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How Do Low-Income Households Respond to Basic Income? Experimental Evidence from Minneapolis*

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Abstract: We study a randomized controlled trial that gave $500 per month for 24 months to 200 low-income people in Minneapolis, Minnesota. 329 people served as controls and received $150 for completing surveys every six months. These basic income payments equal one-third of annual income for the average participant. We pre-specified several methods to address large differential attrition rates and adjust statistical inference for multiple hypothesis testing. We find that basic income causes improvements in food security, housing stability, and financial security; has no effect on labor supply; and improves measures of mental health and self-reported well-being while participants are receiving payments.

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In 2022, the poorest 10 percent of American families, comprising 20 million people, lived on less than $17,100 per year (Guzman and Kollar 2023, Table A-4a). This *equaled* the median cost of rental housing and was just over twice the cost of the 10th percentile rental unit.[1] Their incomes are also unstable and their consumption of necessities, such as food, varies substantially more than it does for richer families (Fisher and Hardy 2023, Hardy and Ziliak 2014). Facing uncertainty and resource scarcity, many very low-income Americans have trouble meeting basic needs (Rabbitt et al. 2024), achieving and maintaining good mental health (Sareen et al. 2011), and planning for their own and their children's future.

In response, policy makers have begun to (re)consider "basic income" programs that give cash benefits with simple (or potentially no) eligibility criteria. More than 150 cities, counties, and states in the US to date have operated small-scale pilot studies or, in places like Cambridge, Massachusetts, actual basic income programs (Stanford Basic Income Lab 2020). Advocates argue that basic income would reduce income inequality, insure against income shocks, facilitate human capital investments for recipients and their children, improve worker mobility, and raise recipients' well-being. Those who view it as a *replacement* for existing safety net programs also describe basic income as cheaper to administer and less burdensome to apply for and receive (Murray 2006). Critics, however, argue that basic income and the tax mechanisms necessary to finance it would disincentivize work and human capital accumulation, raise prices, and lead to spending on socially undesirable goods.

Adjudicating these claims is challenging because we lack good estimates about which behaviors would respond to basic income and by how much for the populations such programs would target.

[1] These statistics refer to gross rent measured in the 2022 American Community Survey (Ruggles, Flood, Sobek , et al. 2024).

For example, estimated effects of existing programs need not apply to basic income. The maximum Earned Income Tax Credit (EITC) amount is close to what many basic income pilots have paid per year ($7,430 versus $6,000), but the EITC's role in encouraging work is thought to arise from the fact that, unlike basic income, it subsidizes wages at low earnings levels (Bastian 2020, Eissa and Liebman 1996, Kleven 2024).[2] Credible income effect estimates, on the other hand, come from populations like Alaska residents (Jones and Marinescu 2022), enrolled members of the Eastern Band of Cherokee (Akee et al. 2010), who differ from the typically urban, low-income populations targeted by existing basic income initiatives, or from lottery players who typically received a large lump-sum transfer (Golosov et al. 2023, Imbens, Rubin, and Sacerdote 2001). This suggests a need for direct evidence on the causal effects of regular income payments for different outcomes and populations.

Valid causal estimates can also inform model-based analyses of scaled-up basic income policies that incorporate dynamic and general equilibrium effects. Several recent papers calibrate equilibrium life-cycle (Conesa, Li, and Li 2023, Daruich and Fernández 2024, Guner, Kaygusuz, and Ventura 2023, Luduvice 2024) or search (Jaimovich et al. 2024, Rauh and Santos 2022) models of universal basic income programs. They reach opposite conclusions about the welfare effects depending on what kind of behaviors, policies, and baseline economies they choose to model. Credible evidence on how low-income households respond to basic income can therefore discipline key aspects of these more sweeping structural exercises.

This paper reports estimated effects of basic income on measures of economic stability, subjective and psychological well-being, and labor supply from a randomized evaluation across 529

[2] Some of the earliest randomized evaluations of social policy in US history, the negative income tax experiments, varied benefit levels and benefit tax rates simultaneously (Ashenfelter and Plant 1990) unlike most targeted basic income proposals under consideration today.

households in Minneapolis, Minnesota. The pilot targeted low-income residents in nine low-income ZIP codes; at baseline, the median household in the study had three people but total income of less than $20,000. Treatment group members received $500 per month for 24 months (July 2022 through June 2024) and members of both the treatment and control groups received $150 for filling out surveys at six-month intervals.

The experimental design resulted in 200 households randomized to the treatment group and 330 to the control group within strata defined by poverty status, the presence of children, and location. The process of verifying eligibility and collecting outcome data showed attrition rates of about 25 percent for the treatment group and 55 percent for the control group. Despite significant differential attrition, we find limited evidence of selective attrition. We nevertheless pre-specified three strategies to account for any selection created by these response patterns (Goodman-Bacon, Nunn, and Palmer 2023). Alongside average within-stratum mean differences, we report results that condition on baseline income, education, and age; condition on baseline outcomes; or use outcome differences relative to the baseline (i.e. difference-in-differences). We feel most confident in a causal interpretation for findings that have a similar magnitude across these specifications.

We estimate average treatment effects for respondents on six primary outcome domains: (i) food security, (ii) housing stability, (iii) financial security, (iv) labor supply, (v) psychological distress (Kessler 10 scale), and (vi) subjective well-being. Food security (USDA) and psychological distress (Kessler et al. 2002) are measured using externally validated aggregation methods, but the other measures are inverse covariance-weighted indices of responses to underlying survey questions expressed in terms of baseline standard deviations. We adjust our inference to control the probability of false positives across the six outcomes domains within each specification (Anderson 2008, Westfall and Young 1993).[3]

Our findings show that cash assistance stabilizes households' economic lives (food security, financial security, and housing stability) and improves direct reports of well-being (subjective or psychological) with no evidence of a reduction in labor supply. This pattern of findings matches several other randomized GBI evaluations. Comparing across these studies suggests that the benefits of basic income in terms of stability and well-being are slightly larger for populations with lower baseline income and worse outcomes, and that these groups also tend to have null or slightly positive (but not statistically significant) labor supply effects. Our population is on the more disadvantaged end of this spectrum.

These results also inform the kinds of structural models used to gauge the potentially large macroeconomic implications of scaled-up basic income policies. Some research this vein implies that basic income would raise taxes and interest rates while drastically lower the quantity and productivity of labor, the capital stock, output per capita, and welfare (eg. Daruich and Fernández 2024). A significant part of the estimate welfare effects of basic income may arise from its effects on the broader economy. Different theoretical formulations, however, imply opposite-signed impacts on macroeconomic performance (eg. Luduvice 2024). To the extent that these disagreements stem from modeling choices that can be guided by experimental research, findings like ours can play a key role in ensuring that macroeconomic debates rest on credible scientific estimates.

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[3] We also report findings for a range of individual survey questions, but we treat these as exploratory outcomes and apply weaker multiple hypothesis test adjustments (controlling the false discovery rate; Benjamini and Hochberg (1995)). We do not find statistically significant results on any pre-specified individual survey outcome and report them in the appendix.

In fact, our findings are only partially consistent with the workhorse life-cycle models used in *ex ante* equilibrium evaluations. Recipients in our study received $12,000 over two years, which is about 2.5 percent of discounted lifetime income for a typical household in our sample.[4] Standard life-cycle models predict small reductions in labor supply, higher savings, and a sustained increase in utility. In contrast, we find no evidence of labor supply reductions and can sometimes reject zero. Treatment group members overwhelmingly reported spending their basic income payments on monthly necessities like food and rent as opposed to savings. Finally, self-reported well-being, a plausible proxy for utility, rises substantially while households receive income support but declines slightly in our final survey wave conducted one to two months after the final disbursement, inconsistent with consumption/utility smoothing across periods. This suggests that more accurate individual behavioral models could improve the ability of *ex ante* theoretical evaluations to predict the macroeconomic effects of basic income policies.

I. WHAT IS "BASIC INCOME" AND WHAT MIGHT IT DO?

The term "basic income" often stands in for any program that provides cash benefits with minimal eligibility requirements. The roots of this idea in the US go back at least to Thomas Paine (King and Marangos 2006) and a version of basic income became popular among economists in the 1940s (Green 1967). Stigler (1946, pg 365), for example, argued that eligibility for income support should be based only on need (not on other characteristics) and that in the "choice between grants in kind and in money…the latter commends itself strongly."[5] By the 1960s, as a legal movement to establish a constitutional "right to income" gained strength (Davis 1993), the federal

[4] This calculation is based on cross-sectional income data for income-eligible Minneapolis household heads in the 2021 ACS 5-year file (see Table 1). We calculate average real family income, assume a life-expectancy of 75 years, and discount age-specific income to age 38 using a three percent discount rate. This yields a present discounted value of (remaining) lifetime income of $478,140.

[5] Burkhauser and Finegan (1993, pg 128) quote Stigler as attributing this early exposition of the negative income tax to conversations with Milton Friedman who later proposed it in his book *Capitalism and Freedom* (Friedman 1963).

government commissioned a series of randomized trials to test household responses to a “negative income tax” (Ashenfelter and Plant 1990). In 1971, President Nixon pushed a basic income for all families with children. Although his bill died in the Senate Finance Committee, it spurred the creation of the EITC (a wage subsidy) and the Supplemental Security Income program (SSI; a federalization of welfare programs for people who are blind, over age 65, or have a disability; Goodman-Bacon and Schmidt (2020)). This spelled the end of federal “basic income”-type proposals for several decades.

Since the mid-2010s, however, basic income has garnered substantial attention. Andrew Yang’s 2020 presidential campaign platform, for example, included a $1,000 per month basic income for adults on the premise that it would remunerate care work, facilitate labor market transitions, improve health, and increase economic growth.[6] Others envision basic income as a remedy for technology-induced job loss stemming from the development and spread of artificial intelligence. For instance, Sam Altman, founder of OpenAI, wrote[7] in 2016 that “as technology continues to eliminate traditional jobs and massive new wealth gets created, we’re going to see some version of this at a national scale,” a view echoed by the head of the Service Employees International Union, Andy Stern.[8] Others, such as Murray (2006) view basic income as a simpler and more efficient replacement for the existing patchwork of safety net programs, an argument dating back to Friedman (1963).

[6] https://2020.yang2020.com/policies/the-freedom-dividend/
[7] https://www.ycombinator.com/blog/basic-income
[8] https://prospect.org/economy/conversation-andy-stern-case-universal-basic-income/

Beyond "just giving people cash," however, basic income is only a loosely defined concept. In practice, any program must answer (at least) three inter-related questions: Who gets the payments? How big are they? Who pays for them?

Universal basic income (UBI) programs, for example, pay benefits regardless of criteria like income, work effort, health, family structure, or citizenship currently used in safety net programs in the US. Cash assistance programs in the US, on the other hand, have always been limited to specific categories of people: single parents, blind people, people over age 65, and people with disabilities. Many current basic income pilots also limit eligibility to groups like this.

The most common limitation on who gets payments, however, is income. Income-targeting (or means-testing) introduces an additional policy parameter: the rate at which payments decline as income rises (the phase-out rate). This parameter was the focus of major reforms to cash welfare programs in the US in 1967, 1981, and 1996. Reducing basic income benefits less than dollar-for-dollar with other income creates smaller disincentives to work than does a one-for-one reduction (essentially a 100% tax rate) or an immediate loss of the entire benefit for anyone over an income threshold (a "benefits cliff"), but it mechanically makes a larger share of the population eligible for some benefit. It also requires administrators to define and measure income, which is costly to both governments and recipients.

Basic income programs directly set benefit levels, but the method of financing also shapes the amount that people actually receive. Some existing basic income-type programs make payments that are a function of "external" revenues. For example, since 1982, Alaska residents receive annual disbursements determined by the five-year annual return on a fund established from the sale of oil rights in the state. Members of certain Native American tribes also receive regular income payments out of casino revenues (Marinescu 2018). A more common conception is that basic income programs would be paid for with income or consumption taxes. Benefit levels are essentially constrained by tax revenue and the net amount that people receive equals the basic income benefit minus their tax contribution.

Some basic income plans are meant to be financed by replacing large parts of the existing safety net. The net transfer that people would receive under such a policy is a complicated function of "how much" they received from the eliminated programs. For cash and near-cash benefits like Social Security and Supplemental Nutrition Assistance Program (SNAP) benefits this comparison is straightforward, but it is not obvious that public health insurance benefits, which make up the bulk of safety net spending the US, can be monetized on an individual level.

a. Predicted effects of basic income

Comprehensive conclusions, or at least conjectures, about what a particular basic income proposal would do to behavior and well-being requires understanding the effects of its payments as well as its financing mechanism. Because these program features necessarily involve the entire population or overhauls of tax and transfer systems, these effects are likely quite complicated. As noted in Imbens, Rubin, and Sacerdote (2001), however, the direct income effects of basic income payments are certainly a key component of this suite of behavioral responses. Moreover, generating hypotheses about what scaled-up basic income programs would do requires a stance on which outcomes may be affected by basic income payments and by how much.

The simplest economic models predict that basic income would make recipients better off through a combination of higher consumption, lower labor supply, and insurance against income shocks. Empirical evidence clearly shows that existing transfer programs such as SNAP, Social Security Disability Insurance (SSDI), SSI, Temporary Assistance for Needy Families, and the EITC raise consumption (e.g. Hastings and Shapiro 2018), although they have very different benefit structures than basic income. The labor supply effects of these programs are difficult to apply to basic income policies because their implicit tax rates on earnings, which are often extremely high (Ilin and Terry 2021), trigger substitution effects. Some programs like the EITC, however, explicitly subsidize wages at low levels of earnings in order to incentivize work.

Pure income effects on labor supply estimated from casino payments or annual disbursements in Alaska, however, are small or zero (Jones and Marinescu 2022, Akee et al. 2018). Similar findings emerge from a handful of basic income trials: small reductions in labor supply (Vivalt et al. 2024) or zero reductions (Liebman et al. 2022, West et al. 2021, Kappil et al. 2024, Jefferson et al. 2024, Yang, Thomas, and Juras 2024) in response to payments equal to about one-third of baseline income. Several US-based studies find that lottery winners reduce their annual earnings by about one to two percent per year, although it is not clear if people respond to this payment structure in the same way they would to a regular payment (Golosov et al. 2023, Imbens, Rubin, and Sacerdote 2001).

Basic income may also change dynamic choices by loosening borrowing constraints that prevent investments in own skills or children's skills; moves; job changes; or entrepreneurship. Little evidence exists on these margins, however (cf. d'Astous et al. 2024, Golosov et al. 2023).[9] Credible evidence does exist, however, on an extreme version of sectoral choice. Deshpande and

[9] Labor supply reductions may be one way to invest in one's children. Structural models of child skill formation (Mullins 2024) and causal estimates (Caetano et al. Forthcoming) find that maternal labor supply can reduce children's test scores, although they disagree on how these effects relate to mother's own education and productivity. To the extent that basic income policies change the return to investing in children's human capital (both by establishing a floor for children's income and potentially by taxing their earnings to finance the basic income program), some models imply that parents will invest *less* in their children (Daruich and Fernández 2024, Heathcote, Storesletten, and Violante 2017). No evidence exists on the strength of this effect, but one experimental study finds that parents' educational investments in their children do not respond after being informed that their children will likely not receive welfare benefits as adults (Deshpande and Dizon-Ross 2023).

Mueller-Smith (2022) show that SSI receipt reduces crime and incarceration, especially for financially motivated crimes.

Research in psychology and behavioral economics also suggests that income support may have benefits for mental health and cognitive functioning. Mani et al. (2013), for example, provide theory of limited mental resources and empirical evidence to show that poverty leads to lower cognitive function and decision-making capabilities.[10] Basic income may alleviate these cognitive burdens by making income and consumption more stable (Bartik et al. 2024, West et al. 2021). Experimental studies of non-cash interventions, such as Medicaid eligibility, find improvements in mental health as well (Finkelstein et al. 2012), although some studies of one-time cash interventions find that mental health actually falls (Jaroszewicz et al. 2023).

Structural economic models address these questions by abstracting from certain policy and choice features, while explicitly modeling how dynamic behaviors and equilibrium outcomes respond to a basic income policy. These tend to focus on UBI policies, but their structure can be used to study other policies as well. Daruich and Fernández (2024), for example, calibrate an overlapping generations model with agents who work, save, and invest in human capital for themselves and their children in response to a log-linear tax and transfer system. They find that a UBI financed by labor taxes would reduce savings incentives and the return to skills, and have large negative welfare consequences. Luduvice (2024), on the other hand, calibrates a model with a more realistic set of tax and transfer programs but no educational investments or intergenerational links and finds that while UBI reduces employment and output, welfare improves. Similarly, Conesa, Li, and Li (2023) find that different combinations of UBI generosity and financing mechanisms produce

[10] Note that these effects, as well as the ones predicted by economic models, almost certainly differ based on household's financial situation. This matters most for UBI policies that necessarily pay benefits to higher-income households who may respond very differently than lower-income households.

different welfare consequences. The conclusions from structural approaches evidently depend critically on assumptions about which kind of behaviors respond to UBI and how strongly.

II. THE MINNEAPOLIS GUARANTEED BASIC INCOME PILOT

Since 2018, when officials in Stockton, California began a two-year pilot study that paid $500 per month to low-income residents for two years, over 150 localities in the US have initiated studies of guaranteed basic income (GBI) programs, many of them using a randomized design (Stanford Basic Income Lab 2020). In July 2021, the City of Minneapolis announced plans to launch its own pilot GBI program using $3 million in federal American Rescue Plan Act (ARPA) funds. Because GBI programs necessarily affect some combination of savings, consumption, and labor supply and typically target low- and moderate-income communities, their direct effects are relevant to researchers interested in poverty, labor markets, and the macroeconomy. The City of Minneapolis determined the policy objectives, made program design decisions, and administered the pilot, and we designed a rigorous pilot study for city administrators to follow and analyzed the resulting confidential and anonymized data.

a. Study Eligibility and Recruitment

The pilot channeled Covid-19 relief funds to low- and moderate-income Minneapolis residents of nine high-poverty ZIP codes. To be eligible for the study, residents of these ZIP codes (55403, 55404, 55405, 55407, 55411, 55412, 55413, 55430, or 55454) had to be age 18 or over, have a status which would make them eligible to receive federal funds, attest to having been adversely affected economically by Covid-19, and have annual household income in 2021 less than or equal to half of area median income.[11] These community-wide eligibility criteria differ from many other

[11] These thresholds are $36,725 for a single-person household, $41,975 for a three-person household, $47,225 for a four-person household, and $52,450 for a four-person household. The full set of thresholds is available here: https://www.huduser.gov/portal/datasets/il.html#year2021

GBI studies in that they are not otherwise restricted to specific populations such as mothers or participants in existing safety net programs.

In Fall 2021, the City of Minneapolis to communicate the objectives of the pilot and recruit prospective participants. Outreach activities included partnerships with community-based organizations; print and online advertisements; interviews in local media; and a landing page on the City website. An initial interest form was open online for the month of December 2021. The first panel (three circles) in Figure 1 documents the initial phase of recruitment. The City received 14,510 submissions, of which 13,379 were unique, and 8,335 appeared to meet geographic, age, and income eligibility criteria. We then randomly selected 1,500 applicants from an anonymized version of this list and the City invited them via email to complete a baseline survey during March 2022 for which they could receive $150 (second panel of Figure 1).[12] The third panel of Figure 1 shows that 530 of the 1,500 people invited to complete the baseline survey did so. One pair of responses was a duplicate. Ultimately, one confirmed-eligible treatment participant became ineligible immediately before their payments began and was replaced with a would-be control participant. Thus, 529 baseline respondents made up the sample that we randomized to the treatment and control groups.

b. Treatment and survey incentives for control units

Treatment group members received $500 per month for 24 months starting in June 2022. Participants could receive benefits via direct deposit or as monthly checks mailed to their current

[12] It included a Data Practices Advisory that prospective respondents were required to affirm having read and understood before submitting information. Eligibility-related items were at the beginning of the survey; responses indicating ineligibility caused text to appear advising respondents accordingly. The written version of the baseline survey was in English, with prominent advisory text directing respondents to live City translation support in six additional languages: Spanish, Hmong, Somali, Lao, Oromo, and Vietnamese.

address. Members of both the treatment and control groups received $150 for completing surveys every six months.[13]

c. Data collection, sharing, and measurement

All data used in this study come from surveys administered by the City and completed online by participants. The City sent surveys to participants via email every six months. Participants had about four weeks to respond and received periodic phone and email reminders to do so. A short time after surveys closed, City staff shared de-identified data with us.

The survey instrument includes questions primarily drawn from existing, publicly available data sources, which allows us to show how our Minneapolis study population compares to populations nationally.[14] In addition to demographic and economic information, our main outcomes come from questions covering six domains:

1. <u>Food security (6 items).</u> This measure comes from the US Department of Agriculture (USDA)'s six-item short form food security survey. [15] Responses to yes/no questions about sufficiency and affordability of food in the last 30 days are summed into a seven-point scale. Consistent with USDA, we consider scores of zero or one to be "food secure."

[13] The city determined these program variables to match related GBI pilots, incentivize control responses, and stay within the $3 million budget. The estimated cost of the study was $2,677,500 (24 months*$500/month*200 treated households + $150 survey payment*530 baseline respondents + $150 survey payment*4 follow-up surveys*330 control households), which left room for the City to cover administrative costs. The Federal Reserve received no payments for work on this study. In practice, attrition meant that not all of these funds were spent by the end of the 24-month period. The remaining funds will pay for two additional rounds of post-pilot data collection (30 and 36 months), which is currently in progress.

[14] A community focus group in fall 2021 facilitated by an external community-based organization provided input on survey design considerations. A second community focus group provided field testing and feedback in January 2022. In pre-testing, typical baseline survey completion time was between 20 and 30 minutes. Sixty items were reserved for capturing information for up to ten household members in addition to the respondent. Survey instruments from all waves are publicly available on the Federal Reserve Bank of Minneapolis website.

[15] https://www.ers.usda.gov/topics/food-nutrition-assistance/food-security-in-the-u-s/survey-tools/

2. Housing stability index (9 items). This measure includes questions about renting/owning one's own shelter, difficulty or lateness paying rent/mortgage, overcrowding, self-reported instability, and concerns or instances of forced moves.
3. Financial security index (11 items). This measure includes questions about self-reported financial situation; whether the household receives income from public assistance, charity, or family/friends; precautionary saving behavior; the ability to pay bills or emergency expenses; and late debt service payments.
4. Labor supply index (6 items). This measure combines questions about labor force participation, employment status, full-time employment status, multiple job holding, and usual weekly hours.
5. Psychological distress (Kessler 10 scale). This is a common, clinically predictive screening tool for psychological distress. Items ask how often respondents experienced various feelings in the last 30 days, with responses on a five-element scale ranging from "none of the time" (scored as one) to "all of the time" (scored as five). Responses are summed (Kessler et al. 2002).
6. Subjective well-being index (3 items). This measure combines responses to questions about self-reported health status, happiness, and life satisfaction.

We also constructed indices that capture housing quantity, health care access, health care use, and the use of low-cost credit (i.e. not using high-cost credit lines such as check cashing services, pawnshops, or payday loans). Our pre-analysis plan treats these as exploratory outcomes along with a set of specific outcomes that contribute to the six main indices, such as employment, multiple job holding, and transportation access. We also asked several multiple-choice or open-ended questions to treatment respondents about how basic income affected them and how they used it. For a full description of these measures see Goodman-Bacon, Nunn, and Palmer (2023).

Our main outcome variables for housing stability, financial security, labor supply, and well-being are inverse covariance weighted indices (Anderson 2008). (Food security and the Kessler-10 score are aggregated using externally validated procedures.) This approach first codes all variables so they go in the "same direction." For example, one binary variable in the housing stability index equals one for respondents who report renting or owning housing, and another equals one for respondents who do *not* report being late on their rent. We then standardize responses to each component question by its baseline mean and standard deviation, calculate the covariance matrix between the measures, and weight them together by the inverse of this matrix. This ensures that strongly correlated responses do not "count twice" in our outcome.

We measure outcomes this way for two main reasons. First, combining many questions into indices reduces the number of statistical tests we perform and therefore the probability of false positives. Second, mean comparisons of indices are more efficient than variable-by-variable comparisons and weighted indices are more efficient than unweighted ones (O'Brien 1984). One cost of this measurement choice, however, is that estimated effects on these outcomes are in baseline standard deviation units which precludes the direct calculation of elasticities from our index estimates.

d. *Target population*

Given the recruitment and selection process, the study population is low-income Minneapolis residents of low-income ZIP codes who chose to apply for the study, passed initial eligibility criteria, and successfully returned their baseline survey. Table 1 compares average baseline characteristics in this study population (column 3) to income-eligible Minneapolis residents in the American Community Survey 5-year file from 2021 (columns 1 and 2; Ruggles, Flood, Sobek, et al. 2024). In terms of demographics, our sample is slightly younger and includes fewer non-Hispanic white residents and more women and parents (reflected in household size and composition) than the city-wide population. Reweighting the ACS data to match our study population's age distribution only partly closes these gaps because the City specifically targeted ZIP codes that have historically been affected by systemic barriers to economic opportunity—which in Minneapolis correlates closely with neighborhood racial composition. Economically, our population closely resembles the broader income-eligible population in Minneapolis, especially after aligning their age distributions. About 40 percent have earned a high school degree or less, just under two-thirds were employed, and average income was about $21,000.

The first two columns of Table 2 present baseline means for our six main outcome variables. The index values do not contain any information on their own because they are centered around zero by construction. Our population has very low levels of food security and high levels of psychological distress, however. About one-third of the baseline sample scores as "food secure" on the USDA measure, which is less than half the level among households with income below 130 percent of the federal poverty level (according to data from the Current Population Survey's Food Security Supplement; Rabbitt et al. 2024). The average Kessler 10 score—about 25, which equals the clinical screening threshold for "moderate psychological distress"—also exceeds values for the target populations of basic income studies in at least four other urban areas (Jefferson et al. 2024, Kappil et al. 2024, West et al. 2021, Yang, Thomas, and Juras 2024).

III. EXPERIMENTAL DESIGN, ESTIMATION, AND INFERENCE

This section summarizes information from our pre-analysis plan describing how we randomized our study population into treatment and control groups, how we planned to handle attrition, and the target parameters, estimators, and inference procedures we used to generate our estimates.[16]

a. Initial Randomization

Unlike trials in populations that share certain features, such as mothers, foster children, or SNAP recipients, our study essentially targeted an entire community and thus produced a quite heterogeneous population. We therefore used stratified randomization to ensure balance along three important dimensions: the presence of children under 18, poverty status, and living in the set of ZIP codes in North Minneapolis. These three binary variables define our eight strata (denoted by j), which had sizes (N_j) between 38 and 112 households. The overall study intended to treat 38 percent (200/529) of participants, so within each stratum we randomly ordered participants and assigned the top $N_j \times \left(\frac{200}{529}\right)$ of them to the treatment group. Because of rounding, this produced 198 treatment-randomized participants.

b. Timing and attrition

The selection of the study population did not involve formal eligibility verification, only self-attestation. Therefore, after randomization but before payments could be made, City officials conducted the intake and verification process for treated participants mostly in April and May, 2022.[17] Some study participants randomized into the treatment group were found ineligible (9), responded to eligibility verification requests only after several months of attempts to reach them

[16] We solicited four anonymous reviews of the pre-analysis plan, for which we thank Abbie Wozniak. and we coded all our analyses on simulated data prior to accessing outcome data. The full pre-analysis plan was registered with American Economic Association's RCT registry on May 5, 2023 (Goodman-Bacon, Nunn, and Palmer 2023). We thank Alex Albright, Natalie Gubbay, and Ayushi Narayan for their review of the code alongside the PAP throughout the project.

[17] Intake also included a consultation about how receipt of GBI payments might affect households' receipt of other benefits.

(4), or never produced documentation of their eligibility (15). In these 28 cases and for the 2 remaining treatment openings after randomization, the City randomly moved initial control participants into the treatment group on a rolling basis as eligibility verifications were completed. After initial eligibility verification but before their payments began, one treatment group participant became ineligible and was replaced with a would-be control participant. This resulted in a total of 201 participants ever assigned to treatment, with 200 actually receiving treatment. This had a small effect on the randomization procedure and reduced the total sample size by 5.5 percent.

These verified-eligible treatment participants began receiving their first $500 monthly payments in June 2022. City officials then began conducting eligibility verification for control households, which continued on a rolling basis through December 2023. At the time of the six-month survey, only about half of the 300 remaining control households had provided eligibility documentation, a rate of attrition that held roughly constant throughout the study. Furthermore, as responses to the six-month survey came in (but prior to any outcome data being shared with us), City officials reported non-trivial survey non-response. Across the four data collections, 40-42 percent of control households and 68-74 percent of treatment households verified their eligibility *and* responded to surveys. We thus observed substantial differential attrition of between 26 and 34 percent. Table 3 shows the total number of respondents and the treatment share within our eight strata across the four survey waves.

Our pre-analysis plan addresses this attrition in two ways. First, we compare baseline covariate and outcome imbalance between treatment and control groups in the estimation sample versus the study population. Column 4 of Table 1 shows baseline characteristics for the 326 respondents who verified their eligibility and responded to any post-baseline survey. They closely resemble the full population on these measures. Columns 5-6 show treatment and control means within the estimation sample. Treatment respondents are about a year and a half younger, less likely to identify as male, have slightly less education, and have marginally lower income than control respondents. Figure 2 helps to quantify these means by plotting standardized differences between treatment and control households in the full sample (dark circles) and the 6-month estimation sample (light circles). None of the covariates nor baseline outcomes differ by more than 0.25 standard deviations, a common rule of thumb for gauging serious imbalance (Imbens and Rubin 2015pg. 277). Evidently, attrition was not related to baseline income, among other things.

Columns 3-4 of Table 2 present treatment and control means in the estimation sample for baseline outcomes (which are also plotted as standardized differences in Figure 2). These show little evidence that attrition generates large imbalances in baseline outcomes. This conclusion is supported by formal tests of two null hypotheses about selective attrition discussed in Ghanem, Hirshleifer, and Ortiz-Beccera (2023). [18]

Failing to find strong evidence of selective attrition suggests either that there is none, in which case simple mean comparisons would identify causal parameters, or that we cannot detect selectivity, in which case simple estimators are biased. To protect against the second possibility, we also pre-specified three different control strategies that condition on baseline information in order to make treatment and control respondents more comparable, which we discuss below.

[18] These are tests of two different types of equalities of baseline outcome means across four groups: treatment responders, treatment attritors, control responders, and control attritors. The null hypothesis that all four groups have equal baseline outcomes is an implication of the assumption that attrition is completely random which implies that treatment/control contrasts identify average treatment effects for the study population. The null hypothesis of equal means between treatment and control respondents and between treatment and control attritors is an implication of a weaker assumption that attrition itself was non-random, but that it was the same by treatment status. In this case treatment/control contrasts identify average treatment effects for the respondent sub-population. We implement these tests within strata and compare results with and without covariates. Results are reported here: https://www.minneapolisfed.org/-/media/assets/topics/labor-market-policies/24-month-results-overview.pdf

c. Target parameters and identification

We define causal quantities and assumptions using traditional potential outcomes notation (Robins 1986, Rubin 1974). $Y_{it}(1)$ and $Y_{it}(0)$ represents unit i's period-t outcome under the treatment or control conditions. The time periods include $t = 0$ for the baseline survey and four follow-up periods measured in months ($t = 6, 12, 18, 24$). We use $R_{it}(1)$ and $R_{it}(0)$ to represent potential response status. Actual response status is $R_{it} = D_i R_{it}(1) + (1 - D_i) R_{it}(0)$, which means that the observed data are:

$$Y_{it} = \begin{cases} D_i Y_{it}(1) + (1 - D_i) Y_{it}(0) & if \quad R_{it} = 1 \\ \quad missing \ \ otherwise \end{cases}$$

Our goal is to estimate average treatment effects for respondents:

$$\tau_{ATE-R,t} = E[Y_{it}(1) - Y_{it}(0) | R_{it} = 1]$$

Ghanem, Hirshleifer, and Ortiz-Beccera (2023) show that this parameter is identified under the following conditional mean independence assumption:

$$E[Y_{it}(d) | D_i, R_i, G_i, X_i] = E[Y_{it}(d) | R_i, G_i, X_i], \qquad d = 0,1, \quad \forall\, t, \tag{1}$$

which says that conditional on stratum (G_i) and baseline covariates (X_i), both potential outcomes are mean independent of treatment status conditional on response status.[19] Identification only

[19] Identification is as follows:

$$\begin{aligned} \tau_{ATE-R,t} &= E[Y_{it}(1) - Y_{it}(0) | R_{it} = 1] \\ &= E[E[Y_{it}(1) - Y_{it}(0) | R_{it} = 1, G_i, X_i\,] | R_{it} = 1] \\ &= E[E[Y_{it}(1) | R_{it} = 1, D_i = 1, G_i, X_i\,] - E[Y_{it}(0) | R_{it} = 1, D_i = 0, G_i, X_i\,] | R_{it} = 1] \\ &= E[E[Y_{it} | R_{it} = 1, D_i = 1, G_i, X_i\,] - E[Y_{it} | R_{it} = 1, D_i = 0, G_i, X_i\,] | R_{it} = 1] \end{aligned}$$

Where the second line uses the law of iterated expectations, the third line uses the conditional mean independence assumption, the fourth line uses the definition of observed outcomes.

requires this equation to hold for $t > 0$, but it has sharp testable implications in the baseline survey where we observe $Y_{it}(0)$ for all units.[20]

d. Estimation and inference

Without covariates, equation (X) suggests a simple plug-in estimator that calculates within-stratum mean differences and weights them by the share of respondents in each stratum, n_j^R. Because of the magnitude of differential attrition, however, we view the use of covariates as important for establishing internal validity. Therefore, we use three different methods to estimate within-stratum effects. The first two are essentially linear outcome regression models (Heckman, Ichimura, and Todd 1997, Słoczyński 2015) that either control for baseline income, a dummy for having a post-high-school degree, and baseline age (Model 1, a conditional-on-X specification); or baseline outcome values (Model 2, a lagged dependent variable, LDV, specification):

$$Y_{it} = \sum_{j=1}^{8} 1\{G_i = j\}\left(\mu_j + \beta_j X_i + \rho_j \dot{X}_i D_i + \tau_X^j D_i\right) + \varepsilon_{ikt} \tag{2}$$

Where $\dot{X}_i \equiv X_i - \bar{X}_{j(i),0}^R$ are deviations of each covariate from its baseline stratum mean among respondents in stratum j. $\hat{\tau}_X^j$ is then an estimate of the conditional ATE-R in stratum j and we average these using each stratum's share of the respondent population as our estimator of $\tau_{ATE-R,t}$:

Three variants of this assumption are worth noting because they imply that the comparisons we make in our estimation identify different causal parameters. Conditional mean independence of $Y_{it}(0)$ only implies that our estimates equal average treatment effects for treated respondents ($R_{it}(1) = 1, D_i = 1$), not all respondents. This is a weaker assumption than equation (1) because it allows for the possibility that attrition is related to treated potential outcomes. A stronger assumption is $E[Y_{it}(d)|D_i, R_i, G_i, X_i] = E[Y_{it}(d)|G_i, X_i]$, which says that potential outcomes are mean independent of treatment *and* response status (not just mean independent of treatment conditional on response status). This permits identification of average treatment effects for the entire study population. Neither assumption affects the estimators we use, just the interpretation of the estimates. Finally, dropping the conditioning on X_i justifies taking simple differences of means within stratum, but restricts attrition more.

[20] This implicitly makes a kind of no-anticipation assumption. While recruitment into the study could have affected outcomes as baseline, treatment had not yet been allocated and so it is not possible for units to have anticipated their treatment status.

$\hat{\tau}_X = \sum_{j=1}^{8} n_j^R \hat{\tau}_X^j$. Our third approach is a difference-in-differences (Model 3; DiD) model that subtracts baseline outcomes from period-t outcomes and estimates a simple mean difference in changes rather than levels. It is identified by a parallel trends assumption, similar to equation (1) but with $Y_{it}(0) - Y_{i0}(0)$ (and no other covariates). One reason to estimate both the LDV and DiD specifications is that in cases where one of their identifying assumptions holds but the other does not, the two estimates bracket the true parameter of interest (Ding and Li 2019).

This pre-specified analysis tests null hypotheses for six outcome variables for each specification. To protect against false positives we present p-values that control the family-wise error rate; i.e. the probability of any false discovery. For details see Goodman-Bacon, Nunn, and Palmer (2023).

IV. ESTIMATED EFFECTS OF GBI IN MINNEAPOLIS

Figure 3 plots mean outcomes by wave separately for treatment and control respondents who verified their eligibility and responded to the 24-month survey. Tables 3-5 report the formal econometric results using the methods described in section III.D. The final rows of each panel present summary estimates that average all post-baseline responses for each respondent. Strikingly, the results tell a consistent story across our distinct estimation techniques.

Table 4 presents results for three measures of economic security: food security (panel A), housing stability (panel B), and financial security (panel C). We find immediate, large, and robust positive effects of basic income payments on food security. At baseline only about one-third of respondents in both groups are recorded as food secure, but after 6 months of GBI payments and in every wave thereafter, about half of the treatment group reports being food secure. The estimated average treatment effect parameter is between 15 and 20 percentage points across all specifications in all periods, and every estimate except one is statistically significantly different from zero after adjusting for multiple hypothesis testing.

These are also meaningfully large effect sizes. A national report from USDA finds that 64 percent of households at or below 130% of the federal poverty line are food secure (Rabbitt et al. 2024). GBI payments close half the gap between our study population and this nationally representative statistic. The effects also align roughly with direct nutritional supports. Pandemic-era SNAP benefits, for example, which raised payments by $110 on average for a higher-income group of households reduced a measure of food insufficiency by 1.2 percentage points (Schanzenbach 2023). Scaling the benefit up and accounting for differences in the populations served brings these results fairly close together.

We also find that GBI tends to increase housing stability, although these findings do not appear immediately and are not as precise. Panel B of Table 4 (and Figure 3) shows that at 18 months, all three of our approaches detect an improvement of between one-sixth and one-fifth of a standard deviation. The results at 12 months are of a similar magnitude but less precise and the findings at 6 and 24 months are smaller and not always significant. Aggregating housing stability responses over all four post-baseline waves, however, yields a precisely estimated positive average effect. Notably, the DiD results are always the largest. This may be a statistical artifact of subtracting the entire baseline treatment/control outcome gap. If housing stability gaps tend to shrink over time then the LDV specification (model 2), which nets out a fraction of the baseline gap, is more accurate. Assuming that either equation (1) or its parallel trends analogue hold, then these two coefficients bracket (fall on either side of) the true ATE-R (Ding and Li 2019).

This pattern makes sense for housing outcomes, especially in contrast to the immediate food security findings. One reason is that housing takes more time to change than food consumption. We do not expect low-income people to break leases, for example, in response to GBI. It is more feasible to wait to adjust housing consumption until a lease expires. The housing stability outcome also includes questions about experiences that may be rare, such as being forced to move (or being worried about a forced move). Finally, the rebound in the estimates at the 24-month survey likely reflects the fact that these data were collected one to two months after the final payment. Recipients therefore knew more about their post-payment housing stability at this wave.

Our results for overall financial security share features with both the food security and housing stability results. We find robust evidence that GBI payments improve financial security by between one-sixth and one-fifth of a standard deviation (panel C of Table 4 and Figure 3). This includes questions about income sources other than earnings (and GBI), savings, and the ability to afford sudden expenses. Yet as with housing stability, we observe attenuation in the effects at the 24-month wave, although these results are generally significantly different than zero. We view the food security, housing stability, and financial security results as first-order effects of giving income support to low-income households. Many of them reported difficulty affording necessities and they used additional income to do so.

One clear behavioral prediction from economic models is that an increase in unearned income should reduce labor supply. This is a direct consequence of valuing leisure time. Static and dynamic models differ on the magnitude of their prediction for short-run labor supply behavior but not in their sign. We do not find evidence that GBI payments caused recipients to reduce their labor supply. In fact, panel D of Figure 3 and Table 5 show that our labor supply index—which combines extensive and intensive margin measures—is higher among treated respondents than control respondents. These results are not generally distinguishable from zero and our specifications that use baseline employment information (models 2 and 3) find smaller results than unadjusted comparisons or ones adjusted only for demographic information.

We view our final two measures—psychological distress measured by the Kessler 10 scale and self-reported well-being—as two ways to summarize causal effects of GBI payments on recipient welfare. Table 6 and the last two panels of Figure 3 show strong evidence of large improvements in both measures at the 12- and 18-month waves, after recipients had received at least $6,000 of payments, but not at the 24-month wave after payments had already ended. Our study population had low mental health scores at baseline, but by 18 months the treatment group average equaled the clinical screening threshold for no distress. The ATE-R estimates for the K10 outcome are about 3.5 points in the LDV and DiD specifications and slightly bigger in the specification that conditions only on characteristics. This is a substantive improvement. One validation study, for instance, found an 11-fold difference in rates of suicidal ideation for people scoring the top versus bottom ranges of the K10 scale (Chamberlain et al. 2009). We find similarly large effects—about one-third of a standard deviation—on the well-being index which is composed of more general questions. We find significant effects on both outcomes when averaging over post-baseline observations as well.

Our estimated effects on well-being outcomes at 24 months, however, are notably smaller (about 0.2 instead of 0.3 standard deviations) and no longer distinguishable from zero. We view this as a reflection of the fact that this survey was administered after payments ended. While City officials clearly communicated throughout the pilot that payments were temporary, in the months leading up to May 2024 the City intensified messaging that payments were ending, and participants had at least several weeks without payments prior to reporting their well-being at the 24-month wave. Therefore, we expect that the stress of losing payment shapes responses at this point. Other GBI pilots find similar patterns, although the rebound in mental health effects sometimes occurs while treatment participants are still receiving payments (Miller et al. 2024).

V. DISCUSSION: GBI EFFECTS IN PILOT STUDIES AND ECONOMIC MODELS

The previous section reported treatment effect estimates that told a fairly clear story about GBI in Minneapolis. GBI stabilized household finances along several dimensions without any observable reduction in labor supply, and this translated into improved mental health and well-being, at least until GBI payments ended. This section discusses our results in the context of other GBI pilots, other research on safety net programs, and the kinds of economic models used to forecast the effects of larger-scale basic income programs.

a. How do the Minneapolis results compare to other basic income findings?

While our study is relatively small, the large number of GBI pilots that are underway today actually represents quite a large number of respondents. These studies use different benefit amounts and durations, target different groups, and measure different outcomes, and this heterogeneity likely contains more information when analyzed together than any single study on its own. Figure 4 attempts to place our findings in the context of several other recently released findings.

Panel A is a scatter plot of estimated treatment effects and confidence intervals for food security against the baseline level of food insecurity in each study. Not all studies detect a causal effect of basic income on food security, but the ones that do appear to come from lower-income populations to begin with. The studies in Minneapolis, Shreveport, Chelsea, and Baltimore all served populations with food security rates below one half and all but Chelsea detected effects on food security (although the Chelsea study did find short-run effects that faded away). The OpenResearch Unconditional Income Study (ORUS) and Birmingham evaluations found no effects but applied to populations with higher food security to start with.

Panel B scatters estimated treatment effects on employment rates against baseline income. The only study to detect negative employment effects, Vivalt et al. (2024), serves a higher-income population at baseline (average income of about \$29,000 compared to less than \$20,000 in most other studies) and has the largest sample size. Studies of lower-income groups find no evidence of disemployment effects: five out of the six point estimates are positive and none is significant. It is possible that labor supply of other household members responds more than that of the respondent (as the ORUS study found), or that there are shifts from full- to part-time employment (see Jones and Marinescu 2022). It appears, however, that income support alone does not have a strong negative effect on labor supply among poorer households, although these effects may become stronger for slightly higher-income populations.

Finally, panel C plots estimated effects on the Kessler 10 scale against baseline K10 scores. It appears that basic income leads to a convergence in the mental health distribution: the largest negative effects on psychological distress come from studies that apply to the populations with the lowest mental health at baseline. The outlier on this figure is the ORUS study which asks a slightly different six-question version of the Kessler scale. We multiply their baseline scores and treatment effects by 1.67, but this translation may not be accurate. Even within the five studies that use the same K10 measure, a gradient in baseline income is evident. Our population has the lowest mental health scores at baseline and the largest treatment effects.

b. How do the Minneapolis results compare to predictions of tractable economic models?

Our experimental design appears to give reliable causal estimates of a (a) *short-run* basic income program (b) given to a *small share* of Minneapolis residents and (c) financed by *one-time federal dollars*. Each of these criteria makes this specific setting different in important ways than realistic basic income policy proposals which would ostensibly be permanent, widespread, and financed by taxes or spending cuts. Our results, however, can inform the kinds of choice models employed in *ex ante* structural exercises meant to evaluate true basic income *policies*.

For example, model-based research on universal basic income uses a life-cycle framework with standard discount parameters and a functional form that implies a geometrically declining consumption path and a labor supply function that follows the path of wages (in accordance with the Frisch elasticity). Unanticipated changes in unearned income, such as from a short-run basic income pilot, simply shift the path of consumption up and the path of hours down. Income shocks therefore shift lifetime utility but do not otherwise change its trajectory.

Our findings match some but not all of these predictions. The predicted magnitudes of the short-run labor supply effect are negative and fairly small. (This is especially true for low-wage workers for whom the implied disutility of work is small.) Relatedly, as one check on their model parameters, Daruich and Fernández (2024) calculate income elasticities of labor supply in response to short-run payment programs of between -0.01 and -0.11. These are roughly consistent with our findings in Table 5, although we obtain positive point estimates and our confidence intervals can sometimes rule out zero.

The strong theoretical prediction of consumption smoothing implies large savings in the short run. While we did not collect data on how households used their basic income payments, we did ask an open-ended question to treatment group members about their top three uses of the money. By far the most common response was that recipients paid for rent and utilities. Almost none reported increasing cash savings, although about one-third of them reported that paying down debt was one of their top three ways they used the money. Therefore, most treatment group members appear not to have saved, which is consistent with our findings about improved food security and housing stability.[21]

Finally, our findings that mental health and subjective well-being increase meaningfully when recipients receive basic income payments and then start to revert even a few weeks after payments end are not consistent with consumption smoothing, the point of which is to avoid large period-to-period swings in utility.

Together these comparisons suggest that traditional economic models used to capture life-cycle decisions made by low-income households may be missing important aspects of their lives. Affordability, for example, is a key challenge. Almost all low-income people *consume* housing each month, but when given extra cash they still find it necessary to spend on housing (Chetty and Szeidl 2007). Alleviating this cash flow problem makes them better off, but possibly not permanently. Bartik et al. (2024) consider whether hyperbolic discounting can explain this pattern in the ORUS basic income experiment (also see Shapiro 2005, Card, Chetty, and Weber 2007). They also appear not to be able to afford to reduce labor supply very much. Individual choice frameworks that capture these features may improve the ability of structural models to incorporate low-income people and policies that aim to support them.

VI. CONCLUSION

This paper reports on findings from a randomized evaluation of providing basic income payments ($500/month for 24 months) to 200 low-income families in Minneapolis, Minnesota from June 2022 through May 2024. After adjusting for the possibility of selective attrition, we find robust evidence that basic income payments (1) stabilize household finances as measured by food

[21] Our financial security measure, for which we estimated large and precise treatment effects, primarily contains questions about whether respondents relied on income sources other than earnings. We do not find evidence that basic income changes respondents' propensity to cover sudden expenses with cash.

security, housing stability, and financial security, (2) have no effect on labor supply, and (3) improve clinically validated measures of mental health and well-being. Not all of these effects are immediate or persistent. In particular, the treatment group's mental health, self-reported well-being, and housing stability all erode slightly in the final survey wave taken one to two months after the payments ended. Comparing these findings to other studies suggests that basic income may have its largest effects on people with the lowest income. The patterns of our findings, namely small labor supply effects and time-varying well-being, are only partially consistent with strict consumption smoothing behavior predicted by simple (tractable) economic theories that form the basis of equilibrium evaluations of hypothetical basic income programs. Reconciling these models with credibly identified causal findings would improve their performance, and designing subsequent studies to answer key modeling questions would be uniquely valuable.

Figure 1. Recruitment, intake, and randomization process

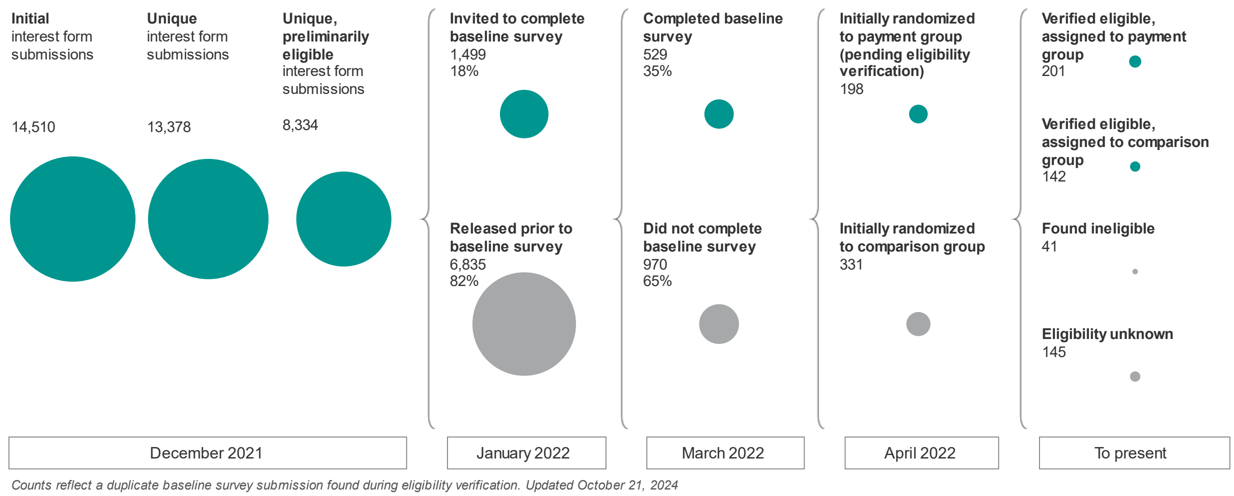
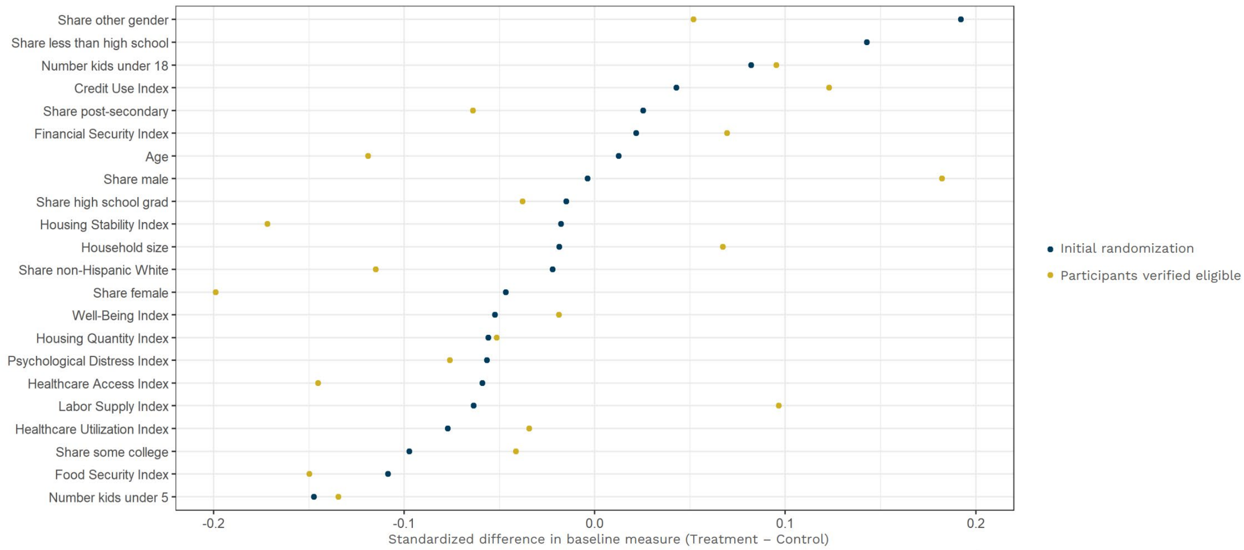


Figure 2. Baseline balance statistics, in the full study sample and verified-eligible sample



Notes: The figure plots standardized differences in the baseline survey, which are defined as $(\bar{x}_T - \bar{x}_C)/\sqrt{\frac{s_T^2+s_C^2}{2}}$. The dark dots use all 529 participants and the light dots use participants who verified their eligibility.

Figure 3. Treatment and control outcomes by survey wave

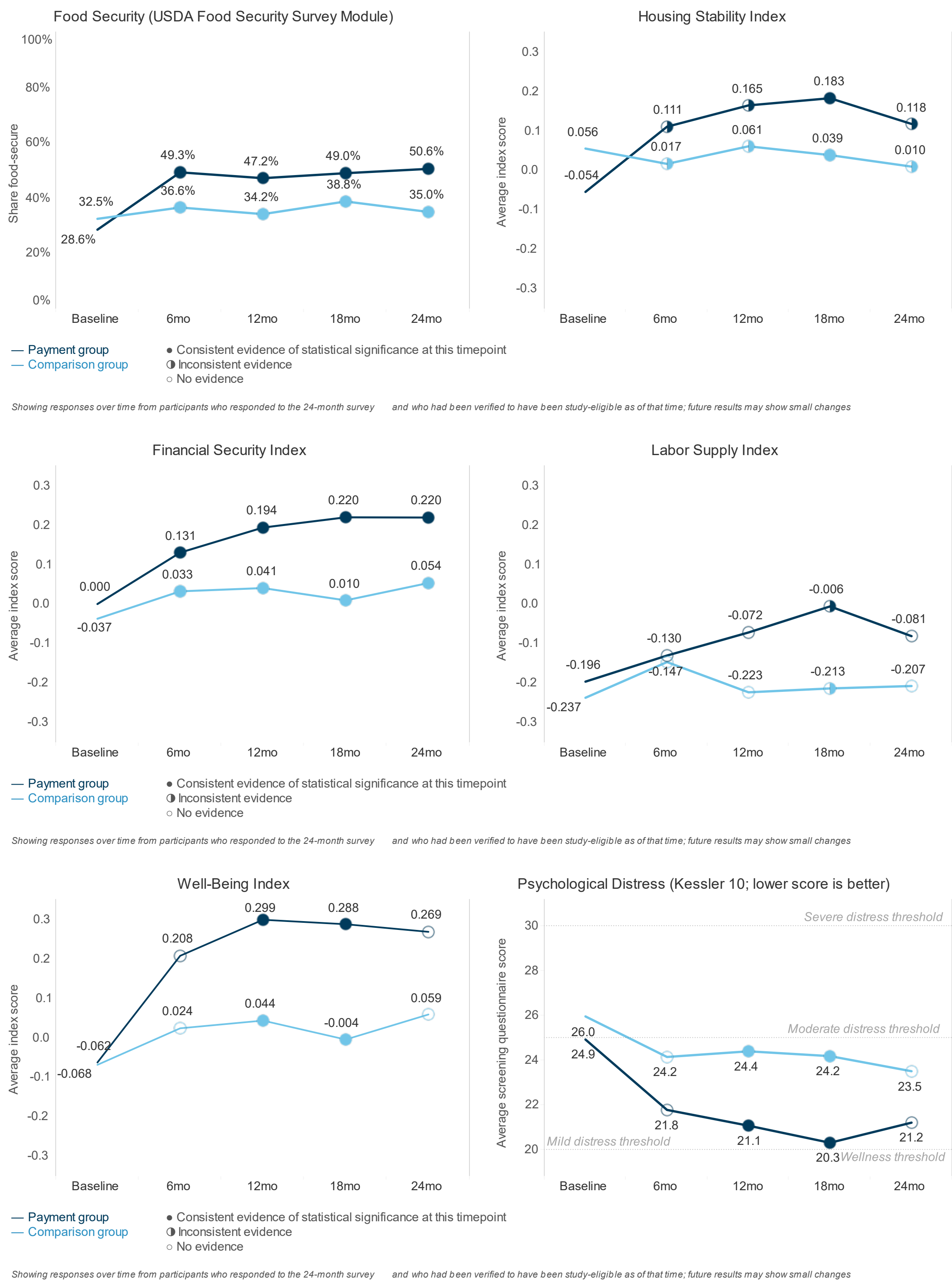
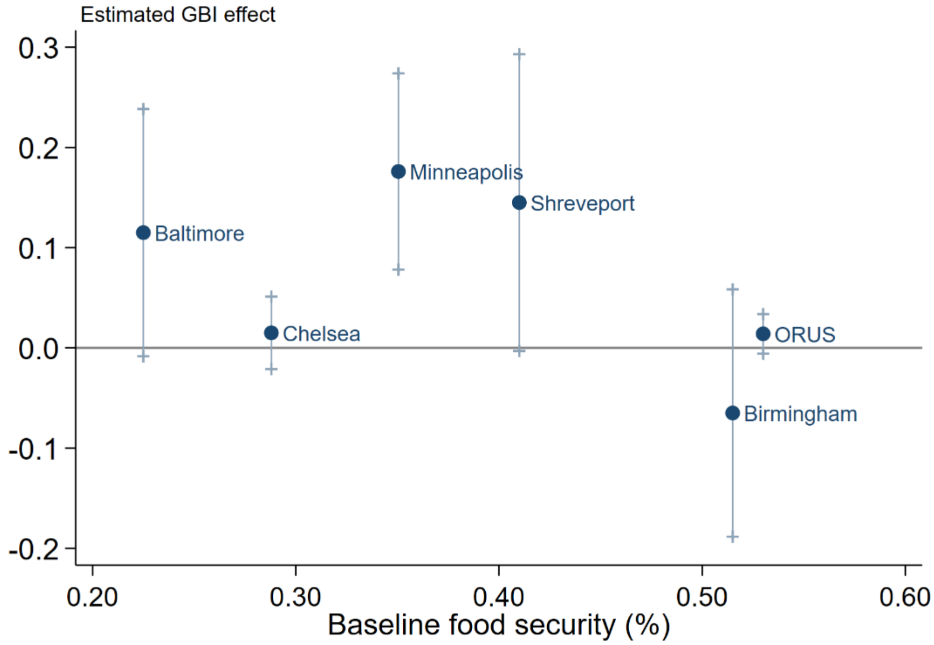
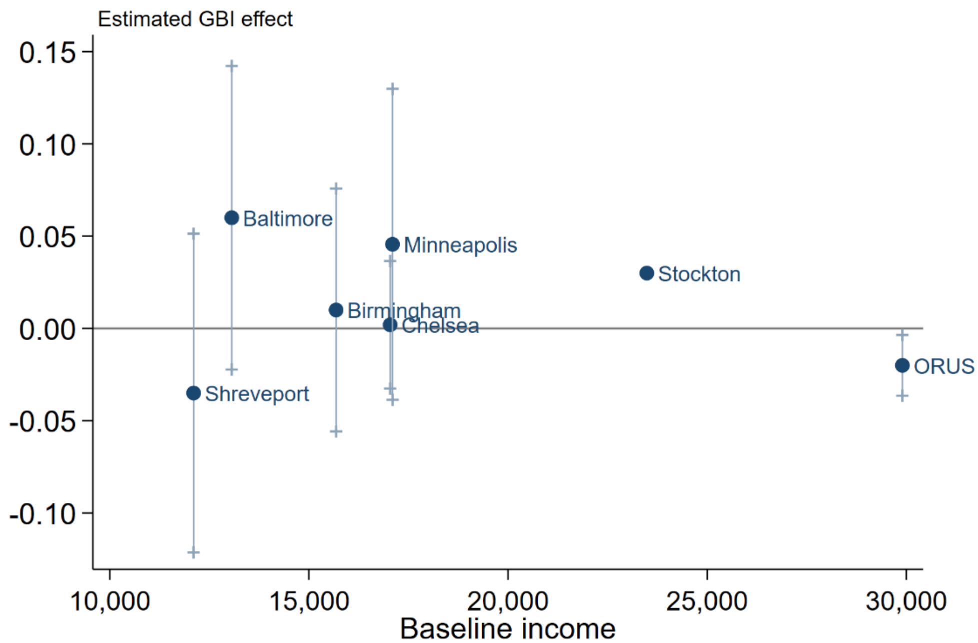


Figure 4. Estimated effects of basic income across studies

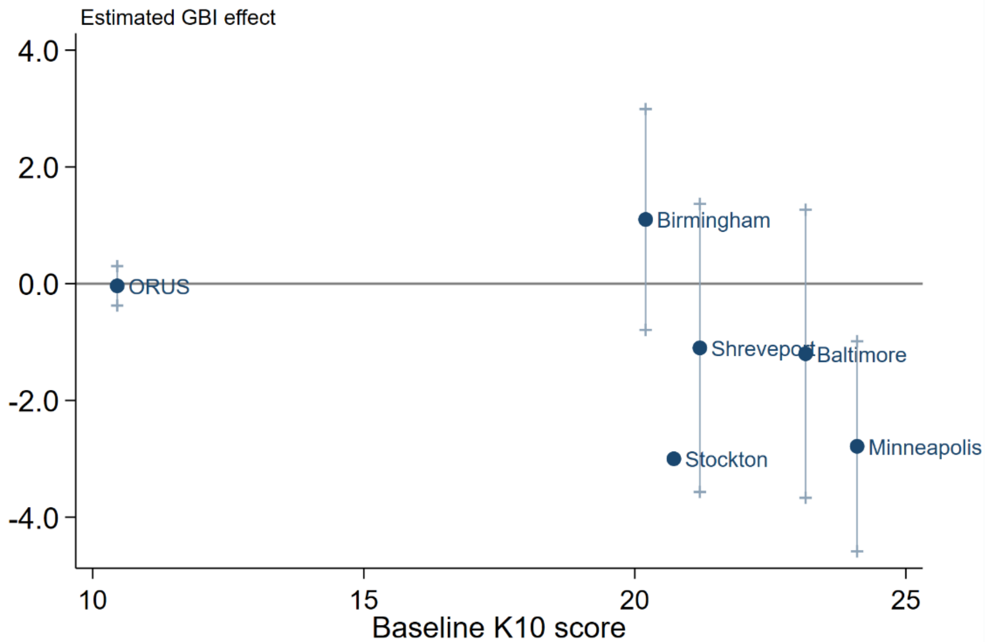
A. Food security effects vs baseline food security



B. Labor supply effects versus baseline income



C. Mental health effects versus baseline mental health



Notes: The figures plot estimated treatment effects of GBI averaged over all post-baseline waves against either baseline outcome means (panels A and C) or baseline income (panel B). The comparisons studies come from Baltimore, MD (Yang, Thomas, and Juras 2024; $1,000/month for 12 months); Birmingham, AL (Jefferson et al. 2024; $375/month for 12 months); Chelsea, MA (Liebman et al. 2022; $400/month for 9 months); Shreveport, LA (Kappil et al. 2024; $660/month for 12 months); Stockton, CA (West et al. 2021; $500/month for 24 months); and a combination of respondents from Illinois and Texas (Bartik et al. 2024, Miller et al. 2024, Vivalt et al. 2024; $1,000/month for 36 months). Panel B uses current employment as the outcome and, for Baltimore, Birmingham, and Shreveport we sum over full time, part time, gig work, and self-employment. We plot a result from the ORUS studies in panel C, but they use the 6-question Kessler scale as opposed to the 10-question version. We plot their estimates multiplied by 1.67.

Table 1. Study population characteristics versus average Minneapolis characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Income-Eligible Minneapolis Households		GBI Study Population	GBI Estimation Sample (verified eligible and responded to any survey)		
	All	Reweighted by age		All	Control	Treatment
			A. Demographics and Household Structure			
Age	42.76	38.40	38.45	38.10	39.07	37.40
Male	0.45	0.43	0.26	0.24	0.21	0.26
Female			0.71	0.72	0.76	0.70
Nonbinary			0.03	0.04	0.03	0.04
Non-Hispanic White	0.45	0.39	0.20	0.22	0.25	0.19
Household Size	2.13	2.46	2.86	2.80	2.72	2.87
Number of Children Under 18	0.69	1.06	1.40	1.42	1.32	1.49
Number of Children Under 5	0.14	0.23	0.41	0.41	0.43	0.40
			B. Education			
Less than HS	0.18	0.19	0.15	0.11	0.05	0.14
HS Degree or Equivalent	0.19	0.19	0.26	0.25	0.26	0.24
Some College	0.28	0.25	0.30	0.31	0.33	0.29
Post-Secondary Degree	0.35	0.37	0.30	0.34	0.36	0.32
			C. Employment and Income			
Employed	0.54	0.64	0.63	0.62	0.63	0.62
Mean Household Income	$20,110	$22,395	$21,194	$20,872	$21,615	$20,333
HH income < $5,000	0.15	0.13	0.15	0.16	0.13	0.17
HH income < $7,500	0.18	0.15	0.22	0.22	0.20	0.23
HH income < $10,000	0.23	0.19	0.29	0.28	0.26	0.30
HH income < $12,500	0.32	0.26	0.37	0.36	0.34	0.38
HH income < $15,000	0.41	0.33	0.44	0.42	0.42	0.42
HH income < $20,000	0.55	0.48	0.54	0.54	0.53	0.54
HH income < $25,000	0.66	0.57	0.64	0.63	0.61	0.65
HH income < $30,000	0.76	0.69	0.72	0.71	0.69	0.72
HH income < $35,000	0.85	0.81	0.81	0.83	0.80	0.85
HH income < $40,000	0.92	0.90	0.87	0.89	0.88	0.89
HH income < $50,000	0.98	0.98	0.95	0.97	0.96	0.97
HH income < $75,000	1.00	1.00	1.00	1.00	1.00	1.00

Notes: The first two columns contain average characteristics for 1,104 respondents who lived in Minneapolis in the 2021 American Community Survey 5-year file and had family income less than half of area median income (Ruggles, Flood, Sobek, et al. 2024). Column 1 are sample-weighted averages for all such respondents, and column 2 reweights ACS respondents to match the age distribution in the GBI study population. Column 3 includes the 529 participants in our baseline sample. Column 4 includes 326 participants who verified their eligibility for the study and respondent to any post-baseline surveys, which is our largest estimation sample. Columns 5 and 6 include 137 control respondents and 189 treatment respondents from this estimation sample.

Table 2. Average baseline outcomes by treatment and control status in the full study population and the estimation sample

	(1)	(2)	(3)	(4)
	GBI Study Population		GBI Estimation Sample (verified eligible and responded to any survey)	
	Control	Treatment	Control	Treatment
Food Security Index	0.34	0.29	0.32	0.27
Housing Stability Index	-0.03	-0.04	0.04	-0.07
Financial Security Index	0.01	0.02	-0.05	-0.01
Labor Supply Index	-0.20	-0.25	-0.24	-0.18
Psychological Distress Index	25.00	24.40	25.72	25.04
Well-Being Index	0.00	-0.04	-0.06	-0.07
N	329	200	137	189

Notes: The table shows averages of our six outcome variables in the baseline survey by treatment and control status in the full study population (columns 1 and 2) and the largest estimation sample (columns 3 and 4).

Table 3. Respondents and Treatment Probabilities by Stratum

Kids	ZIP Group	Poverty	Respondents (Treatment Probability)				
			Baseline	6 months	12 months	18 months	24 months
No	0	No	49 (39%)	31 (48%)	25 (60%)	27 (59%)	25 (60%)
No	0	Yes	44 (36%)	24 (42%)	22 (36%)	23 (43%)	22 (45%)
Yes	0	No	71 (38%)	52 (44%)	45 (49%)	47 (51%)	47 (51%)
Yes	0	Yes	112 (42%)	75 (52%)	64 (53%)	65 (58%)	66 (59%)
No	1	No	69 (45%)	45 (58%)	44 (55%)	45 (56%)	43 (58%)
No	1	Yes	61 (39%)	33 (61%)	27 (67%)	27 (67%)	27 (67%)
Yes	1	No	38 (39%)	23 (52%)	21 (52%)	20 (55%)	21 (57%)
Yes	1	Yes	85 (39%)	41 (56%)	40 (57%)	38 (66%)	39 (69%)

Notes: ZIP group 0 includes North Minneapolis and ZIP group 1 includes central Minneapolis (largely Cedar Riverside and Phillips neighborhoods).

Table 4. Estimated average treatment effects of basic income on economic security

Month	Control Mean (N)	Treatment Mean (N)	Within-stratum mean difference	Model 1 (X)	Model 2 (LDV)	Model 3 (DiD)
A. Food Security (Share)						
6	0.35	0.5	0.14	0.16	0.16	0.19
	128	168		(0.06)	(0.05)	(0.06)
				[0.04]	*[0.01]*	*[<0.01]*
12	0.32	0.48	0.15	0.14	0.16	0.19
	126	155		(0.06)	(0.05)	(0.06)
				[0.11]	*[<0.01]*	*[0.01]*
18	0.36	0.49	0.13	0.16	0.14	0.17
	125	167		(0.06)	(0.05)	(0.06)
				[0.05]	*[0.02]*	*[0.02]*
24	0.35	0.5	0.16	0.21	0.17	0.21
	120	168		(0.06)	(0.05)	(0.06)
				[0.01]	*[0.01]*	*[0.01]*
Average	0.35	0.5	0.14	0.15	0.15	0.2
	137	189		(0.05)	(0.04)	(0.05)
				[0.02]	*[<0.01]*	*[<0.01]*
B. Housing Stability Index						
6	-0.01	0.09	0.1	0.15	0.12	0.19
	128	168		(0.08)	(0.06)	(0.06)
				[0.17]	*[0.23]*	*[<0.01]*
12	0.02	0.14	0.12	0.13	0.14	0.2
	126	154		(0.07)	(0.06)	(0.06)
				[0.19]	*[0.08]*	*[0.01]*
18	0.02	0.18	0.18	0.18	0.23	0.26
	125	166		(0.06)	(0.06)	(0.07)
				[0.03]	*[<0.01]*	*[<0.01]*
24	0	0.11	0.12	0.12	0.15	0.2
	120	167		(0.05)	(0.07)	(0.07)
				[0.14]	*[0.16]*	*[0.04]*
Average	0	0.13	0.13	0.16	0.18	0.23
	137	189		(0.06)	(0.05)	(0.05)
				[0.02]	*[<0.01]*	*[<0.01]*
C. Financial Security Index						
6	0.01	0.15	0.15	0.16	0.15	0.13
	128	167		(0.04)	(0.04)	(0.05)
				[<0.01]	*[0.01]*	*[0.03]*

12	0.01	0.18	0.19	0.19	0.15	0.13
	126	154		(0.05)	(0.04)	(0.05)
				[<0.01]	*[<0.01]*	*[0.05]*
18	-0.01	0.22	0.23	0.26	0.22	0.2
	125	165		(0.05)	(0.04)	(0.05)
				[<0.01]	*[<0.01]*	*[<0.01]*
24	0.05	0.21	0.18	0.21	0.16	0.13
	120	166		(0.05)	(0.05)	(0.05)
				[<0.01]	*[0.02]*	*[0.08]*
Average	0.01	0.2	0.19	0.21	0.18	0.15
	137	188		(0.04)	(0.03)	(0.04)
				[<0.01]	*[<0.01]*	*[<0.01]*

Notes: The table shows results by survey month (column one) as well as average effects across the four post-treatment survey waves. Columns two and three show means and sample sizes for treatment and control groups. Column four shows the average within-stratum unadjusted mean difference (which is why it is not exactly equal to the overall mean difference between the first two columns). Columns five through seven show point estimates, robust standard errors (in parentheses) and family-wise-error-rate-adjusted *p*-values (italicized in square brackets) from our three controlled specifications. Column five controls for baseline income, education, and age (separately by stratum). Column six controls for the baseline outcome value (also separately by stratum; denoted LDV for lagged dependent variable). Column seven is a difference-in-difference specification (DiD) that equals average within stratum differences in the outcome *change* relative to baseline. The three outcomes are food security (panel A), housing stability (panel B), and financial security (panel C).

Table 5. Estimated average treatment effects of basic income on labor supply

Month	Control Mean (N)	Treatment Mean (N)	Within-stratum mean difference	Model 1 (X)	Model 2 (LDV)	Model 3 (DiD)
6	-0.17	-0.13	0.09	0.01	0.02	0.02
	127	166		(0.09)	(0.07)	(0.07)
				[0.9]	*[0.7]*	*[0.77]*
12	-0.23	-0.05	0.24	0.17	0.14	0.15
	124	153		(0.08)	(0.07)	(0.07)
				[0.13]	*[0.11]*	*[0.07]*
18	-0.21	0	0.28	0.23	0.17	0.15
	124	164		(0.09)	(0.07)	(0.08)
				[0.03]	*[0.08]*	*[0.09]*
24	-0.21	-0.09	0.19	0.2	0.05	0.06
	120	162		(0.1)	(0.08)	(0.08)
				[0.15]	*[0.59]*	*[0.74]*
Average	-0.2	-0.07	0.18	0.16	0.06	0.06
	137	187		(0.07)	(0.05)	(0.06)
				[0.04]	*[0.38]*	*[0.39]*

Notes: The table is structured in the same way as Table 4. The outcome is the labor supply index.

Table 6. Estimated average treatment effects of basic income on psychological and subjective well-being

Month	Control Mean (N)	Treatment Mean (N)	Within-stratum mean difference	Model 1 (X)	Model 2 (LDV)	Model 3 (DiD)
			A. Kessler 10 Scale			
6	23.87	22.07	-1.84	-2.37	-1.13	-1.14
	128	168		(1.13)	(0.86)	(0.99)
				[0.16]	*[0.35]*	*[0.44]*
12	24.64	20.96	-3.96	-3.94	-3.78	-3.76
	126	155		(1.24)	(0.88)	(1.13)
				[0.02]	*[<0.01]*	*[<0.01]*
18	24.4	20.58	-4	-4.9	-3.28	-3.22
	125	167		(1.2)	(0.89)	(1.13)
				[<0.01]	*[<0.01]*	*[0.02]*
24	23.51	21.22	-2.54	-3.15	-1.75	-1.06
	120	168		(1.38)	(1.06)	(1.18)
				[0.13]	*[0.33]*	*[0.52]*
Average	23.99	21.12	-2.83	-3.33	-2.17	-1.99
	137	189		(1.06)	(0.74)	(0.92)
				[0.01]	*[0.01]*	*[0.1]*
			B. Well-Being Index			
6	0.04	0.19	0.15	0.21	0.14	0.16
	128	167		(0.11)	(0.08)	(0.08)
				[0.16]	*[0.19]*	*[0.14]*
12	0.01	0.28	0.3	0.33	0.3	0.31
	126	155		(0.11)	(0.07)	(0.07)
				[0.02]	*[<0.01]*	*[<0.01]*
18	-0.03	0.28	0.34	0.42	0.34	0.32
	125	165		(0.11)	(0.08)	(0.09)
				[<0.01]	*[<0.01]*	*[<0.01]*
24	0.05	0.26	0.24	0.23	0.19	0.2
	120	165		(0.11)	(0.08)	(0.08)
				[0.15]	*[0.1]*	*[0.1]*
Average	0.04	0.27	0.23	0.26	0.21	0.22
	137	189		(0.1)	(0.06)	(0.07)
				[0.03]	*[<0.01]*	*[0.01]*

Notes: The table is structured in the same way as Table 4. The outcomes are the Kessler 10 score (panel A) and the subjective well-being index (panel B).

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