

# Design for “The causal effects of sustained unconditional cash transfers: Experimental evidence from two U.S. states” – Employment outcomes

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PRELIMINARY

## Abstract

The regular provision of unconditional cash transfers to individuals is a tactic to fight poverty that has attracted significant interest from researchers and policymakers. Despite this interest, many fundamental questions about the effects of receiving sustained unconditional cash transfers remain. Open Research Lab, a nonprofit research organization, aims to help address this absence of data by conducting the U.S.’s first large-scale randomized trial of a guaranteed income. This document describes the design and analysis plan for the study. In the experiment, 1,000 individuals will receive \$1,000 per month for 3 years. A control group of 2,000 individuals who receive \$50 per month will serve as the comparison group. The study offers an opportunity to inform both the debate over unconditional cash assistance and other questions about the effects of income that typically elude causal identification. This document focuses on the design of the study and the employment outcomes.

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# 1 Introduction

Since the late 1960s, income inequality in the United States has risen dramatically and the share of income going to the bottom half of the income distribution has fallen by over a third (Piketty, Saez and Zucman 2019). Intergenerational mobility has fallen, wage growth has stagnated for all but the most skilled, and the official poverty rate remains essentially unchanged despite decades of robust economic growth (Chetty and Hendren 2018*a;b*; Congressional Research Service 2019; U.S. Department of Health and Human Services 2016). Individuals and communities are struggling as opportunities are increasingly concentrated in urban areas and among the highly skilled. These trends have increased political and social divisions (e.g., Dorn et al. 2016), and the ability of existing social programs to stem them is limited.

Research shows that the current social safety net leaves many Americans cycling in and out of poverty and/or categorically ineligible for aid (Shaefer and Edin 2013; Danziger 2010; Ben-Shalom, Moffitt and Scholz 2012). The patchwork of programs is complex, costly to administer, and difficult to navigate. Take-up rates are often low, particularly among those most in need (Bhargava and Manoli 2015; Finkelstein and Notowidigdo 2019). Due to the high marginal tax rates and eligibility “cliffs” introduced at moderate income levels, families who do find work often face a difficult trade-off between earnings and the benefits they rely on for survival.

In response to these challenges, policymakers at state and local levels around the country have

become increasingly interested in exploring unconditional cash transfers as a solution. Research points to negative economic, social, and psychological feedback loops that keep individuals without a steady income “trapped” in poverty. Sustained unconditional cash transfers seek to break these feedback loops. Interest in unconditional cash assistance has recently skyrocketed, but the debate often relies on conjecture, stereotypes, and studies that are out-of-date, have important methodological shortcomings, or were conducted in very different contexts. This lack of data and experience impedes rigorous policy analyses and data-driven political debate.

To help guide academic, policy, and political debates, we plan to conduct an experiment that will provide new evidence about the effects of sustained unconditional cash transfers in the U.S. We are collaborating with two non-profit organizations that will implement a cash assistance program. Our partners will recruit approximately 3,000 individuals across two U.S. states and randomly assign 1,000 in total to receive \$1,000 per month for 3 years. We will conduct extensive quantitative measurement of outcomes related to individuals’ economic, social, and physiological self-sufficiency and well-being, as well as gather data on how individuals use their time and money and how their receipt of monthly cash transfers impacts their children and those in their households. We are partnering with state and local government agencies and private entities to measure many outcomes with administrative data. A single study cannot answer all questions about the effects of a guaranteed income, but we view this experiment as the strong foundation for a broader research agenda moving forward.

The experiment also offers the opportunity to speak to policy debates about unconditional cash assistance programs. Most directly, the study will provide evidence that will inform debates about the design of public benefits, including whether benefits should be provided as cash or in-kind, whether they should be provided monthly or annually, and whether transfer programs should be extended to groups that they do not traditionally target (such as young adults without children). More broadly, the study will allow us to better understand the relationship between income, work, and well-being generally, and it can provide new evidence on the mechanisms underlying rich-

poor gaps in policy-relevant outcomes such as education, health, and time use. For example, unearned income may relax liquidity constraints and facilitate investments in health, human capital, or geographic mobility that may provide long-run returns to households. Unearned income may also change individual bargaining power with employers, landlords, family members, romantic partners, and others. Additionally, unearned income may reduce the cognitive burdens that may be created by scarce resources (Mani et al. (2013)), causing individuals to make different decisions. We discuss a broad array of additional channels through which unearned income may influence outcomes in subsequent sections.

## **2 Existing Research**

Much of the existing literature on unconditional cash transfers in developed countries focuses on estimating effects on labor supply. Traditional economic theory predicts that unconditional cash transfers should cause individuals to work less (e.g., Becker 1965), while also consuming more of most goods. By providing nonwage income, cash transfers make household incomes less dependent on labor market earnings; this “income effect” allows households to consume more leisure. Based on this insight, much of the literature on unconditional cash transfers and welfare programs more broadly has focused on quantifying and understanding the determinants of income effects (Chan and Moffitt 2018).

Less work has been done measuring how unconditional cash transfers influence household consumption, which is the other impact of unconditional cash transfers predicted by traditional economic theory. Moreover, richer models suggest that unconditional transfers could have more nuanced effects than those predicted by traditional theory due to liquidity constraints, behavioral mechanisms, social interactions and spillovers, and other factors. More recent research has started to provide evidence on these broader effects of unconditional cash transfers.

In this section, we summarize this literature. Later, we go into more detail and characterize the

contribution of this project relative to the existing literature for particular topics and outcomes.

## **2.1 Early experiments on unconditional cash transfers**

To examine the effects of a negative income tax (NIT) on the labor supply of recipients, the U.S. government conducted four randomized experiments between 1968 and 1980, while the Canadian government sponsored one. A number of studies have aggregated the findings on reduced labor supply among participants across the four U.S. experiments, and these estimates range between a 5% and 7.9% reduction in the number of hours worked annually per individual for men; a 17% to 21.1% reduction for married women with children; and a 7% to 13.2% reduction for single women with children (Burtless 1986; Keeley 1981; Robins 1985).

The goal of the experiments was to examine the effect of a guaranteed income on labor supply, but supplemental analyses revealed positive effects on birth weight, homeownership, health, children's academic achievement, the number of adults pursuing continuing education, and other indicators of well-being (see, e.g., Hanushek et al. 1986; Widerquist et al. 2005; Murnane, Maynard and Ohls 1981; Weiss, Hall and Dong 1980; Rea 1977; Kehrer and Wolin 1979; Keeley 1980*b*; Baumol 1974; Maynard 1977; Elesh and Lefcowitz 1977; Maynard and Murnane 1979; Kaluzny 1979; O'Connor and Madden 1979). Similarly, a reexamination of Canada's guaranteed annual income experiment in the 1970s using health administration data shows a significant decrease in hospitalizations—particularly due to accident, injury, and mental health concerns—and an overall reduction in health service utilization among guaranteed income recipients relative to controls (Forget 2011; 2013). These overall improvements in health may lead to significant savings in health system expenditures.

Despite their path-breaking design, these experiments were plagued by nonrandom selection, errors in randomization protocols, differential attrition, nonparticipation, and systematic income misreporting, calling their results into question (Hausman and Wise 1979; Greenberg and Halsey 1983). Even without these empirical issues, the experiments were begun a half-century ago in

a different economic and political context, so the results may not generalize to the present day. Moreover, the 1970s studies also did not track a number of outcomes that more recent research suggests may play key mediating roles in the effects of unconditional cash transfers. The proposed study will employ research tools unavailable during the NIT experiments to generate a more holistic picture of the effects of the supplemental income on individuals. Tracking expenditures and financial data and leveraging a mobile application and web-based surveys to gather data on time use enable us to investigate how the cash transfers are spent and whether individuals are able to make investments that promote long-term economic self-sufficiency and build savings to help weather shocks and reduce vulnerability.

## **2.2 Evidence from the Earned Income Tax Credit (EITC)**

The expansion of the Earned Income Tax Credit (EITC) in the early 1990s provided another opportunity to examine the effects of exogenous increases in income. Because it is linked to the amount earned, the EITC also affects beneficiaries' incentives to be employed and the number of hours worked, creating a substitution effect in addition to the income effect discussed above. Empirical research has suggested that the EITC increased labor force participation but had negligible impacts on hours worked (Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Nichols and Rothstein 2016). Eissa and Hoynes (2004) show that while there is a positive increase in the labor supply of married men, the increase is more than offset by the reduction in labor force participation by married women, leading to an overall decrease in the total labor supply of married couples. There is ongoing debate about these estimates, however, as more recent analyses suggest that the observed effects on the extensive margin may be confounded by the simultaneous effects of welfare reform and a strong economy (Kleven 2018; 2020).

Additional research has investigated the effects of the EITC beyond measures of labor supply. By transferring money to lower-income households, the EITC substantially reduces the fraction of households in poverty. These gains are concentrated among families near the poverty level, how-

ever, and the EITC has little impact on those who are very poor (Meyer 2010). One analysis of maternal health before and after the expansion documented improvements in self-reported health and mental health as well as reductions in the counts of risky biomarkers for cardiovascular diseases, metabolic disorders, and inflammation (Evans and Garthwaite 2014). Another EITC study found reductions in low infant birth weight that may be at least partially attributable to notable decreases in smoking during pregnancy and increases in prenatal care. More generally, the authors highlight that there are positive externalities to safety net programs that may lead policymakers to underestimate the benefits (Hoynes, Miller and Simon 2015). Other welfare reforms, such as Connecticut's Jobs First program, bundled multiple reforms together, making it difficult to determine the effects of individual components (Kline and Tartari 2016).

### **2.3 Natural Experiments**

Unlike unconditional cash transfers, programs like the EITC affect beneficiaries' incentives to be employed and the number of hours worked because the amount of the benefit is linked to the amount of earned income. To address this limitation, several studies have examined the labor supply of lottery winners. Lottery studies generally find that the income effects of these transfers are modest. Using earnings data from the tax records of consenting Massachusetts lottery players, Imbens, Rubin and Sacerdote (2001) estimate that individuals with winnings up to \$100,000 reduce their earnings from labor by about 11 percent of the exogenous increase in income provided by their prize. The effect is larger for individuals between 55 and 65, and the marginal propensity to earn actually increases for those with the lowest pre-lottery earnings, although the effect is not statistically significant.

In a study of Swedish lottery winners, Cesarini et al. (2016) also find negative effects on labor supply, though much smaller in magnitude than earlier studies. The authors report that pretax earnings decrease by approximately 1.1 percent of the payout amount per year, mainly due to a reduction in wages from working fewer hours. It is also important to note that, for lottery winners



with a large lump sum or large monthly payments, negative effects on labor supply could also be attributed to higher marginal tax rates on wages. Furthermore, the lottery studies generally either had small samples (Imbens, Rubin and Sacerdote 2001) or took place in policy contexts very different from the U.S. (Cesarini et al. 2016).

Other recent quasi-experimental evidence of responses to exogenous increases in income comes from examinations of the Alaska Permanent Fund and casino disbursements to Native American families in the U.S. The Alaska Permanent Fund provides an annual unconditional cash transfer to every resident of the state. In 2019, this transfer amounted to \$1,606. Feinberg and Kuehn's (2018) analysis using data from the American Community Survey shows a negative effect of dividend receipt on hours worked. In contrast, Jones and Marinescu (2018) employ synthetic controls using data from the Current Population Survey and find no effect on the extensive margin and a small positive effect on the intensive margin. Available data was insufficient to determine if the latter is a result of people shifting from full to part time work or more people entering the labor force part time. A study of the effects of casino disbursements to Native American families found that a \$4,000 annual increase in income per adult had no effect on parental labor force participation (Akee et al. 2010).

In addition to the effects on labor supply, some of the recent quasi-experimental papers have examined broader outcomes. Research on casino disbursements to Native American families finds that an average increase in annual household income of \$1,750 is associated with statistically significant reductions in obesity, hypertension, and diabetes (Wolfe et al. 2012). Casino windfall cash disbursements have also been linked to higher achievement and educational attainment, reduced incidence of risk behaviors in adolescence, improvements in children's mental health, and better parent-child relationships (Akee et al. 2010; 2018; Costello et al. 2003). The Swedish lottery study found that winners consumed fewer mental health medications after winning, particularly those targeting anxiety (Cesarini et al. 2016). Though they did not report statistically significant changes in health service utilization and other indicators of health, the generalizability of the results to the

U.S. context is questionable given the presence of universal health coverage and a generous social safety net.

## **2.4 Unconditional Cash Transfers in Developing Countries**

There is also an important literature on cash transfers in a developing country context. Most of this work focuses on conditional cash transfers and children's outcomes (reviewed, for example, in Fiszbein et al. 2009). However, some studies leverage unconditional cash transfers and consider employment outcomes. Banerjee et al. (2017) review seven government-run cash transfer programs, plus Haushofer and Shapiro's evaluation of a Give Directly program in Kenya (2016), and find no systematic effect on labor supply on either the intensive or extensive margin.

One of the largest and most widely available of these recent cash transfer programs was the 2011 policy enacted in Iran that distributes the equivalent of 28% of the median per capita household income to over 70 million individuals. Despite the size of these transfers, no impacts were found on labor force participation (Salehi-Isfahani and Mostafavi-Dehzoeei 2018). Individuals under thirty worked slightly less, though the effect was not statistically significant, and there were very small positive effects on labor supply for some groups (e.g., women and men in industrial and service sectors). These results may not generalize to the U.S., given the significant contextual differences.<sup>1</sup>

Other studies have focused on the impacts of cash transfers targeted at business owners or workers in particular industries (de Mel, McKenzie and Woodruff 2008; Blattman, Fiala and Martinez 2014; Fafchamps and Quinn 2017; McKenzie 2015). Schady and Rosero's (2007) analysis of data from an Ecuadorian unconditional cash transfer program reveals no impact on the labor supply of recipients. In a study of three-generation households in South Africa, Bertrand, Mullainathan, and Miller 2003 find a sharp decline in both the extensive and intensive margin in working-age individuals' labor supply when an older individual in the household receives a pension.

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<sup>1</sup>There is also a large literature on conditional cash transfers in developing countries we do not review here.

## 2.5 Recent Experiments

More recently, there have been a growing number of conditional and unconditional cash transfer pilots in high-income countries. In the U.S., there have been two recent experiments with conditional cash transfers (CCTs) in New York City and Memphis, Tennessee, but results were mixed. The transfers reduced poverty and led to modest improvements in other areas that varied across sites, but researchers did not observe expected gains in academic achievement, employment, and health (Miller et al. 2016; Riccio and Miller 2016). However, a disproportionate amount of the cash rewards went to more advantaged families; in households that earned more rewards, parents had higher education levels and were more likely to be employed and married. There are a number of possible explanations for the lack of impact, including challenges with implementation, the complexity of the incentives, the process of documenting participation, and the small amount of money relative to the cost of living.

Finland recently piloted a basic income scheme targeted to those experiencing long-term unemployment. Two thousand unemployed individuals were randomly selected to receive 560 euros per month unconditionally for two years in lieu of traditional unemployment benefits. Final results are due in 2020, but no significant impacts were found on labor market participation in preliminary analyses (Kangas et al. 2019). It is important to note, however, that the control group was asymmetrically affected by changes to the unemployment system implemented in the middle of the experiment that require unemployment benefit recipients to prove they are looking for a job in order to continue receiving financial assistance. Though survey response rates were low, survey data indicated that basic income recipients experienced less stress, fewer symptoms of depression, and better cognitive functioning than the control group. Positive effects were also found on financial well-being, trust, and confidence in their future possibilities (Kangas et al. 2020).

## **3 Sample Definition and Sampling Procedures**

### **3.1 Population**

#### **3.1.1 Eligibility Criteria**

We define the population of interest as all individuals with Social Security Numbers between the ages of 21 and 40, inclusive, whose self-reported total household income in the calendar year prior to enrollment did not exceed 300% of the federal poverty level (FPL). In addition, we will exclude individuals that receive Supplemental Security Income (SSI) or Social Security Disability Income (SSDI), live in public housing or have a Section 8 voucher (also called Housing Choice Voucher) or other housing subsidy, and live in households in which another member receives SSI. Receiving an income supplement could jeopardize individuals' eligibility for housing assistance and SSI, and getting back on these benefits is very difficult and may take years. Losing this assistance could cause permanent harm, so these individuals will be excluded from the study.

#### **3.1.2 Geography**

The study will be conducted in regions in two states. Within each state, we chose a mixture of urban counties with large city centers, urban counties with medium-sized city centers, suburban counties, and rural counties.<sup>2</sup> We selected 1-5 counties of each type in each state that are demographically representative of counties of that type in the region. Nationally, roughly 19% of households that meet the eligibility criteria for the cash assistance program live in rural areas, 35% live in suburban

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<sup>2</sup>Counties are divided into rural, suburban, small urban, medium urban, and large urban based on the share of households living in rural census tracts, the population density, whether the county is the largest in its metropolitan or micropolitan area, and population. Rural counties are those that have at least 50% of the population living in rural census tracts or population densities of less than 100 per square mile. Suburban counties are those that are not rural counties, but are not the largest city in their metropolitan or micropolitan area and have populations of less than two million. Small urban counties are those non-rural counties that are the largest in their micropolitan area but have urban cores of smaller than 40,000 people. Medium urban counties are those that are the largest in their metropolitan area, but have population densities of less than 10Expenses00 per square mile and populations of less than one million. Large urban counties are those that are the largest in their metropolitan area and have populations of at least one million or densities of greater than 1000 per square mile.

areas, less than 1% live in small urban areas, 17% live in medium-sized urban areas, and 28% live in large urban areas. Small urban counties make up a small share of the overall eligible population (less than 1%), so we excluded them from the sample. We aimed to recruit a sample that roughly matched these population shares, but we oversampled large urban areas to reduce recruitment and survey costs. This approach resulted in a sample of program participants composed of 13% individuals living in rural counties, 18% living in suburban counties, 16% living in medium urban counties, and 53% living in large urban counties.

### **3.1.3 Demographic Characteristics**

In addition to the geographically stratified sampling described above, we used stratified random sampling to ensure that low-income individuals are over-represented in the sample of program participants and the share of males and females is approximately proportionate to their shares of the eligible population (which is roughly 62% female). Table 1 reports basic summary statistics of both eligible mailer respondents and enrolled program participants and compares both groups to the population mean characteristics computed using the American Community Survey for eligible households living in study counties. We report estimates of the eligible population both unweighted and reweighted to reflect the FPL group and county type stratification variables that were used.

On most dimensions, the characteristics of the sample closely match the eligible population in study counties. Our sample is slightly poorer, less likely to be Hispanic, and more likely to be female than eligible households as a whole. The biggest differences between our sample and the full eligible population are that our sample is more likely to report having a college degree and to be a renter than the eligible population.

**Table 1: Study Sample Characteristics Compared to Eligible Population**

	Eligible Population Comparison (ACS)				Study Sample		
	Full US Population		Study Counties		Eligible Mailer Respondents		Enrolled Active Survey Group
	Unweighted	Reweight to Match Enrolled Sample FPL Distribution	Reweight to Match Enrolled Sample FPL and County	Reweight to Match Enrolled Sample FPL and County Type	Unweighted	Reweight to Match Enrolled Sample FPL and County Type Distribution	Unweighted
(1)	(2)	(3)	(3)	(4)	(5)	(6)	
<b>Panel A. Key active group stratification variables</b>							
Income < 100% of FPL	0.25	0.37	0.37	0.37	0.38	0.37	0.37
Income 100-200% of FPL	0.36	0.39	0.39	0.40	0.31	0.40	0.40
Income 200% + of FPL	0.38	0.23	0.23	0.23	0.30	0.23	0.23
Rural County	0.26	0.22	0.13	0.13	0.15	0.13	0.13
Suburban County	0.32	0.34	0.18	0.18	0.18	0.18	0.18
Medium-Sized Urban County	0.16	0.18	0.16	0.16	0.18	0.16	0.16
Large Urban County	0.24	0.26	0.53	0.53	0.49	0.53	0.53
<b>Panel B. Demographic Characteristics</b>							
Any Children	0.59	0.60	0.59	0.62	0.62	0.59	0.58
HH Size	3.4	3.3	3.2	3.3	3.1	3.2	3.0
Age < 30	0.52	0.53	0.54	0.54	0.41	0.54	0.54
White (non-hispanic)	0.59	0.53	0.46	0.40	0.45	0.46	0.46
Black (non-hispanic)	0.17	0.22	0.25	0.30	0.30	0.27	0.30
Hispanic	0.17	0.18	0.21	0.25	0.19	0.23	0.22
Female	0.57	0.59	0.59	0.61	0.69	0.69	0.67
HH Income	36,204	29,822	29,549	30,158	28,715	28,297	28,800
College Degree or more	0.17	0.15	0.16	0.15	0.28	0.29	0.26
Renter	0.56	0.66	0.69	0.67	0.79	0.84	0.85
	919,395	904,792	904,792	35,086	14,708	14,708	3,000

Notes: This table compares the study sample to estimates of the characteristics of the study in the US as a whole. Eligible individuals are those ages 21-40 with household incomes of less than 300% of the federal poverty line. Columns (1) - (4) report estimates of the characteristics of eligible households using the American Community Survey (ACS) 2013-2017 pooled sample. Column (1) presents the unweighted means for eligible individuals, Column (2) reweights this sample to match the enrolled sample distribution of income groups as a share of the FPL (which was a stratification target when assigning individuals to the active survey group), Column (3) reweights the ACS sample to match both the income group distribution and the county-type distribution in the enrolled active survey group sample, and Column (4) presents estimates of characteristics of eligible individuals in study counties, reweighted to match the enrolled sample FPL group and county type distribution. Columns (5)-(7) report characteristics of the study sample. Columns (5) and (6) report characteristics of eligible respondents to the mailer and online advertisement recruitment methods. Column (5) is unweighted, while Column (6) is reweighted to match the enrolled sample FPL and county type distribution. Column (7) reports the unweighted mean of the ultimate enrolled active survey group (i.e. the 3000 individuals assigned to the active group who answered the baseline survey).

## 3.2 Sampling Frames

### 3.2.1 Address-based Sampling

The majority of the sample—approximately 87%—was recruited through mailers. We selected addresses in eligible Census tracts from Target Smart ([targetsmart.com](http://targetsmart.com)). This vendor appends commercial data on name, income, race, and other available information to addresses from a variety of state and commercial sources. We understand that the accuracy of these commercial data varies widely, but using the data for targeting significantly improved the efficiency and cost of recruitment in pilots of the mailing strategy. About 69% of mailers were targeted to individuals

who appear income and age eligible on the basis of these commercial data. We refer to these as the “targeted mailers”.

To ensure that we did not systematically exclude from the sample individuals who are income and age eligible but did not appear as eligible in the commercial data (for example, because they moved or lost a job recently, they have missing or incomplete information in the commercial data, or they do not appear in any of the commercial data), the remaining 31% of the mailers were sent to addresses that were chosen randomly without regard to information from the Target Smart data. We refer to these as the “untargeted mailers.” Where data on names was available, we randomly selected one name per household to whom to address the letter.<sup>3</sup> We appended “or Current Resident” to the end of each name.

We sent mailers to Census tracts roughly in proportion to their share of the eligible population within the county type in the region. For example, if a Census tract contains 2% of the eligible households in rural counties in a state, that county was sent roughly the number of mailers required to ensure that the tract represents 2% of the ultimate sample. The number of mailers this procedure required for each tract depended on the share of households in the tract that are eligible for the program, the targeting effectiveness of the commercial data, and the share of respondents we aimed to recruit using targeted versus untargeted mailers. Ultimately, we sent mailers to 1,138,130 unique addresses, making up about 23% of households in the average Census tract in the study.<sup>4</sup>

To identify the optimal mailing strategy and generate variation in selection into the study, we randomized both the number of letters sent to each address (ranging from one to four) and the gift card incentive offered for completing the online screening questionnaire, which ranged from \$0 to \$20. Roughly 2% of mailed households received one letter, 55% received two letters, 26% received three letters, and 17% received four letters. In terms of gift cards amounts, 37% of households received no gift card, 21% received \$5, 17% received \$10, 2% received \$15, and 23%

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<sup>3</sup>For the “targeted” mailers and 50% of the “untargeted” mailers, we randomly selected one name per household among those names that appear age eligible in the commercial mailer data.

<sup>4</sup>The exact share varies with response and eligibility rates across different geography types.

received \$20.

### **3.2.2 Alternative Recruitment Methods**

In an effort to include in the sample participants selected differently from those who chose to respond to mailers, we employed two alternative methods to recruit the remaining 13% of the sample. First, the partner organizations purchased ads on the Facebook and Instagram platforms that were shown to all age eligible individuals located in program counties. Participants recruited through this method make up about 1 percent of study participants.

Second, the partners placed ads on the Fresh EBT platform. FreshEBT is a free mobile application developed by Propel ([www.joinpropel.com](http://www.joinpropel.com)) that allows Supplemental Nutrition Assistance Program (SNAP, also known as food stamps) recipients to check their balance and manage their benefits. FreshEBT has over 4 million users nationwide, including more than 180,000 active users in the program counties. The partner organization recruited app users in eligible zip codes by placing ads for the study within the app. Participants recruited through this method comprise roughly 12% of study participants.

### **3.2.3 Mitigating Spillovers Between Participants**

We took three primary measures to reduce potential spillovers between study participants (either through direct interactions or through changing housing or labor market conditions). First, we sent mailers in 6 waves, composed of 0.4%, 9.5%, 19%, 25%, 20%, and 26% of the total mailers, spread out over 8 months. We stratified the number of mailers sent across each wave within a Census tract. This meant that, at most, 6% of households in the average tract received a mailer during any given mailer wave.<sup>5</sup>

Second, we capped the number of households we randomized into the program participation

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<sup>5</sup>There are a few rural counties where we needed to send mailers to essentially all households within the county during the course of recruitment.



group at 2 for each Census block and 20 for each Census tract. This reduces the probability that participants in the program interact socially.

Third, prior to randomization into treatment and control, we conducted a survey of study participants to ask if they knew anyone else in the study and, if so, who that person was. Individuals who knew another person in the program were randomized in clusters with the other person(s) they knew in the study to avoid spillovers between people with different treatment status. For more details, see Section 4.5 below.

## **4 Recruitment and Randomization Procedures**

### **4.1 Recruitment to Eligibility Survey**

#### **4.1.1 Mailers**

The non-profit organizations implementing the cash assistance program first sent the mailers described above, informing individuals they may be eligible to participate in a new program in which participants receive “\$50 or more” per month for three years. The mailers directed recipients to a website where they could register their interest in the program and complete a short eligibility screening survey. This screening survey collected demographic data that was used to verify eligibility for the program (e.g., household size and income to determine if respondents’ incomes were below the cap, age, participation in public assistance programs). Respondents were also presented with an e-consent form to give the research team permission to access their administrative data. In order to facilitate linkages to administrative data, individuals who consented to share admin data had the option of providing their social security numbers during this process. Consent to share admin data was not a requirement for program participation, and it did not affect the probability of being selected for the program or randomized into the treatment group.

The partner organizations provided a phone number on the letter that people could call with

questions or to receive assistance accessing and completing the survey. Ultimately, 38,823 individuals responded to the mailers and completed the eligibility survey, of whom 12,745 were program eligible (33%).

#### **4.1.2 Facebook and Instagram**

As described above, each implementing partner organization purchased ads that appeared on Instagram and in the Facebook news feeds of users in all eligible counties who are predicted to be age-eligible for the program. The ads ran for 1-3 weeks and had varied levels of concentration, as measured by ad spending, by zip code group in each state; more money was spent on ads in zip code groups with the highest poverty rates.

The ad included a thumbnail picture of a calculator and a notepad with a list of monthly bills and text announcing a new program in which “Participants will receive \$50 or more per month.” Clicking a button that said “Learn more” directed respondents to a website hosted by each partner organization that included a brief description of the program, contact information for questions, and a link to complete the same online eligibility survey that mailer recipients completed.

#### **4.1.3 FreshEBT**

Also as described above, each implementing partner organization posted ads on the FreshEBT app to users in eligible counties. These notices ran for 1-2 weeks and advertised a “new financial assistance program” in which “selected participants receive \$50 or more per month.” When a user clicked the “Learn More” button, they were directed to a short form that collected their email address, phone number, age, and zip code. Age-eligible respondents who confirmed that they live in an eligible zip code were sent an email that provided instructions to complete the same online eligibility survey administered to individuals recruited through other methods.

## 4.2 Randomization 1: To In-Person Enrollment or Passive Monitoring

We then randomized individuals to be targeted for in-person enrollment or to remain in an “administrative data only” control group. Though individuals in the latter group will not participate in any research activities, their de-identified administrative data can be used for comparison on outcomes measured using these data.

Once we had a pool of eligible individuals, we blocked participants by demographics (age, gender, and race) and pre-treatment values of high-priority outcomes collected in the eligibility survey. We randomly assigned participants to the **“administrative data control”** or the **“program participation”** sample. To ensure that we met our demographic quotas<sup>6</sup> in the program participation group, we sent a larger number of mailers than required to reach our sample size and then randomly selected the program participation group to satisfy the demographic quotas. This means that participants had different probabilities of assignment to the “administrative data control.” We include all eligible screener respondents who are not randomized into the program participation group in the administrative data control group, but we will reweight the administrative data control group to have the same demographic averages as the program participation group.

In total, 9,504 individuals were placed in the “administrative data control” group, of whom 55% consented to share their non-health related administrative data, yielding an admin control group of 5,266.<sup>7 8</sup>

We plan to compare outcomes measured using administrative data for the administrative data control group to the control group enrolled in the main study (as described in Randomization 2

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<sup>6</sup>There are three demographic quotas that we targeted for the sample. Specifically, we designed the randomization to ensure that i) the share of women in the sample resembles the share of women in the eligible population in study counties; ii) the sample is least 20% non-Hispanic White, 20% Black, and 20% Hispanic; and iii) the household income of at least 30% of the sample is 0-100% of the federal poverty level (FPL), the household income of at least 30% is 101-200% of FPL, and the household income of no more than 25% of the sample is 201-300% of FPL.

<sup>7</sup>Individuals in the admin control group are disproportionately in the middle and high income groups (with household incomes of 101%-200% and 201%-300% of the FPL) given the need to assign households with incomes of 0-100% of the FPL to the program participation group with higher probability in order to achieve our sample income group target goals.

<sup>8</sup>A smaller proportion, 51%, agreed to also share health related administrative data.

below). This comparison will reveal whether participation in the study and receipt of the \$50 per month transfer had any effects on outcomes.<sup>9</sup>

### 4.3 In-Person Enrollment

The partner organizations then attempted to enroll individuals who had been randomized into the group targeted for in-person enrollment into the cash assistance program. As part of this enrollment, we administered the baseline survey to program participants who consented to take part in the research. We contracted with the University of Michigan Survey Research Center (SRC), a survey research firm with extensive experience fielding national studies, to manage recruitment and conduct in-person enrollment and baseline surveys. SRC employees aimed to ultimately complete 3,000 enrollments from the larger pool of possible participants. During the first 3 weeks of an attempted enrollment, interviewers made a total of 12 phone calls to primary and secondary phone numbers and sent follow up emails and text messages. The non-profit partner reached out to the individual at least once during week 4 if no contact had been made, and a different interviewer attempted 3 additional phone calls in week 5. If there had been no response after 6 weeks, we put contact on hold for two months before making another call and sending another text. If there was still no response, interviewers continued to call and text at least once per month until 3,000 participants had been enrolled.<sup>10</sup>

The in-person enrollment proceeded as follows:

- SRC staff first explained the purpose of the cash assistance program and the program pro-

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<sup>9</sup>When conducting any such estimation, our estimand will be the average treatment on treated effect (ATT), weighting to the sample actually targeted for enrollment in the program. We had originally planned to conduct pooled analyses that estimated treatment effects by pooling our main analysis with an analysis that compared this “administrative data control” group to the treatment group that received the cash assistance. However, due to many participants having either very low or very high probabilities of assignment to the administrative data control group and the lower than anticipated take-up rate of the study among those assigned to the group targeted for in-person enrollment (due in part to COVID-19, which required enrollment to be done over the phone rather than in person), we do not plan to pursue this estimator for our final analysis. Our power calculations indicated that it would only increase our statistical precision by approximately 2%.

<sup>10</sup>Depending on response rates after the two-month break, interviewers in some cases attempted to reach individuals by visiting their home up to three times. In-person outreach stopped in March 2020 due to the COVID-19 pandemic.

cedures. Everyone was informed that they will receive "\$50 or more" each month for three years and that the specific amount will be randomly assigned, but the fact that some participants will receive \$1000 each month was not disclosed. This reduces the likelihood that the control group will know they are in the control group, as that knowledge may change their behavior in ways that would bias the results (including differential take up or attrition and a negative reaction to learning one is receiving less than others). Additionally, we did not want the prospect of a large cash transfer to coerce anyone into participating in the study.

- Individuals who agreed to participate in the program were enrolled in accordance with the procedures established by the non-profit organizations implementing the program.
- SRC staff then explained the purpose of the research and the study procedures.
- The explanation included the incentive structure for participation in research activities: \$50 each for completing in-person baseline, midline, and endline surveys, \$15 for each mobile baseline survey, \$10 for each short monthly survey, and \$10 per month for completing short activities on a mobile app. These incentives are taxable (unlike the cash assistance gifts), so we will send participants a 1099 if the participation incentive payments exceed \$600 per calendar year, although we intend to keep incentives under the threshold.

During study enrollment, the enumerators:

- Obtained informed consent and contact information for friends and family that can help us locate the participant if we cannot reach them.
- Collected names and demographic information for other members of the household and a description of their relationship to the participant, to help document spillover effects.
- Helped the participant install the custom mobile app and showed participants how to use it, if the participant had a smartphone and consented to using a mobile app.

- Administered the first and most comprehensive baseline survey, including collecting biomarkers (height, weight, and blood pressure).
- Helped the participant set up direct deposit for the research incentive payments. If the participant already had a bank account, the interviewer logged in to a custom-built payments processing system and allowed the participants to verify their bank account information. If participants did not have a bank account, they were given the option of opening an account at Chime Bank, an online bank with no monthly fees, no minimum balance, and no overdraft fees. If they chose this option, they received a Visa debit card in the mail within 7 business days.

#### **4.3.1 Changes to Enrollment in Response to COVID-19**

Enrollment began in October 2019, and 1,317 individuals were enrolled and completed the in-person baseline survey by March 14, 2020. On March 15, 2020, the University of Michigan imposed restrictions prohibiting all in-person research activities in response to the COVID-19 pandemic. All outreach was suspended and no enrollments were conducted for approximately six weeks. During that time, we worked with SRC to make the necessary adjustments so that interviewers could enroll participants and administer the baseline survey over the phone. With the exception of biomarkers and the cognitive tasks, all other data could be collected over the phone. Enrollments resumed in late April and all remaining participants were enrolled remotely by October 6, 2020. Ultimately, 44% (1317) of enrolled individuals were enrolled via an in-person baseline survey and 56% (1683) were enrolled via phone.

#### **4.4 “Long Baseline”**

Enrollments took place over a 12 month period (the “long baseline”). During this time, random assignment to treatment had not yet taken place; all participants who had been enrolled were

receiving the control group cash assistance gift of \$50 per month. In the month after a participant was enrolled, we administered three additional waves of web-based baseline surveys, notifying participants by text and email. These “mobile baselines” allowed us to collect data on outcomes that were not included in the in-person baseline. We also began distributing short web-based surveys each month that took approximately 10 minutes to complete. The purposes of these surveys are 1) to gather additional pre-treatment data to increase the precision of the estimates, and 2) to identify individuals likely to attrit from the study under the \$50 condition.

The desire to identify participants likely to attrit is primarily driven by concerns over differential attrition. As previously noted, the 1970s NIT experiments were plagued by differential attrition. Differential attrition also seems likely *ex ante*; even though participants will continue receiving their \$50 (in the control group) or \$1,000 (in the treatment group) monthly payments regardless of whether they participate in all of the surveys, individuals receiving \$1,000 per month may nevertheless be significantly more responsive than those receiving only \$50. In case this differential attrition occurs, we hope we can identify a large subsample *ex post* that did not exhibit differential attrition, as defined by their *ex ante* responsiveness. For example, we might conclude: “We see differential attrition on average, but among those who answered at least 2 of the 3 pre-randomization baseline surveys, we do not.” We will not, however, exclude any participants from randomization or change the probability of assignment to the treatment group based on whether they continue responding to surveys during the “long baseline.”

## **4.5 Randomization 2: Treatment and Control Groups**

After all 3,000 individuals had been enrolled, we randomly assigned them to the “**treatment**” (\$1,000 per month) and “**program control**” (remain at \$50 per month) groups.

We used blocked and clustered random assignment as follows:

1. *Clustering.* We first formed clusters of individuals based on information that a small num-

ber of study participants knew each other. We placed individuals who reported knowing each other into the same cluster, such that they would always receive the same treatment assignment.

2. *Selecting the Waitlist.* We next selected a stratified random sample of 300 individuals in each state to be placed in a waitlist group. Only individuals not in a cluster with other individuals were eligible for this waitlist group. Within this waitlist group in each state, we formed 10 blocks of 30 observations, blocking on a number of pre-treatment characteristics. We then placed the observations on the waitlist in order such that each 10 observations contained one randomly sampled observation from each of the 10 blocks.
3. *Blocking.* We next “collapsed” the data to the cluster level to conduct a cluster-level random assignment. (The vast majority of individuals are in a cluster of size one with no other observations, but around a dozen clusters were of size two or three.) We then formed blocks of clusters as follows. We first formed strata based on race/ethnicity, income group, and state; any clusters with more than one individual within them were placed in their own strata. Within these strata, we formed blocks of three based on several dozen pre-treatment covariates using the `blockTools` package in R. When the number of clusters in a strata did not evenly divide into three, there were either one or two leftover clusters in a strata after the first round of blocking. We then conducted a second round of blocking for these leftover clusters, again forming blocks based on a set of pre-treatment covariates using `blockTools`.
4. *Random Assignment: blocks.* Within each block of three, we selected one of three observations to be in the treatment group and placed the remaining two in the program control group. Given that the number of clusters did not evenly divide into three, within the final block we sampled from the vector  $\{0, 0, 1\}$  without replacement to assign treatment within the final block.
5. *Random Assignment: waitlist.* After the first random assignment, we computed the number



of *individuals* (not clusters) in each state that had been placed in the treatment group. Because the clusters are not of equal size, the number of individuals placed in the treatment group during the first random assignment step varies by randomization. We then calculated how many remaining individuals  $N$  from the waitlist would need to be placed into the treatment group in order for 1/3 of each state to be in the treatment group. For example, our target was to place 501 participants in one state (1/3 of the 1503 enrolled) into the treatment group; if 401 participants had been randomly assigned to the treatment group in the first randomization, we would place 100 of the state's 300 observations on the waitlist into the treatment group.

Recall that the waitlist had already been placed in a random order within each state. To select the individuals on the waitlist that would be initially placed in the treatment group, we simply selected the top  $N$  individuals on the waitlist.

6. *Re-randomization.* After conducting a randomization, we conducted a series of balance checks across several dozen pre-treatment covariates. Each pre-treatment covariate was associated with a different  $p$ -value floor, with covariates we deemed to be more important assigned a higher floor. We rejected any randomization where the  $p$ -value on a  $t$ -test was below the  $p$ -value floor for any of the individual variables. We also conducted an  $F$ -test for the joint significance of all of the same set of pre-treatment variables by outcome area and rejected a randomization if the  $p$ -value on any of these  $F$ -tests was over 0.25.

Through simulation, we verified that this procedure resulted in all observations having an exactly 1/3 probability of being in the treatment group.

## 4.6 Intervention

After random assignment, participants in the treatment and control groups will be notified about the amount of the cash transfer they will receive each month and the schedule for disbursements.

The intervention in this study is an exogenous increase in income in the form of unconditional cash transfers. The transfers (\$50 monthly for the program control group and \$1,000 monthly for the program treatment group) will be delivered by the implementing non-profit organizations via direct deposit to the participants' bank accounts.<sup>11</sup> All participants will be notified monthly when the payment is deposited into their account.

Receipt of the treatment transfers and the nominal transfer for the control group is not conditional on participation in any of the research activities and individuals can use the money however they choose. Note that the transfers are provided as a gift from a non-profit organization and will not be subject to income tax.

#### **4.6.1 Waitlist**

Participants may not wish to receive the \$1,000 per month transfer (e.g., because they do not feel comfortable taking money they did not “earn,” or because it affects their eligibility for other benefits). During the first three months of the program, if any individuals assigned to the treatment group refuse the \$1,000 per month transfer, we will go to the next person on the randomized waitlist in their state and offer that person the transfer instead.

### **4.7 Outcome Measurement**

#### **4.7.1 Monthly Surveys**

We plan to use Qualtrics to conduct monthly web-based surveys. Participants will be notified by a text message and an e-mail containing a personalized link to the survey, and we will ask them to complete the questionnaire at their convenience within 2 weeks. We will send reminders to nonresponders, and \$10 will be deposited to participants' bank accounts immediately upon

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<sup>11</sup>The implementing partner organizations work with participants who do not have a bank account and who decline to or are unable to open a Chime account to ensure that they are able to receive direct deposits via a reloadable debit card or payment transfer app.

completion. We plan to keep the surveys very short to reduce fatigue.

Maintaining regular contact allows us to identify changes in employment, housing, education, and other variables for which a change will trigger an additional module asking about the reasons for the change and collecting new data on relevant measures (e.g., housing quality following a move, job satisfaction and earnings for new job, etc.). We will spread the modules to be administered less frequently across months to keep the length fairly consistent. Questions pertaining to variables with higher likelihood for measurement error or misreporting due to difficulty remembering will be asked more frequently.

If we see large differential attrition from these surveys, we may abandon them and focus on collecting data during the midline and endline surveys. However, we do see the monthly surveys as an important way to maintain contact with respondents, and response rates were very high (over 90%) throughout the pilots.

#### **4.7.2 Midline Survey**

The survey firm will administer an in-person midline survey 15-18 months after the treatment group begins receiving \$1000 per month.

#### **4.7.3 Endline Survey**

The survey firm will administer an in-person endline survey towards the end of year 3, several months before the cash transfers will end. Respondents in the treatment group may behave differently during the last few months of the program in anticipation of the payments ending, so we will conduct this survey a bit early, starting at 2.5 years into the program and ending at least 3 months before the transfers cease. We hope to conduct long-run follow ups in the future after the program has ended to observe whether effects persist.

#### **4.7.4 Administrative Data**

We will gather a variety of administrative data which is described in more detail below.

### **4.8 Mobile Phone Application**

Participants have the option to download a mobile phone application created for the study. We will use this mobile app for both passive and active data collection for consenting participants. We will administer 2-4 short activities each month through the app; participants who choose not to or are unable to download the app will be able to complete these activities via a web interface. From the subset of participants who consent to share anonymized location data, we will passively collect GPS location and accelerometer data from the participants' phones that we can connect to other data sources to potentially improve the precision of our estimates.

## **5 Estimation**

To estimate treatment effects, we will compare outcomes for individuals who were assigned to the treatment group to individuals who were assigned to the “program control” group. In an alternative specification, we will take advantage of the fact that we collect repeated measures over time to analyze treatment effect dynamics. When necessary to combine multiple outcomes (e.g. to form indices) we will estimate models using seemingly unrelated regression. To be more specific, we will run one seemingly unrelated regression for each outcome in an index, and then test the joint hypothesis that the weighted sum of the treatment effects are zero while using robust standard errors. The weights will be determined as described below in the “combining data from multiple sources” section.

## 5.1 Waitlist

Within the waitlist group, we will follow the approach of (De Chaisemartin and Behaghel 2020). We will separately estimate the TOT of the \$1,000 per month among the observations not in the waitlist. Finally, for our estimates, we will compute a precision-weighted average pooling the estimates for the waitlist group and for the observations not in the waitlist.

## 5.2 Regression Adjustment to Increase Precision

In general, we will compute regression-adjusted treatment effects using the procedures outlined in Bloniarz et al. (2016), using the LASSO to select baseline covariates to use for regression adjustment, then including the selected covariates in an OLS regression with the treatment indicator present. These OLS regressions with clustered standard errors will represent our main estimates and standard errors. For robustness, given the re-randomization process, we will also compute a set of standard errors by permutation, using 100,000 permutations that also passed our randomization criteria.

In some instances, we will be unable to merge our survey data with the administrative data outcomes for the TOT component of our estimator. In these cases, we will always include all of the pre-treatment values of the administrative data outcomes on the right hand side of our regressions unless otherwise specified.

We will present unweighted estimates for our primary results.

## 5.3 Adjusting for Multiple Comparisons

We will organize our outcomes at four levels:

1. **Topic.** E.g., political outcomes, health, time use, labor supply, geographic mobility, financial health, child outcomes, material hardship, cognitive, intrahousehold, psychosocial outcomes. One can think of each topic as representing one academic paper.

2. **Family.** This is the level at which we will conduct the multiple comparison adjustment. Therefore, each paper will make false discovery rate (FDR) adjustments within each family of outcomes in the paper. E.g., intergroup attitudes, political attitudes, political participation.
3. **Outcome.** Each family will have multiple outcomes. E.g., attitudes on social issues, attitudes on economic issues.
4. **Outcome Measures.** An outcome may be composed of multiple measurements. E.g., an economics attitudes index might be composed of ten different survey items about different economic issues.

We will categorize all outcomes into outcomes, families, and topics *ex ante*.

We plan to compute “sharpened” FDR-adjusted  $p$ -values that control the rate of false positives within the family of tests to be no more than the nominal level. We will use the procedure as outlined by Anderson (2008). We will report per comparison  $p$ -values in addition.

We will treat ordinal outcomes as continuous by default.

We will place secondary outcomes in separate families from primary outcomes and clearly label them as secondary.

## 5.4 Combining Data from Multiple Sources

Unless specified otherwise, in cases where we collect both midline and endline survey outcomes, we will combine the midline and endline outcomes to increase precision (McKenzie 2012); the main outcomes of interest will be a weighted average of the midline and endline outcomes, with 30% of the weight on the midline outcomes and 70% of the weight on the endline outcomes. We will also report the midline and endline results separately. We will also report a version that incorporates the data collected via the monthly or online surveys, collapsing the monthly surveys by pre/post midline and constructing a monthly survey outcome putting an analogous 30/70 weight on pre versus post-midline quarterly surveys. This set of monthly survey outcomes will then be

combined with the midline/endline aggregated outcomes with 30% weight being applied to the monthly surveys and 70% applied to the midline/endline surveys. Note that we will estimate all effects on individual  $\times$  time period data (i.e., data will not be collapsed to the individual level). For outcomes collected at frequencies other than midline and endline (e.g., monthly), results will be reported by year unless otherwise noted in the PAP. For selected outcomes collected on a frequent basis, we will look at time trends; these cases will be specified in the discussion of the outcome measures.

## **5.5 Attrition**

We will test for differential attrition from the surveys and, should this prove to be an issue, we will present a set of results correcting for differential attrition. We will check for balance in attrition rates using the same set of covariates that we used to test for balance at randomization. We are fortunate that we will have a variety of administrative data outcomes which are significantly less likely to be subject to substantial attrition.

We will conduct two-stage sampling for midline and endline data collection to minimize attrition-related bias by concentrating resources and efforts on a randomly chosen subset of the cases that are the most difficult to reach (and adding weights accordingly). We will also keep track of the number of contacts required to reach each participant for each survey. We will consider using the randomly assigned intensive follow-up and number of contacts required to reach each participant to construct attrition adjusted treatment effect estimates.

## **5.6 Heterogeneous Treatment Effects**

Given the sample size and the many hypothesis tests we already plan to conduct, we are concerned about statistical power. Therefore we will pre-register that all heterogeneous treatment effect estimates will be considered exploratory unless explicitly pre-specified otherwise. PAPs for some

outcome areas may specify hypothesis tests for heterogeneous treatment effects and note them as exploratory or non-exploratory.

## **5.7 Characterizing “Treatment” of Control Group Participants**

Not all eligible respondents who complete the online eligibility screener will be randomly selected to participate in the program and study. As a result, we have access to an additional “control” group of individuals who consented to passively provide administrative data but will not be contacted by the research team. Using this “administrative control” group can help us shed light as to whether the program has any effects on the “program control” group, either as a result of the \$50 monthly payments, the survey incentives, or the act of completing surveys themselves. We will use this group to characterize any such effects on outcomes measured using administrative data that might be present in the program control group.

## **5.8 Elicitation of Forecasts**

We will be eliciting forecasts for several key outcomes on the Social Science Prediction Platform. We expect to receive forecasts from other researchers, those working in policy or non-profit organizations, and the general public. These forecasts can help in gauging the novelty of our results. There are not currently standard ways of presenting comparisons of *ex ante* forecasts with research results, but we anticipate including some comparisons, if only in an appendix. In comparing our research results to the *ex ante* forecasts, we will focus on comparing our results to the predictions of researchers in economics unless otherwise specified. The outcomes that we will forecast are indicated with an asterisk in the section on outcomes below.



## **5.9 Other Notes**

The survey questions and analyses described here are contingent on securing sufficient funding to gather the requisite data.

# **6 Sustained Unconditional Cash Transfers and Employment**

## **Outcomes**

Receiving sustained unconditional cash transfers could affect both labor market supply and labor market outcomes.

There have been many studies of labor supply elasticities; Keane (2011) and McClelland and Mok (2012) provide recent reviews focusing on responsiveness to wages and taxes. Studies of the Earned Income Tax Credit and Negative Income Tax experiments tend to find small, negative effects on labor supply (e.g. Eissa and Liebman (1996), Price and Song (2017)). Most meta-analyses of cash transfer programs in a developing country context also find mixed, sometimes negative effects (Kabeer and Waddington (2015), Bastagli et al. (2016)).

In contrast to much of the existing literature, our focus is on a relatively young sample whose career trajectories may be more malleable. Some of the labor supply effects previously observed in other studies were driven by older adults retiring sooner as a result of the transfers (Price and Song (2017)), an effect that is unlikely to occur in our setting. We may expect to see greater career changes, potentially including some individuals leaving employment to pursue further training or some electing to stay unemployed longer while searching for a better job rather than settling for an available but less desirable job.

This study also has the opportunity to speak to an on-going debate in the literature as to whether expansions of the social safety net lengthen unemployment but ultimately result in better job matches between job seekers and employers. This literature has historically focused on changes

in the generosity of employment insurance, but similar arguments could apply to job search under the increased security of monthly cash transfers. The literature, mostly from other countries (e.g. Germany, Austria), is mixed, with Centeno (2004), Caliendo, Tatsiramos and Uhlenhorff (2012), and Nekoei and Weber (2017) finding that more generous benefits enable better jobs, while another strand of the literature finds no such effects (e.g. Card, Chetty and Weber (2007), Lalive (2007), van Ours and Vodopivec (2008)). In addition to drawing from other countries with more generous social safety nets, past papers in this literature have often had limited information on job quality, inferring job quality from income or the duration that the post-unemployment job was held. In contrast, we will have access to a rich array of variables we can use to identify quality of employment.

To capture possible shifts in job quality, we thus ask very detailed questions about the nature of respondents' current jobs, paying particular attention to their stability and formality. We also focus on whether their jobs are "high quality" as indicated by the hourly wage and work hours available, non-wage benefits such as whether training is offered by an employer, and a variety of self-reported measures of quality of work life.

Given the importance that has been placed in the literature on disability and the role of disability in long-term unemployment outcomes (Autor and Duggan (2003)), we also probe whether respondents are disabled and, if so, if they expect to return to work in the future. This could potentially serve as a valuable explanatory variable. Importantly, at the end of the program, after the transfers are stopped, we will also follow up to see what the likelihood is of a participant applying for disability or other benefits; however, these post-program outcomes will be detailed in a separate pre-analysis plan. We additionally catalog barriers to employment that could be mitigated by a cash transfer, such as whether the respondent has had to miss work due to illness or lack of transportation. These outcomes are considered separately to disability as they may be more immediately responsive to cash transfers.

The security of regular cash transfers could help to make self-employment more attractive, so

we will also consider entrepreneurial activity.

Finally, we will also analyze human capital formation: i.e., if we do observe recipients working slightly less, what are they doing with their time instead? Are they pursuing formal or informal education to gain skills which could help them obtain better jobs in the future? The effects of sustained cash transfers on future labour market outcomes could help to address concerns about the cost of extending such transfers as a social policy (Hoynes and Rothstein 2019). Other outcomes that we are measuring in separate pre-analysis plans that can contribute to our understanding of long-run effects are child development outcomes, time use outcomes, outcomes predictive of long-run health, and outcomes relating to social problems that are very expensive to address such as homelessness, drug use and crime.

Due to large empirical differences in observed careers, we will disaggregate each family's index and several key outcomes noted in the text by sex.<sup>12</sup> These disaggregated results will be presented as main results along with estimates pooled across sex. For some outcomes flagged in the text, we will separately consider heterogeneity by age or whether the participant had a college degree at baseline.

## **7 Employment Outcomes: Survey Measures**

This section describes the survey measures that will be used in each family of outcomes.

As noted, apart from considering the measures in each family separately, we will report a single index for each family (following, e.g., Finkelstein et al. (2012)). On occasion, an outcome measure will be pre-specified to be excluded from this index as it may be useful descriptively but not make sense to include in an index.

In addition to considering families of outcomes through indices, we will report results tables that disaggregate by each outcome measure. We will focus particular attention on three outcomes

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<sup>12</sup>Unless otherwise noted, those self-identifying as “other” at baseline will be pooled with those self-identifying as “female” for analyses.

critical for policy decisions: the employment rate, annual hours worked, and annual earnings. We will also include discrete choice questions and construct models of job search and selection to test whether individuals find better jobs if they can search longer or if this is associated with negative outcomes. Approximately 30% of our sample is unemployed at baseline, making progression from unemployment particularly important.

Finally, we will consider several outcomes that are descriptive and will not be included in any family index: type of employer (e.g. private sector), industry of work, and job title. Industries can be linked through NAICS codes and job titles through SOC and OCC codes to data from the Bureau of Labor Statistics (BLS) and the Economic Census to explore whether there is growth in occupations with higher or lower average monthly unemployment rates or a lot of turnover, as represented by the share of hires and separations out of total employment.

While the following sections focus mostly on the questions asked in the in-person surveys, as we expect these surveys to have the highest response rate, related mobile survey questions are sometimes available and will be used in robustness checks.

## **7.1 Family 1: Labor Supply Elasticity**

We will ask respondents to provide details of their current employment and work histories at baseline/midline/endline, as well as asking related questions, described below. This family is divided into four groups of outcomes:

1. Labor supply elasticity: a) whether the respondent is currently employed (G2: 1=working for pay, 0=unemployed and looking for work or unemployed and not looking for work) (\*); b) how many hours the respondent is working total, across their main job and any other jobs (I7+J9\* for employed; 0 for unemployed) (\*); c) the respondent's reasons for unemployment, if unemployed (H2). B21 will be used in an alternative specification for item b); it contains less information but may be less sensitive to outliers.

As an exception to our typical protocol, we will also present estimates of the income elasticity of labor supply ( $\eta$ ) and estimates for the extensive ( $\eta_e$ ) and intensive ( $\eta_i$ ) margin separately from the index. For greater comparability with past studies on the NIT experiments and the EITC (e.g., Eissa and Hoynes (2004), Robins (1985)), our preferred measures for this purpose will be the average annual hours worked and the employment rate; we will also plot the quarterly employment rate to highlight any trends.

We will calculate the income elasticity of labor supply as  $\eta = \frac{Y}{L} \frac{\partial L}{\partial v}$  where  $Y$  is net-of-tax total income,  $L$  is labor supply, and  $v$  is virtual income, taking a conventional approach (e.g., Rothstein (2010)). We will also calculate the extensive and intensive elasticities  $\eta_e = \frac{Y}{p} \frac{\partial p}{\partial r}$  where  $p$  is participation and  $r$  is non-labor income and  $\eta_i = \frac{Y}{h} \frac{\partial h}{\partial v}$ , where  $h$  is labor hours. In considering income changes, we will consider changes from baseline income.

Of the two lottery studies that are perhaps the closest to this paper, Cesarini et al. (2016) finds support for a “separate spheres” household model (Lundberg and Pollak 1993) while Golosov et al. (2021) cannot reject a unitary household model. We will present elasticities both using individual-level data on the participant and their partner, if any, as well as household-level income. The pre-analysis plan on intrahousehold outcomes describes how the estimates of partner income, labor force participation and labor hours will be measured in detail; we will prioritize administrative data for income and baseline/midline/endline survey measures for participation and labor hours, analogous to how we construct participant outcomes.

Recall that in our setting, participants were eligible to apply for the program based in part on falling below a household income threshold, and we randomized which individual in the household the recruitment mailer was directed to. However, it is still possible that who applied with the household, and hence receives the transfers, is non-random.<sup>13</sup> This is important because it means that if we observe differences in responses between participants and their partners, it may be due

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<sup>13</sup>If multiple adults within the same household applied, one would have been randomly selected to participate since one individual from each address was randomly selected to form the active survey group, but this was fairly rare.

to selection. We will test whether who we addressed the mailer to predicts who applied. We will also ideally test for baseline differences between the participants and their partners using tax return data for married spouses filing jointly, as this would provide the most accurate source of data on participants' spouses. As a fallback measure, we will consider whether participants reported that they were the primary breadwinner or caregiver in the household, based on survey data. These analyses will be considered secondary.

In the unitary household model, household income should not depend on which individual in the household participates in the program. As an exploratory analysis, we can examine to what extent this appears to be the case by testing whether dual-earner households respond differently depending on the participant's sex or relative wage, following Golosov et al. (2021).

In our setting, it is possible that other household members' income or labor supply may react to the transfers. Further, we are following participants over an extended period of time, and household composition and membership could change over time, either exogenously or as a result of the transfers. We will test both of these hypotheses (i.e., that there may be individual income or employment spillovers to others in the household, particularly for romantic partners, and that household composition may change as a result of the transfers, again with special focus on romantic partners). Details are provided in the pre-analysis plan on intrahousehold outcomes. Assuming we do not observe significant income or employment spillovers, we will prefer to treat partners' income as exogenous and include it in non-labor income, along with other transfers; if we do observe differences, we will prefer the unitary household model. In either case, we will also present these elasticities by subgroup: participants who are single household heads at baseline; participants who are married or cohabiting at baseline; participants with children at baseline; and participants without children at baseline (the latter two groups overlap with the first two groups). All these subgroup analyses will be exploratory.

Other non-labor income, in either model, includes government transfers and gifts. The cash transfers were designed to have minimal impact on benefits and are not taxable. Nonetheless,

if we know that government transfers were mechanically reduced for any participants as a result of the transfers, we will consider the net amount they received rather than the gross (i.e., cash transfer minus reduction in benefits). To further maximize comparability with existing studies, we will ideally use total income net of capital gains from administrative data (McClelland and Mok (2012)). While the main results will use net-of-tax income, we will also present results using pre-tax income given that the differences observed in Cesarini et al. (2016) and Golosov et al. (2021) depended in large part on which metric was used.

We will also consider the minimum and maximum work hours an individual reports (G11 and G12 in preferred specification; workhrs\_1 and workhrs\_2 in the mobile survey data). However, these outcomes will be exploratory.

We will conduct additional exploratory analyses to consider whether there is heterogeneity in the first two outcome measures (1a and 1b) by age (under 30 or 30 and over), sex (female or male), or education (with or without a college degree) at baseline. We will also consider heterogeneity by whether the participants had randomly received three or more mailers recruiting them to the study (as an indicator of more intense recruitment)<sup>14</sup> and whether the participant was recruited through the FreshEBT app, though we do not expect this to be significant given the very small subset of participants recruited through this means. It should be emphasized that these are exploratory analyses. The heterogeneity analyses by age, sex or education will be secondary, and the latter tests tertiary. FDR adjustments for the primary outcomes (index measures) will be run across primary outcomes; FDR adjustments for any outcomes described as secondary will be applied based on the number of tests in the primary analyses plus the number of tests in the secondary analyses, following Allcott et al. (2020); similarly, FDR adjustments for tertiary outcomes will be applied based on the number of tests among the primary, secondary, and tertiary outcomes, though we presently anticipate these last tests will only be contained in appendix tables.

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<sup>14</sup>We can also consider the incentive randomly offered, but there is not much variation in this.

## 7.2 Family 2: Employment Preferences and Job Search

This family of outcomes captures whether individuals prefer more, less, or different work. The results of this family of outcomes should be interpreted carefully and in conjunction with results from other families of outcomes. For example, searching for more work is not unambiguously a positive outcome: it could indicate work effort, but it could also indicate inadequacy of one's current position.

1. Preferences for employment: a) whether a respondent is employed or if unemployed would prefer to be working (H1b) (1 if so, 0 if not); b) how much work they want (here we will combine responses for those who are employed, who answered “more”, “same” or “fewer” (I8) with responses for those who are unemployed, who answered “full-time”, “either” or “part-time” (H4); by default, if someone is unemployed and wanting any kind of work, that counts as “more” and if they are not seeking a job that counts as “same”). The responses to items a) and b) will be also considered separately descriptively for those who are unemployed, despite selection into employment; for item b), we will revert to using a categorical scale that takes the value 3 if H4=“full-time”, 2 if H4=“either”, 1 if H4=“part-time” and 0 for those unemployed and not seeking employment. Selection will be addressed in this separate analysis using a Heckman correction. As a robustness check, we will use `jobsearch_2` for item b), however, as there is likely to be more attrition in the mobile surveys, this will not be the preferred specification. We will additionally descriptively consider reasons why individuals left their previous jobs (`leftjob_2` and `newjob_3`). All descriptive outcomes will not be included in the FDR adjustments for primary outcomes but be included in FDR adjustments accounting for the number of primary and secondary outcomes, following Allcott et al. (2020).
2. Active search: a) if employed, whether the respondent is seeking a new or additional job (G7 and G7a); if unemployed, whether the respondent is seeking a job (G2: 1=unemployed



and looking for work) (\*); b) how many job applications they made recently (G7b and H3a from the in-person modules, which ask about number of applications in the last 4 weeks for those who are employed and unemployed, respectively), or 0 if no job search; c) if they were looking for a job in the last three months (jobsearch\_1); d) how many interviews they had in the last three months (jobsearch\_8 or 0 if no job search); e) number of actions taken to search for a job (jobsearch\_6 or 0 if no job search). As a robustness check for unemployed respondents, G2 in item a) will be replaced by H3, which asks about actions taken in the last 4 weeks to look for new work; this measure might be less prone to social desirability bias, but G2 is more comparable to the question asked of individuals who are employed (G7 and G7a). Analysis of the baseline data suggests high correlation between H3 and G2. In another robustness check, item b) will separately be replaced by a similar measure from quarterly mobile trigger modules, jobsearch\_7, asking about the number of applications in the last 3 months, however, this will not be the preferred specification due to likely higher attrition. It should also be emphasized that less active search could be as a result of having obtained a better job, so the interpretation of this outcome depends on the trajectory of employment and quality of employment families. Due to the possibility of recall bias, this latter number will be re-scaled so as to minimize the average root-MSE when compared with the in-person survey measures). Specifically, we will multiply the three-month measures by 4/13 to put them in terms of a 4 week period and then multiply by the factor that equalizes the mean. We will present items a) and b) for those who are employed and unemployed separately as a descriptive measure outside the index, to help us interpret the main results. As with other descriptive outcomes, they will not be included in the FDR adjustments for primary outcomes but will be part of FDR adjustments for primary and secondary outcomes. If the response rates to the mobile modules is below 90% for any survey round, the primary specification will be restricted to the data from other sources, dropping items that rely only on data from the mobile surveys. However, as a robustness check we will also use the mobile

survey responses, averaging responses over the pre/post midline period.

### **7.3 Family 3: Duration of Unemployment**

We may expect sustained unconditional cash transfers to result in a longer period of time spent searching for employment, potentially resulting in better matches. Thus, in this section, a lengthy period of unemployment may be a positive outcome if respondents are also searching for jobs and, ideally, ultimately able to find better employment (including self-employment). The questions asked in this family can be thought of as indicating the duration of unemployment, but do not, by themselves, determine whether the observed effect should be considered a positive or negative outcome; for that, one should consider the selectivity of job search, employment trajectory and quality of employment families.

1. Duration of unemployment: a) average duration of non-employment (\*). This will be constructed using an in-person survey question (H1a) and mobile survey questions (leftjob\_1\* and newjob\_2\*) in the preferred specification, counting each spell of non-employment only once (this is important, for example, if respondents answer multiple surveys about the same period of non-employment; the longest continuous reported spell would then be used) and averaging across spells of non-employment. newjob\_2\* gives duration of non-employment for those who have found a new job, but if someone left a job without finding a new one then leftjob\_\* will indicate the time of non-employment so far; b) longest period of non-employment since baseline (constructed using H1a, leftjob\_1\* and newjob\_2\*) (\*). For the job search index, we will restrict attention to question a), with someone with no periods of non-employment being counted as having “0” average duration of non-employment. As a robustness check, we will use only in-person survey data, but as the in-person survey is asked relatively few times, it is more likely some individuals will not report ever being non-employed during this period (the robustness check will be considered as a secondary

outcome in the FDR corrections). Finally, we will also consider results for item a) using the in-person survey data restricted to those who state they are searching for a job (G2) (i.e., the unemployed). Though this is an important outcome and will be considered as a primary outcome in the FDR corrections, it will not be included in the index to avoid double-counting. These individuals could be selected, so we also use a Heckman correction to account for this.

## **7.4 Family 4: Selectivity of Job Search**

This family of outcomes only applies to those who are searching for a job, so will be considered separately. In the main specification, the mobile survey data (jobsearch\*) will be replaced by the corresponding in-person data (i.e. H6 and H6a, H7-H11) for those who are unemployed, as we expect a higher response rate to the in-person surveys; the mobile data will be scaled to match the mean of the respective in-person survey data for those for whom those data are available. As a robustness check, we will use only the mobile survey data. For those who are unemployed, we will also consider the in-person survey measures separately, bearing in mind there will be selection in employment status that we will address using a Heckman correction. In addition, we will descriptively consider the conditions under which an unemployed respondent would take a job (e.g. high income potential, potential for advancement, convenient location, etc.) (H5). This measure will not be included in the index, being secondary, and not included in the FDR adjustment for primary outcomes, but again be adjusted based on the number of tests in the primary analyses plus the number of tests in the secondary analyses. Further, as a participant's subjective expectations as to when they would find a job may be incorrect, this outcome will also be presented separately as a secondary outcome and not combined with selectivity in the index.

1. Selectivity of job search: a) if searching for a job, how long the respondent would be willing to search (H6 and H6a and jobsearch\_4); b) willingness to make sacrifices for a job (e.g. take a low-paying job, take a job in a different field, take a job with a long commute, etc. We will

count the total number of sacrifices the individual says they are willing to make as a negative indicator of selectivity) (H7 - H11 and jobsearch\_5\*); c) if searching for a job, ln income of jobs applied for, as gauged by mapping job titles of jobs applied for to the BLS Occupational Outlook Handbook (jobsearch\_9\*) (if multiple job titles are provided, the ln of the average income will be used); d) willing to take any job (H4a).

2. Expectations: a) if searching for a job, perceived likelihood of finding a job they would accept in 6 months (averaging across H12 and leftjob\_3, counting each spell of unemployment only once).
3. Reservation wage: a) the participant's reservation wage (H5b, H5c).

In addition to building an index as described above, we will also ask some questions that will be analyzed through discrete choice models. First, we will model selectivity of job search. We will ask respondents about the jobs they have applied for and match the job titles they enter with job titles in the BLS Occupational Outlook Handbook, which provides characteristics of those jobs such as median pay, job outlook, and on-the-job training, all of which we will assume independently enter into participants' utility function. We will then model their choice of jobs to apply for as a discrete choice problem in which they select which jobs to apply for among those which they might realistically attain based on their previous education and work experience. (For each job title, the BLS also contains details on typical entry-level years of education required and work experience in a related occupation, both of which we have data on for study participants.)

Second, we will separately estimate a discrete choice model of job selection in order to better understand their preferences and their job search strategy. Job selection may differ from job search because job search is a function of both participants' expectations of employer preferences and participants' own preferences, while job selection more narrowly captures participant preferences (though respondents may still select jobs in part because of their expectations for future employment). For this second component, we will ask participants for details about job offers that they

**Table 2:** Attributes and Levels for the Job Preferences Discrete Choice Experiment

<b>Attribute</b>	<b>Levels</b>
Work from home	Required; allowed; disallowed
Flexible schedule	Can set own, though number of hours is fixed; cannot set own
Number of hours per week	10; 20; 30; 40; 50; 60
Hourly wage*	\$7.25; \$9.50; \$12; \$14.50; \$17; \$19.50; \$22; \$24.50; \$27; \$29.50

\* The current minimum wage in Texas is \$7.25 per hour and the current minimum wage in Illinois is \$12 per hour. Since it is important to elicit preferences over the entire space of possibilities that respondents may face, respondents in Texas may view some choice sets with lower options than they would in Illinois. However, the amounts are otherwise designed to align as closely as possible. We also include many different levels for this attribute to allow us to capture a wide range of possible preferences.

received that they rejected, including details about wages and non-wage benefits such as the ability to work from home, flexibility in scheduling, and the number of hours of work per week. This will enable us to estimate preferences by considering the offers rejected and accepted, similar in identification strategy to Stern (2004) but leveraging a multinomial logit regression. Since the set of job offers that people report may not be very comprehensive, we will supplement this analysis with a discrete choice experiment for robustness. Similar to Mas and Pallais (2017) and Maestas et al. (2018), we will provide respondents with repeated sets of two hypothetical jobs with different randomly assigned attributes to choose between. Table 2 lists the different attributes included in this experiment and their levels.

The analysis of job offer acceptances and hypothetical choices will allow us to construct willingness-to-pay estimates, i.e., how much individuals would be willing to trade off in terms of wages for each non-wage benefit:

$$WTP = -\beta_n/\beta_w$$

where  $\beta_n$  is the coefficient on each non-wage benefit, in turn, and  $\beta_w$  is the coefficient on wages. The estimates of the discrete choice model for job applications will help us understand how transfers affect job seekers' job application strategies. We will include several standard attention checks in the experiment, all of which participants need to pass in order for their responses to be included

in this analysis.

For both the model of job search and the model of job selection, we will use the experimental variation provided by the transfers to consider whether the cash transfers affect respondent choices.

Finally, we will also ask participants for details of job offers received, including wages offered and any non-wage benefits such as ability to set own hours; the ability to work from home; perceived advancement opportunities (Likert scale); employer discretion at setting hours; and whether on-the-job training is provided. We will ask participants which of these offers, if any, they accepted. This will enable us to examine whether the transfers enable them to turn down more job offers, and whether the jobs they ultimately accept are higher-quality jobs according to their own reported preferences. We will also consider whether estimated time and risk preferences appear to be associated with greater selectivity or duration of job search and whether these estimates depend on the log of baseline household income.

## **7.5 Family 5: Employment Trajectory**

This family captures the respondents' employment trajectory. We will ask respondents to provide information on their current main job at baseline as well as their most recent job that they are not doing anymore, conditional on their having that other job within the past year (if employed) or within the past three years (if unemployed). This means we will be able to make three types of comparisons for the first five outcomes in this family (the latter outcomes do not depend on one's past job):

1. Comparing pre- and post-treatment trendlines. Changes in the trend lines will be exploratory only as they are based on the relatively small and likely selected set of people who worked at baseline and had another job within the past 12 months (in particular, considering variables L1a, L5, L3b, L2, L3, and L3a).
2. Investigating changes from respondents' most recent job using data on their current job if

employed or most recent job if unemployed but having held a job in the last three years.

3. Investigating changes from respondents' current job (at baseline) treating those who are unemployed as having responses of 0 for all items except 1b), 1c) and 1e), 4) and 5), where it would not make sense and these items would need to be excluded.

Our preferred approach will be #3, to conserve sample size. Still, results for other approaches may be presented as exploratory, and results from #1 may be particularly interesting. Where applicable, we will also plot trajectories graphically.

1. Trajectory of occupations: occupation will be elicited through job title (I5 and J5b, weighted by number of hours worked at that job) and matched to BLS data from the Occupational Outlook Handbook. The following statistics will then be included as part of the index: a) ln of median pay (i.e.  $\ln(\text{follow-up median pay} - \text{baseline median pay})$ ); b) job outlook (%); c) typical entry-level years of education (12 = high school diploma or equivalent, ...); d) on-the-job training (0 = none, ...); e) work experience in a related occupation (0 = none, ...). As an alternative specification, we will use measures from mobile surveys, in which every time an individual reports getting a new job we ask its title (newjob\_4), for matching with BLS data, as well as asking how it compares to their old job on a number of dimensions (newjob\_17\*), and take the average across these questions.
2. Trajectory of earnings and benefits: a) ln of total annual work-related earnings (I7, I9, I9a, I10\_s\_1 through I15b for the primary job, and equivalent questions for each non-primary job or 0 for those who are unemployed); b) number of benefits (count of I16\_\_s\_\* and corresponding benefits for each other job), weighted by the number of hours worked at that job (I7 and J9\* or 0 for those who are unemployed).
3. Trajectory of work hours: a) hours worked at the respondent's current jobs (I7 and J9\* or 0 for those who are unemployed).

4. Trajectory of nature of work: a) whether the respondent was a regular employee or a temp or contract worker at their main job (I20).
5. Trajectory of stability of work: a) duration of each current job (I9, I9a, J10\*), weighting responses for each job by hours worked at that job (I7 and J9\*).
6. Aspirations: respondents will be asked to provide the job title they hope to have in 10 years' time (G15). This job title will be matched to BLS data and the index previously described will be re-computed for their aspirational job.
7. Subjective career prospects: a) whether the respondent thinks their career prospects have improved over time (Likert scale) (G14).

Outcomes 6 and 7, being subjective but very important, will be considered separately and not in the index.

The above measures consider the in-person survey data, but we will also consider the corresponding mobile survey-based questions where available, in an alternative specification. Further, we will separately report the extent of churn in jobs (job turnover), leveraging the frequent reporting of changes of jobs in the mobile trigger surveys (newjob\*, leftjob\*, incomchg\*, jobintro\_1\_1\_2, jobintro\_1\_2\_2) in conjunction with the in-person survey data, being careful to avoid double-counting (e.g. through the mobile and in-person surveys asking about overlapping time periods).

It should be noted that a respondent's most recent job that they no longer hold may have been either a main job or a secondary job. This does not pose a problem so long as we consider all jobs that an individual might hold at a given point in time. However, we cannot straightforwardly compare measures beginning with "I" (current main jobs), "J" (current secondary jobs) and "L" (most recent job they no longer hold) without further assumptions. We will approach this challenge by two approaches: 1) conservatively comparing only like to like across surveys (e.g., "I" and "J"



to “I” and “J” measures, ignoring “L” measures); 2) classifying each job into bins based on hours worked (e.g., less than 10 hours of work per week or 10 or more hours of work per week; less than 20 hours of work per week or 20 or more hours of work per week) and making comparisons only among jobs in the same bin. The conservative approach will be preferred for the main specification, but the latter will be required for approach #1.

Reasons for leaving past jobs will also be descriptively considered (L7, L7a, leftjob\*).

We will also separately consider whether there is any heterogeneity in the trajectory of employment outcomes by age and whether the participant had a college degree at baseline, but this will be exploratory.

## **7.6 Family 6: Quality of Employment**

This section captures various factors often associated with quality of employment. Unlike the other families, this family of outcomes and “Barriers to Employment” will focus exclusively on those who are employed and will not be combined with other questions among those not employed. Recognizing that there will be selection, we will use a Heckman correction to account for this selection and present results with and without this correction to aid interpretation. Some of the outcomes will have their sign reversed prior to combination in an index; this is noted in the text.

1. Adequacy of employment: a) if the respondent is employed part-time in their main job but would prefer to work full-time (negative) (I24); b) respondent would prefer to work more hours in their current main job (negative) (I8); c) the number of jobs held by the respondent apart from their main job (negative) (G4); d) the reason provided for the number of jobs (G5). When constructing an index, the last question will be considered descriptive. We will also count the total number of W-2s and 1099s from administrative records (also asked in M3 and M5) in an alternative specification.
2. Hourly wage: a) the respondent’s hourly wage (\*); if the respondent works more than one

- job, this is their total income from employment weighted by the number of hours worked at each job, using the income and hour questions noted in the financial health pre-analysis plan.
3. Employment quality: a) number of non-wage benefits (count of I16\_\_s\_1\* and corresponding benefits for each other job, weighted by hours worked at that job (I7 and J9\*); b) whether training is offered by the main employer (I17); c) whether formal training is offered at the main job (I17a\*=3 or 7); d) whether training is offered during work hours (I17a\*=4, 5, or 6); e) whether respondent must work a night shift / irregular shift / split shift / rotating shifts / be on-call (negative) (I22 and I23 and J18\*), weighted by hours worked at that job (I7 and J9\*). In an alternative specification we will consider the number of non-wage benefits provided by either the main job or other jobs (count of I16\_\_s\_1\* and J9\*, where each benefit is included at most once in the count) and present results descriptively by type of benefit (this enables us to consider, e.g., the share of individuals that have health insurance at either their main job or another job).
  4. Quality of work life: a) how hard it is to take time off (I28), with “never tried” recoded to the mean; b) flexibility of schedule (I28a, with answers of 1 and 5 coded as 1 and answers of 2, 3, or 4 coded as they are); c) advance notice of schedule (I27); d) whether job demands interfere with family life (negative) (I29); e) whether the respondent has decision-making input in their job (I30); f) safety and health conditions (workqual\_2); g) whether the work activities are boring (negative) (workqual\_3); h) whether the respondent experienced fair treatment by their supervisor (workqual\_4); i) whether the job is a good fit with the respondent’s experience and skills (I31); j) opportunities for promotion (I32); k) overall satisfaction (I19a); l) whether the respondent plans to leave the job in the next year (negative) (I34); m) whether the respondent faces age discrimination at work (negative) (workqual\_5); n) whether the respondent faces racial or ethnic discrimination at work (negative) (workqual\_6); o) whether the respondent faces sex discrimination at work (negative) (workqual\_7); p) satisfaction with

compensation (I18); q) satisfaction with non-wage aspects of the job (I19); r) number of stressors in their work environment (negative) (workqual\_8); s) whether a scheduled shift was cancelled with less than 24 hours notice in the last month (G13 and workhrs\_3). These questions apply to the respondent's current main job, and all are Likert scale measures. Only data from in-person surveys will be used in the primary specification in the case of more differential attrition in mobile surveys.

5. Stability of employment: a) whether the respondent is performing contract or freelance work as their main job or other job (negative) (I6 and J8\*), coding "don't know" as "no" and weighting responses for each job by hours worked at that job (I7 and J9\*); b) how many months they have been employed in the past year (K1\*); c) how long they have spent at their current main job or other job (I9, I9a, J10\*), weighting responses for each job by hours worked at that job (I7 and J9\*); d) how many jobs they have held in the past 12 months (negative) (G4 or 0 if unemployed + K3); e) whether their job has a salary (1) or they are paid hourly or by task, tips, etc. (0) (I10\* and J11\*), weighting responses for each job by hours worked at that job (I7 and J9\*); f) whether their job is a temp job (negative) (I20); g) how long the respondent expects to remain in their job (I21), taking the midpoint of each range and coding "one year or more" as 18 months. Participants will also be asked how many jobs they have held in the past 2 years (K4), which may have more variation than how many jobs they have held in the past 12 months but could potentially be less accurate; it could also include a few pre-treatment values at midline. For robustness, the index will also be calculated using K4 at baseline and endline in lieu of item d).
6. Formality of employment: a) the percent of reported income not on W-2s (negative) (using administrative records for the W2s and total income from the in-person survey as described in the financial health pre-analysis plan); b) whether the respondent reports any informal jobs such as babysitting, home repairs, home cleaning, cooking and catering, sewing (negative)

(M9\* excepting renting property). As an exploratory measure not included in the index, we will also note whether the respondent reports any “gig economy” jobs such as Uber, TaskRabbit, or online surveys (M9\*=9, 10, 13, 14), but we expect too few individuals will participate in one of these jobs to consider it except descriptively.

7. Relative firm rating: this measure will be constructed by linking the company names of respondents’ employers to data on those employers such as from Indeed.com ratings. The average firm rating will be considered an alternative proxy of quality of employment, conditional on there being at least 10 reviews, but since not all firms may have a rating this measure will be presented separately and not be part of the index for this family. The number of reviews and the number of employees, if reported, will also be used to descriptively discern whether individuals are more likely to be employed at larger or smaller firms. If fewer than 10% of participants work at a firm with a rating, these outcome will not be presented.

Since individuals may take on multiple jobs in part due to need, to aid in interpretation we will construct a parallel index that focuses only on respondents’ main job, ignoring any secondary jobs and not weighting responses across jobs by hours worked.

It is also possible that some people may prefer freelance or informal work. Thus, in an alternative specification we will calculate the same index excluding the stability and formality of work outcomes. We will also separately consider heterogeneity by whether the participant had a college degree at baseline, but this will be exploratory.

## **7.7 Family 7: Barriers to Employment**

As described under “Quality of Employment”, this family of outcomes will exceptionally be on only those who are employed and we will use a Heckman correction to account for selection.

1. Barriers to employment: a) whether the respondent had to miss work due to illness (G8); b) whether the respondent had to miss work due to lack of transportation (G9); c) whether the

respondent had to miss work due to lack of childcare (G10).

## **7.8 Family 8: Disability**

Disabilities can be exacerbated by certain jobs, and having the security of cash transfers could improve individuals' health. A large literature also finds that a growing number of people are claiming disability insurance, though in absolute terms this cannot account for much unemployment (Autor and Duggan (2003), Staubli (2011)). For these reasons, disability is an important outcome to capture, however, we may not expect much change in disabled status over the duration of the study, so this set of outcomes will be reported separately from other barriers to employment, which one may expect to be more responsive to the treatment. The primary outcome measure considered in this family is whether the respondent does not work due to disability, and beyond our interest in this measure for its own sake, we expect this measure to be useful as a control variable in other regressions.

1. Disability: a) whether the respondent has a disability (negative) (items dis\_1\_1 - dis\_1\_6); b) whether the respondent has a health problem or disability that limits the kind or amount of work they can do (negative) (dis\_1\_7); c) how much their worst disability or health problem limits the kind or amount of work they can do (negative) (dis\_2); d) how long this health problem or disability has affected the kind or amount of work they can do (negative) (dis\_3).

## **7.9 Family 9: Entrepreneurship**

In this section, we will gather data not just on entrepreneurial activities but on “precursors” to entrepreneurial activity like entrepreneurial intention and entrepreneurial orientation. We expect that more people will exhibit changes in entrepreneurial orientation or entrepreneurial intention than entrepreneurial activity, both because some barriers to entrepreneurial activity may remain even with the transfers and because some intentions may not translate to activity even in the absence of

any barriers.

1. Entrepreneurial activity: a) whether the respondent ever started or helped to start a business (entrep\_1); b) whether the respondent knows anyone who ever started or helped to start a business (entrep\_2); c) whether a family member who started a business lives in the respondent's household (entrep\_3).
2. Entrepreneurial intention: a) on a 0 to 10 scale, the respondent's interest in starting a business (entrep\_4); b) on a 0 to 10 scale, how likely they think they would be to start a business in the next 5 years (entrep\_6); c) whether or not the respondent has an idea for a business (entrep\_5).
3. Entrepreneurial orientation: a) the respondent's risk preferences, as gauged by respondents' choice of one of six lotteries based on the flip of a fair coin ( $Z1$ ), taking the midpoint of each range of consistent risk preferences; b) the respondent's self-reported willingness to take financial risks, on a 0 to 10 scale (gpref\_2). The latter measure follows Falk et al. (2015), while the first follows Eckel and Grossman (2002).

We will consider heterogeneity by age and whether the participant had at least some college education at baseline, but this will be exploratory.

## **7.10 Family 10: Human Capital Formation**

This section focuses on variables relating to formal and informal education. Receiving cash transfers may provide individuals with the security to pursue more education and cause individuals to work less in the short run in pursuing education. This set of outcomes can thus help interpret some of the changes we may see in other outcome families and also provide guidance as to whether individuals may be gaining new skills that could help them in the labor market.

1. Formal education: a) enrollment in post-secondary (associate, certificate, etc.) programs (\*); b) progress in program (total years of education completed); c) completion of GED or post-secondary programs; d) contact hours; e) area of concentration. Our preferred specification will use administrative data, and the last two measures will be descriptive and not included in the index or FDR adjustment for primary outcomes, and instead be adjusted based on primary plus secondary outcomes.
2. Informal education: a) participation in any other education, including on-the-job training, noncredit study, private study, online courses, or training offered by other groups such as employment centers (B19a = 6, school\_4 = 6, 7, 8, 9, 11, else 0 if no such participation); b) extent of participation in these activities (part-time = 1, full-time = 2: B19, school\_2, else 0 if no such participation). We also ask for hours of schooling for those pursuing it part-time but as few do this outcome will be exploratory only. Descriptively, we also ask why individuals are pursuing education (school\_5). These measures will be self-reported through surveys and any such participation over the entire study period counted.

The surveys also ask about formal education, and we will compare the survey responses with the administrative records. In particular, we will compare 1a, 1c and 1e with survey-based measures (B19a or school\_4 = 2, 3, 4, 5; new completion of any items in B20 of 3 or higher; and B20a and B20b, respectively). If we observe differences, it may be due to bias in self-reporting, which would help us gauge how much we should trust the self-reported measures of informal education. However, informal education could also be less likely to be remembered, or alternatively could be exaggerated. As relatively few participants may indicate they are pursuing informal education during the study, these outcomes will be considered exploratory.

To form the index, we will consider whether the respondent reported receiving any education or job training (a binary variable that takes the value 1 if B18 = 1 or G6 = 1 at midline or endline or if administrative records show education) and descriptively report whether or not individuals plan

to start a job training program ( $G6 = 2$ ).

Finally, we will descriptively consider hours spent on school, but this will be exploratory and not in the index (school\_3 if part-time according to school\_2 and 40 hours a week if full-time).

We will consider heterogeneity by age, but this will be exploratory.

## **8 Employment Outcomes: Administrative Data Measures**

In addition to the survey-elicited measures, we will also leverage administrative data, where possible. In particular, we are working to find a collaborator within the federal government to match individuals in our sample to tax return data and report back impacts on their records of aggregate income, including income as reported in W-2s, 1099s, passive income, and federal transfers. We are also seeking to use state UI data, which have the advantage that they are available quarterly. How these data will be used to capture income is discussed in a separate pre-analysis plan. We are additionally planning to use National Student Clearinghouse data for the education outcomes.

The administrative data play a direct role in outcome “a” under “Formality of employment” in the “Nature of employment” family, as described in the survey-based outcomes section. If the participant has W-2 income, this can also serve as an alternative measure of outcome “a” under “Stability of employment” in the same family. Both measures will be considered in alternative specifications, for robustness; however, the administrative data will be used in our preferred specification. Likewise, administrative data will be the preferred data source for the “Formal education” outcomes, while the survey data will provide robustness checks.



## 9 Conclusion

### 9.1 Known Limitations

Our study has several limitations. First, the limited nature of the RCT does not permit us to simulate the macroeconomic conditions of the government introducing an unconditional cash transfer program to all residents of the United States who meet broad eligibility criteria. If recipients are spending the money helping friends and family who would receive their own cash transfer under the policy, the treatment is diluted and the likelihood of the hypothesized effects is undermined. Similarly, the dispersed sample precludes our ability to capture the multipliers and general equilibrium effects identified in the theoretical literature and observed in studies in developing countries. The dispersed study also precludes studying the effect of sustained unconditional cash transfers on cultural attitudes towards work and other social spillovers. Despite these limitations, we selected a geographically dispersed population for several reasons. Most importantly, the intervention is very expensive and our sample size is constrained by the budget. A geographically saturated study would likely cost billions of dollars, and we would not have enough statistical power to detect effects with a geographically saturated study with our budget.

A second limitation is the time-bound nature of our treatment. The 3-year timespan of the intervention is obviously not the same as a perceived long-term guarantee, and individuals may behave differently knowing that the transfers are time-limited (Hoynes and Rothstein (2019)). Nevertheless, a study at the scale proposed in this analysis plan will allow us to provide timely evidence to inform ongoing policy debates and future research on this topic.

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