Design for "The causal effects of sustained unconditional cash transfers: Experimental evidence from two U.S. states" – Mobility and Housing Outcomes

Alexander W. Bartik* David E. Broockman† Sarah Miller‡
Elizabeth Rhodes§ Eva Vivalt¶

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 participants 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 geographic mobility and housing outcomes.

*Assistant Professor, University of Illinois at Urbana-Champaign. abartik@illinois.edu, https://alexbartik.com

†Associate Professor, University of California, Berkeley

‡Assistant Professor, University of Michigan Ross School of Business

§Research Director, Open Research Lab

¶Assistant Professor, University of Toronto

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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 richpoor 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*c*;*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, however, 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-Dehzooei 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.[1]

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.

[1] 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.[2] 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

[2]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 1000 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 additional 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 Populaton Comparison(ACS)				Study Sample		
	Full US Population			Study Counties	Eligible Mailer Respondents		Enrolled Active Survey Group
	Unweighted	Reweighted to Match Enrolled Sample FPL Distribution	Reweighted to Match Enrolled Sample FPL and County	Reweighted to Match Enrolled Sample FPL and County Type	Unweighted	Reweighted to Match Enrolled Sample FPL and County Type Distribution	Unweighted
	(1)	(2)	(3)	(3)	(4)	(5)	(6)
Panel A. Key active group stratification variables							
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
Panel B. Demographic Characteristics							
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 thoe 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 sttudy 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 reruitment methods. Column (5) is unweighted, while Column (6) is reweighted o match the enrolled sample FPL and county type distribution. Column (7) reports the unweighted mean of the ultimate enrolled actve 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`). 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.[3] 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.[4]

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%

[3]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.

[4]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`) 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.[5]

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

[5] 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[6] 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.[7] [8]

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

[6]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.

[7]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.

[8]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.[9]

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.[10]

The in-person enrollment proceeded as follows:

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

[9] 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%.

[10] 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.[11] 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

[11]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.

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 family-wise error rate (FWER) 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 FWER-adjusted p-values that control the probability of a false positive within the family of tests to be no more than the nominal level. We will use the Westfall and Young procedure as outlined by Anderson (2008). We will report per comparison p-values in addition.

To estimate effects on each outcome, unless otherwise noted, we will estimate a standardized treatment effect across all components in the family by estimating effects for each component jointly with pooled OLS and standardizing each component's estimate by the standard deviation of the component in the control group (following, e.g., Finkelstein et al. (2012)). Note that, as Finkelstein et al. (2012) also note, this implicitly weights every component within an outcome equally. The exceptions are cases where there is an existing procedure for combining outcomes, such as for established econometric or psychometric scales; we will explicitly note these cases.

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 Midline Data

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. 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 it. We are fortunate that we will have a variety of administrative data outcomes which will not be subject to this issue.

In addition to identifying a large subsample ex post that did not exhibit differential attrition, as defined by their ex ante responsiveness during the long baseline, we will consider two-stage sampling for midline and endline data collection to minimize attrition-related bias by concentrating resources and efforts on a subset of the cases that are the most difficult to reach (and adding weights accordingly).

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.

For any unanticipated issues that do not appear in the PAP, we will use the Green Lab SOP (see Lin and Green 2016), at `https://github.com/acoppock/Green-Lab-SOP`.

6 Unconditional Cash Transfers and Housing and Geographic Mobility

Some of the most important choices households make are what homes, neighborhoods, and cities to live in. Receiving large, unconditional cash transfer could affect people's mobility choices, including housing choice, neighborhood choice, and long-distance migration, through five primary channels. First, unconditional cash transfers may relax household liquidity constraints, allowing them to make the upfront payments required to invest in better housing, buy a home, or move to a new neighborhood or labor market. Second, direct income effects of unconditional cash transfers may lead even individuals without liquidity constraints to make different mobility and housing choices.[12] Third, housing, neighborhood, and migration decisions are joint decisions with labor supply and many other household decisions. Consequently, if unconditional cash transfers have effects on other decisions that are made jointly or interact with mobility and housing decisions, then mobility and housing decisions may also be affected. For example, households that change their labor supply decisions may simultaneously change the neighborhood they choose to live in because their commuting needs may change. Fourth, unconditional cash transfers may reduce stress and cognition, making it easier for households to make forward looking investments, such as moving into a new house, neighborhood, or labor market. Finally, unconditional cash transfers may make it easier for households to smooth housing consumption when experiencing labor market or other shocks to earnings, for example avoiding eviction, moving in with family, or other types of housing instability. These mobility decisions are of particular interest because they may be one mediating mechanism behind effects of unconditional cash transfers on labor market outcomes, consumption, health, children's outcomes, criminal justice, or financial outcomes.

Researchers have investigated the income elasticity of housing choices and spending, neighbor-

[12]For a household with an average annual income of $30,000 in after-tax labor market income over the lifecycle, the value of the basic income provided to the treatment group represents roughly 6% of the value of discounted lifetime income (using a discount rate of 5 percent), representing a moderate sized lifetime income effect.

hood choice, migration, and other mobility decisions in a number of settings. In the US, evidence on these topics comes from the Negative Income Tax (NIT) experiments (also called the Income Maintenance Experiments) and from a number of housing allowance and voucher experiments run by the Department of Housing and Urban Development (HUD), starting with the Housing Allowance Demand Experiments in the 1970s, and continuing with the Moving to Opportunity (MTO) and Moving to Work (MTW) demonstration projects in the 1990s and 2000s.

Both the NIT and housing voucher experiments or allowance experiment tend to find positive, but modest, elasticities of housing expenditures with respect to income, with estimates ranging from .10 to .50 (Hanushek et al. (1986), Friedman and Weinberg (1981)). Experiments in the US on the effects of housing vouchers find that recipients choose neighborhoods that are less disadvantaged on a number of dimensions, such as having lower crime and poverty rates (Kling, Liebman and Katz (2007), Collinson and Ganong (2018)), although the magnitudes are not always large or precisely estimated (Jacob and Ludwig (2012)). The NIT studies were not designed to estimate effects on neighborhood choice.

Many of the policy experiments described above precluded using the cash transfers or housing allowances to migrate across labor markets (or cities in some cases). One exception was the Seattle and Denver sites of the NIT experiments. Keeley (1980*a*) finds that the NIT payments increased migration rates across different labor markets (defined as metropolitan areas) by about 50%, from 7% to around 11%. He finds that the NIT also induces migrating households to choose metro areas with higher levels of natural amenities.

In developing country contexts, several studies have found large effects of income shocks on migration (see e.g. Bryan, Chowdhury and Mushfiq (2015) and Angelucci (2015)), while others find negative effects (Abramitzky, Boustan and Eriksson (2013)). Bazzi (2017) argues that this can be reconciled by the fact that many income shocks studied in the literature may both reduce liquidity constraints preventing migration, while also increasing the opportunity cost of moving if income opportunities rise in the home labor market (which reduces migration). However, it's unclear how much results from lower-income countries generalize given the greater prevalence of seasonal migration, the more prominent role of joint migration with extended family or friends, and differences in migration opportunities and costs. Additionally, some of these studies (i.e. Bryan, Chowdhury and Mushfiq (2015)) concerned transfers that were explicitly framed to participants that they could be used for a particular purpose (a bus ticket), which also may influence their affects relative to an unconditional cash transfer that is unframed.

Our study differs from the above literature in several notable dimensions. First, in contrast to much of the US literature, we study an unconditional cash transfer that isn't restricted to a particular type of spending (such as the housing allowance programs), taxed away at a high rate (such as the NIT experiments), or restricted to be used in particular cities. Second, most of the existing US experiments focus on residents of large cities, while we include in our sample households in small cities, large cities, suburban areas and rural areas. The mobility response of households in these different geographic types may be quite different and important for thinking about the effects of a basic income program. Third, we focus on a relatively young sample. Migration and mobility of all types decline sharply with age, so in this younger sample we may expect to see greater location changes, potentially including some individuals making long-distance moves to seek out new labor market or personal opportunities. Finally, we ask more detailed qualitative questions of respondents of their perceptions or their housing unit, neighborhood, and labor market, mobility search behaviors, and self-reported reasons for moving, providing a richer picture of mobility outcomes and decisions than previous work.

We will disaggregate outcomes across four different dimensions along which we expect heterogeneity in mobility outcomes to be particularly important. These dimensions are household liquidity[13], baseline labor market quality (as measured by the Family 7 index below), baseline neighborhood quality (as measured by the Family 5 index below), and whether or not the individ-

[13]Which we'll measure as liquid assets plus unused lines of credit divided by consumption per month, including debt service payments as a part of consumption

ual has children who live with them. We disaggregate along these four dimensions because they are either related to mobility constraints (liquidity), potential mobility gains (neighborhood and labor market quality), and preferences over neighborhood or labor market characteristics (whether the individual has children who live with them(. These disaggregated results will be presented as main results along with estimates pooled across households of all types.

Finally, we will also analyze household finances, labor market, and material well-being outcomes: i.e. if we do observe recipients moving to different housing units, neighborhoods, or cities, how are these choices affecting the household financial position, labor market outcomes, or material well-being? Financial, labor market, and material well-being outcomes are described in separate PAPs.

7 Mobility Outcomes: Survey and Location-Based Measures

This section describes the survey based measures that will be used in each of ten families. In addition to the survey-elicited measures, we will incorporate measures of neighborhood and labor market characteristics quality using data from publicly available data sources. We call these "location-based" measures. These include data from the US Census Bureau and the Bureau of Labor Statistics on neighborhood and labor market characteristics,public safety data on crime rates, data on local amenities from Safegraph, Google-Maps and other sources, local government data on school inputs and outcomnes, measures from other researchers and neighborhood upward mobility levels, and a variety of other publicly available data sources.

Apart from being considered separately, the measures in each family will also be combined into an index by whether or not the household has children, the households' liquidity level, and the household's baseline neighborhood and labor market quality, and this index will constitute an additional outcome variable. We expect each measure to have different degrees of relevance and importance to the overarching theme of the family. Nonetheless, we will generally not impose our own views of the relative importance of the individual components within an index, but will standardize each outcome and create an additive index of the standardized items (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. Measures of particular interest will be flagged in the following subsections.

In several of the sections below, we refer to "neighborhoods" and "labor markets". We will define neighborhoods using the 2010 US Census Tract definitions (Bureau (2019)) and labor markets by the commuting-zones constructed for the year 2000 by the Economic Research Service (ERS) at the Department of Agriculture (Tolbert and Sizer (1996),Service (2020)), unless otherwise stated.[14]

Unless otherwise mentioned, we will use the self-reported place of residence in the end-line for measures of mobility, housing quality, neighborhood quality, labor market quality, and home-ownership (Families 1-9). We will use a combination of the monthly trigger modules, the midline survey, the endline survey, and administrative data to measure household housing unit moves, living with family friends, and other housing instability measures in Family 10. In descriptive analysis and robustness checks, we will explore the time-path of moves during the study period.

7.1 Family 1: Moved Housing Units

We will ask respondents about their current address, housing and neighborhood search behavior, as well as asking related questions, described below. We will supplement these survey measures with administrative data on place of residence from administrative data sources, which we will describe in more detail below. This family is divided into three groups of outcomes:

1. Changed housing units*
2. Active Search for new home: a) Interested in moving homes, b) Looking for a new home, c) Any active housing-search behaviors in the last quarter.

[14]For some measures, census tract measures are not available or are not appropriate. For example, for measures of local amenities we will use the weighted number of amenities within a fixed distance of the respondent's exact address.

3. Reason for changing housing units. This outcome will be descriptive only.

7.2 Family 2: Moved Neighborhoods

As with the measures in Family 1 on moving homes, we will construct measures of moving neighborhoods using questions on current address and housing search behavior, as well as asking related questions, described below. We will supplement these survey measures with administrative data on place of residence from administrative data sources, which we will describe in more detail below. This family is divided into three groups of outcomes:

1. Changed neighborhoods : a) neighborhood (census tracts) different from previous neighborhood*, b) New residence at least 2 miles from previous residence c) changed jurisdiction (i.e. moved cities), d) changed school district, e) distance moved if respondent moved neighborhoods but did not leave the labor market. Outcomes (b)-(e) will be descriptive only.

2. Active Search for New Neighborhood: a) Interested in moving neighborhoods, b) Looking for a new neighborhood, c) Any active new neighborhood-search behaviors in the last quarter, d) # of active neighborhood search behaviors in last quarter

7.3 Family 3: Moved Labor Markets

Households may take advantage of a basic income to make investments in longer-distance labor market moves, such as changing counties, labor markets, states, or regions. As with Families 1 and 2, we will measure workers' labor market using their self reported address and supplement these data on self-reported addresses with administrative data on addresses.

1. Long-distance migration: a) moved labor markets* (i.e. commuting zones), b) moved cities, c) moved counties, d) moved states, f) changed census divisions, g) changed census regions, h) number of miles moved (including 0s), i) number of miles moved conditional on moving labor markets.. (b) - (i) will not be included in the index and will just be descriptive.

2. Long-term migration search: a) looking to move to different area, b) # of actions taken in trying to move to new area.

3. Reported reason for moving labor markets. This last question is just descriptive and will not be included in the index.

7.4 Family 4: Quality of Housing

Households may take advantage of unconditional cash transfers to move into higher quality housing or housing that is better suited to their families needs. As described above, this may reflect both the direct income effects of a basic income, but also the relaxation of liquidity constraints preventing the paying security deposits, moving costs, or down payments and possibly changes in bargaining power with the landlord (if, for example, tenants can more credibly threaten to move out). Note that we will include all study participants in the sample, regardless of whether or not they changed housing units during the course of the study.

We will report descriptive results where we disaggreggate the change in quality of housing for movers by self-reported reason for changing housing units.

1. Number of bedrooms per household member

2. Number of bathrooms per household member

3. Plumbing, electrical or heating problems

4. Rats, mice or bugs (self-reported)

5. Noise (self-reported)

6. Too little space (self-reported)

7. Conflict with landlord (self-reported)

8. Overall, self-reported, unit quality

9. Type of housing unit (i.e. detached single family, attached single family, 2-3 unit building, 4-9 unit building, 10 or higher unit building). This last outcome is descriptive only.

7.5 Family 5: General Quality of Neighborhood

This section details outcomes related to the general quality of respondent's neighborhoods. Households may use a basic income to choose a neighborhood with a different mix of characteristics, such as transportation access, public safety, and other neighborhood characteristics. This family focuses on general quality, while Family 6 focuses on neighborhood characteristics particularly valuable to families with children. We measure these neighborhood characteristics both by merging in information from public sources on neighborhood characteristics and from directly asking participants about their perceptions of their neighborhood. Note that we will include all study participants in the sample, regardless of whether or not they changed neighborhoods during the course of the study.

We will also report descriptive results where we disagreggate the change in quality of neighborhood for movers by self-reported reason for changing neighborhoods.

1. Local amenities: a) # of super-markets in neighborhood, c) # of retail businesses in neighborhood, d) # parks in neighborhood, e) # of libraries in neighborhood, f) # of restaurants and bars in neighborhood. We will measure these amenities by using Safegraph data (SafeGraph (2020)). We will compute the weighted number of entities within a one to two mile radius of the individual's residence. We will only include entities that the Safegraph data suggests are open (i.e. the entity has a minimum number of visits during recent time periods).

2. Job-access: a) job market-access (computing by weighted average distance to jobs constructed by Owen and Murphy (2018)), b) Average commute-time in neighborhood, and c) respondent's own, self-reported, commuting time. The last two questions will be descriptive.

Own, self-reported commute time will only be computed descriptively only for households who work.

3. Pollution: a) Satellite based measure of PM 2.5 in most recent available year (vanDonkelaar and Martin (2019), Hammer et al. (2020)). We will use the North American Regional Estimates (VA.NA.03) measured using $.01° \times .01°$ grids. We will then merge this grid with tract boundaries from the Census Tigerline shapefiles (Bureau (2019)).

4. Neighborhood disadvantage: a) Poverty rate, b) housing unit vacancy rate, c) adult unemployment rate, d) adult employment to population rate, e) share of children under the age of 18 living within only one-parent, f) median household income. These will all be computed using the American Community Survey (ACS) census tract five-year pooled sample.

5. Public Safety: a) neighborhood violent crime rate, b) neighborhood property crime rate.[15] c) Respondent self-reported feeling of safety during the day, e) respondent self-reported feeling of safety at night, e) any household member purse snatched in previous 12 months, f) any household member threatened with knife or gun in previous 12 months, g) any household member beaten, assaulted, stabbed, or shot in previous 12 months.

6. Social environment: a) self-reported good relations with neighbors, b) self-reported people can be trusted.

7. Racial Segregation: This outcome is not necessarily ordered in a clear way, so it will not be included in the index, but is still an important component of housing and neighborhood choice: a) Neighborhood racial dissimilarity index relative to commuting zone average (i.e. the proportion of the neighborhoods residents that would have to move to equalize the census

[15] If we can obtain tract level crime data from the primary police department covering tracts where at least 75% of where the sample lives, we will measure census tract violent and property crime rates from the cities where tract level crime data is available and interpolate crime rates using police department reported crime and census tract characteristics. If we cannot obtain city-level crime data from at least 75% of the sample, we will instead use police department level crime data or an alternative data source.

tract racial and ethnic characteristics to the commuting-zone average), b) Isolation of individual's own ethnic and racial group (i.e. the proportion of respondent's own race/ethnicity members in census tract). For the purposes of these outcomes, we will group race and ethnicity into Non-Hispanic White, Non-Hispanic Black, Hispanic, and other. We will use the ACS census tract level characteristics data to measure the racial and ethnic composition of neighborhoods.

7.6 Family 6: Quality of Neighborhood for Children

This section details outcomes related to the quality of respondent's neighborhoods for children. Households may use a basic income to choose a neighborhood with better characteristics for their children, such as school average achievement, neighborhood upward mobility, presence of parks, and concentration of other school-aged children. We measure these neighborhood characteristics both by merging in information from public sources on neighborhood characteristics and from directly asking participants about their perceptions of their neighborhood. Note that we will include all study participants in the sample, regardless of whether or not they changed neighborhoods during the course of the study.

We will report descriptive results where we disaggregate the change in quality of the neighborhood for children for neighborhood movers by self-reported reason for changing neighborhoods.

1. Childhood focused local amenities: a) # parks in neighborhood, b) # of playgrounds in neighborhood, c) # of schools in neighborhood, d) # of libraries in neighborhood, e) of daycares in the neighborhood. We will measure these amenities by using Safegraph data (SafeGraph (2020)) and measuring the weighted number of entities within a one to two mile radius of the individual's residence. We will only include entities that the Safegraph data suggests are open (i.e. for example the entity has a minimum number of visits).

2. Local school composition adjusted test-scores: a) nearby public elementary school aver-

age achievement relative to student characteristics, b) nearby public high-school average achievement. These measures will be computed by using the Texas (Agency (2019)) and Illinois (of Education (2019)) state publicly released test scores and district boundaries and school locations from the Institute for Education Science, the National Center for Education Statistics' School Locations and Geo-assignments file (for Education Statistics (2020)), and supplemented with data from other states that participants may move to and data compilers like SchoolDigger (SchoolDigger (2020)).[16] To make units comparable, we will convert school average achievement levels to z-scores. We will adjust for school student composition and local characteristics by estimating models predicting test scores using school effects and student, school, and neighborhood characteristics. We will use the estimated school effects in the index. For school level-measures, households will be linked to their zoned elementary or secondary school when available. When within-district zoning boundaries are not available, we will link households with the closest elementary and high school within their school district.

3. School inputs: a) Elementary school pupil-to-teacher ratio (school-level), b) Secondary school pupil-to-teacher ratio (school-level), c) average primary teacher salaries (district level), d) district level secondary school teacher salaries (district level), e) primary student spending-per-pupil (district level), f) secondary student spending-per-pupil (district level). As above, for school level-measures, households will be linked to the closest elementary and high school within their zoned school district. We will use the same data-sources as described above.[17]

4. Neighborhood upward mobility: a) Chetty et al. (2018) neighborhood upward mobility measures (or most recent version of those measures)

[16]Note, we will only include these educational measures if we can construct data on test scores and school characteristics closet public school for at least 75% of the sample.

[17]Note, as above, we will only include these educational measures if we can construct data on test scores and school characteristics for at least 75% of the sample.

5. Family friendliness: a) share of households with children under 18 in the household, b) share of population that is children under the age of 18. Both of these variables will be measured using the American Community Survey (ACS).

7.7 Family 7: Quality of Labor Market/Metro area

In this section, we present outcomes related to the quality of the labor market or metro area that the respondent lives in. Unconditional cash-transfers may facilitate long-distance migration to geographic areas with desirable features, both by allowing those who would not move otherwise to move, and by allowing households to make more costly or difficult moves. These longer distance moves may allow households to move to better labor markets, closer to family or friends, or to areas with better public or private amenities. Note that we will include all study participants in the sample, regardless of whether or not they changed labor markets during the course of the study.

We will report descriptive results where we disagreggate the change in labor market quality for labor market movers by self-reported reason for moving labor markets.

1. Labor market amenities: a) violent crime rate, b) property crime rate constructed using the Federal Bureau of Investigation (FBI) Uniform Crime Records (UCR) following a procedure similar to the one used in Bartik et al. (2019) to construct county-level crime-records and then aggregate , c) per-capita school-spending constructed from the average of the Census of Local Governments in 2017 and 2022, d) natural amenities index from data constructed by McGranahan (1999) f) Chetty and Hendren (2018*b*) estimates of labor market upward mobility for children.

2. Labor market quality: a) employment to population ratio for ages 25 to 64 for respondent's education group, b) median annual income for respondent's education group, e) BLS projected job-growth for respondent's education group, e) recent population growth for respondent's education group using the American Community Survey (ACS). Measures a) and b)

will be computed using ACS county-level data aggregated to the labor market (commuting-zone) level. Outcome c) will be measured using the local-area employment growth projections derived by state Labor Market Information (LMI) for each state where respondents live (these projections are created based on the BLS national employment projections). Occupation level predictions will be converted to educational group predictions by constructing a weighted average of occupation growth (weighting using baseline employment share) for occupations in the respondent's education group. Educations groups will be defined as less than high school, high school, some-college and associates degree, and college plus.

7.8 Family 8: Housing costs

1. Own housing costs: a) Monthly rent for renters or imputed monthly rent for homeowners based on estimated value of owner-occupied home.

2. Neighborhood Housing Costs: a) median home-prices from the American Community Survey (ACS), b) median home-prices using administrative house price data, c) median rent from the ACS, d) administrative data on local rents. a) and b) will be equally weighted to create a measure of local home-values and c) and d) will be equally weighted to create a measure of local rents. These measures will then be combined using the census tract share of renters (measured using the ACS).[18] This outcome will be descriptive only and will not be included in the family index.

3. Labor market housing costs: a) median home-prices from ACS, b) median home-prices using administrative house price data, c) median rent from the ACS, d) median rent using administrative data on rents. a) and b) will be equally weighted to create a measure of labor market home-values and c) and d) will be equally weighted to create a measure of local rents. These measures will then be combined using the commuting zone share of renters

[18] Outcomes c) and d) will only be included if we are able to link neighborhoods with administrative data on housing unit values and rents.

(measured using the ACS).[19] This outcome will be descriptive only and not included in the family index.

7.9 Family 9: Housing Stability

Unconditional cash transfers may change recipients' ability to smooth consumption in responses to income or other shocks. For many individuals, housing takes up a large share of consumption and, consequently, households must sometimes make significant changes to their housing situation in response to economic changes. This housing instability can have significant effects on individuals and their families.

1. # of housing moves above one
2. Any evictions/foreclosures
3. # Evictions/foreclosures*
4. Lived temporarily with family or friends
5. Stayed in shelter, car or other non-permanent housing
6. Report struggling to pay rent or mortgage
7. Borrowed money to help pay rent or mortgage

7.10 Family 10: Homeownership

In this outcome, we discuss outcomes related to homeownerhip. A basic income may make it easier for households to own their own home by helping them to afford a downpayment, be approved for a loan, or make mortgage payments and avoid foreclosure.

[19] Outcomes b) and d) will only be included if we are able to link individual addresses with administrative data on labor market level housing values and rents.

1. Homeownership: a) Does the respondent own their residence*, b) became homeowner (previously renter). The second outcome is just descriptive.

2. Homeownership sharing: a) Homeownership shared, b) number of people ownership is shared with. This second outcome is just descriptive.

8 Mobility Outcomes: Administrative Data Measures

In addition to the survey-elicited measures, we are seeking to use state unemployment insurance data and credit report data, which will provide information on place of residence. We will also seek to match individuals using address information to private and public administrative data on the value or rental costs of their homes and compile public records of evictions and eviction related court proceedings. When both administrative and survey measures of home-values exist, we will use both measures, as described above. When both administrative and survey measures of monthly rent exist, we will use survey measures.

The administrative data will also play a direct role in measuring evictions and eviction related court proceedings under “b” under “Housing Stability” in the “Migration” family. When both administrative and survey measures of evictions outcomes exist, we will use administrative measures of evictions as our preferred measure and use survey based measures as a robustness check.

Finally, the administrative data also play a role in measuring all outcomes in Families (1)-(8), which require knowing whether a person’s address (including whether they moved houses, neighborhoods, or larger geographies and matching them with neighborhood characteristics). However, there are complications with using credit records, unemployment insurance data, and other administrative measures to measure location (DeWaard, Johnson and Whitaker (2019)). Most notably, these measures disproportionately miss households who do not have credit records, are unemployed, or do not file taxes, which are disproportionately low-income households. For example, Brevoort, Grimm and Kamabara (2016) find that Commercial Credit Panel (CCP) misses roughly

11 percent of US adults overall and up to 30 percent of adults in low-income neighborhoods. This missing data problem in low-income neighborhoods is exacerbated during business cycle fluctuations Wardrip and Hunt (2013). Consequently, although we'll use administrative data to measure place of residence for alternative specifications and robustness checks, survey data measures of place of residence will be used in our preferred specification.

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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