, equivalent to leaving the trial. One reason that a participant might leave the trial is because they forget to fill out the surveys repeatedly, or they do not believe that survey completion is valuable for themselves or the research team. We will incorporate several economic and behavioral insights into our design to reduce the likelihood of attrition. First, we pay participants a minimum of $20 per month simply for being enrolled. Second, we pay well for each survey they complete ($40—a working wage of approximately $60/hour, far above the likely average hourly salary for participants in our trial). Third, participants will be sent three

[o] These events will be virtual at the start due to current COVID-19 social distancing guidelines, but may ultimately turn into in-person events held around the city (e.g., at community centers or libraries).

email reminders for each survey to ensure that they remember to complete the surveys. Fourth, we will communicate with households that fail to complete a survey to emphasize the importance and benefits of remaining in the trial, ask them why they failed to complete the survey, and remind them that they will be removed from the trial if they miss two surveys in a row. Finally, to incentivize completing the study, we will use an unbalanced payment schedule: participants receive a $100 bonus payment if they are still enrolled at the end of the eighteenth month, and get a chance at $1,000 if they complete all the surveys.

Nevertheless, some participants may choose not to continue with the study. If a participant informs the non-profit partner organization that they want to leave the study, or if they fail to complete two surveys in a row, we will mark them as having dropped out of the trial. To gain insights into why a person may want to leave, the non-profit organization will ask participants who initiate the leaving process about their concerns. We will also aim to conduct qualitative interviews not only with participants who are active in the trial, but also those who left and any peer groupmates they may have left behind.

We will closely monitor the missing data and attrition rates throughout the trial. Should they be higher than expected, we will analyze the information participants provided on why they failed to complete surveys and/or wanted to leave. Building on this information, we will then devise a strategy jointly with the non-profit organization and the partner government agencies to alter the retention strategy. We detail our analytical strategies for addressing missing data and attrition below.

Non-Compliance. Non-compliance means that a participant who is assigned to a specific treatment is not compliant with the treatment. We do not expect serious issues of non-compliance in our trial. This is because in some treatments, participants are—by design—almost always compliant (i.e., we minimize opportunities for them not to be). In the C and CS treatments, participants will receive the unconditional cash transfers directly into their preferred accounts on a pre-programmed schedule. This means that participants in the C and CS arms are always compliant with the *receipt* of the cash component, unless a person refuses to receive payment, which we believe to be highly unlikely.

Nevertheless, there may be varying degrees to which participants in the C and CS treatments *use* the money provided. For participants who choose to have their money deposited onto the online platform, and for participants who consent to providing us with their bank account information, we can proxy for whether a participant used their money by measuring whether they withdrew it from their accounts.

Importantly, however, we believe that there may be psychological benefits to simply knowing that one has funds available, regardless of whether or not one uses them. To better understand *beliefs* about the availability of funds, each quarter we ask participants how much money they have received from the non-profit organization over the last three months. C and CS participants who indicate that they have received less than $1,500 will be marked as "cash underestimaters" for that quarter.

In the social treatment arms, non-compliance is harder to define. Non-compliance could naïvely be perceived as someone who does not engage with their peer group. However, we emphasize that ceasing to actively participate in some of the peer group activities is not necessarily a failure to comply. The aim of the social component of the S and CS treatments is to *encourage* randomly assigned participants to engage with a peer group of peers and give them the *option* to develop other social ties through the online platform—and not to require them to do so. While households in the S and CS arms are encouraged to meet monthly, it is the decision of each member of the peer group to decide how often they participate, if at all. Moreover, the treatment is likely to also operate through channels beyond the monthly meetings (e.g., WhatsApp groups or individual meetings between peer group members). Finally, the treatment might offer a psychological benefit that does not require meetings or interaction with peer group members at all: simply knowing that the peer group exists as a source of potential advice and support may be enough for households to receive some benefits of the S and CS treatments.

Nonetheless, we will measure the extent to which participants are engaged with their peer group through several methods. First, we will have administrative data about their activity level and activity type (e.g., offering help, posting questions) on the online platform, including whether they have chosen to leave their peer group on the platform. Second, we will ask S and CS participants basic knowledge questions about their peer groups in Months 3 and 18 to measure their awareness of the treatment (e.g., their beliefs about whether they are in a peer group, how many people are in their peer group). Third, each survey we will ask them how much contact they have had with their peer group in the previous month (where "contact" includes anything like virtual or in-person meetings, phone calls, WhatsApp group chats, etc.) and when is the last time they met with their peer group. Participants who do not communicate with their peer group on the online platform and indicate having had no contact with anyone from their peer group will be marked as "non-contacters" for that quarter.

The "cash underestimaters" and "non-contacters" will not be treated differently in our main analysis, as our main specification uses an Intent-to-Treat (ITT) approach. However, in exploratory analyses, we will separately examine the outcomes of participants who were "compliant" with the treatment using a Treatment-on-the-Treated (ToT) analysis.

Contamination. Contamination refers to cases where participants who are in one treatment also receive elements of another treatment. For example, this would be the case if a participant in our NTC treatment learns from a friend (see also *Spillovers* below) that s/he is meeting with other members in regular peer group meetings and that participant then joins the regular peer group meetings. In this case, the participant who was initially assigned to the NTC treatment has received elements of the S treatment—their own assigned treatment has been contaminated. We will measure contamination from the social treatment by asking participants in the NTC and C arms if they have been meeting with any peer groups in the study.

Contamination from the cash component of the C or CS treatment arms would occur if a participant not in a cash arm received money from those in the C or CS treatments. This will be captured through our survey questions. We will also be able to measure potential contamination across the S and CS treatments by observing whether S or CS participants chose to create groups on the online platform with study participants who are in the other social arm.

In our main specification, contamination is not treated separately, as we use an ITT strategy. However, in exploratory analysis, non-social arm participants who appear to have received components of the social treatment, and non-cash arm participants who appear to have received components of the cash treatment, will be analyzed separately.

Spillovers. A spillover in our context is defined as a participant learning about the treatment status of another participant who is in their network or neighborhood and being influenced by that participant's treatment. We believe that spillovers will be fairly limited in the trial, as participants are largely encouraged to keep their treatment arm status private. The exception to this is in the S and CS arms, where sharing information, advice, and even money within a peer group is not a spillover but a design feature (and therefore not technically a spillover but part of the treatment). We believe spillover outside the peer group is relatively unlikely to occur in the S and CS arms because interactions are encouraged within their own peer group only.

Nonetheless, we will aim to understand and study the impact of potential spillovers. First, to measure spillovers, we will ask participants in Months 6 and 12 whether they personally know anybody who is part of the trial (but not in their peer group). Participants who indicate that they know at least one other person who is a part of the trial (other than people who are in their peer group) will be asked to describe, in as much detail as possible, the features of those individuals' experiences with the study (e.g., what they are being asked to do and what they are getting from the non-profit organization). The purpose of these questions is to assess in a non-leading way whether participants recognize that their acquaintances are receiving the same treatment as them or different treatment(s), and if different treatment(s), which one(s).[p] While these measures are likely imperfect, we have designed them to be conservative: these estimates are only a rough proxy of participants' *awareness* that other participants are receiving some other treatment, but not that the treatment effects have actually spilled over. In other words, our measures are a conservative upper bound of any actual spillover.

While we do not expect spillover effects to be large, we will nonetheless take them into account in our analyses. Importantly, because one of our primary research questions specifically pertains to the effect of social capital on outcomes, we can leverage the existence of these spillovers to help answer that question. In exploratory analyses, we will use the spillover data (e.g., natural variation in the share of a participant's network that is in a different treatment arm)

[p] Although asking explicitly about how many acquaintances are receiving the cash and/or social treatments could lead to more precise estimates, such questions could also themselves inadvertently create spillovers by suggesting to (previously unaware) participants that other participants may be receiving such treatments.

as additional explanatory variables. Although these analyses will not, of course, speak to the causal effects of increased treatment intensity, they will allow us to capture correlations. In addition, the Out of Sample Control group (see the *Out of Sample Control Group* section below) will provide additional data on a control group that is not only not treated, but also likely completely unaware of the study or the treatment arms within it.

Missing Data and Multiple Imputation. Missing data can be the result of a participant forgetting to fill out a survey when prompted to do so, or from attrition when a participant completely drops out of the trial. If our strategies above (see *Attrition*) are insufficient to keep a participant engaged in the trial, we will proceed as follows in our analysis. First, in our main analysis, we will not alter, or impute, missing values. This means that our regressions will not take missing values into account and only consider survey responses that have been completed. However, such a strategy could be problematic if attrition or missingness is systematic and, in particular, differential by treatment status, which can bias our estimator. Thus, second, we will conduct a prediction exercise with the goal of identifying systematic patterns in attrition and missingness (see *Differential Attrition and Prediction Analysis* below). In addition, we will apply a multiple imputation method for missing values in time series data [73]. In robustness checks, we will examine whether our results hold when missing values are imputed.

Differential Attrition and Prediction Analysis. To test the extent to which attrition, survey missingness, non-compliance, and contamination are systematic in our data, we will conduct two analyses. To start, we will measure the extent of potential differential attrition between the NTC arm and each treatment using the following specification:

$$attrit_{ip} = \beta_0 + \beta_1 C_i + \beta_2 S_i + \beta_3 CS_i + \varepsilon_{ip}$$

where *C* is equal to 1 if participant *i* in peer group *p* is in the Cash-only treatment, *S* is equal to 1 if the participant is in the Social-only treatment, and *CS* is equal to 1 if the participant is in the Cash+Social treatment (see further specification details in the *Identification and Econometrics* section below). If high levels of attrition are found, we will adjust for the potential effect of such attrition using Lee bounds in our robustness checks (see also [9]). We will repeat the same estimation with survey missingness, non-compliance, and contamination as outcome variables.

Second, we will aim to identify whether, in addition to treatment status, any observable characteristics in the baseline survey can predict whether a participant will leave the trial, be non-compliant, or receive another treatment (i.e., contamination). Since we have a large number of potential explanatory variables, we will use a machine learning approach for this prediction problem. Specifically, we will use a classification algorithm (e.g., Random Forest) to identify whether, on the basis of baseline characteristics, we can correctly predict participants' likelihood of attrition or contamination [74,75]. For continuous dependent measures such as number of surveys missed, we will use a Lasso regression [75,76]. We will show both the model fit as well as any important variables that arise from this analysis. If any systematic patterns are identified, we will discuss these and describe how they affect the interpretation of our results.

Power Analysis. Taking all of the above into account, we conducted a power analysis for a given budget, which in our case is approximately $5,300,000. To maximize statistical power across our four treatment arms given our budget constraints, we consider the differential costs of our treatments to assign sample sizes optimally [77]. Because the C and CS arms are more than 12 times as expensive as the NTC and S arms, we will assign more households to the NTC and S arms than the C and CS arms. We aim for our final sample to be 216 households in each of the cash arms (C and CS) and 534 households in each of the non-cash treatment arms (NTC and S), for a total of 1,500 households at endline. Furthermore, we assume that 10% of the cash arms and 20% of the non-cash arms will leave over the course of the trial. Therefore, we plan to recruit approximately 1,816 participants at the start of the trial to ensure our final sample is approximately 1,500.

Table 2 summarizes our power calculations using a set of conservative input values. To calculate the detectable difference given this sample size, we make the following assumptions. We conservatively assume an intraclass correlation of 0.05 and a correlation between baseline measurements and outcomes of 0.10. We further conservatively assume that all peer groups in the S and CS arms will be eight households, the maximum number allowed. To adjust for multiple hypotheses, we use a conservative Bonferroni correction, dividing a standard 0.05 significance level by four—the total number of outcomes we study, described in further detail below. The sample sizes used in these calculations reflect our predicted sample sizes at the end of the eighteenth month, after approximately 17% of the sample has left the trial. Thus, these estimates are a lower bound on our power earlier in the trial, when presumably fewer households will have left the trial. With these assumptions and a target rate of 90% power, we calculate standardized effect sizes across outcomes. Because our outcomes are in standardized effect sizes, this calculation applies to each of our key outcomes (financial, psychological, health, and family well-being).

Because of the longitudinal nature of our study, we can leverage the multiple endline measurements to increase our power (or, equivalently, to decrease our minimum detectable effect [MDE] size while holding our power level constant). Specifically, we can treat each quarter after the baseline as an endline measurement, giving us six endline measurements. We assume a correlation of 0.50 between follow-up measures. At 90% statistical power, we find that we are able to detect an MDE between 0.14 and 0.23 (Table 2) for pairwise comparisons between treatment arms. The code for the power analysis is available on our preregistration page.

To put our power calculations into context, an effect size of 0.20 is typically considered a “small” behavior change but, in field settings, typically large enough to be economically meaningful—depending on the cost of the intervention (e.g. [78–80]). Conversely, an effect larger than 0.20 would be particularly encouraging, yet not unexpected for our context. For example, a recent study of UCT found an effect of 0.26 on stress reduction [9]. Given that our statistical power is 90% and we are able to detect effect sizes as small as 0.14, we are well powered to reject even small effect sizes. This ability to speak to small effects would be particularly important if we did not find support for our hypotheses. We discuss the implications of this at the end of the results section.

Table 2. Minimum detectable effect size for pairwise comparison between treatment arms. Abbreviations for treatment arms: NTC = No Treatment Control; C = Cash-only; S = Social-only; CS = Cash+Social.

	MDE for pairwise comparisons with 6 endline measurements
NTC vs. C	**0.15**
NTC vs. S	**0.14**
NTC vs. CS	**0.18**
C vs. CS	**0.23**
S vs. CS	**0.17**
C vs. S	**0.17**

Data Preparation and Cleaning

Data Exclusions. We do not plan on excluding any observations. Peer groups that disband and where two or fewer people remain will be asked to report their status to the non-profit. The non-profit will then attempt to match the remaining households to other peer groups such that everyone is in a peer group of at least three people. We will create an indicator variable in our analysis that captures which participants were reassigned to a new peer group in that month.

Outliers and Data Transformations. To account for outliers, we will employ a 95% winsorization on all unbounded quantitative survey responses and administrative data (e.g., savings, debt accumulation, earned income). In addition, in case we find that the skew and kurtosis of our unbounded data (e.g., income or debt) exceeds recommended thresholds, we will employ commonly used data transformations, such as the natural log (e.g., see [81] for a similar analysis strategy) or the inverse hyperbolic sine transformation [82].

Unreliability of Measures. Measures which are not sufficiently reliable would impede our ability to test our hypotheses. If the reliability of any self-report measure is below 0.60, we plan to iteratively remove items that have the lowest item-total correlation until a reliability of at least 0.60 is met (see [81]).

Outcome Measurement

Description of Indices. For both brevity and reliability, we construct indices for each of our main outcomes (financial, psychological, health, and family well-being). The Appendix lists each measure in detail, and below we provide an overview of the indices.

Composite Indices Construction. We will construct composite indices using a technique described in detail in ref. [83], which we summarize here. First, all outcomes will be oriented such that higher values are "better." Next, we will demean the outcomes and divide each one by the control group standard deviation at each time point. These "transformed" outcomes can then be compared on a common scale. After standardizing the effect sizes, we will create a weighted average of the transformed outcomes for each individual household for each measure in each domain. The weight of each input is equal to the sum of the row entries in the inverted covariance matrix in each domain. This weighted average will be applied to the measures we use in our analyses.

As noted in ref. [83], with this procedure, the final outcome measure ignores missing values. As described above, we will also conduct robustness analyses where missing values are imputed, which in turn means the indices will include the imputed values at that point. Moreover, outcomes within a given domain that are highly correlated with one another receive relatively little weight within the index, while outcomes that are not highly correlated (and thus carry additional information) receive comparatively more weight. In robustness checks, we will also construct simple Z-score indices for comparison.

Components of Main Outcomes Indices. We will assess outcome measures on four main dimensions: financial well-being, psychological well-being, physical health, and family well-being. All outcomes refer to the individual respondent unless otherwise stated.[q]

Financial Well-Being

Subjective financial well-being

Perceived ability to afford household's needs

Perceived ability to afford household's wants

Ability to meet an unexpected $400 expense [46]

Savings stock (self-reported or, if available, from bank account data)

Amount of late fees charged (self-reported or, if available, from bank account data; reverse coded for the index)

[q] We may change some survey questions after the trial has begun in response to shifts in the COVID-19 public health or policy landscape. All changes will be reported.

Number of bills last month unpaid or partially paid (reverse coded for the index; [46])

Psychological Well-Being

Cantril's Ladder: extent to which one is living one's "best life"

Feelings of agency [84]

Optimism [85]

Self-esteem [86]

Positive mental health [87,88]

Anxiety, fear, or distress (reverse coded for the index; [89])

Depression (reverse coded for the index; [90])

Physical Health

Sleep quality [91]

Diet quality

Food security [92]

Exercise

Family Well-Being

Perceived extent to which family's financial and time resources are adequate [93,94]

Children's general happiness [95]

Children's nutrition

Number of school days children missed (reverse coded for the index)

Children's grades

Self-assessed quality as a parent [96]

Relationship with children

Relationship with partner [97]

In addition to these preregistered outcomes, we will also gather data on a number of more exploratory outcomes. They include number of hours worked, debt, amount of welfare benefits received, consumption amount and type, intrahousehold bargaining, beliefs about intergenerational economic mobility in the US [98], working memory as measured through the digit span task [99], and cognitive capacity as measured through Raven's Progressive Matrices [100]. These measures will be analyzed separately.

Data and Statistical Framework

Generalizability. We will examine how representative our experimental sample is of the general population of low-income Americans. To this end, we will compare the characteristics of our enrolled sample to the Out of Sample Control group (see the *Out of Sample Control Group* section below), as well as the general population. We will discuss the potential implications and economic significance of any observed differences.

Descriptive and Summary Statistics. We will report descriptive statistics (means, medians, and standard deviations) for each composite and individual measure of the indices. We will also report summary statistics by treatment arm.

Balance at Baseline. To ensure that our randomization yielded balance across treatment arms, we will test whether the composite indices in the baseline survey, participants' time-invariant covariates (e.g., gender) and financial health at sign-up (e.g., household earned income as a percent of the federal poverty line) are equal across the treatment arms. We will use the same statistical specification and inference approaches as described for our main analysis (see the *Identification and Econometrics* section below), except that we will not condition on baseline outcomes on the right-hand side of the equation. If any of these checks reveal imbalance for one or more variables, we will discuss the implications and economic significance of this imbalance, as well as include those variables in robustness checks to our main specifications.

Multiple Hypotheses Testing Corrections. To address multiple hypotheses testing concerns, we will employ a Benjamini-Hochberg approach [101]. This approach uses a step-down False Discovery Rate (FDR) method of controlling Type I error rates, which, relative to Familywise Error Rate (FWER) approaches, yields higher statistical power [83]. The Benjamini-Hochberg procedure ranks the naïve *p*-values of related comparisons (i.e., the outcomes in our setting) and divides the rank of each *p*-value by the number of tests (i.e., four outcomes). We will use a standard significance threshold of 5% and an FDR threshold of 10% and report the adjusted *p*-values in our regressions (for details, see [83]; for recent empirical examples, see [102,103]).

Basis for Statistical Inference and Economic Significance. Our statistical inferences will be based on *p*-values and standardized effect sizes. We will use a frequentist approach to test for statistical significance in our data, adjusting for multiple hypotheses testing as described above. Alongside effect sizes and *p*-values, we will report corresponding 95% confidence intervals. In addition to statistical significance, we will also evaluate effect sizes on the basis of their economic significance and their relative size to each other (including in light of the differential costs across treatments). Specifically, since our indices are based on standardized effect sizes, we are able to "benchmark" each effect against another [104], speak to their relative cost effectiveness, and compare our effect sizes to prior work with similar outcomes.

Identification and Econometrics. Our identification strategy is based on random assignment to one of four treatment arms (NTC, C, S, and CS). We will use an Intent-to-Treat (ITT) approach where we maintain households' initial assignment for the main analysis. We will use analysis of covariance (ANCOVA) to estimate the treatment effects. Following ref. [105], we will condition on the baseline measure of the composite index of interest to improve statistical power. Our main specification is as follows:

$$y_{ip,t>0} = \beta_0 + \beta_1 C_i + \beta_2 S_i + \beta_3 CS_i + \delta y_{ip,t=0} + t + \varepsilon_{ip} \quad (1)$$

where y is the outcome of interest (e.g., financial, psychological, health, or family well-being index value) for individual i in peer group p (if applicable) at time t. C is an indicator variable that equals 1 if the participant is in the Cash-only arm, S is an indicator variable that equals 1 if the participant is in the Social-only arm, CS is an indicator variable that equals 1 if the participant is in the Cash+Social arm, and $y_{ip,t=0}$ is the baseline measure of the composite index for participant i. Finally, t is a linear time-trend term, taking values corresponding to each quarter (0, 3, 6, 9, 12, 15, and 18). The omitted arm is the NTC arm. We cluster robust standard errors $\boldsymbol{\varepsilon}$ at the peer group p level (if a participant is in the S or CS arm), reflecting the two-stage randomization described above.

Model Specifications for Primary Outcomes. Following Eq. (1), we will use the following three specifications in our main analysis. In the first specification, we will pool all observations across all time points t, while in the second and third specifications, we will pool across the first nine months and second nine months of the trial, respectively.

The intuition is as follows: we are interested in the overall treatment effects across time. However, we recognize that some treatment effects might arise sooner, while others might take time to mature. We also recognize that some effects may decay over time, while others may accelerate. Finally, some effects may be distorted by the finite duration of the study, e.g., participants in the cash arms may feel calm and optimistic about their financial futures early in the trial, but renewed stress when they realize that the benefits will soon be ending. These possibilities are captured by the latter two specifications. We do not have *a priori* predictions on these temporal effects, and describe further exploratory analyses for potential temporal effects below.

Our measure of "success" on each dimension will be derived from the first main specification. To test our hypotheses (see below), we will look at the statistical significance of each of the three treatment indicators as specified in Eq. (1).

Results

In this section, we present our confirmatory hypotheses and what we can learn from the trial should we find mixed evidence for these hypotheses or only null effects. We also provide an overview of our planned exploratory analyses.

Hypotheses

For the main outcomes, we make the following confirmatory hypotheses. We use the term "better" as shorthand to refer to higher values on the scales.

Hypothesis 1. Compared to those in the NTC arm, participants in the C arm will have better financial, psychological, health, and family well-being outcomes.

Hypothesis 2. Compared to those in the NTC arm, participants in the S arm will have better financial, psychological, health, and family well-being outcomes.

Hypothesis 3. Compared to those in the C arm, participants in the CS arm will have better financial, psychological, health, and family well-being outcomes.

Hypothesis 4. Compared to those in the S arm, participants in the CS arm will have better financial, psychological, health, and family well-being outcomes.

We will test these hypotheses with an ANCOVA as described in Eq. (1) by examining the statistical significance of the treatment indicator coefficients and testing for their equivalence.

Interpretation of Potential Mixed or Null Results

Here we explore several scenarios in which our key hypotheses are not supported by the data. While we are well powered to find effect sizes that are smaller than those found in other unconditional cash transfer studies (e.g., consider that ref. [9] found a 0.26 standard deviation reduction in stress following UCT), it is important to consider what we can learn from this trial if we find mixed or null results. Given our ability to detect small effects, we believe that both significant results and null results would be of economic and policy relevance. For example, if a result is not statistically significant, the ability to offer a small confidence interval around the estimated treatments effects would be valuable information for policymakers. In addition, a post-study examination of the size of the standardized coefficients (compared to each other and other studies we have reviewed above) will offer practical and theoretically valuable insights, including the treatments' cost effectiveness.

We re-examine our hypotheses in light of potential mixed or null results. For example, we have proposed that financial and social capital will have additive or potentially even multiplicative effects, i.e., that the combination of financial and social capital will lead to an improvement that is greater than receiving either component treatment alone. If this is the case, we would find support for Hypotheses 3 and 4 as discussed above. Specifically, should we find that $0 < \boldsymbol{\beta}_1 < \boldsymbol{\beta}_3$, it would show (1) that unconditional cash transfers improve the well-being of low-income households and (2) that, in addition, encouraging people to take advantage of their existing and new social networks can further improve those outcomes at almost no cost. Similarly, should we find that $0 < \boldsymbol{\beta}_2 < \boldsymbol{\beta}_3$, it would suggest that social capital—while helpful on its own—may not be enough to address the hardships that people in poverty face; some financial assistance can further improve outcomes.

If, on the other hand, $\boldsymbol{\beta}_1$, $\boldsymbol{\beta}_2$, and $\boldsymbol{\beta}_3$ are all larger than 0 but not significantly different from each other, it would suggest that our financial and social interventions, as well as combining the two, are in a sense substitutes. This would be an important finding in its own right; since the social intervention is much cheaper to implement than the cash intervention, it could have major policy implications.

Another possibility is that we find varied effects of our interventions across our outcomes. For instance, the CS arm might have a beneficial effect for some outcomes but not others. If this were the case, our results would still be insightful: they would allow us to better understand the nuanced ways in which financial and social capital influence low-income households. That is, the results can shed light on precisely which outcomes the provision of financial and social capital can change, and—just as crucially—which outcomes such capital is less likely to change.

We also need to consider the possibility that our data do not show evidence for significant improvements in any treatment arm. For example, if the C treatment does not lead to better outcomes (i.e., $\boldsymbol{\beta}_1$ is not statistically different from 0), the first question might be to ask whether participants were aware of the cash we provided them. For this reason, every quarter, participants will be asked to report how much cash they have received from the non-profit organization.

Assuming that participants were aware of the cash transfers, a second question to ask would be whether the cash we will have delivered was large enough to make a difference in people's lives. One way to answer this question is to review other studies that examined the effect of additional income for US households. The range of income increases studied is wide, varying from about $4,000 [23], to $2,000 [106], and anywhere from $331 to $2,072 per year [67].[r] In all three of these cases, the authors found positive effects on outcomes that overlap with our own (e.g., health, children's educational attainment). In our study, households in the C and CS arms receive $6,000 annually. While in the case of a null result of the cash intervention we cannot rule out the possibility that our payments were insufficient, we believe the size of our cash intervention is of a magnitude for which we can expect positive effects.

As mentioned above, some of our study population will likely receive state-level financial support from the partner government agency through a program called Transitional Aid to Families with Dependent Children (TAFDC). TAFDC recipients will not be treated differently from non-TAFDC recipients in our primary analyses—recipient status will only be recorded and controlled for in robustness checks. However, we can take advantage of natural variation in welfare receipts to better understand the effects of providing larger amounts of cash on household outcomes. For example, some households receiving TAFDC may reach the 24-month limit of their state benefits during our trial period, while others may have just started receiving TAFDC before they enter our study. Leveraging this exogenous variation in when households reach their limit can allow us to better understand the relationship between the size of cash grants and outcomes. In particular, such analyses could provide hints about whether the cash transfers we provide in the context of the trial are sufficiently large to generate measurable effects, and if not, how large they might need to be to generate such effects.

Finally, we will collect a range of data that could help us better understand where participants' money goes and why it may or may not have a detectable effect on outcomes. For

[r] Another relevant comparison may be that of the average Supplemental Nutrition Assistance Program (SNAP) benefits in Massachusetts. According to the agency that administers these benefits, average SNAP benefits before the pandemic were around $220 per month; our cash intervention is therefore about double of this widely-used program.

instance, we will observe and ask participants to report what they use their money on, including whether they are paying off debts, investing money in education or a business, or sharing it with friends or family. Should we find null or even opposing effects, these data may provide clues on why the money may not have helped as expected.

If households in the S arm do not experience better outcomes (that is, $\boldsymbol{\beta}_2$ is not statistically different from 0), we can conduct further analyses to examine both endogenous and (quasi-)exogenous variation in peer groups. First, all participants in the S and CS arms will be asked a few questions in Months 3 and 18 to determine whether they have understood the basic elements of their treatment. For instance, they will be asked how many people are in their peer groups and what their initials are. An inability to answer such questions would suggest that participants do not have a basic understanding of and/or engagement with the study parameters, and thus that the problem may have been in communication or engagement rather than in the effectiveness of social capital *per se.*

Second, while not causal, we plan to explore to what extent participants who are more actively participating in peer group meetings and the social platform are potentially benefitting more from the social capital opportunities offered to them. We include several questions in our surveys to identify to what extent participants communicate with their peer groupmates, what their peer group experience has been like, and how willing they are to ask their peer groupmates for help. Furthermore, peer groups may choose to meet at different intervals and not all participants might attend all meetings. Using these variations, we can learn about the effect of endogenous “treatment intensity” on outcomes.

Other insights may be gained by studying variation that is more plausibly exogenous. For instance, while during the COVID-19 pandemic peer group meetings will presumably primarily be virtual, if and when it is safe to do so, the meetings may be in person. In such a case, the distance between households in a (randomly assigned) peer group or their access to public transit may provide us with an instrument for causal estimation of treatment intensity on outcomes. Assuming that distance or access to public transit is associated with meeting frequency and attendance, being randomly assigned to a peer group in which one lives further versus closer to the other peer groupmates may thus provide insights into how meeting with one's peer group affects outcomes.

Finally, our comprehensive data allow us to employ different exploratory analyses and a mixed-method approach to identify why we may not have seen the hypothesized effects. For example, qualitative interviews both with participants who left the trial and those who did not, as well as open response comments in the surveys, will all contribute to our understanding of why our treatments may not have shifted outcomes.

Additional Analyses

Out of Sample Control Group

While using surveys to measure outcomes has many advantages, it also generates several concerns. First, although we hypothesize that the financial and social capital interventions will improve outcomes over the counterfactual state in which no interventions are given, it is also possible that the *absence* of these interventions, should it be made salient to the NTC arm, actually worsens the NTC arm's outcomes (see the *Spillovers* section above). Second, it is possible that the survey itself serves as a kind of treatment that can shift outcomes, whether by providing payments for taking the survey, generating demand effects, or making salient behaviors or outcomes the households themselves want to improve. Finally, we cannot rule out the possibility of differential survey completion across treatment arms or differential demand effects.

For these reasons, in addition to the four treatment arms discussed above (NTC, C, S, and CS), we also add a second control group to our trial: recipients of the state government agency's welfare benefits who were eligible for the trial and did not enroll. Because the households in this group will not be paid for their participation, they will not be aware that they are in a study, and we will only have access to objective outcome measures (e.g., verified income, employment, applications to welfare programs, amount of welfare benefits received, and speed with which SNAP benefits are used), the households in this group can serve as an additional "Out of Sample Control" (OSC) group that helps to address the concerns raised above.

To analyze the data from this group, we will first limit the other four treatment arms in the main sample (the NTC, C, S, and CS arms) to only the sample of people who receive benefits from the state government agency with which we partner. Next, we will conduct the same analyses as those described above, conducting pairwise comparisons between the OSC group and each of the treatment arms in the main sample for the variables available for all groups. Because the households in this OSC group will likely differ from the households in the main sample on various dimensions, we will also aim to match them on detailed demographic and financial characteristics, including zip code, household composition, age, gender, race, and income, to help ensure the validity of these additional comparisons.

Exploratory Analyses

In addition to the confirmatory hypothesis analyses described above, we will also conduct the following exploratory analyses, grouped into four overarching questions.

Heterogeneity in Treatment Effects

First, we will seek to understand for whom the treatments are most effective. We will have a rich set of covariates from two data sources. First, participants will be asked to provide a wide range of information at sign-up, including their demographics and comprehensive financial information. Second, we will have administrative data from the state government agency with which we partner. For participants who received welfare benefits from this agency in the five years before the start of the trial, we will observe their history of being in the partner government agency's system since January 2016 (e.g., cumulative amount of time receiving benefits); detailed financial data (e.g., place of employment, whether the participant is meeting the program's work requirements); and receipt of federal and state benefits (the amount of benefits received, the reason that a household stopped receiving benefits). These data will enable us to explore, for example, whether the treatment effects differ by poverty level at baseline, single versus dual-parent households, the age of the participant's dependents, and the extent to which a participant felt like she had support from her network at baseline.

Temporal Patterns

Second, we will test for the temporal pattern of our effects—i.e., when we begin to observe effects, when they taper off, and when they disappear. For instance, in these exploratory analyses we will test whether (1) the effects of unconditional cash transfers are strongest in the first few months and subsequently taper off (e.g., see ref. [9]); (2) the effects of the social capital intervention are relatively linear over time as participants continuously build stronger relationships with their peer group members; and (3) the additive or possibly multiplicative effects of the CS arm reflect both a short-term boost similar to the C arm and a continuous improvement similar to the S arm.

Longer-Term Follow-Up

Should we see promising effects of one or more treatments at Month 18, we will administer one or more follow-up surveys after the main trial has concluded to capture any longer-term effects.

Characteristics of Successful Social Capital Treatments

Fourth, we will explore what peer group characteristics are associated with the greatest shifts in outcomes. For instance, we plan to explore factors like peer group size and how well the members of the peer group knew each other before beginning the trial. The random rather than endogenous structure of the peer groups will enable us to make causal claims about the effects of these characteristics.

Data and Materials Availability

A de-identified and masked dataset will be deposited on the Harvard Dataverse. We will take two steps to ensure participant confidentiality: (a) only the data to reproduce our main analysis will be posted; and (b) we will remove any identifiable information and create synthetic datasets in case remaining information is identifiable. The power analysis code and output, the peer group matching code, the statistical analysis code, and the survey instrument used in the current research will also be posted on the Harvard Dataverse.

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