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THE EMPLOYMENT EFFECTS OF A GUARANTEED INCOME: EXPERIMENTAL EVIDENCE FROM TWO U.S. STATES

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ABSTRACT

We study the causal impacts of income on a rich array of employment outcomes, leveraging an experiment in which 1,000 low-income individuals were randomized into receiving $1,000 per month unconditionally for three years, with a control group of 2,000 participants receiving $50/month. We gather detailed survey data, administrative records, and data from a mobile phone app. The transfer caused total individual income excluding the transfers to fall by about $1,800/year relative to the control group and a 4.1 percentage point decrease in labor market participation. Participants reduced their work hours as a result of the transfers by 1-2 hours/week and participants' partners reduced their work hours by a comparable amount. Among other categories of time use, the greatest increase generated by the transfer was in time spent on leisure. Despite asking detailed questions about amenities, we find no impact on quality of employment, and our confidence intervals can rule out even small improvements. Treated participants broadly increase expenditures, led by spending on non-durable goods and services, with smaller increases in spending on durable goods and human capital. We observe no significant effects on degree attainment, though the magnitudes of the estimated effects generally appear larger among younger participants. Measures of subjective well-being are higher among treated participants in the first year of the transfers but then revert to control group levels. Overall, our results suggest a moderate labor supply effect that does not appear offset by other productive activities.

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I Introduction

The design and success of cash-based public poverty alleviation programs depend critically on how income affects beneficiaries' labor supply and other employment-related outcomes. Means-tested transfer programs distort returns to work, causing some individuals to reduce work hours or earnings to preserve eligibility. These concerns have contributed to growing interest in unconditional cash transfer and guaranteed income programs, which avoid explicitly disincentivizing work. However, even without steep phase-outs, unconditional cash transfer programs may still reduce labor supply through income effects, increasing the fiscal costs of public programs. At the same time, transfers that relax credit or liquidity constraints could potentially support entrepreneurship or human capital formation, enable longer job searches that yield better matches, reduce barriers to employment, or facilitate productive non-work activities such as caregiving. Understanding the net effect of cash transfers therefore requires examining impacts across several dimensions. We consider the effects of a large unconditional cash transfer program leveraging a combination of administrative records, enumerated and online surveys, and data from a custom mobile phone app, providing a detailed view of how many key outcomes change due to an income effect.

Extensive research has examined the impacts of income on labor supply (Krueger and Meyer 2002; Chetty 2012). Much less is known about the impact of unearned income on other significant aspects of the labor market, including quality of employment, entrepreneurship, and human capital investment. We also have limited evidence on how additional income affects recipients' broader allocation of time, or how recipients might trade off work and competing priorities such as home production, caregiving, leisure, and self care when more resources are readily available. These outcomes are difficult to measure using the administrative and survey data sets typically used in existing research, yet they can be important in predicting the long-run impacts of cash transfers, as well as being valuable to understand in their own right. Given the increased interest in cash transfer programs, such as the Child Tax Credit and the Alaska Permanent Fund, this paper also provides timely micro-level evidence on these broader effects.

We investigate the causal effects of income on employment and related outcomes by analyzing a program implemented by two non-profit organizations that distributed \$1,000 per month for three years to 1,000 low-income individuals randomized into the treatment group, with an additional 2,000 participants randomly assigned to receive \$50 per month as the control group. We merge rich survey data, with response rates of 97% at midline and 96% at endline, with administrative records and data from a custom mobile phone application. By collecting and merging a comprehensive set of outcome variables, we are able to answer questions that have previously eluded causal estimation. For example, if people work a little less, as prior literature might suggest, what do they do with their time instead? This question has important policy implications: decision-makers may want to know whether participants engage in activities with positive spillovers, such as education or caregiving, and understanding how participants spend their time may also be informative of what improves their well-being by their revealed preferences. Moreover, if the transfers enable unemployed participants to search longer for work, does that translate into any changes in the quality of their employment? Are there effects on entrepreneurship or human capital investments? Are increased expenditures largely concentrated in non-durable or durable goods and services? Do the transfers lead to lasting improvements in participants' subjective well-being?

The transfer program we study is particularly policy-relevant as it targets lower-income individuals, the focus of virtually all cash transfer programs in the U.S. Individuals between the ages of 21 and 40 whose total household income did not exceed 300% of the Federal Poverty Level (FPL) in 2019 were eligible to participate, with most of the sample falling below 100% or 200% of the FPL. Participants reported an average household income of about $29,900 in 2019, so the transfers represented a 40% increase in household income. The sample approximated the broader U.S. population among those who meet the program's income and age eligibility criteria, and we ensured balance between treatment and control groups across a wide range of baseline characteristics. The study's experimental design allows us to estimate the causal effects of the transfer with minimal assumptions, and we pre-registered our analyses.[1]

Examining the effects of the cash transfers on income and labor supply using a combination of state Unemployment Insurance (UI) records and survey data, we find total individual income excluding the transfers fell by about $1,800 per year relative to the control group, with these effects growing over the course of the study. This relative decrease takes place during a period when nominal wages were rising rapidly for low-income workers, including both the control and treatment group. The program caused a 4.1 percentage point reduction in the extensive margin of labor supply and a 1-2 hours/week reduction in labor hours for participants. The estimates of the effects of cash on income and labor hours represent a 5-6% decline relative to the control group mean, a moderate effect. Inter-

[1] AEARCTR-0006750. Changes since the pre-analysis plan was registered are described in Appendix G.

estingly, partners and other adults in the household appear to adjust their labor supply by about as much as participants. For every dollar received, total household income excluding the transfers fell by about 28 cents, and total individual income fell by around 16 cents. Estimates show broadly similar trends using administrative data alone and survey data alone, though some treatment effects appear to have larger magnitudes in administrative data.

We captured time use using a combination of 24-hour time diaries delivered through a mobile phone app on randomly-selected weekdays and weekend days and survey questions adapted from the American Time Use Survey (ATUS). The time diaries and ATUS-style questions support the findings for employment. Treated participants primarily use the time gained through working less to increase leisure. We can reject even small changes in several other specific categories of time use that could be important for gauging the policy effects of an unearned cash transfer, such as time spent on childcare, exercising, searching for a job, or self improvement.

Despite asking extremely detailed questions about workplace amenities, we find no substantive changes in any dimension of quality of employment and can rule out even small improvements, rejecting increases in the index of more than 0.022 standard deviations and wage increases of more than 58 cents. Treatment group participants expressed more interest in entrepreneurial activities and a greater willingness to take financial risks, but the coefficient on starting a business is close to 0 and not statistically significant. Administrative data from the National Student Clearinghouse (NSC) show no significant impacts on post-secondary education overall, though larger point estimates are observed among younger participants, which could potentially help to explain the labor supply effects within this subgroup. Those in the treatment group also self-report increased rates of disabilities that limit the work they can do, perhaps due to increased medical care. We observe significant impacts on duration of non-employment and unemployment. The average spell of non-employment in the control group lasted 7.8 months; the treatment increased this by 0.8 months. Compared to estimated impacts of UI benefit extensions, this effect is relatively small (Cohen and Ganong 2024). Treated participants were more likely to have recently applied for work but applied to fewer positions on average. We see no significant reductions in barriers to employment.

Most of the transfers were allocated to consumption expenditures, led by non-durable goods and services, though we also see increases in spending across other categories, most notably on human capital and durable goods. Despite some treated participants moving labor markets, labor market quality does not significantly improve. We see no significant changes in marriage or divorce on net.

Finally, we see temporary improvements in subjective well-being across various measures in year one, but these revert in years two and three, mirroring the patterns in the effects of this intervention on stress and mental health in Miller et al. (2025) and on measures of financial health in Bartik et al. (2025).

This study builds on the previous literature in several ways. Previous studies estimated the effects of programs that affected both income and the returns to working, such as the Earned Income Tax Credit (EITC) or a Negative Income Tax (NIT) (e.g., Ashenfelter and Plant 1990; Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Nichols and Rothstein 2016). However, programs like these affect beneficiaries' labor market incentives because the size of the benefit is linked to the amount of earned income. To isolate a pure income effect, several studies have examined lottery winners. However, 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. 2017). Lottery players may also be selected, such as being generally higher-income and more risk-loving than the individuals a public guaranteed income program might target (Golosov et al. 2024). Other recent quasi-experimental evidence of responses to exogenous increases in income comes from studies of the Alaska Permanent Fund (Feinberg and Kuehn 2018; Jones and Marinescu 2022), which was relatively small in magnitude ($1,606 USD in 2019), and casino disbursements to Native American families in the U.S. (Akee et al. 2010).[2]

In contrast to the preceding literature, a key advantage of this study is the ability to combine experimental variation in a large unconditional cash transfer with uniquely rich data. Existing studies largely rely on administrative data sets with limited information on the individuals, despite theoretical and empirical evidence that contextual factors and preferences matter (*e.g.*, Cox and Oaxaca 1990; Atkinson and Micklewright 1991; DellaVigna and Paserman 2005; Boswell, Zimmerman and Swider 2012). We collect very detailed data about participants from administrative records and surveys, enabling a more nuanced understanding of their labor supply and time use decisions situated within the context of other choices they face. The administrative data include quarterly earnings and employment information reported by employers to UI agencies from the two states from which participants were recruited, as well as NSC data on post-secondary educational outcomes. The survey data were collected through a combination of in-person and phone-based surveys implemented by the Survey

[2]There is also an important literature on cash transfers in a developing country context, *e.g.*, Bertrand, Mullainathan and Miller (2003); Fiszbein et al. (2009); Banerjee et al. (2017); Mostert and Castello (2020). These results are important but may not generalize to the U.S., given the significant contextual differences.

Research Center (SRC) at the University of Michigan as well as frequent web-based surveys and a mobile phone app.

The comprehensive data collection enables us to analyze marginal propensities to earn and consume with weaker assumptions relative to the literature. For example, lottery studies generally have to infer consumption from a model in which individuals are assumed to smooth consumption over time according to the permanent income hypothesis, governed by Stone-Geary utility and an assumed discount rate. However, as we observe consumption directly, we do not need to do this. Moreover, since treated participants do not appear to save a substantial portion of the transfers, on net, we can show that these models do not describe our participants well. Compared to the literature, participants in our study appear to spend nearly all the money they receive each month on increased consumption or reduced labor. Since estimates of the marginal propensity to earn (MPE) greatly depend on the denominator, *i.e.*, how much of the transfers participants allocate to spend that period as opposed to saving to spend in future time periods, these data are important in accurately understanding labor supply responses.

Due to the detailed data collection, this study also allows us to speak to an ongoing debate in the literature as to whether expansions of the social safety net lengthen unemployment but ultimately result in better job matches between job seekers and employers. This literature has historically focused on changes in the generosity of employment insurance, but similar arguments could apply to job search under the increased security of monthly cash transfers. The literature, mostly from European countries, is mixed (Centeno 2004; Card, Chetty and Weber 2007; Lalive 2007; van Ours and Vodopivec 2008; Caliendo, Tatsiramos and Uhlendorff 2012; Nekoei and Weber 2017). In addition to drawing from other countries with more generous social safety nets, past papers in this literature have often had limited information on job quality, inferring job quality from income or the duration that the post-unemployment job was held. In contrast, we have a rich array of variables we can use to identify quality of employment and characterize the jobs participants are applying to.

Our study also contrasts with recent work on several randomized cash transfer programs. Chelsea Eats, in Chelsea, MA, provided $400/month between November 2020 and August 2021. This study focuses primarily on food consumption and financial well-being and does not find significant effects on employment or work hours (Liebman et al. 2022). Baby's First Years provided low-income new mothers in a "high" cash arm with $333/month for 72 months, starting in May 2018-July 2019, with an additional "low" cash arm receiving $20/month. These transfers were provided on a debit card labeled “4MyBaby”, and participants were spread across four U.S. cities. The evaluators did not find any effects on maternal employment (Sauval et al. 2024; Stillwell et al. 2024). Jaroszewicz et al. (2024) examine a U.S. program which randomized individuals to receive a one-time transfer of either $500, $2,000, or nothing between July 2020 and May 2021. They find small negative effects on earned income and null effects on employment. The Compton Pledge provided transfers of $450 per month on average over a two-year period (Balakrishnan et al. 2024). They find moderate decreases in both income excluding the transfers and consumption. Relative to the treatments investigated in these studies, the program we study provided a substantially larger total transfer per participant through a combination of higher monthly payment, longer duration, or both. The duration of the program may be important given that in our study we observe different effects over time. We benefited from extremely high survey response rates and limited differential attrition and importantly leverage administrative records, which appear to show a larger effect on labor supply than the survey data alone would suggest. Finally, we collected a wider range of employment variables than any existing study.

Our results demonstrate that monthly cash transfers have a moderate effect on labor supply and that this decline in formal sector production is not fully offset by substitutions towards other productive activities like human capital investments or home production. We also do not find support for other hypothesized benefits to long-run employment, like an improved quality of job fit, though it is possible that a subset of participants are making investments with payoffs that will take longer to observe. However, even abstracting from any potential longer-run benefits, the marginal value of public funds (MVPF) of the transfers remains close to one, because the observed labor supply responses generate only modest declines in tax revenue. For a policymaker interested in cash transfers, the main benefits of such a program would flow through the increased choice they offer participants in how to spend their time and invest for the future or the increased consumption they allow, even if relatively few use the transfers for any one given purpose.

In the following sections, we describe the sample and approach in more detail, present results, examine heterogeneity and compare our findings to the broader literature.

II The OpenResearch Unconditional income Study (ORUS)

II.A Recruitment

The study took place in two sites: ten counties in north central Texas, including the Dallas area, where the cash assistance program was implemented by a local 501(c)(3) non-profit organization, and nine counties in northern Illinois, including the Chicago area, where an identical program was implemented by an Illinois-based non-profit. Both sites combined participants living in urban counties (from the counties containing Dallas, Forth Worth, or Chicago, respectively), suburban counties, medium-sized urban counties, and rural counties. The sites are depicted in Figure I.

A total of 3,000 people were enrolled in the program. Individuals between the ages of 21 and 40 whose total household income did not exceed 300% of the FPL in 2019 were eligible to participate. To prevent participants from losing key benefits, the implementing organizations excluded individuals from households receiving Supplemental Security Income (SSI) as well as individuals receiving Social Security Disability Insurance (SSDI) and those living in publicly-subsidized housing. Extensive effort was taken to protect eligibility for other public assistance programs. In Illinois, SB 1735 was passed, protecting SNAP, TANF, child care assistance, Medicaid, and energy assistance.[3] In Texas, only Medicaid and energy assistance were protected, but benefits in Texas are less generous and eligibility criteria more restrictive. Appendix Table A.1 summarizes the protection status of specific programs. The transfers were not conditioned on research participation and were considered gifts from non-profit organizations and not taxable income.

Potential participants were recruited in three ways. Most participants (87%) were recruited via a mailer inviting them to participate in a cash assistance demonstration program in which they would receive "$50 or more" each month if they were chosen to enroll. Addresses within program counties were selected to receive mailers based on information from a commercial data vendor which provided address data and demographic details about residents at each address. Approximately 69% of mailers were sent to individuals who appeared to be eligible based on age and household income, while 31% of mailers were sent without any targeting to avoid systematically excluding individuals who were eligible whose data might be missing or inaccurate. Mailers were addressed to a maximum of one person at each address, and "or Current Resident" was appended to the address line. Interested recipients were then directed to a website to complete a brief intake survey to determine eligibility. To encourage responses, recipients were offered randomized incentives of $0 to $20 to complete the survey, and online gift cards were delivered immediately upon completion via email to increase trust. Individuals who did not respond to the initial contact were randomly assigned to receive between 0 and 4 follow-up letters. A flowchart of the recruitment process is provided in Figure II.

A smaller share of participants were recruited by alternative methods. Approximately 1 percent

[3] Further details on the bill are provided in Appendix Figure B.1.

of the sample was recruited through Facebook and Instagram advertisements shown to users who appeared eligible for the program based on age and county. An additional 12 percent were recruited through "Fresh EBT", a free mobile application used by over 4 million Supplemental Nutrition Assistance Program (SNAP) recipients nationwide to check their balance and manage their benefits. Advertisements were displayed only to users in eligible zip codes.

II.B Randomizations

There were two randomizations, described in more detail below. The first randomized individuals either into the main study sample, where those enrolled would later be eligible to receive an unconditional transfer of either $50/month or $1,000/month for three years, or out of the main study sample. The second randomization, conducted after enrollment in the main study sample was complete, randomly assigned individuals to either the treatment or control group.

II.B.i Randomization to the Main Study Sample

The first randomization selected the main study sample from eligible applicants. This randomization was structured to ensure that the study sample satisfied three design criteria. First, the sample needed to include a minimum of 20% non-Hispanic White, 20% Black, and 20% Hispanic participants. Second, it needed to reflect the program's income targets: at least 30% of participants below 100% of the FPL, a minimum of 30% between 100% and 200% of the FPL, and no more than 25% between 200% and 300% of the FPL (we refer to these bins as "FPL groups"). Third, in terms of gender representation it needed to broadly reflect the gender distribution in the eligible population according to data from the American Community Survey (ACS), though this criterion was applied flexibly to meet FPL group by state targets. To achieve the desired sample, we blocked participants on demographic characteristics and randomized a larger share from some blocks to the study sample.

II.B.ii Enrollment

After the first randomization, contact information for individuals selected into the sample of potential participants was shared with SRC on a rolling basis. Participants were first enrolled in the $50/month cash transfer program before being invited to participate in the research. Consenting individuals then completed a comprehensive baseline survey and were asked if they wished to allow the research team to access their administrative data. As part of the enrollment procedures, participants also provided bank account information so funds could be transferred via direct deposit. For the 348 individuals without an existing account, an online bank account was created so they could receive their transfers.[4] Enrollment was conducted in person from October 2019 to March 2020, then by phone through October 2020 until all 3,000 individuals were enrolled.

The extended baseline period was intentional. First, it allowed us to obtain a large amount of pre-intervention data through monthly surveys. Second, it enabled us to monitor early attrition before conducting the second randomization to the $50 or $1,000 monthly transfers. We believed attrition might be highest in the first few months of the study, and this design ensured that treatment assignment could be balanced on baseline participation. Participants were paid $10 for each monthly survey and $50 for the longer enrollment survey. Everyone received the $50/month unconditional transfer during this baseline period. Participation in the program did not depend in any way on participation in research activities.

We tested whether the population enrolling in the study was different from the broader population by re-weighting the population in the ACS to match our FPL group and county type stratification variables. While we cannot rule out differences in unobservables, it is reassuring that differences in observables appear small (Table I). Participants are comparable to the broader population on all measures, except for being slightly more likely to rent, have a college degree, or be female.

II.B.iii Randomization to Treatment or Control

After enrollment, we conducted the second randomization to assign participants to receive either $50 per month ("control") or $1,000 per month ("treatment") unconditionally for three years. We focus our analyses on differences between these two groups.

In the second randomization, all participants had an equal 1 in 3 probability of assignment to the treatment group. We implemented a blocked random assignment process to ensure balance across key strata and imposed a minimum p-value for differences between the treatment and control group on a wide range of baseline covariates. A balance table focusing on employment outcomes is presented in Table II. At baseline, 58% of participants were employed, total household income in the year before enrollment averaged about $29,900, 17% held a second job, 57% had children living in the household, and 33% were living with a romantic partner. The average household size was 3.0 people (including the participant), and about 20% of the sample had a bachelor's degree.

During enrollment, we identified a handful of participants who knew each other or lived at the

[4] In a few cases in which a participant did not pass KYC to open a bank account, a reloadable debit card was provided.

same address (such as a large apartment building). We grouped these individuals into "clusters", and each cluster was assigned to either treatment or control together. This process produced 18 clusters of 2 people and 2 clusters of 3 people; all remaining clusters contained a single participant. Given that randomization to treatment or control occurred at the cluster level, standard errors in our analyses are also clustered at this level.[5] We conducted simulations to confirm that every cluster had a 1 in 3 chance of assignment to the treatment group. Further experimental details are provided in Appendices C and D.

II.C Cash Transfers

After the second randomization, participants in the treatment group were notified of their increased transfer amount. Both treatment and control participants were reminded of the three-year transfer timeline, and the implementing partners reinforced this information repeatedly during the final year of the program.[6] The cash transfers were fully unconditional, and participants in the treatment and control arms continued to receive them regardless of research participation.

Enrollment in ORUS concluded in October 2020. Randomization into treatment and control took place immediately thereafter, and treatment ran from November 2020 through October 2023. This timing means that the intervention overlapped with COVID-related disruptions, particularly in the first year, while years 2 and 3 occurred after vaccines became widely available. Since our analysis relies primarily on data from 2022 and 2023, with 2023 weighted most heavily, our results predominantly reflect the post-COVID-19 era, particularly compared to other cash transfer pilots.

III Data Collection and Outcome Measures

We collected four types of data: (1) administrative records; (2) data from in-person and phone interviews conducted by SRC; (3) data from web-based surveys; and (4) data collected using a custom mobile phone application.

III.A Administrative Data

We leveraged data on income and employment from Illinois and Texas UI agency records. Employers are required to report quarterly employment and earnings for all employees in UI-covered positions

[5] There is one exception: the Texas UI data provider did not permit the cluster variable to be included in the environment due to privacy concerns. In these data we cluster at the individual level, but do not expect this to meaningfully affect our inference given that very few participants were randomized in clusters.

[6] Most participants had been enrolled for several months prior to the start of the treatment period, receiving $50/month during this period. This extended enrollment period built trust and familiarity with the program. Qualitative interviews suggest participants understood and believed the three-year duration of the transfers.

to state agencies. UI records cover most formal employment, but miss some independent contractors and other "alternative" work arrangements, which recent estimates place in the single- to low-double-digit percent range (Katz and Krueger 2019; Bernhardt et al. 2023; Graham et al. 2022).

A total of 87.5% of participants consented to administrative record linkage. In Illinois, participants were matched to UI agency records by SSN within the Administrative Data Research Facility, while in Texas, the matching was done within the Texas Education Research Center. Among those who provided a full SSN, nearly all were able to be matched, but providing a full SSN was optional. Of those who consented to share administrative data, but not conditioning on provision of SSN, we obtained match rates of 71% in Illinois and 73% in Texas. In Illinois, we were able to analyze survey data alongside the administrative records.[7,8] Given that the ultimate match rate is lower than in Golosov et al. (2024) and Cesarini et al. (2017), we present results using Lee bounds in the appendix (Appendix Table A.8) as a robustness check. Match rates were very similar between treatment and control groups, so the results from the Lee bounds analyses are consistent with our main findings.[9]

We also linked participants to administrative data on post-secondary educational enrollment and completion from the NSC. The NSC covered 97% of post-secondary institutions in the U.S. over the transfer period (Clearinghouse 2025). These data include information on enrollments, progress in the degree, and degree attainment, as well as descriptive details about the fields of study pursued. We supplement these records with survey data for those who did not consent to linkage.

Finally, we leverage information on debt from individual-level linkages of consenting participants to credit report data from a major credit reporting agency and again supplement with survey-reported debt measures for those who did not consent to external linkages.

III.B Enumerated Survey Data

Trained enumerators from SRC conducted surveys with participants prior to the start of the cash transfer payments ("baseline"), after approximately 18 months of transfer payments ("midline"), and after approximately 30 months of payments ("endline"). The midline interviews took place between

[7] In Texas, we were unable to bring such detailed data into the administrative data environment, though we did bring in 56 baseline covariates.

[8] In general, we construct outcome variables in the administrative data so as to match the survey data, *i.e.*, using data for an individual based on the quarter in which they took the survey.

[9] Requesting consent to examine administrative data early in recruitment was essential in ensuring balance across treatment and control groups. First, the initial online screener asked potential participants whether they would consent to share their administrative data. This was not required and did not affect the odds that someone would be selected for the study, but most participants provided consent at this stage. At the time of enrollment in the baseline survey, participants were asked again if they would like to provide consent for their administrative data to be analyzed. 78.7% provided consent by baseline, and this increased to 87.5% by endline. Participants were asked prospectively for consent to analyze outcomes in administrative data for 30 years.

April 3 and August 2, 2022, while the endline ran from March 30 to August 15, 2023. The endline was intentionally scheduled several months prior to the final transfer to avoid capturing changes in behavior from anticipation of the program ending. A timeline of the main study events appears in Figure III.

To limit response burden, some survey modules were administered as separate online surveys following the SRC surveys. Participants received $50-$100 for completing the SRC-administered surveys and $15-$30 for each accompanying online survey.[10]

Response rates for the SRC surveys were very high. At the time of the midline survey, approximately 1.5 years into the cash transfer period, when a participant might have been enrolled in the study for 2 years, we obtained a response rate of 97%. At endline, a year later, we obtained a response rate of 96%.[11]

III.C Web-based Survey Data

We measured many outcomes using data from monthly surveys administered via the Qualtrics web-based survey platform.[12] These surveys included ATUS-style questions on time use with different lookback periods as a complement to the mobile app-based time diaries, as well as questions on job search, quality of employment, job satisfaction, hours worked, intrahousehold employment outcomes, housing search and labor market mobility, subjective well-being, and participation in formal and informal education and training, among other outcomes. Participants received $10 for each completed survey.

The questions that were asked on each survey varied from month to month, and key modules were repeated several times per year, providing multiple opportunities to collect information that might have been missed in any one survey. In our analyses, we aggregate participant responses to online surveys at the annual level.

Response rates to the monthly web-based surveys remained high throughout the study: 98% of participants completed at least one survey in year 1, 96% in year 2, and 94% in year 3 (Appendix Figure B.2).

[10] At baseline, a $50 kept appointment bonus was offered near the end of the enrollment period, and at midline and endline participants were randomly assigned to receive a kept appointment bonus of $0, $25, or $50. During the final weeks of the mobile endline surveys, incentives were increased from $15 to $30.

[11] For the associated online follow-up surveys, we obtained response rates of 93.7%, 91.0% and 89.2% at midline and 95.2%, 93.2%, 91.1% and 88.6% at endline.

[12] The online modules were not asked at the time of the midline and endline to avoid survey fatigue.

III.D Mobile Application Data

Participants also used a mobile phone application developed by Avicenna Research for both passive and active data collection for the study. Daily time diaries are widely regarded as the gold standard for measuring time use, and the app provides a user-friendly calendar interface that allows respondents to report all of their activities in a 24-hour period by dragging activities into time slots. The interface supports reporting of both primary and secondary activities (*e.g.*, cooking while watching television). Each month, participants were asked to complete time diaries on a randomly-selected weekday and weekend day each and were compensated $5 per diary completed.[13] A screenshot of the interface appears in Figure IV.

The time diaries achieved high response rates and were collected frequently, yielding a rich set of repeated measures. The web-based surveys achieved even higher response rates, but were administered less frequently. We present results for both modalities.

III.E Response Rates and Attrition

We took several proactive steps to minimize attrition and non-response. First, the control group was provided with a small monthly transfer ($50), which likely played an important role in maintaining engagement; other unconditional cash programs with high response rates, such as Baby's First Years, have also compared high- and low-cash arms (Sauval et al. 2024). At enrollment, we also asked participants to provide the contact information of two other people who could be reached in case the participant's contact information was no longer valid, and participants were asked to update this information at midline and endline. The monthly web-based surveys offered another opportunity for participants to update their contact information, and the frequent contact enabled us to proactively reach out to participants to reduce attrition. These surveys were intentionally designed to be short and engaging to sustain interest, with response times and attrition patterns closely monitored, and we adjusted survey content over time based on participant feedback. Participants received multiple email and text reminders to respond to surveys, along with periodic higher-effort outreach such as phone calls and postcards that appeared to be handwritten. Other measures were also taken to build trust: for example, we built a custom payments system that automatically initiated the direct deposit of incentive payments to participants' bank accounts immediately upon completion of each survey or app activity.

[13] In May 2021, we switched from asking participants to complete two time diaries per month to asking them to complete two time diaries in 7 of the 12 months of the year to reduce response burden.

Attrition was extremely low, particularly given the length of the study. Differential attrition was only 1.7% at midline and only 3.2% at endline. For the monthly online surveys, we observed no significant differential attrition in years 1 and 2 after pooling across surveys within the year, and 4.3% in year 3. Differential attrition in the app-based time diaries averaged 6.0% across the three years of the study.[14]

Although attrition was very limited, we take several measures to mitigate concerns that differential attrition could bias results. First, we prioritize administrative data outcomes, for which we do not observe differential attrition. Second, we verify that respondents and non-respondents appear similar across a wide range of baseline covariates (Appendix Tables A.2-A.6). Third, we report Lee bounds estimates that conservatively correct for potential selection at the expense of precision. Fourth, we present a set of results restricting attention to the midline and endline surveys, which achieved particularly high response rates. Finally, for outcomes with baseline values, we implement a difference-in-differences approach that relies on parallel trends rather than on balanced respondent composition. All robustness checks are included in the appendix.

IV Method

Our main analyses estimate the effect of the cash transfers on employment outcomes through the following specification:

$$Y_i = \alpha + \beta Treated_i + \gamma X_i + \varepsilon_i \tag{1}$$

where Y represents a given post-treatment outcome variable, i represents the individual participant, *Treated* is an indicator variable denoting treatment status, and X is a matrix of Lasso-selected controls.[15]

Given that we have outcome data from multiple time periods, we pre-specified how these measured would be aggregated. Our preferred specification pools results across time periods, increasing statistical power and generating a single aggregate measure capturing effects over the study period, though also we present disaggregated results. We pre-specified that we would place more weight on the endline outcomes (70%) than the midline outcomes (30%), and that we would similarly place

[14] 4.4% in year 1, 7.5% in year 2, and 6.5% in year 3.

[15] For outcomes measured with administrative data in Texas, privacy constraints limited us to importing 56 baseline covariates (demographics, employment, income, household composition, relationship, and county type). We ran Lasso within the secure environment using this restricted set. In Illinois, we could include the full set of baseline covariates.

more weight on online survey responses in year 3 (50%) than in year 2 (30%) or year 1 (20%). We place more weight on the results from later years for three reasons. First, if the effects of the transfers accrue over time, this approach would better capture them. Second, one unique feature of our study is the relatively long transfer duration, and we are primarily interested in changes that might occur over extended periods of time. Third, by focusing on later years, we anticipate that our findings will have greater external validity given the COVID-19 pandemic potentially affecting the first year of the study, as it did for many cash transfer programs around that period. We also pre-specified that we would place more weight on the SRC-enumerated survey data (70%) than online survey data (30%), given that the SRC data may be higher-quality and have less non-response bias. If a participant is missing data from a particular survey year (*e.g.*, they have endline but no midline data), we re-distribute the weight from the missing time period to the non-missing periods. Then, to estimate equation (1) on these pooled outcomes, we collapse the survey by individual level outcomes to the individual level, taking the weighted average over all non-missing time periods, yielding one observation per participant in our regression. Participants are included in the regression analysis for a particular outcome if they have at least one non-missing measure of that outcome during the treatment period; otherwise, their outcome is missing and they are not included.

Given the number of outcomes we examine, we take steps to correct for multiple hypothesis testing. Here, we take two approaches. First, we generate summary index measures to reduce the number of primary hypothesis tests, following Kling, Liebman and Katz (2007). Constructing a hierarchy of outcomes, we group related measures into "families" of outcomes, with several "components" capturing the same theoretical construct within a given "family", and specific "items" (*e.g.*, responses to a survey question or a specific outcome variable in administrative data) within the "component". For example, one family of outcomes we consider is the impact of the transfers on quality of employment, but there are many dimensions to quality of employment. One dimension of interest is participants' day-to-day experience at work, which could include factors such as whether they face discrimination at work or whether their boss treats them fairly. Questions asking about these factors ("primary items") would be combined with similar questions under a "quality of work life" component, which in turn would be combined with other components in the "quality of employment" family. The index measures are constructed by taking the standardized estimates from item-level analyses and aggregating them within components using seemingly unrelated regression. The component-level estimates are then combined into families by averaging the standardized effects. Prior to being combined in an index, items are reversed if necessary so that a positive treatment effect consistently represents a positive impact. For interpretability, we also report all item-level estimates in raw units (neither standardized nor reversed). Sometimes items in a family are pre-specified as "secondary" and not included in the index.

Our second approach to reduce the risk of "false positives" is to report false discovery rate (FDR) adjusted q-values for estimates in the main results. Following Guess et al. (2023), we organize estimates into tiers for multiple comparison adjustments. This approach recognizes that some estimates may be higher-priority than others and, as long as treatment of these estimates is pre-specified, we can conduct secondary analyses without penalizing higher-priority tests. The family-level estimates constitute the top tier, and all family-level estimates are pooled when constructing q-values. Component-level estimates occupy the next tier and are pooled with the family-level and other component-level tests within the family. Primary items are pooled with all family-level, component-level, and item-level tests within the family. The last tier of the hierarchy includes exploratory analyses, including any secondary items, subgroup analyses, and estimates disaggregated by time period. Further details are provided in Appendix E.

V Results

V.A Income, Labor Supply, and Time Use

The transfers led to substantial reductions in earned income. Rows (1) and (2) of Table III show a decline in total household income excluding the transfers of about $4,300 per year (with a standard error of $1,000) and a decline in total individual income excluding the transfers of $2,400 (s.e. $700) per year. These estimates are based on survey questions that asked participants to report a single number for their household income and a single number for their individual income.[16] Additionally, participants were asked about their earnings from each specific job they held as well as other sources of income. Summing across these categories, we estimate a negative effect on total calculated individual income of $1,400 (s.e. $900) per year in row (3). This estimate is slightly smaller in magnitude than the estimate obtained in row (2), but the control mean of the calculated measure is slightly larger, suggesting that individuals recalled more income when prompted to consider more finely-grained

[16] Specifically, the question on household income asked about total household income, while the question about individual income asked for one number representing total income earned from employment, so the latter question was aggregated with questions about temporary work not already reported, passive income and other sources of income such as government transfers in constructing this outcome variable.

sources of income.

Considering the sub-components of this calculated measure separately, we observe the decrease in total calculated individual income is driven by declines in individual salaried/wage income in row (4). Self-employment income represents the next-largest category of income according to the control mean, and we do not observe a quantitatively large or significant decline here (row 5). Income from supplementary gig work contributes an average of $400 per year on average in the control group, and we do not see large or significant changes in this category either (row 6). While it is possible that this category is more meaningful for some participants, on net we do not see substantial impacts. Participants have essentially no passive income, and we do not detect any effect on this category of income (row 7). "Other" income (row 8) consists of gifts from family and friends as well as monetary government transfers.[17] These contribute a meaningful share of participants' total income, but the point estimate suggests these may only decrease as a result of the program by about $100 (s.e. $200) per year on net, a difference that is not statistically significant. Breaking government transfers out separately (row 9), we see a $100 (s.e. $100) decline per year, but this result is also not statistically significant.

Turning to consider administrative data from UI records, we estimate a $1,700 (s.e. $900) decline in annual individual salaried/wage income in row (10). This point estimate is larger than the survey-only estimate of $1,300 (s.e. $800) per year in row (4). Pooling UI data for those who consented to share these data and could be matched with survey data for those who did not consent to share administrative data, we obtain an estimate of about $1,600 (s.e. $900) per year (row 11).[18] The pooled results in row (11) differ from the UI data-only results in row (10) by only about $160 and are not restricted to those who were matched, so we treat the pooled results as our preferred measure. Appendix Table A.47 contains a more detailed comparison of administrative and survey data, and Appendix K describes our approach to pooling across data sources.

Given that we have multiple income measures, it may be useful to consider their respective strengths and weaknesses. As a first step, we may think that the pooled UI and survey data estimate in row (11) has less noise and hence is likely more accurate than the survey-only estimate in row (4). If we prefer the pooled measure to the survey-only measure, the calculated total individual income measure in row (3) may be understated by at least a similar amount. This would suggest a

[17] In-kind benefits are treated separately and examined in a later section on benefits.

[18] All measures in Table III apply 30% weight to the midline data and 70% weight to the endline data, or comparable time periods in the case of administrative records. Results broken down by survey are available in Figure VI.

decrease in total individual income of about $1,800/year, aggregating across rows (11) and rows (5)-(8).[19] Similarly, if we believe the calculated measure in row (3) is more accurate than the measure in row (2) which is based largely on an answer to a single survey question, then the total household income decline in row (1) may be too high, as it was also elicited by asking participants to provide one aggregate number. A back-of-the-envelope calculation suggests $3,200 may be a more reasonable magnitude for this estimate.[20]

These estimates can be used to calculate participants' MPE. We define the MPE as the change in earnings as a share of the change in the flow of unearned income. In a dynamic setting such as ours, this could differ substantially from the change in earnings relative to the total change in wealth. Participants' MPE depends on two decisions: 1) how much of the transfer to allocate for consumption today (whether consumption of leisure or other expenditures) versus consumption in the future; and 2) how much to consume in leisure today out of the total amount reserved for today.[21] The first of these decisions can be approximated by considering changes in participants' net worth as a measure of how much is allocated to the future. However, the estimated impact on net worth depends on measurement choices, such as how to treat assets like real estate held by relatively few participants; further, the confidence intervals on these estimates are relatively wide, and we may be missing some inputs to net worth. To be conservative, we use a range of plausible values for effects on net worth when calculating the MPE based on Bartik et al. (2025). Specifically, we assume the impacts on net worth fall somewhere between -$2,000 (*i.e.*, treated participants end up with $2,000 more net debt than the control group over the course of the program) and $5,000. The latter estimate is optimistic given our data, but it acknowledges the wide confidence intervals around the results for net worth and possible underreporting. Table III reports a range of possible MPE values for each type of income based on these alternative assumptions. The adjusted total household and individual income values of $3,200 and $1,800 discussed above would yield estimates of the MPE of 0.28 and 0.16, respectively, assuming no saving. We also compute elasticities. Since the control group receives $50/month, the effects we observe are due to the treatment group receiving an additional $950/month, and the elasticities are

[19]The estimates in rows (5)-(8) sum to -$204, and aggregating with a point estimate of -$1,590 in row (11) yields a preferred total individual income measure of -$1,793.

[20]Specifically, if we deflate the total household income estimate (-$4,333) by the ratio of our preferred -$1,793 estimate for total individual income to the value in row (2) (-$2,396), we get -$3,244. We did not ask participants to provide a breakdown of the income of other household members into different categories because the literature suggests that they would likely not be able to do this accurately (*e.g.*, Zagorsky 2003).

[21]Formally, the MPE is calculated as the ratio of the change in earnings over the total amount allocated for consumption today. Typically, the numerator uses pre-tax earnings and the denominator post-tax changes in income. The transfers we consider are not taxable, as a gift, so they are treated as though they are post-tax.

calculated accordingly. Further details on these calculations are provided in Appendix J.

Turning to consider impacts on labor supply, we observe effects on both participation and hours worked (Table IV). In the survey data, the number of hours worked per week declines by about -1.3 (s.e. 0.6) as a result of the transfers (row 1). This estimate is based on employed and non-employed individuals and is a function of both reductions in hours among those working and reduced entry into employment. Treated participants appear 2.3 (s.e. 1.2) percentage points less likely to be employed in the survey data (row 2). Similar to how we saw larger impacts on income in the UI data compared to survey data, we estimate a larger effect on labor supply using the UI data: a 6.6 (s.e. 2.0) percentage point decrease in employment using the UI data alone (row 3) and a 4.1 (s.e. 1.7) percentage point decrease in employment using the pooled UI and survey data (row 4). Some of the difference between these extensive margin estimates could result from contract, temporary, and gig work not being included in the UI data. We observe lower rates of employment in the UI data in both the treatment and control groups overall, consistent with these types of jobs not being included. However, as we will show in the quality of employment section, we do not find evidence that participants substitute into these types of work in response to the transfers. As with income, Appendix Table A.47 compares administrative and survey measures in detail, and Appendix K describes our pooling approach. The pooled measure will again be our preferred measure.

We translate our results into labor supply elasticities according to $\eta_e = \frac{NY}{\partial v}\frac{\partial p}{p}$ and $\eta_i = \frac{NY}{\partial v}\frac{\partial h}{h}$, where p denotes labor force participation, h denotes total hours worked (including zeros), ∂v is the change in virtual income (the transfer), and NY is net-of-tax income. The elasticity η_e captures the extensive margin, while η_i, because it is defined over unconditional hours, reflects a combined response through both participation and hours conditional on working, rather than a purely intensive margin. We estimate η_e=-0.19 and η_i=-0.16 among participants.[22]

Turning to consider the employment responses of other individuals in the household, we estimate a reduction of 2.5 (s.e. 0.8) in total hours per week worked by the participant and their partner (row 5 of Table IV), and 2.4 (s.e. 0.9) hours per week when we include all household members (row 6). These estimates represent a 5.0-6.1% decrease relative to the control mean, close to the roughly 6.7% decline in total household income suggested by our preferred adjusted estimate of $3,200. The difference between this estimated decrease in household work hours and estimated decrease in total household income could reflect small, imprecisely-estimated reductions in other transfers or slight

[22]Results for hours conditional on employment are presented in Appendix Table A.9. The estimated intensive margin (conditional-hours) elasticity is -0.07.

underestimation of the decline in total work hours. The estimates for the work hours of others in the household were not pre-specified and are subjected to a large penalty in the FDR corrections, but they are significant before those corrections are applied. The magnitude of the effect on partners' hours is roughly comparable to the effect we observe on the participants' own work hours.

Time use data from the mobile app show similar impacts. As described in section III, participants were asked to complete two detailed 24-hour diaries on a bi-monthly basis, recording activities in 15-minute increments. Figure V shows the treatment effects on time use estimated from these data. Between reductions in "market work" and "other income", we estimate a decline of about 1.4 hours per week of work, consistent with the employment module survey questions about hours worked at each job. Appendix H presents robustness checks, including an alternative coding of overlapping activities and a LLM-based classification of text responses for those who entered free text in an "other" category, and provides additional results, such as impacts on time spent with other people (*e.g.*, time spent with friends, children, or alone). It also provides results from ATUS-style enumerated and quarterly time use surveys. In these surveys, we observe a 1.5 hours per week reduction in work (Appendix Figure B.9), which aligns closely with estimates from the mobile time diaries. The extra time participants have from reduced work is used largely for leisure,[23] non-commuting transportation, and other activities (Figure V).[24]

The app asked participants who they were with while engaging in activities. Appendix Figure B.6 shows that time spent with various categories of people does not meaningfully change; all effects are small and insignificant after FDR adjustment. The only category that exhibits a significant change (before adjustment) is a reduction in time "with my boss".

The estimated effects on income, labor supply, and time use are broadly consistent with each other. A $1,800 reduction in annual individual income represents a 5.4% decrease relative to the control mean, while a 1.3-1.5 hour/week reduction in work (from the labor supply survey modules, time diaries, or the quarterly ATUS-style surveys) represents an approximately 4.3-5.6% decline relative to these variables' control means. As we will see when discussing the employment quality results, average wages were unaffected by the treatment, so any small differences between these estimates

[23]Social leisure is only marginally significant before the multiple hypothesis testing correction and solitary leisure is not significant, but they represent the largest category if pooled.

[24]The survey-based time use measures asked participants about different categories in which they could spend their time and did not explicitly have a "social" or "solitary" leisure category and did not distinguish between "market work" or "other income-generating activities". Still, the survey-based time questions showed similar decreases in hours/week worked. In terms of increases in time spent on certain categories, the only category that stands out is a very small increase in time spent on "finances"; people in the treatment group spent approximately 0.3 hours/month more on this activity before adjusting for the FDR (Figure B.9).

could not be explained by a differential effect among those with higher wages, though individuals with higher total household income at baseline did have larger reductions in income as a result of the transfers. Weekly work hours may be somewhat understated in survey data, similar to what we see for income.

Overall, the effect on labor supply appears to be driven by the extensive margin. This would be consistent with many low-income jobs offering limited flexibility in work hours (Lachowska et al. 2023). Indeed, at baseline, 44% of employed participants reported a preference to work either more or fewer hours, indicating some inflexibility in work hours. While gig work may be more flexible (Garin et al. 2023), income from this work comprises a relatively small share of participants' total income. Further analysis suggests that the effects on employment and work hours are largely driven by those leaving their primary job rather than leaving second, third, or fourth jobs (Appendix Table A.9). This argues against a narrative in which individuals working multiple jobs quit a second job and implies a somewhat greater share of the reduction in work hours comes from the extensive margin than we see in much of the literature. For example, Golosov et al. (2024) find roughly half the response is through the extensive margin, with larger extensive margin responses among low-income households. Participants report heterogeneous reasons for not working (Appendix Table A.10).

Treatment effects on income and employment (Figures VI and VII) and time use (Figure B.11-B.14) grow over the course of the study. Restricting attention to the quarterly UI data, the time trend appears evident in both the event study results in Figure VII and the quarter-by-quarter estimates (Appendix Figure B.5). This pattern would be consistent with increasing detachment from the labor market, but it would also be consistent with individuals gradually reallocating time toward non-employment activities such as pursuing education. We will return to this hypothesis when presenting results for effects on human capital investment, but it does not appear that pursuing higher education explains most of the observed reduction in labor supply.

One interesting note, however, is that treated participants may start to "catch up" towards the end of the study. In both quarterly administrative records and survey data, we cannot reject null effects in some quarters close to the end of the program, even though we detect significant negative impacts in the second and third years (Figure VII and B.5). This trend may reflect the approaching end of the transfers, as we also observe participants taking a larger number of actions to search for a job in the final year of the program, consistent with, *e.g.*, Card, Chetty and Weber (2007). In the UI data, there is still a relatively large difference in the magnitudes of the point estimates on employment and earnings between the treatment and control groups after the program ends, but this difference is smaller than it was during the program and no longer statistically significant for any of the measures.[25]

V.B Other Employment Outcomes

Figure VIIIa summarizes other effects on employment outcomes at the family level, in standard deviations. Item-level results for each index appear in Appendix Tables A.12-A.28. For ease of interpretation, even when an increase in an index represents a potentially "negative" outcome, such as increased reports of disability or unemployment, estimates in this figure are reported as-is (not reversed).

The largest increases are in the indices for disability and duration of unemployment. While one might expect disabilities to remain relatively stable over a three-year horizon, that is not necessarily the case if treated participants can leverage the transfers to improve their health or, conversely, if recipients get more care and therefore are more likely to get diagnosed with a disability. It is also possible that if individuals participate less in the labor market, they may be more likely to think of themselves as disabled as a form of self-signaling (to mitigate any stigma associated with non-employment). We find a significant 4.2 percentage point increase in the likelihood of self-reporting any disability (on a base of 31 percent in the control group) and a 4.2 percentage point increase in the probability of reporting a health problem or disability that limits the work they can do (on a base of 28 percent in the control group) (Appendix Table A.11). Participants also report slightly worse disabilities or health problems that have persisted for slightly longer periods of time. Somewhat reassuringly, none of these measures is significant at endline (Appendix Figure B.15), which might support the hypothesis that participants received diagnoses early in the program that were partially treated or became less salient by the end of the program.

Duration of unemployment and non-employment goes up, as one might expect if, with the transfers, people feel less pressure to accept a new job or raise their reservation wage. The average spell of non-employment causally increases by 0.8 months relative to the control group mean of 7.8 months, with treated participants' longest spell of non-employment increasing by 0.8 months relative to the control group mean of 8.8 months (Appendix Table A.12).[26] These estimates are relatively small com-

[25]Given that we were not able to match all participants in the UI records, we include some results applying Lee bounds to the main UI-based estimates in Appendix Table A.8. Results are broadly comparable.

[26]We construct two main types of variables to examine impacts in this domain. First, we measure the average and longest duration of non-employment over the entire study using an employment history timeline that captures when participants left or started *any* job, including second, third, or fourth jobs. Second, we use cross-sectional measures of how long participants reported being non-employed or unemployed at the point in time at which they answered a survey. The cross-sectional measures yielded slightly smaller estimated effects on lower control group means. The number of months of non-employment in the last year, a supplementary exploratory measure which was not pre-specified, may be downward

pared to what one might expect from analyses of UI benefit extensions (Cohen and Ganong 2024); substitution effects generated by UI and not by unconditional transfers could explain part of this gap.

We also see some shifts in entrepreneurship. This outcome may be particularly important from a policy perspective, as the negative labor supply effects we observe could potentially be partially offset if participants start new businesses as a result of the transfers. New businesses could generate productivity gains that would not show up in the data. The entrepreneurship index is comprised of three components: "entrepreneurial orientation" captures willingness to take financial risks, drawing on both a survey measure and risk preferences from an incentive-compatible multiple price list experiment; "entrepreneurial intention" includes questions such as whether or not the respondent has an idea for a business and the respondent's self-reported likelihood of starting a business in the next five years; "entrepreneurial activity" captures whether participants actually start a business or are close to someone who started a business. We find significant increases in entrepreneurial orientation and intention, but this did not translate into significantly more entrepreneurial activity (Appendix Table A.14). The point estimate on the latter is positive but very small, and it is possible that very few people are inclined to become entrepreneurs in general. We pre-specified that we would consider entrepreneurial orientation and intention as potential precursors to entrepreneurial activity, and it remains possible that there is an effect that is too small to be detected in our sample. Our confidence intervals include an increase as large as 2.5 percentage points. There also appears to be a time trend: the treatment effect on entrepreneurial activity grows over time and the point estimate is only marginally insignificant in year three (Appendix Figure B.16). It should be noted that the businesses started are not very large.[27]

Education is a particularly important determinant of long-term employment outcomes and hence the long-run cost-effectiveness of cash transfers (Hoynes and Rothstein 2019). There is a positive point estimate on the human capital index in Figure VIII, which is perhaps notable despite its insignificance since the sample includes many older adults who might be less likely to return to school. Appendix Table A.13 reports the full results. We pre-specified that we would conduct heterogeneity analysis by baseline age for human capital outcomes since younger people tend to have higher rates of return to investment in education and may be more likely to pursue post-secondary education in response to the transfers. Participants who were in their 20s at baseline qualitatively appear to have obtained

biased since non-employment spells can exceed a year.

[27] Examples from open-ended questions include screen-printing t-shirts or purchasing a vending machine for an apartment block.

more years of post-secondary education (Appendix Table A.42), but the effect is not significant and is offset by older participants obtaining if anything slightly less education.

Participants were also asked about barriers to employment. A common theoretical motivation for cash transfers is that they could help individuals overcome challenges preventing them from working, such as a lack of transportation or childcare. However, we do not find significant impacts on self-reported barriers to employment (Appendix Table A.16).

We do not see significant changes in the family-level indices for job search and selectivity, but we do observe significant changes in a few items within these indices which tell a consistent story. In particular, it appears that receiving unconditional cash transfers made recipients more likely to search for a job and apply for a job (Appendix Table A.17). They also report higher likelihoods of taking specific search actions, such as looking at job postings or contacting friends or relatives to find work (Appendix Table A.18), though the total number of distinct search actions taken does not change significantly. Interestingly, while treated participants were more likely to apply for a job, they reported applying to about 0.8 fewer jobs in the last 3 months (relative to a control mean of 5.5 applications in that time period) and interviewing for fewer jobs as well. These effects are significant before but not after the multiple hypothesis testing correction. These results suggest that while treated participants are more likely to search for work, they either search a little less intensively or more selectively.[28]

To disambiguate between searching less intensively or more selectively, we consider if there are changes in the types of jobs participants applied for. The index value in Figure VIII indicates that there were limited differences in the types of jobs participants applied for overall, and item-level analyses in Appendix Table A.20 tell a similar story. In exploratory analysis of self-reported requirements for taking a job, treated participants are more likely than control participants to say that interesting or meaningful work or work with flexible hours is a requirement, but these results do not survive the FDR correction (Appendix Table A.21).

Given the debate in the literature as to whether quality of employment should go up or down in response to a cash transfer, we included a large number of questions relating to quality of employment, divided into several components. Unlike the other families of outcomes which are estimated unconditionally, this family focuses exclusively on employed participants, as questions about job quality do not apply to those who are not employed. Note that there is some selection into our ability to observe these outcomes since employment changed in the treatment group relative to the control group. How-

[28] Alternative specifications in Appendix Table A.19 provide supporting evidence.

ever, the extensive margin effects on labor supply were sufficiently moderate (4.1 percentage points in the pooled administrative and survey data) that we believe the estimates are still largely directly interpretable.

This quality of employment index leverages 35 primary items across six components: adequacy of employment, employment quality (including benefits and training), informal work, hourly wage, stability of employment, and quality of work life (day-to-day experiences). Despite the very detailed questions, we find no evidence of changes in quality of employment, and for most items we can reject even small effects (see Appendix Table A.22 for the component-level indices and Appendix Table A.23 for the raw item measures). For example, wages decline by 13 cents on average and we can reject increases of more than 58 cents per hour. We can reject declines in the family-level index of greater than 0.028 standard deviations or improvements of more than 0.022 standard deviations. Two clusters of variables did show some significance. First, in the stability of employment component, the number of jobs held in the past 12 months (or, descriptively, in the past two years) is lower among treated participants, though this could reflect reduced labor supply rather than being a measure of quality of employment. Second, in the quality of work life component, treated participants report slightly fewer opportunities for promotion, more scheduled shift cancellations with less than 24 hours notice in the last month, and a larger number of stressors in their work environment relative to the control group. None of these changes remain significant after FDR adjustment, and point estimates were generally small across the board.[29]

V.C Other Outcomes

The largely–though not universally–negative effects on employment outcomes contrast with more positive impacts on other outcomes. Figure VIIIb summarizes index-level effects (in standard deviations) for other families of outcomes. Notably, there are relatively large changes in consumption and geographic mobility. The following subsections describe the different outcomes in more detail.

V.C.i Consumption

Most studies that estimate MPEs do not directly observe consumption.[30] Instead, they typically infer it based on assumptions about how much people might smooth consumption over time or impute

[29] Appendix Table A.24 provides further exploratory analyses within this family of outcomes, including a more detailed breakdown of which specific benefits are offered by participants' employers.

[30] Note that in this paper we refer to all expenditures as consumption. In practice, some expenditures are made on durable goods or services like health care that may generate a higher flow of future consumption as well.

it using a combination of data on earnings and assets or asset returns.[31] In this study, we collect detailed survey data on consumption from enumerated surveys at baseline, midline, and endline and quarterly online survey modules. Table V expands on the results in Figure VIII by reporting estimates for total monthly expenditures and broad categories of expenses generated from these survey data.[32] Non-durable goods and services account for the largest absolute increase in spending, off of the largest control group mean. Impacts on expenditures on human capital, durable goods, housing, and "other" expenditures are much smaller and roughly equal in magnitude to each other. In terms of percent increases relative to the control group, expenditures on human capital, durable goods, and "other" expenditures increase the most, but these large percent increases are from relatively low control group means.

These results imply that roughly 30% of the transfers go to monthly consumption.[33] However, given the estimated employment effects and very limited asset accumulation (on the order of $0 to $2,000) and increases in debt (of around $1,000 to $2,000) in the treatment group relative to the control over the course of the study (Bartik et al. 2025), we expect that estimates in Table V understate true consumption.

There are several plausible reasons for the consumption results to be understated. First, in our main regressions we follow the pre-analysis plan and winsorize outcomes at the 99th percentile to limit the influence of outliers. If treated participants have different needs and use the transfers to make diverse purchases (one of the theoretical advantages of unconditional cash transfers), this winsorization would mechanically attenuate the estimated effect. We therefore also conduct median regression on unwinsorized data; these results show slightly larger impacts of about $341 per month, or 36% of the transfers (Table A.60). Second, a large literature suggests that consumption tends to be systematically understated in surveys (*e.g.*, Bee, Meyer and Sullivan 2012). The Consumer Expenditure Survey (CEX), for example, has been found to capture only about 73% of comparable consumer expenditures as measured in the Personal Consumer Expenditures (PCE) data in 2022 (Bureau of Labor Statistics

[31] A common approach is to assume the permanent income hypothesis holds and assume a certain discount rate (*e.g.*, Golosov et al. [2024], who also use asset returns to calibrate savings).

[32] In this table, non-durable goods and services include food and non-alcoholic beverage consumption, inside and outside the home; utilities, phone, cable, and internet; non-durable transportation expenditures; clothing, apparel, and personal care expenditures; housekeeping supply expenditures; spending on alcohol, tobacco, marijuana and gambling; recreation and entertainment expenditures; vacations and trips; and expenditures on pets. Housing expenditures include rent, mortgage, home insurance and property tax expenditures. Human capital expenditures include education expenses but also health expenditures, childcare and expenditures on children. Durables include car payment and insurance expenditures and household expenditures such as on furnishings and appliances. Other expenditures include gifts or loans to family and charity, a small amount in debt payments, and other expenses. Outcome variables were constructed at the survey year level.

[33] We divide observed impacts by $950 to reflect the treatment–control differential in monthly transfer size.

2023).

In Table VI, we consider what share of the transfers might go to consumption (as opposed to income or net worth) under different assumptions. Column (1) reports unadjusted estimates. Column (2) rescales item-level consumption using PCE/CE ratios.[34] Column (3) assumes that the effect on net worth is $5,000 and allocates the remainder to consumption proportionately. Finally, Column (4) assumes all under-reporting is in consumption alone. The true share of the transfer spent on goods and services likely falls between the estimates in Columns (3) and (4).

V.C.ii Labor Market Mobility

Apart from changes in consumption, we observe sizable effects on where participants live over the course of the study, which can affect labor market outcomes (*e.g.*, Chetty and Hendren 2018). By the end of the transfer period, approximately 50% of control participants had moved housing units at least once since baseline, with most moves occurring across neighborhoods, defined as a different Census tract (Bartik et al. 2025). Moves across labor markets, defined as moves to a different commuting zone, were less common. Pooling across time periods, 12% of control households moved labor markets, and the treatment appears to have increased labor market moves by 1.8 percentage points (Appendix Table A.25); the largest treatment effects were in year 1 (Appendix Figure B.29). Treated participants also reported more active labor market search behaviors and indicated significantly greater interest in moving labor markets, leading to a significant effect size of 0.09 standard deviations on the overall index for labor market mobility.

Perhaps because most moves occur within the same labor market, we do not see significant changes in the quality of labor markets in which participants reside (Figure VIII). The only exception at the item level is that treated participants are somewhat more likely to live in areas where the BLS projects more job growth for their education group, though this result is only marginally significant and does not survive the FDR adjustment (Appendix Table A.26). Further, all differences are relatively small in magnitude. However, just because participants are not necessarily moving to labor markets with markedly different characteristics does not mean their moves are not economically meaningful. First, revealed preference suggests that moving was welfare-enhancing for them even if it did not improve their employment prospects. Second, moves within a commuting zone could still

[34] This rescaling is done by matching the PCE/CE ratios in Bureau of Labor Statistics (2023) to item-level consumption prior to aggregation. We use the most disaggregated categories of consumption for this comparison, since the ratio of the PCE/CE can differ greatly by item. Further details are provided in Appendix I.

affect proximity to certain labor markets.

V.C.iii Subjective Well-Being

One might expect that working less and consuming more in response to the transfers could increase overall happiness. Some past work on lottery winners does report lasting improvements in well-being (Lindqvist, Östling and Cesarini 2020). In our setting, we instead find temporary gains in subjective-well being that fade over time, consistent with the literature on the hedonic treadmill (Brickman, Coates and Janoff-Bulman 1978; Frederick and Lowenstein 1999). We measure subjective well-being in several ways, including eliciting measures of life satisfaction, satisfaction across various domains such as satisfaction with one's standard of living, health, and time for enjoyable activities, and a measure of affect balance using the Scale of Positive and Negative Experience (SPANE) (Diener et al. 2010). The subjective well-being index and all components are insignificant on aggregate (Appendix Table A.27).

Figure IX, which plots the results over time, demonstrates the dynamic nature of the estimates. In year 1, the transfers positively affect all components of subjective well-being. By year 2, the effects are smaller and no longer significant, and by the end of the study the estimates are insignificantly negative. These results highlight the importance of the relatively long duration of the study: had we only followed participants for one year, we might have reached different conclusions about the effects on well-being. In companion papers, we show similar patterns–meaningful beneficial effects that fade out quickly–for outcomes related to mental health, stress, and food security (Miller et al. 2025), as well as subjective perceptions of financial health (Bartik et al. 2025).

V.C.iv Take-Up of Benefits

The expected effect of the transfers on receipt of public benefits is theoretically ambiguous. On the one hand, some literature suggests that low-income individuals may be particularly bandwidth constrained and less likely to take up benefits with onerous application processes; the cash transfers could reduce constraints and thereby facilitate higher take-up. Conversely, the transfers may partially substitute for some benefits or make participants feel less need to apply for them. Further, we expect a small mechanical effect on benefits: though most benefits were protected, some participants in Texas may have become temporarily ineligible for food assistance. Overall, we do not detect statistically significant effects on benefits take-up (Figure VIII). Benefits decrease by about $200 per year, but this estimate is very noisy (Appendix Table A.28).[35] To the extent that receipt of the transfers reduced benefits, however, the elasticity estimates may slightly understate the income effects of the transfers.

V.C.v Relationship Status

Finally, to interpret the effects on employment and income, we also examine potential changes in household composition. The initial analyses of the negative income tax experiments suggested an increase on marriage dissolution, raising concerns that cash transfers may affect relationships and family structure. If the transfers we study caused people to leave the household, that could mediate the observed effects on total household income. However, we find no evidence that the treatment caused any significant changes household structures on net (Figure VIII). A relatively small share of participants were married at baseline, and other types of relationships were more common, so we asked about relationships in several different ways. We observe no effect on being divorced, on having a spouse or partner, or on being in any romantic relationship (Appendix Table A.29). If anything, there is some indication that treated participants experienced more changes in relationships with romantic partners outside the household, but not with partners within the household.[36]

VI Discussion

VI.A Heterogeneity in Treatment Effects

We pre-specified several heterogeneity analyses based on participants' baseline attributes. These subgroup analyses all are adjusted for multiple hypothesis testing as described in Section IV and Appendix E. Since these analyses were pre-specified as exploratory, they are unlikely to be significant after FDR correction unless the estimated effects are quite large. Nonetheless, it may be informative to consider the point estimates and broad trends observed across different measures.

The treatment effects on income appear stronger among those whose baseline income was above the FPL (Table A.31). These estimates are consistent with what one might theoretically expect with decreasing returns to income. They are also in line with other lottery studies that find larger labor supply responses among higher-income individuals (Golosov et al. 2024). Our results confirm that this pattern holds even at lower absolute income levels. The fact that income effects are different for

[35] Benefits in this section include both monetary benefits as well as non-income benefits such as SNAP and WIC which are excluded from the estimates of government benefits under the "Income" family.

[36] Exploratory analyses of self-reported reasons for relationship dissolution do not reveal clear patterns (Table A.30). Treated participants are somewhat more likely to report that they ended the relationship, rather than it being ended by their partner or by mutual agreement, but this result does not survive the FDR adjustment.

relatively low-income individuals has implications for redistribution: the income effects of redistribution would not "cancel out" when transferring from richer to poorer individuals, but rather we observe smaller income effects among recipients with lower income at baseline.

We also observe interesting heterogeneity in treatment effects by education. Treated participants without a bachelor's degree at baseline appear to reduce their income and employment by more than those with a degree (Tables A.32 and A.36). In fact, participants with a bachelor's degree had insignificant increases in salaried/wage income, while potentially reducing supplemental income from gig work. While these subgroup analyses are exploratory, they align with heterogeneity analyses by age: negative labor supply effects appear larger for participants in their 20s at baseline (Table A.37), and we see qualitatively larger positive effects on formal education among those in this younger age group (Table A.42), though these latter estimates are generally not significant and do not survive FDR correction. This suggests a plausible story in which younger participants may be more likely to use the money to pursue post-secondary education and work less while they do so. However, these results are only suggestive, and we cannot rule out alternative explanations for the larger labor supply reductions among younger participants or those without a college degree. The quality of employment estimates are broadly comparable between those who had a bachelor's degree at baseline and those who did not, though potentially slightly more negative for those without a bachelor's degree at baseline (Tables A.45-A.46). Again, these subgroup analyses should be interpreted with caution given the large number of items tested.

We also pre-specified heterogeneity analyses by sex since the literature often finds large empirical differences in response to other transfers (*e.g.*, Keane 2011). In the survey data, differences in impacts on income and employment by sex are mixed. Males in the treatment group may have experienced slightly larger reductions in income relative to the control group according to one self-reported measure (Table A.33), while females may have slightly larger treatment effects on labor supply (Table A.38). In general, we do not detect significant differences by sex.

For entrepreneurship, we pre-specified heterogeneity analyses by baseline age and education. These results are relatively noisy, but there may be somewhat larger effects on entrepreneurial intention among those without a bachelor's degree at baseline and those in their 30s at baseline (Appendix Tables A.43-A.44).[37]

[37] Additionally, we pre-specified two more exploratory heterogeneity analyses, described in Appendix M, which were designed to focus more on attributes of participants that relate to how they were recruited to the sample. Heterogeneity by state and by the presence of children in the household is also explored in this appendix, though these analyses were not pre-specified.

VI.B Comparison to Other Transfers

Much of the evidence on cash transfers in high-income contexts has come from studies of lottery winners where, as in our setting, a relatively pure income effect can be observed. However, lottery winnings are typically paid as lump sums or long-term annuities, making them difficult to compare directly to sustained monthly cash transfers. Most lottery studies assume that winners follow the permanent income hypothesis, saving a large share of their winnings for future time periods and spending them down gradually (Golosov et al. 2024; Cesarini et al. 2016; Imbens, Rubin and Sacerdote 2001). Under this framework, a large lump sum could be converted to an annuity or even monthly transfers. However, our participants do not appear to follow the permanent income hypothesis under the discount rates typically assumed. Even under the most optimistic estimates, they do not save anywhere near the share of the transfers implied by this model.[38]

Instead, our MPE estimates align more closely with estimates from the lottery literature if we assume that, in our context, recipients treat monthly transfers as current spending money. Appendix Table A.66 compares estimates of the MPE from lottery studies with our MPE estimates. The MPEs from the lottery studies, calculated under the permanent income hypothesis as in Golosov et al. (2024), resemble our estimates that assume individuals do not save the transfers for future periods. Further supporting the idea that our different results may be capturing a real difference between how people think of large, lump-sum transfers or long-term annuities as opposed to monthly transfers, we can calculate a MPE similar to what we obtained in this study if we look at the estimated effects from a monthly transfer in another program, Baby's First Years (Sauval et al. 2024), and make the same assumption that participants do not save the transfers for future periods.

Several factors may explain this difference. There may be something about receiving a lump sum that encourages people to think about saving or investing a large share for the future, or conversely something about receiving monthly transfers that encourages participants to think of the transfers as income intended for ongoing expenses. Selection may also play a role. Compared to lottery winners, our study participants are younger and have lower incomes and thus may face tighter constraints that lead them to spend a larger share of the transfers on basic needs. The lottery studies have typically not been able to directly observe savings or consumption, and it is possible that if such outcomes were observed and analyses were restricted to individuals socioeconomically similar to our sample, those

[38] As discussed in Appendix N, under typical assumptions, the permanent income hypothesis would imply that treated participants should save about 90% of the transfers for use after the transfer period ends, spending only about $95 more per month than control group participants. This does not fit our data, as we observe much more than this being spent.

recipients would also exhibit lower savings rates.

Despite differences in saving behavior, our results align with the lottery literature in other respects. Consistent with Golosov et al. (2024), we find that participants with higher baseline income reduce their labor supply more (Table A.31).

It may also be informative to compare our results with those from studies of the EITC (*e.g.*, Eissa and Liebman 1996; Eissa and Hoynes 2004; Kleven 2024) and related programs like Paycheck Plus (Miller et al. 2016; Yang et al. 2022). The EITC and Paycheck Plus provide refundable tax credits to low-income workers who file taxes, with Paycheck Plus designed to supplement the EITC for adults without dependent children who receive relatively small EITC payments. Unlike lottery winnings or monthly unconditional cash transfers, these programs directly incentivize work by conditioning benefits on earnings. Their overall effects on incentives to earn are complicated, however: because credit amounts phase in and then phase out with earnings, the programs can increase labor incentives for low earners while reducing them for higher earners near the phase-out level.

Evaluations of the EITC have focused on households with children. Eissa and Liebman (1996) find that the EITC increased employment among single women with children, while Eissa and Hoynes (2004) show that the same program may reduce labor supply for married women with children given that they are often secondary earners. The recent evaluations of the Paycheck Plus experiments provide supporting evidence that this type of program can increase employment. In New York, treated participants were 2-3 percentage points more likely to be employed in years 2 and 3 of the program (Miller et al. 2018); in Atlanta, the effect was about one percentage point and not statistically significant (Yang et al. 2022).[39] The programs had no significant effect on earned income in either site in any year.[40] Overall, the fact that EITC-like programs are designed to encourage work may help explain why their effects on labor supply differ from the effects of unconditional cash transfers, though the samples and payment structures are not directly comparable.

We also compare our estimates to prevailing expert beliefs about cash transfers by surveying economists to elicit their *ex ante* predictions of our findings. As described in DellaVigna, Pope and Vivalt (2019), expert forecasts can be a valuable tool for judging the novelty of research findings. We collected forecasts from a subset of researchers affiliated with the National Bureau of Economic

[39] A synthesis of the two studies finds an aggregate effect of just under 2 percentage points in years 2 and 3 (Miller, Katz and Isen 2022).

[40] Estimating a MPE is not feasible for these programs given the structure of the credits: the credit amount varies depending on the level of earned income.

Research (NBER) and from users of the Social Science Prediction Platform (SSPP).[41] Overall, NBER affiliates predicted labor supply effects relatively accurately, though they expected somewhat smaller effects on individual salaried income. They anticipated more positive effects on hourly wages and human capital investments and more negative effects on job search than we observed. Appendix O discusses these forecasts in detail.

Finally, we can use the estimated treatment effects in this paper to assess the MVPF of the program we study. A full accounting of the MVPF would need to incorporate the long-term effects on children, particularly as programs affecting children often have higher social returns (Hendren and Sprung-Keyser 2020). The short-run effects reported in this paper may therefore understate the net benefits. However, it should be noted that even if there were no longer-term gains and the transfers affect welfare only by providing recipients with additional resources, the associated fiscal externality would be modest. Since the transfers are non-taxable and participants face low marginal income tax rates, changes in tax revenue arise primarily through reduced payroll taxes. The total implied reduction in tax revenue is approximately 4 cents per dollar transferred, yielding an MVPF close to one and comparable to that of other cash transfer policies targeting low-income households. Appendix J.3 provides further detail on these calculations.

VII Conclusion

After decades of shifting welfare assistance from direct cash payments to in-kind benefits, unconditional cash transfers have reemerged as a potential tool to alleviate poverty and provide beneficiaries the flexibility to spend money to meet their own needs. At the same time, some policymakers have raised concerns that such transfers may lead beneficiaries to reduce their labor supply, potentially increasing dependence on future transfers, weakening long-run labor market attachment and job prospects, and raising the fiscal cost of the transfers. Alternatively, if cash transfers help beneficiaries search for higher quality or better fitting jobs, start new businesses, or make human capital investments that boost future earnings, these transfers may ultimately be productive.

Our results provide support for both sides of this debate. On the one hand, the transfer we study generated significant reductions in individual and household market earnings. Spillovers to other household members—who also reduced their labor supply—imply that the total amount of work withdrawn from the market is fairly substantial. Moreover, we do not find evidence of the type of job

[41] https://www.socialscienceprediction.org/.

quality or human capital improvements that would offset these losses, and our confidence intervals allow us to rule out even small positive effects on these outcomes. On the other hand, treated participants showed more interest in entrepreneurial activities and greater willingness to take risks due to the transfers, which could improve future earnings and lead to additional economic benefits over time. Further, exploratory subgroup analyses suggest that not all responses to the transfer were identical: older participants experienced very little change in labor supply, whereas younger participants reduce their work hours but may be more likely to pursue additional education. Finally, the fact that some of the transfer was used to reduce work shows that additional leisure quickly becomes much more valuable than income from work and the consumption it enables. Put differently, the marginal value of leisure rises more rapidly than the marginal value of consumption as income rises, at least given the kind of work available to our sample.

While the our data collection was extremely comprehensive, future work would improve our understanding of the long-term impacts. In particular, follow-ups could examine the extent to which labor market effects persist after transfers end and shed light on how participants' children fare as they grow up, outcomes which may be particularly important for policy decisions. Additional work would also be needed to understand the potential general equilibrium effects that might arise should such a program be implemented at scale.

Other outcomes, documented in companion papers, may also be relevant for policy. In Miller et al. (2025), we find that the transfers largely did not affect participants' mental or physical health. Mental health improves in year one but reverts to baseline levels by year two, similar to our findings for subjective well-being, and we rule out even small improvements in physical health, including as captured by biomarkers. In Broockman et al. (2024), we find no effects on political preferences or participation, though there is some evidence of mood misattribution. Bartik et al. (2025) report the transfers led to short-term improvements in self-reported financial health but no substantial changes in net worth or financial behaviors such as delinquencies during the transfer period. Krause ⓡ al. (2025) find limited effects on children and parenting overall. Parents increase spending on children but report that their children have more developmental difficulties and stress, potentially reflecting increased monitoring, and there are no substantial improvements in educational outcomes or changes in fertility.

Our analysis demonstrates that even a fully unconditional cash transfer induces moderate labor supply reductions among lower-income adults. Since virtually all existing large-scale cash transfer programs in the U.S. are means-tested, they embed additional disincentives to work beyond the income effects we observe in this study. In our setting, participants reduce their labor supply not because of program design features that penalize work, but because, as their incomes rise, the marginal value of leisure becomes relatively larger than the marginal value of consumption. While decreased labor market participation is generally characterized negatively, policymakers should take into account the fact that recipients have demonstrated–by their own choices–that time away from work is something they prize highly.

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Table I: Study Sample Characteristics Compared to Eligible Population

	Eligible Population Comparison (ACS)			Study Sample		
	Full US Population		Study Counties	Eligible Screener Respondents		Enrolled Active Survey Group
	Unweighted	Reweighted to Match Enrolled Sample FPL and County Type Distribution	Reweighted to Match Enrolled Sample FPL County Type Distribution	Unweighted	Reweighted to Match Enrolled Sample FPL County Type Distribution	Unweighted
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Key active group stratification variables						
Income < 100% of FPL	0.24	0.34	0.34	0.30	0.34	0.33
Income 100-200% of FPL	0.36	0.41	0.41	0.33	0.41	0.40
Income 200%+ of FPL	0.40	0.24	0.24	0.37	0.24	0.24
Rural County	0.27	0.13	0.13	0.13	0.13	0.13
Suburban County	0.32	0.18	0.18	0.22	0.18	0.18
Medium-Sized Urban County	0.17	0.16	0.16	0.15	0.16	0.16
Large Urban County	0.23	0.53	0.53	0.51	0.53	0.53
Panel B. Demographic Characteristics						
Any Children in Household	0.58	0.58	0.62	0.57	0.59	0.57
HH Size	3.35	3.23	3.30	3.14	3.19	2.98
Age < 30	0.47	0.48	0.48	0.49	0.49	0.49
Non-Hispanic Black	0.16	0.25	0.30	0.25	0.26	0.30
Hispanic	0.18	0.22	0.26	0.22	0.22	0.22
Non-Black and Non-Hispanic	0.66	0.52	0.44	0.53	0.52	0.48
Female or Other	0.57	0.59	0.61	0.68	0.69	0.68
HH Income	37,001	30,618	31,233	32,327	29,255	29,913
College Degree or More	0.18	0.16	0.16	0.28	0.25	0.20
Renter	0.57	0.69	0.67	0.82	0.84	0.79
N	833,477	820,369	31,662	14,573	14,573	3,000

This table compares characteristics of our sample with characteristics of the full US population and the population of the study counties, reweighted to match the enrolled sample's FPL groups (matching the percent in each bin) and county type distribution. Our sample is very similar along most dimensions, though our participants are a little more likely to be renters, have a college degree, or be female. It should be noted that columns (4) and (5) use data from the online screener while column (6) uses baseline survey data and NSC, so the numbers may differ slightly.

Table II: Descriptive Statistics: Baseline Covariate Balance

	Treatment	Control	p-value	N
Demographic				
Age	30.169	30.035	0.542	3000
Female/Other	0.672	0.681	0.627	2999
Non-Hispanic Black	0.294	0.305	0.536	3000
Hispanic	0.220	0.214	0.709	3000
Non-Black and Non-Hispanic	0.486	0.481	0.798	3000
Household Size	2.943	2.996	0.435	3000
Number of Other Adults in the Household	0.684	0.716	0.347	3000
Any Children	0.568	0.571	0.897	3000
Has Disability	0.338	0.310	0.130	2927
Bachelor's Degree	0.202	0.205	0.866	2594
Employed	0.578	0.586	0.675	3000
Income and Employment				
Total Household Income ($1000s)	29.951	29.894	0.942	2920
Total Individual Income ($1000s)	21.258	21.171	0.917	2871
Work Hours/Week	21.733	22.141	0.631	2988
Has a Second Job	0.168	0.173	0.712	2987
Months Employed in the Past Year	7.214	7.268	0.778	2979
Number of Jobs in the Past 1 Year	1.403	1.439	0.457	2965
Number of Jobs in the Past 3 Years	2.685	2.620	0.485	2959
Searching for Work	0.494	0.510	0.429	2984
Started or Helped to Start a Business	0.316	0.296	0.268	2928
Housing				
Lived Temporarily with Family or Friends	0.262	0.281	0.286	2954
Stayed in Non-Permanent Housing	0.086	0.084	0.811	2954
Housing Search Actions in Last 3 Months	0.255	0.242	0.447	2929
Number of Times Moved in the Past 5 Years	1.328	1.358	0.468	2951
Relationships				
Is in a Romantic Relationship	0.627	0.621	0.749	3000
Lives with a Partner	0.331	0.324	0.681	3000
Married	0.221	0.222	0.951	3000
Divorced	0.077	0.081	0.706	2996
Monthly Consumption ($1000s)				
Total Consumption	3.357	3.307	0.449	2835
Non-durable Goods and Services	1.832	1.828	0.916	2946
Housing Expenditures	0.687	0.661	0.231	2907
Human Capital Expenditures	0.409	0.390	0.441	2969
Durable Goods Expenditures	0.304	0.321	0.171	2924
Other Expenditures	0.119	0.114	0.533	2995

This table shows the baseline levels of a number of different variables relating to the employment outcomes considered in this paper. The treatment and control groups look comparable for all items. All variables in this table are based on survey data, except for having a bachelor's degree which is based on NSC data.

Table III: Impact of Guaranteed Income on Annual Earned and Other Unearned Income (in $1,000s)

	Control Mean	Treatment Effect	MPE	Elasticity	N
Panel A: Survey Data					
(1) Total household income	48.2 (33.9)	-4.3***††† (1.0) [0.001]	-0.36 - -0.45	-0.39	2898
(2) Total individual income	33.5 (25.1)	-2.4***††† (0.7) [0.009]	-0.20 - -0.25	-0.32	2855
(3) Total calculated individual income	36.6 (27.0)	-1.4* (0.9) [0.127]	-0.12 - -0.14	-0.18	2881
(4) Individual salaried/wage income	26.0 (26.2)	-1.3 (0.8) [0.360]	-0.10 - -0.13	-0.25	2920
(5) Self-employment income	5.9 (13.7)	-0.1 (0.5) [0.642]	-0.01 - -0.01	-0.08	2902
(6) Income from supplementary gig work	0.4 (1.3)	-0.1 (0.0) [0.351]	-0.01 - -0.01	-2.23	2925
(7) Passive income	0.0 (0.2)	0.0 (0.0) [0.351]	0.00 - 0.00	0.69	2923
(8) Other income	4.7 (6.1)	-0.1 (0.2) [0.635]	-0.01 - -0.01	-0.04	2935
(9) Government transfers	3.6 (4.9)	-0.1 (0.1) [0.800]	-0.01 - -0.02	-0.11	2961
Panel B: UI Data					
(10) Individual salaried/wage income	21.2 (23.6)	-1.7* (0.9) [0.270]	-0.14 - -0.18	-0.35	1907
Panel C: Pooled UI and Survey Data					
(11) Individual salaried/wage income	22.0 (24.2)	-1.6* (0.9) [0.282]	-0.13 - -0.16	-0.32	2258

This table shows the impacts of an unconditional cash transfer on other income outcomes for participants and their households, excluding the transfers, in $1,000s. As an exception, the income family of outcomes was pre-specified to not have its components aggregated in the same way as most other families; instead, total calculated individual income is "promoted" to the family level. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Items that are italicized are secondary outcomes for the sake of the FDR corrections, and unitalicized rows here refer to single primary item components. The MPE range associated with each estimate is calculated assuming net asset accumulation of -$2000 to $5000 over the course of the study. The main text describes adjustments to row (1) and (3) to form the preferred estimates cited elsewhere in the paper as -$3,200 and -$1,800, respectively. All measures are survey-based except for the pooled UI and survey data estimate and the UI data estimate. Appendix K describes the approach to pooling. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table IV: Impact of Guaranteed Income on Employment

	Control Mean	Treatment Effect	Elasticity	N
Panel A: Participants				
Survey Data				
(1) Hours worked per week	30.28 (19.83)	-1.29**††	-0.16	2940
		(0.63)		
		[0.042]		
(2) Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.11	2962
		(0.01)		
		[0.400]		
UI Data				
(3) Whether the respondent is employed	0.61 (0.44)	-0.07***	-0.31	1907
		(0.02)		
		[0.127]		
Pooled UI and Survey Data				
(4) Whether the respondent is employed	0.63 (0.43)	-0.04**††	-0.19	2275
		(0.02)		
		[0.033]		
Panel B: Other Household Members				
Survey Data				
(5) Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.48***	-0.22	2945
		(0.78)		
		[0.175]		
(6) Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.39***	-0.18	2945
		(0.92)		
		[0.302]		
(7) Total number of hours participant's parents in household work per week	3.22 (12.07)	0.02	0.02	2941
		(0.37)		
		[1.000]		
(8) Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.20	1.36	2945
		(0.23)		
		[1.000]		
(9) Number of other household members which are employed	0.47 (0.61)	-0.01	-0.08	2943
		(0.02)		
		[1.000]		

This table shows the impacts of an unconditional cash transfer on the labor supply of participants. As an exception, this family of outcomes does not have its components aggregated in the same way as most other families; instead, the pooled UI and survey data value for employment status is "promoted" to the family level. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Items that are italicized are secondary outcomes or exploratory (post-pre-analysis plan, i.e., the lowest FDR tier) items for the sake of the FDR corrections. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Estimates are provided in terms of raw units. All measures are survey-based except for the pooled UI and survey data estimate and the UI data estimate. Appendix K describes the approach to pooling. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table V: Impact of Guaranteed Income on Monthly Consumption

	Control Mean	Treatment Effect	N
Total Consumption	4169 (1871)	**284*****†††	2988
		(49)	
		[0.001]	
Non-durable goods and services expenditures	2032 (946)	133***†††	2987
		(26)	
		[0.001]	
Housing expenditures	809 (592)	32**†††	2977
		(17)	
		[0.009]	
Human capital expenditures	526 (480)	44***†††	2988
		(15)	
		[0.004]	
Durable goods expenditures	524 (404)	41***†††	2987
		(14)	
		[0.004]	
Other expenditures	278 (364)	34***†††	2987
		(11)	
		[0.004]	

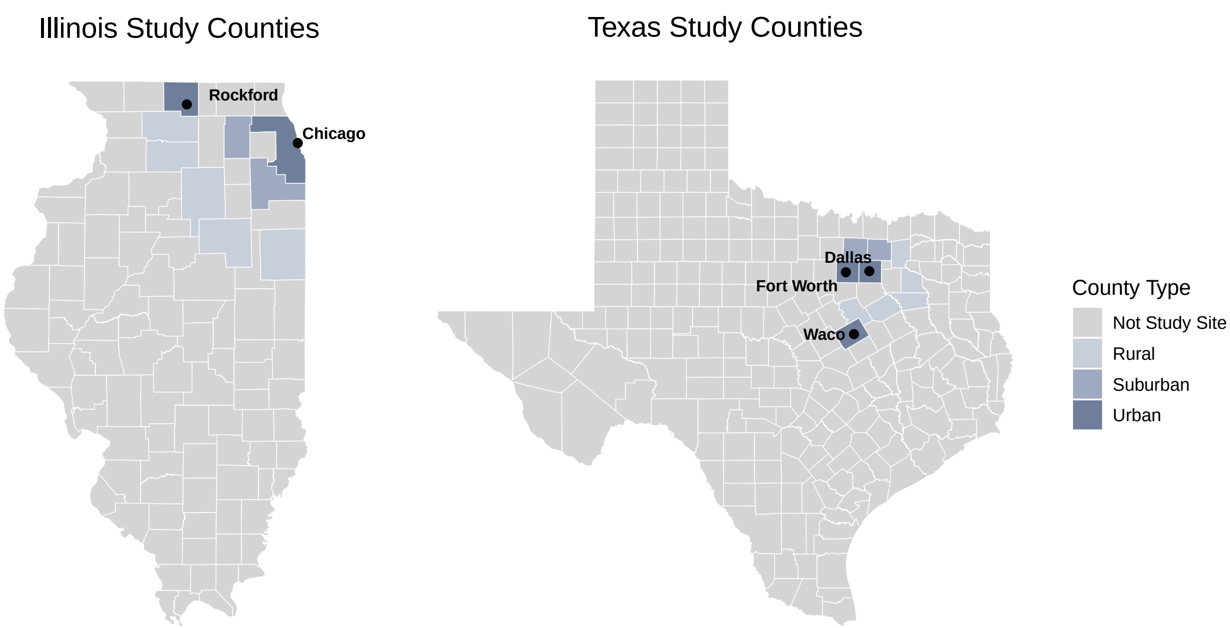
This table shows the impacts of an unconditional cash transfer on monthly aggregate consumption and main categories of spending. In this table, non-durable goods and services include food and non-alcoholic beverage consumption, inside and outside the home; utilities, phone, cable, and internet; non-durable transportation expenditures; clothing, apparel, and personal care expenditures; housekeeping supply expenditures; spending on alcohol, tobacco, marijuana and gambling; recreation and entertainment expenditures; vacations and trips; and expenditures on pets. Housing expenditures include rent, mortgage, home insurance and property tax expenditures. Human capital expenditures include education expenses but also health expenditures, childcare and expenditures on children. Durables include car payment and insurance expenditures and household expenditures such as on furnishings and appliances. Other expenditures include gifts or loans to family and charity, a small amount in debt payments, and other expenses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table VI: Marginal Propensities to Spend and Earn out of $1

	Main Results (1)	Rescale Expenditures by Ratio of PCE/CE (2)	(2) + Assume Change Net Worth $5,000 + Allocate Remainder to Consumption (3)	Allocate All Under-Reporting in (2) to Consumption (4)
Expenditures				
Durable goods	0.04	0.06	0.07	0.09
Human capital	0.05	0.10	0.12	0.15
Non-durables (excluding housing)	0.14	0.25	0.30	0.38
Housing services	0.03	0.03	0.04	0.05
Other	0.04	0.05	0.06	0.08
Income				
Household income	-0.28	-0.28	-0.28	-0.28
Household balance sheet				
Net worth	-0.02	-0.02	0.15	-0.02
Unexplained				
Residual	0.44	0.25	-	-

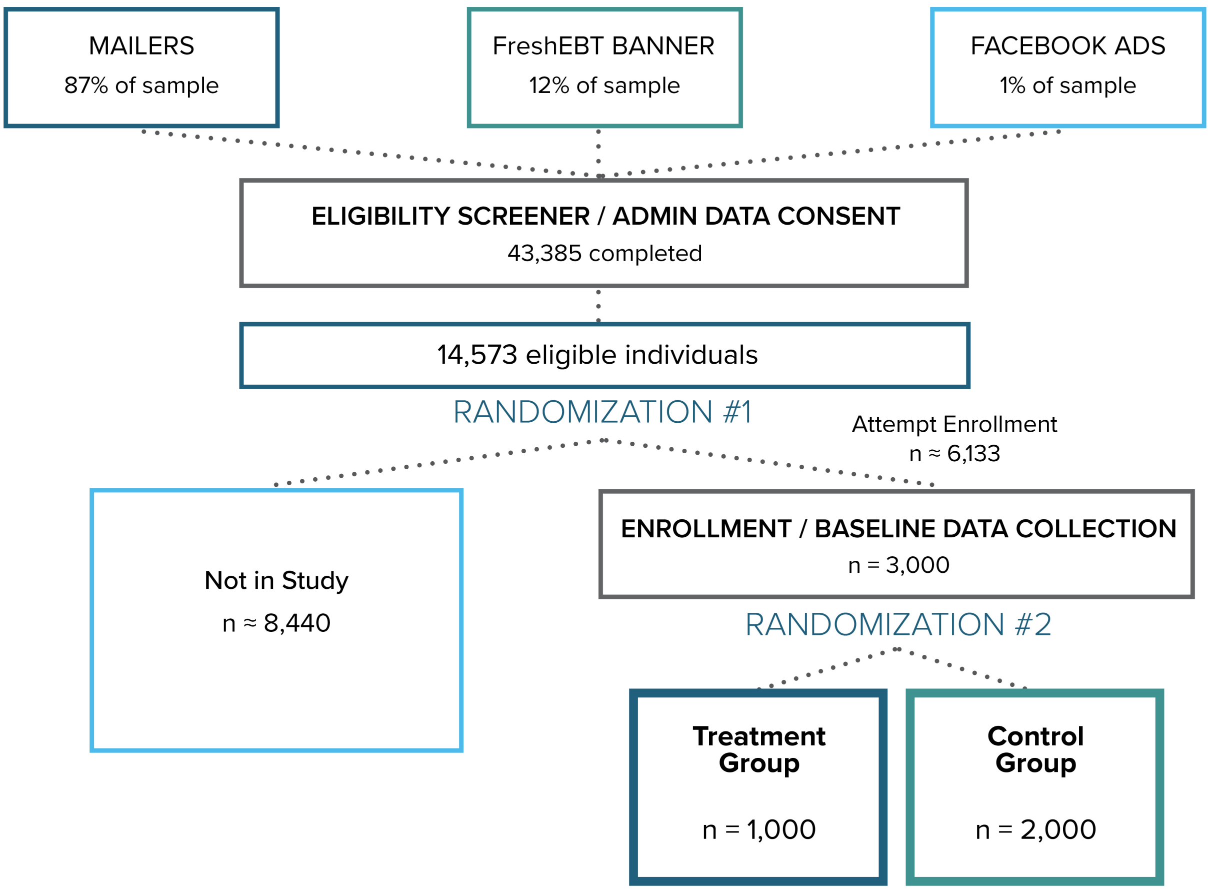
This table shows how participants allocate the transfers across different categories of spending, in particular to expenditures on goods and services, leisure (reduced income), and net saving. Per Bartik et al. (2025), there is a slight imprecisely-estimated decrease in net worth. Column (1) is calculated as the main estimate for each item over the amount that treated participants receive over control participants each year (*i.e.*, $950*12). Treatment effects are annualized before division. Column (2) rescales the expenditures by the ratio of the PCE/CE, per calculations in Appendix I, rescaling at the item-level before aggregating to the category-level. Column (3) assumes net worth increases by $5,000 over the course of the study (the largest amount we consider reasonable given the estimates in Bartik et al. [2025]) and then allocates the remainder to expenditures, per the relative shares in Column (2), on the assumption that income is relatively well-captured in the data. Column (4) allocates all under-reporting to expenditures, per the relative shares going to each type of consumption in Column (2). The "Other" category of expenditures includes spending on debt payments, which one may not want to think of in the same way as spending on other expenditures, but this represents a very small share of spending. Throughout, we use our adjusted estimate of household income of -$3,244, discussed in the text, which yields the -0.28 MPE shown in this table; unadjusted, this number could be slightly higher per Table III. Results from median regressions using unwinsorized consumption data would further increase the share allocated to consumption, per Appendix Table A.60.

Figure I: Location of Study



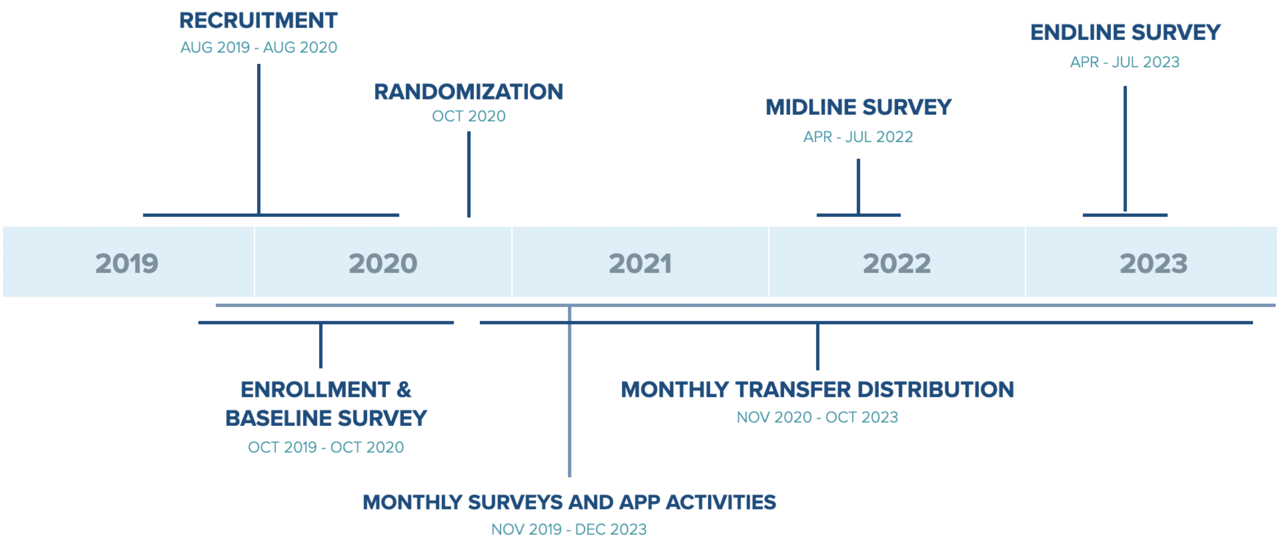
This figure plots the location of the sites in the study.

Figure II: Flowchart of Recruitment Process



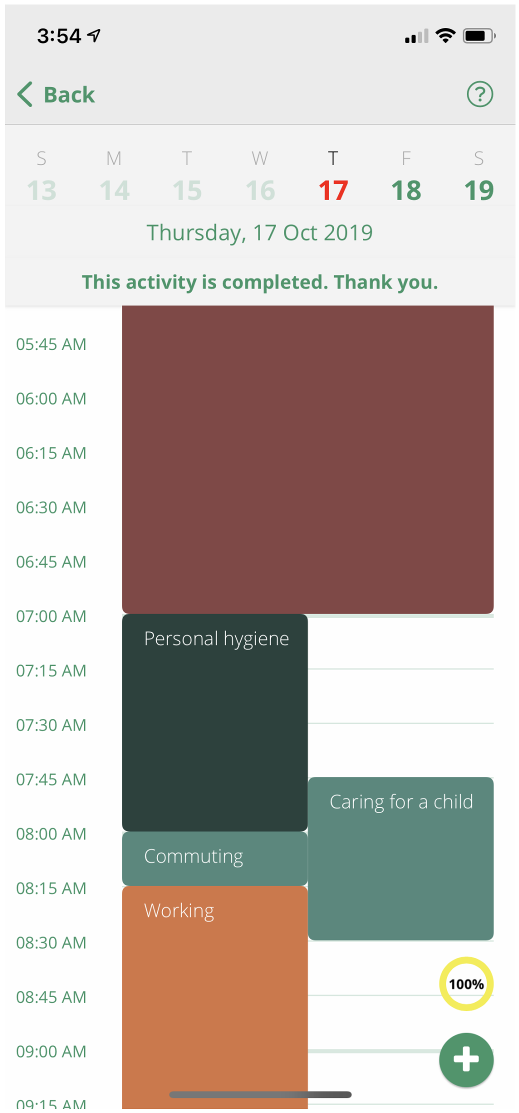
This figure shows a representation of the recruitment process. For the first randomization, individuals were blocked on demographic characteristics, with those in each block having a different probability of being selected in order to meet desired sample characteristics. For the second randomization, participants had the same probability of selection to treatment.

Figure III: Timeline of Study



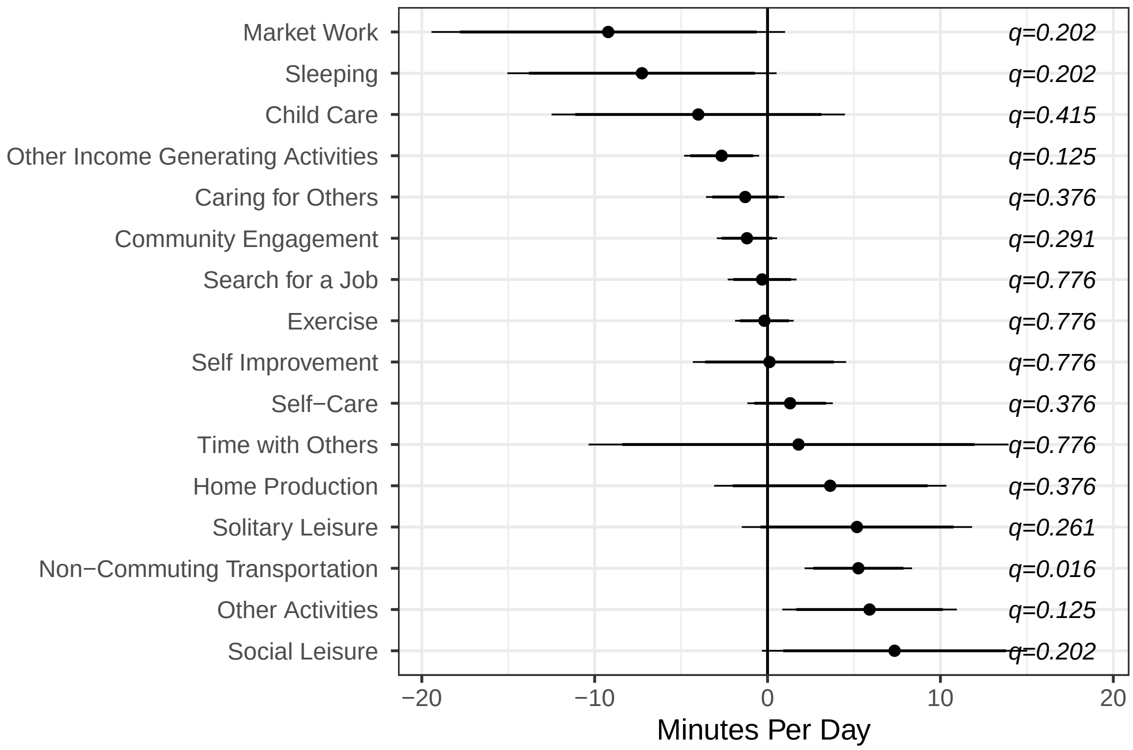
This figure shows a timeline of the program and study.

Figure IV: Time Use Mobile App



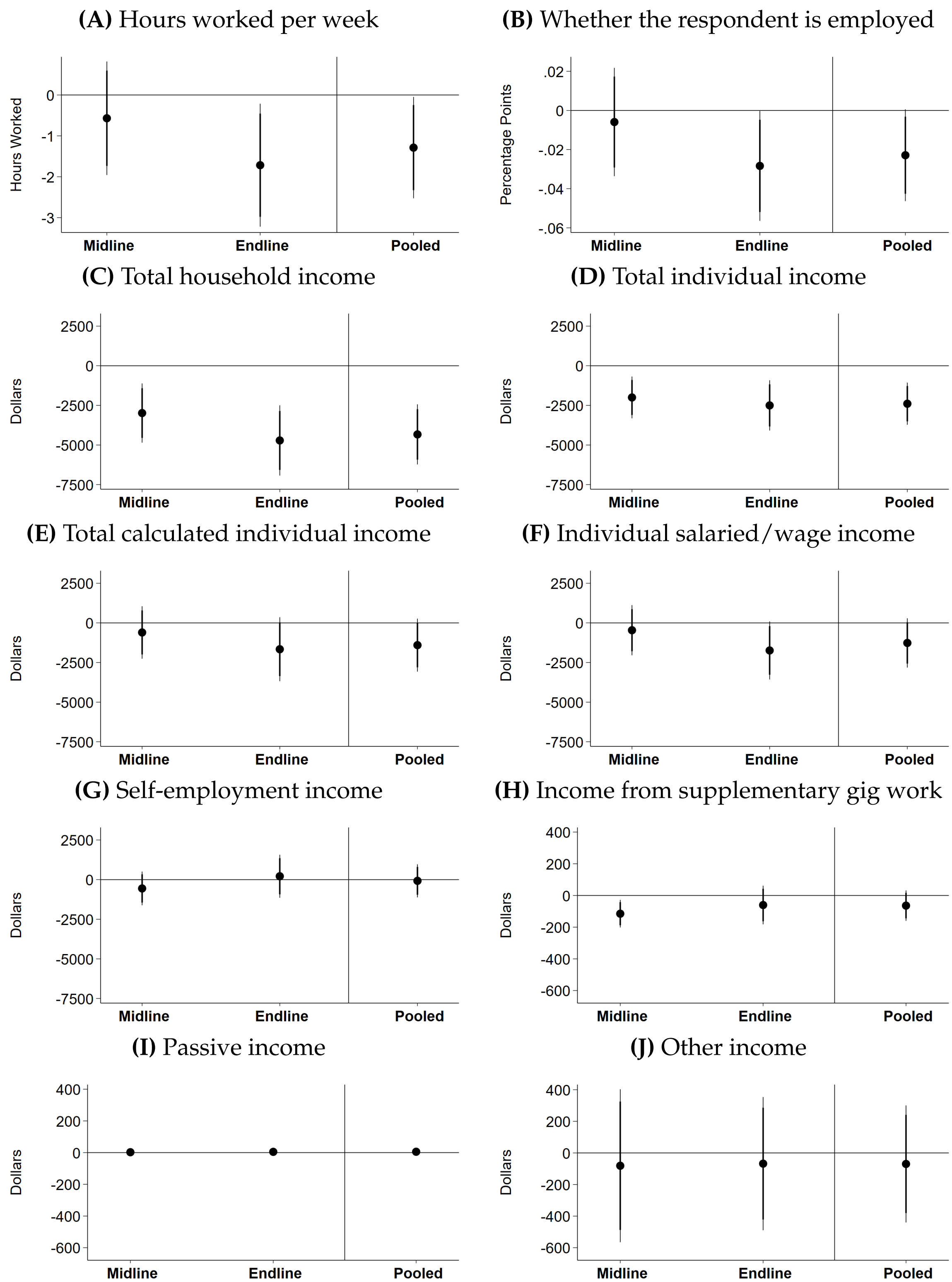
This figure shows a screenshot of the mobile phone application participants used to fill in time diaries on a randomly-selected weekday and weekend day each month.

Figure V: Time Use Results: Mobile App



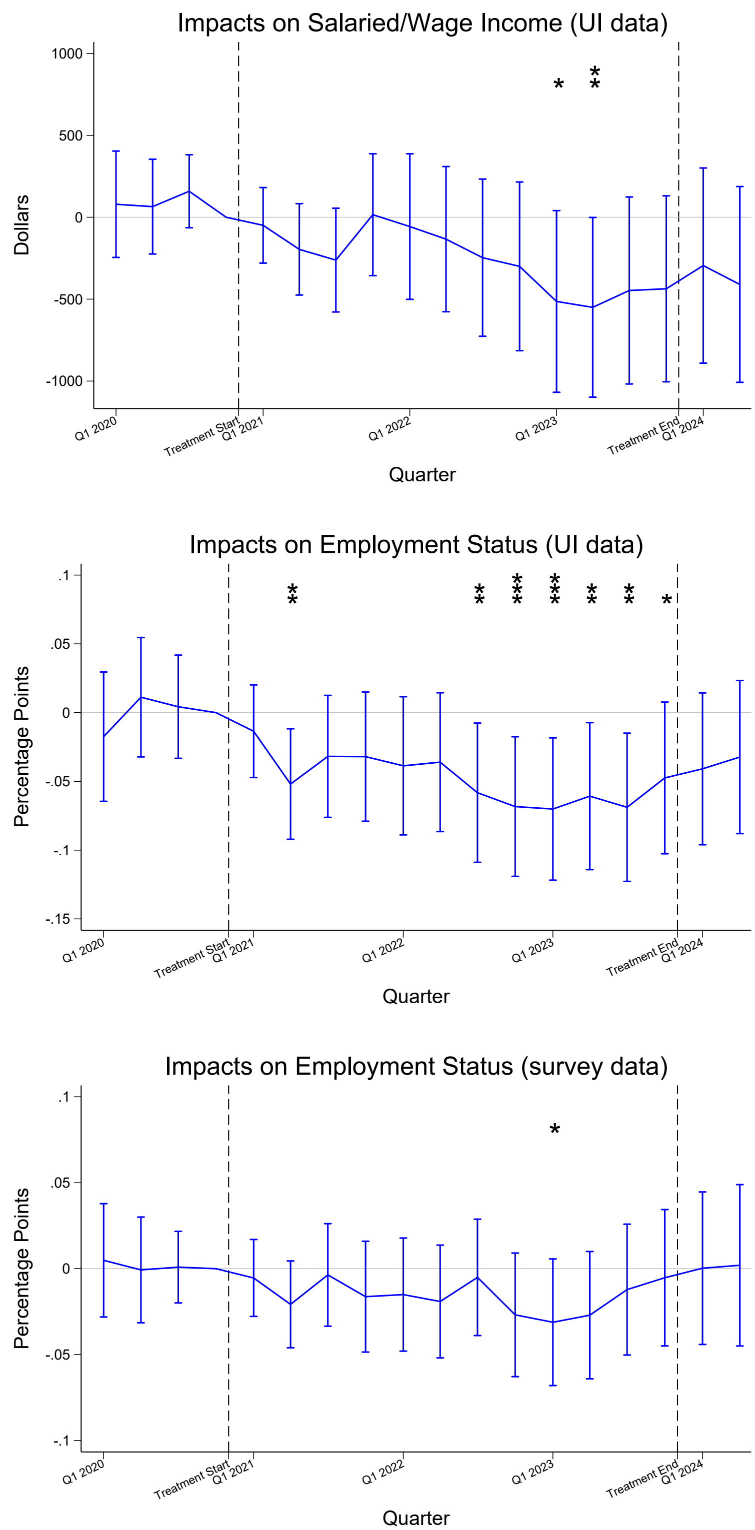
This figure shows the main results from the time diaries. Treatment effects and confidence intervals are displayed, while q-values are provided alongside. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure VI: Estimated Effects on Annual Income and Employment Measures, Enumerated Survey Data



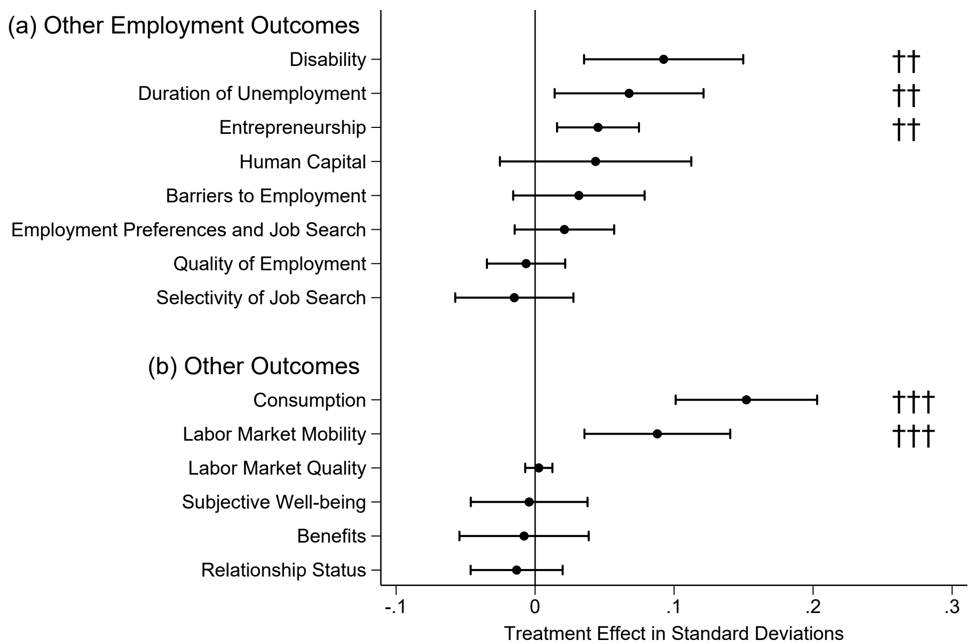
This figure plots the results for treatment effects on annual income and employment over time from the enumerated survey data, showing a clear time trend in the major categories of income and that treatment effects on employment are trending more negative towards the end of the study. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure VII: Event Study Results for Income and Employment



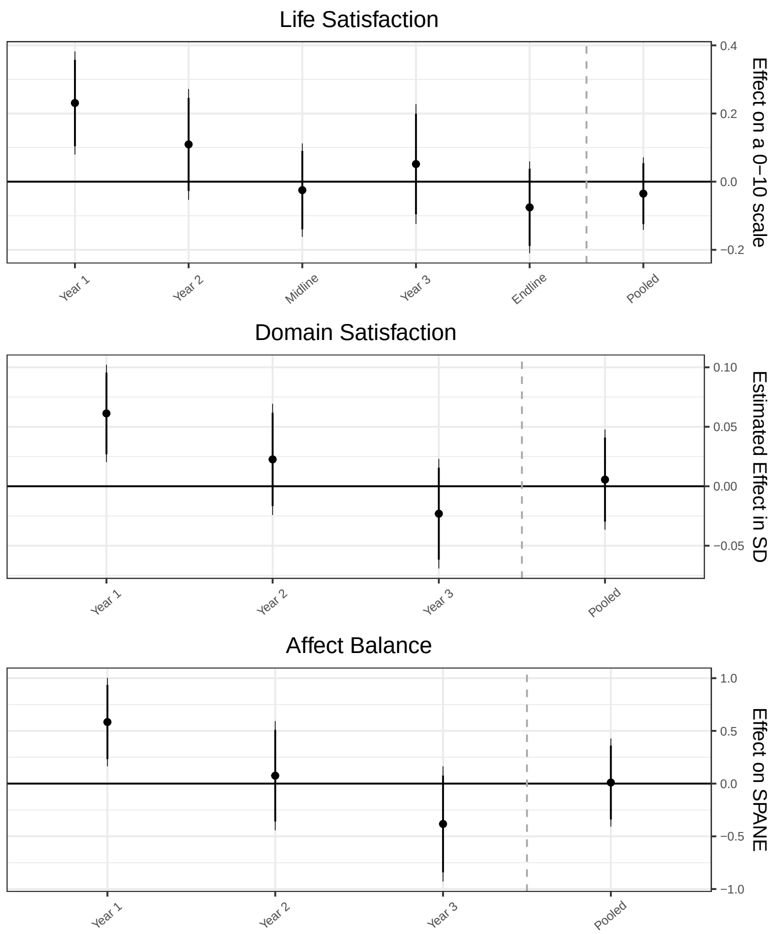
This figure plots the results for income and employment over time, leveraging an event study analysis. The data points represent estimated effects for the preceding quarter, while 95% confidence intervals are shown. Results from Illinois and Texas are pooled in these figures, following Appendix K. Q3 of 2020 represents the last pre-treatment period and is omitted in these figures. Stars show significance at conventional levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). The first two subfigures use data from UI records in each state, while the third uses survey data. No controls are included in these regressions.

Figure VIII: Family-Level Index Results for Other Index Measures



This figure plots the results for family-level indices in standard deviations, with 95% confidence intervals provided and significance per q-values denoted by daggers. For example, there was a significant increase by 0.09 standard deviations in the disability index, indicating that more individuals in the treatment group reported having a disability, with a q-value below 0.05. While we normally reverse "negative" outcomes, we present them unreversed in this figure for interpretability.

Figure IX: Results for Subjective Well-Being Over Time



This figure plots the results for subjective well-being over time, using survey data. All measures appear significant in year one, with effects fading over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

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The Employment Effects of a Guaranteed Income: Experimental Evidence from Two U.S. States

Online Appendix

Eva Vivalt   Elizabeth Rhodes   Alexander Bartik   David Broockman   Patrick Krause
Sarah Miller

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A Appendix Tables

Table A.1: Protection of Benefits

Benefit	Illinois	Texas
Medicaid	Eligibility was not affected	Eligibility was not affected
SNAP	Eligibility was not affected	First $300 per quarter did not affect SNAP, but the remaining amount of the transfer was considered unearned income for the purposes of determining eligibility and the amount of the benefit
TANF	Eligibility was not affected	First $300 per quarter did not affect TANF, but the remaining amount of the transfer was considered unearned income for the purposes of determining eligibility and the amount of the benefit
Housing Assistance	Did not affect eligibility for Chicago Housing Authority, other localities not eligible to participate	Not eligible to participate
SSI	Not eligible to participate	Not eligible to participate

This table shows which major benefits were preserved or not preserved in Illinois and Texas.

Table A.2: Baseline Characteristics of Respondents to Any Qualtrics Survey in Year 1 vs. Non-Respondents

	Respondents			Non-Respondents		
	Control	Treatment	p-value	Control	Treatment	p-value
Demographic						
Age	30.078	30.203	0.574	28.196	27.933	0.847
Female/Other	0.687	0.679	0.683	0.435	0.200	0.071
Non-Hispanic Black	0.307	0.294	0.499	0.239	0.267	0.835
Hispanic	0.212	0.220	0.603	0.304	0.200	0.407
Non-Black and Non-Hispanic	0.482	0.485	0.851	0.457	0.533	0.610
Household Size	2.999	2.947	0.445	2.848	2.667	0.705
Number of Other Adults in the Household	0.717	0.680	0.284	0.674	0.933	0.351
Any Children	0.573	0.570	0.851	0.457	0.467	0.946
Has Disability	0.312	0.338	0.157	0.226	0.364	0.398
Bachelor's Degree	0.205	0.203	0.907	0.209	0.161	0.639
Employed	0.585	0.575	0.574	0.609	0.800	0.138
Income and employment						
Total Household Income ($1000s)	29.928	29.917	0.989	28.455	32.304	0.633
Total Individual Income ($1000s)	21.190	21.219	0.973	20.325	24.090	0.531
Work Hours/Week	22.182	21.489	0.417	20.413	37.733	0.016
Has a Second Job	0.174	0.167	0.640	0.130	0.200	0.551
Months Employed in the Past Year	7.254	7.199	0.778	7.889	8.200	0.762
Number of Jobs in the Past 1 Year	1.433	1.395	0.437	1.711	1.933	0.575
Number of Jobs in the Past 3 Years	2.613	2.647	0.713	2.911	5.133	0.064
Searching for Work	0.508	0.495	0.504	0.587	0.467	0.424
Started or Helped to Start a Business	0.295	0.316	0.264	0.303	0.300	0.980
Housing						
Lived Temporarily with Family or Friends	0.285	0.262	0.202	0.079	0.250	0.203
Stayed in Non-Permanent Housing	0.085	0.085	0.964	0.026	0.167	0.216
Housing Search Actions in Last 3 Months	0.241	0.251	0.582	0.290	0.636	0.049
Number of Times Moved in the Past 5 Years	1.363	1.321	0.316	1.105	1.909	0.124
Relationships						
Is in a Romantic Relationship	0.622	0.626	0.829	0.565	0.667	0.482
Lives with a Partner	0.324	0.330	0.766	0.283	0.400	0.420
Married	0.222	0.220	0.912	0.217	0.267	0.708
Divorced	0.081	0.078	0.805	0.087	0.000	0.043
Monthly Consumption ($1000s)						
Total Consumption	3.310	3.360	0.449	3.190	3.101	0.600
Non-durable Goods and Services	1.827	1.832	0.904	1.869	1.855	0.884
Housing Expenditures	0.663	0.688	0.252	0.576	0.617	0.844
Human Capital Expenditures	0.390	0.410	0.435	0.385	0.369	0.918
Durable Goods Expenditures	0.322	0.303	0.126	0.274	0.379	0.383
Other Expenditures	0.115	0.120	0.498	0.101	0.067	0.351

This table compares the baseline characteristics of participants who responded or did not respond to a Qualtrics survey in Year 1 of the study.

Table A.3: Baseline Characteristics of Respondents to Any Qualtrics Survey in Year 2 vs. Non-Respondents

	Respondents			Non-Respondents		
	Control	Treatment	p-value	Control	Treatment	p-value
Demographic						
Age	30.113	30.163	0.823	28.337	30.467	0.094
Female/Other	0.685	0.681	0.841	0.581	0.400	0.086
Non-Hispanic Black	0.304	0.294	0.604	0.337	0.300	0.706
Hispanic	0.214	0.223	0.566	0.233	0.133	0.203
Non-Black and Non-Hispanic	0.483	0.483	0.999	0.430	0.567	0.199
Household Size	3.018	2.948	0.317	2.512	2.833	0.347
Number of Other Adults in the Household	0.726	0.680	0.183	0.500	0.833	0.077
Any Children	0.575	0.569	0.741	0.465	0.567	0.339
Has Disability	0.311	0.338	0.154	0.274	0.391	0.336
Bachelor's Degree	0.204	0.204	0.995	0.231	0.161	0.342
Employed	0.588	0.571	0.384	0.547	0.800	0.006
Income and employment						
Total Household Income ($1000s)	29.993	29.993	1.000	28.332	29.081	0.907
Total Individual Income ($1000s)	21.223	21.094	0.878	20.140	26.391	0.192
Work Hours/Week	22.174	21.256	0.285	21.570	36.467	0.005
Has a Second Job	0.173	0.161	0.430	0.174	0.333	0.100
Months Employed in the Past Year	7.249	7.163	0.659	7.698	8.700	0.284
Number of Jobs in the Past 1 Year	1.420	1.375	0.357	1.872	2.267	0.241
Number of Jobs in the Past 3 Years	2.575	2.653	0.400	3.605	3.700	0.896
Searching for Work	0.510	0.494	0.412	0.500	0.500	1.000
Started or Helped to Start a Business	0.297	0.310	0.490	0.261	0.565	0.010
Housing						
Lived Temporarily with Family or Friends	0.284	0.265	0.290	0.208	0.154	0.536
Stayed in Non-Permanent Housing	0.082	0.088	0.606	0.104	0.038	0.277
Housing Search Actions in Last 3 Months	0.242	0.252	0.552	0.233	0.391	0.140
Number of Times Moved in the Past 5 Years	1.360	1.323	0.377	1.342	1.440	0.778
Relationships						
Is in a Romantic Relationship	0.627	0.630	0.891	0.500	0.567	0.530
Lives with a Partner	0.330	0.333	0.844	0.198	0.267	0.455
Married	0.228	0.223	0.751	0.093	0.167	0.330
Divorced	0.081	0.077	0.695	0.081	0.100	0.767
Monthly Consumption ($1000s)						
Total Consumption	3.313	3.351	0.567	3.216	3.603	0.444
Non-durable Goods and Services	1.831	1.824	0.840	1.791	2.149	0.110
Housing Expenditures	0.667	0.689	0.321	0.541	0.615	0.548
Human Capital Expenditures	0.385	0.410	0.315	0.505	0.376	0.301
Durable Goods Expenditures	0.322	0.303	0.113	0.293	0.358	0.479
Other Expenditures	0.114	0.119	0.480	0.131	0.120	0.771

This table compares the baseline characteristics of participants who responded or did not respond to a Qualtrics survey in Year 2 of the study.

Table A.4: Baseline Characteristics of Respondents to Any Qualtrics Survey in Year 3 vs. Non-Respondents

	Respondents			**Non-Respondents**		
	Control	Treatment	p-value	Control	Treatment	p-value
Demographic						
Age	30.140	30.222	0.714	28.803	28.100	0.541
Female/Other	0.694	0.676	0.351	0.504	0.567	0.531
Non-Hispanic Black	0.307	0.295	0.488	0.277	0.333	0.555
Hispanic	0.209	0.222	0.436	0.285	0.167	0.135
Non-Black and Non-Hispanic	0.484	0.483	0.994	0.438	0.500	0.540
Household Size	3.019	2.950	0.324	2.737	2.867	0.702
Number of Other Adults in the Household	0.717	0.682	0.307	0.723	0.767	0.821
Any Children	0.578	0.572	0.748	0.482	0.533	0.610
Has Disability	0.312	0.339	0.156	0.266	0.280	0.916
Bachelor's Degree	0.203	0.205	0.909	0.230	0.161	0.321
Employed	0.585	0.573	0.513	0.591	0.767	0.048
Income and employment						
Total Household Income ($1000s)	29.863	29.902	0.960	30.746	34.888	0.280
Total Individual Income ($1000s)	21.234	21.133	0.905	20.623	27.285	0.130
Work Hours/Week	22.195	21.368	0.340	21.664	34.100	0.009
Has a Second Job	0.176	0.165	0.472	0.139	0.233	0.256
Months Employed in the Past Year	7.254	7.162	0.639	7.343	9.267	0.011
Number of Jobs in the Past 1 Year	1.434	1.376	0.243	1.515	2.333	0.004
Number of Jobs in the Past 3 Years	2.597	2.637	0.667	2.905	4.267	0.020
Searching for Work	0.510	0.492	0.362	0.489	0.533	0.662
Started or Helped to Start a Business	0.294	0.311	0.371	0.331	0.542	0.058
Housing						
Lived Temporarily with Family or Friends	0.289	0.264	0.162	0.183	0.222	0.660
Stayed in Non-Permanent Housing	0.082	0.087	0.634	0.099	0.074	0.816
Housing Search Actions in Last 3 Months	0.241	0.255	0.404	0.242	0.240	0.975
Number of Times Moved in the Past 5 Years	1.367	1.317	0.234	1.264	1.654	0.141
Relationships						
Is in a Romantic Relationship	0.626	0.630	0.844	0.562	0.633	0.468
Lives with a Partner	0.329	0.334	0.804	0.263	0.300	0.687
Married	0.226	0.221	0.742	0.168	0.267	0.259
Divorced	0.083	0.078	0.668	0.058	0.033	0.517
Monthly Consumption ($1000s)						
Total Consumption	3.304	3.357	0.426	3.384	3.581	0.583
Non-durable Goods and Services	1.825	1.825	0.995	1.893	2.084	0.371
Housing Expenditures	0.664	0.687	0.288	0.639	0.740	0.325
Human Capital Expenditures	0.382	0.414	0.213	0.472	0.318	0.137
Durable Goods Expenditures	0.321	0.303	0.152	0.321	0.370	0.575
Other Expenditures	0.114	0.120	0.455	0.116	0.107	0.741

This table compares the baseline characteristics of participants who responded or did not respond to a Qualtrics survey in Year 3 of the study.

Table A.5: Baseline Characteristics of Respondents to the Enumerated Midline vs. Non-Respondents

	Respondents			Non-Respondents		
	Control	Treatment	p-value	Control	Treatment	p-value
Demographic						
Age	30.075	30.149	0.741	29.160	31.300	0.123
Female/Other	0.683	0.675	0.678	0.613	0.550	0.615
Non-Hispanic Black	0.307	0.296	0.565	0.267	0.200	0.522
Hispanic	0.213	0.221	0.636	0.240	0.200	0.698
Non-Black and Non-Hispanic	0.480	0.483	0.893	0.493	0.600	0.395
Household Size	3.002	2.947	0.423	2.867	2.850	0.969
Number of Other Adults in the Household	0.717	0.685	0.364	0.720	0.650	0.728
Any Children	0.573	0.569	0.851	0.520	0.550	0.813
Has Disability	0.310	0.337	0.149	0.279	0.421	0.258
Bachelor's Degree	0.206	0.205	0.942	0.182	0.081	0.112
Employed	0.587	0.580	0.737	0.560	0.450	0.385
Income and employment						
Total Household Income ($1000s)	29.940	30.133	0.805	29.515	21.969	0.086
Total Individual Income ($1000s)	21.184	21.388	0.809	20.998	14.525	0.139
Work Hours/Week	22.208	21.825	0.657	20.440	16.300	0.410
Has a Second Job	0.174	0.167	0.623	0.147	0.150	0.971
Months Employed in the Past Year	7.275	7.216	0.758	7.027	6.900	0.919
Number of Jobs in the Past 1 Year	1.443	1.400	0.385	1.347	1.500	0.665
Number of Jobs in the Past 3 Years	2.619	2.678	0.533	2.640	3.000	0.590
Searching for Work	0.508	0.492	0.399	0.533	0.600	0.594
Started or Helped to Start a Business	0.297	0.313	0.369	0.279	0.471	0.153
Housing						
Lived Temporarily with Family or Friends	0.282	0.261	0.235	0.254	0.316	0.603
Stayed in Non-Permanent Housing	0.081	0.086	0.628	0.141	0.105	0.663
Housing Search Actions in Last 3 Months	0.241	0.256	0.370	0.265	0.211	0.607
Number of Times Moved in the Past 5 Years	1.364	1.325	0.347	1.229	1.389	0.566
Relationships						
Is in a Romantic Relationship	0.625	0.631	0.729	0.533	0.450	0.511
Lives with a Partner	0.327	0.334	0.702	0.253	0.200	0.607
Married	0.224	0.223	0.918	0.173	0.150	0.800
Divorced	0.080	0.076	0.704	0.107	0.150	0.624
Monthly Consumption ($1000s)						
Total Consumption	3.312	3.361	0.459	3.237	3.214	0.886
Non-durable Goods and Services	1.829	1.831	0.964	1.827	1.941	0.571
Housing Expenditures	0.662	0.685	0.285	0.660	0.773	0.499
Human Capital Expenditures	0.389	0.414	0.321	0.424	0.180	0.001
Durable Goods Expenditures	0.324	0.305	0.134	0.243	0.243	0.982
Other Expenditures	0.115	0.120	0.489	0.109	0.077	0.280

This table compares the baseline characteristics of participants who responded or did not respond to the enumerated midline survey.

Table A.6: Baseline Characteristics of Respondents to the Enumerated Endline vs. Non-Respondents

	Respondents			Non-Respondents		
	Control	Treatment	p-value	Control	Treatment	p-value
Demographic						
Age	30.050	30.140	0.687	29.903	31.050	0.419
Female/Other	0.687	0.674	0.478	0.553	0.650	0.415
Non-Hispanic Black	0.308	0.296	0.498	0.243	0.250	0.945
Hispanic	0.208	0.222	0.404	0.320	0.150	0.068
Non-Black and Non-Hispanic	0.483	0.482	0.947	0.437	0.600	0.179
Household Size	3.008	2.952	0.411	2.806	2.850	0.925
Number of Other Adults in the Household	0.715	0.692	0.492	0.748	0.350	0.016
Any Children	0.574	0.571	0.883	0.505	0.550	0.713
Has Disability	0.311	0.334	0.207	0.276	0.450	0.147
Bachelor's Degree	0.204	0.205	0.970	0.220	0.141	0.299
Employed	0.585	0.582	0.871	0.602	0.400	0.096
Income and employment						
Total Household Income ($1000s)	29.927	30.134	0.793	29.862	25.926	0.390
Total Individual Income ($1000s)	21.201	21.355	0.856	20.722	18.392	0.696
Work Hours/Week	22.119	21.827	0.735	22.632	18.050	0.445
Has a Second Job	0.173	0.168	0.749	0.184	0.150	0.699
Months Employed in the Past Year	7.263	7.229	0.858	7.272	6.850	0.703
Number of Jobs in the Past 1 Year	1.434	1.403	0.530	1.549	1.450	0.760
Number of Jobs in the Past 3 Years	2.586	2.676	0.326	3.238	3.150	0.929
Searching for Work	0.510	0.491	0.340	0.505	0.600	0.432
Started or Helped to Start a Business	0.293	0.312	0.299	0.354	0.500	0.261
Housing						
Lived Temporarily with Family or Friends	0.286	0.262	0.170	0.192	0.316	0.282
Stayed in Non-Permanent Housing	0.082	0.086	0.749	0.101	0.158	0.511
Housing Search Actions in Last 3 Months	0.242	0.257	0.375	0.224	0.150	0.396
Number of Times Moved in the Past 5 Years	1.363	1.322	0.335	1.306	1.579	0.286
Relationships						
Is in a Romantic Relationship	0.626	0.629	0.859	0.544	0.650	0.370
Lives with a Partner	0.326	0.336	0.600	0.291	0.200	0.366
Married	0.223	0.225	0.908	0.204	0.100	0.187
Divorced	0.081	0.077	0.724	0.078	0.050	0.620
Monthly Consumption ($1000s)						
Total Consumption	3.299	3.359	0.367	3.495	3.606	0.646
Non-durable Goods and Services	1.824	1.828	0.898	1.945	2.093	0.474
Housing Expenditures	0.662	0.685	0.304	0.649	0.870	0.118
Human Capital Expenditures	0.382	0.417	0.171	0.533	0.162	0.000
Durable Goods Expenditures	0.323	0.306	0.156	0.275	0.291	0.828
Other Expenditures	0.114	0.118	0.606	0.109	0.164	0.229

This table compares the baseline characteristics of participants who responded or did not respond to the enumerated endline survey.

Table A.7: FDR Tiers

	Pooled Across Midline/Endline and Monthly Surveys	Pooled Across Midline/Endline Surveys Only (Omitting Monthly Surveys)	Estimates At Each Time Period (e.g., at midline, in year 2, etc.)
Family	K0	K0	K3
Primary Components	K1	K1	K3
Primary Items	K2	K2	K3
Secondary Items	K3	K3	K3
Tertiary Items	K3	K3	K3
Heterogeneous treatment effects	K3	K3	Not calculated
Any post-PAP tests	K4	K4	K4

This table shows the tiers used for the FDR corrections used in this paper. Within a family, the adjustments for tests at the K0 level would be done only among the set of tests at the K0 level; the adjustments for tests at the K1 level would pool those at the K0 level plus those at the K1 level, and so on. This hierarchy of adjustments prioritizes those tests that are "higher" up the index, following Guess et al. (2023). "Any post-PAP tests" refers to those tests that were not specified in a pre-analysis plan. Appendix E provides further detail.

Table A.8: Impact of Guaranteed Income on Income and Employment: Lee Bounds using UI Data

		Treatment Effect	Lower Lee Bound	Upper Lee Bound
Income (Annual salary/wage income in thousands of dollars)	Midline	-0.41	-2.07**	-0.03
		(0.87)	(0.81)	(0.88)
	Endline	-2.67**	-4.87***	-2.03*
		(1.08)	(1.01)	(1.10)
	Pooled	-1.75*	-3.68***	-1.35
		(0.94)	(0.88)	(0.96)
Employment (in percentage points)	Midline	-0.04**	-0.06***	-0.02
		(0.02)	(0.02)	(0.02)
	Endline	-0.07***	-0.09***	-0.05**
		(0.02)	(0.02)	(0.02)
	Pooled	-0.07***	-0.08***	-0.05**
		(0.02)	(0.02)	(0.02)

This table compares the estimated impact of the guaranteed income program on income and employment using Lee bounds. Effects are estimated with the UI data, for those who consented to share these data and could be matched based on provided information, aggregated across states using fixed-effects meta-analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Impact of Guaranteed Income on Employment: Second/Third/Fourth Jobs

	Control Mean	Treatment Effect	N
Whether the respondent has a second job	0.20 (0.35)	-0.01 (0.01) [1.000]	2939
Whether the respondent has a third job	0.06 (0.20)	0.00 (0.01) [1.000]	2939
Whether the respondent has a fourth job	0.02 (0.10)	-0.01 (0.00) [0.760]	2939
Hours per week worked at 1st job	27.27 (17.98)	-1.46** (0.57) [0.337]	2939
Hours per week worked at 2nd job	2.41 (5.69)	-0.06 (0.21) [1.000]	2937
Hours per week worked at 3rd job	0.49 (2.37)	-0.02 (0.09) [1.000]	2938
Hours per week worked at 4th job	0.10 (0.94)	-0.03 (0.03) [1.000]	2939
Hours worked per week (conditional on working)	40.43 (14.44)	-1.02* (0.59) [0.707]	2408
Hours per week worked at 1st job (conditional on having 1st job)	36.39 (12.95)	-1.07** (0.53) [0.487]	2404
Hours per week worked at 2nd job (conditional on having 2nd job)	12.88 (11.48)	-0.23 (0.80) [1.000]	795
Hours per week worked at 3rd job (conditional on having 3rd job)	8.94 (8.23)	-0.57 (1.13) [1.000]	259
Hours per week worked at 4th job (conditional on having 4th job)	7.78 (7.19)	-1.37 (1.93) [1.000]	58
Maximum number of hours worked in a typical week	32.70 (19.46)	-1.57*** (0.59) [0.191]	2984
Minimum number of hours worked in a typical week	21.84 (15.29)	-0.87* (0.46) [0.418]	2984

This table provides exploratory analysis of impacts on whether participants reduced hours in particular at first/second/third/fourth jobs. As usual, unconditional estimates are presented for the sake of maintaining the causal interpretation of the estimate, so for example if someone does not have a third job they would be coded as working 0 hours at that third job. These questions were secondary or exploratory post-pre-analysis plan items in the Labor Supply family and have been adjusted for multiple hypothesis testing accordingly. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.10: Impact of Guaranteed Income on Employment: Reasons for Not Working

	Control Mean	Treatment Effect	N
Not working due to inability to find child care	0.07 (0.22)	0.01 (0.01) [0.684]	2942
Not working due to attending school	0.04 (0.16)	0.01 (0.01) [0.819]	2942
Not working due to caring for elderly	0.02 (0.13)	0.01 (0.01) [0.684]	2942
Not working due to have given up looking for work	0.04 (0.17)	-0.01 (0.01) [0.684]	2942
Not working due to illness	0.07 (0.23)	0.01* (0.01) [0.512]	2942
Not working due to lack in necessary skills	0.08 (0.24)	0.01 (0.01) [0.875]	2942
Not working due to other reasons	0.06 (0.19)	0.01 (0.01) [0.730]	2942
Not working due to personal or family responsibilities	0.13 (0.29)	0.01 (0.01) [1.000]	2942
Not working due to preferring to stay at home	0.09 (0.26)	0.00 (0.01) [1.000]	2942
Not working due to lack in transportation to/from work	0.06 (0.20)	0.01 (0.01) [0.760]	2942
Not working due to suitable work being unavailable	0.13 (0.29)	0.01 (0.01) [0.875]	2942

This table provides exploratory analysis of self-reported reasons participants provided for why they were not working. As usual, unconditional estimates are presented for the sake of maintaining the causal interpretation of the estimate, so if someone is employed they would be treated as having answered no to a question. These questions were secondary items in the Labor Supply family and have been adjusted for multiple hypothesis testing accordingly. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.11: Impact of Guaranteed Income on Disability

	Control Mean	Treatment Effect	N
Disability Index		**-0.09*** ††**	**2875**
		(0.03)	
		[0.014]	
Disability Component		-0.09*** †††	2875
		(0.03)	
		[0.002]	
Participant has a health problem/disability	0.31 (0.42)	0.04*** †††	2875
		(0.01)	
		[0.004]	
Participant has a health problem/disability that limits the work they can do	0.28 (0.41)	0.04*** †††	2873
		(0.01)	
		[0.004]	
How much the participant's worst health problem/disability limits the amount of work they can do (1-7 scale)	1.11 (1.71)	0.15*** †††	2873
		(0.05)	
		[0.004]	
How long the participant's health problem/disability has affected the work they can do (more than 1 year continuously or intermitently, less than 1 year)	0.73 (1.06)	0.08** †††	2874
		(0.03)	
		[0.005]	

This table shows the impacts of an unconditional cash transfer on disability. The top-level index decreases significantly by about 0.09 standard deviations, representing an increase in disability. The q-values on the component and the top-level family index measures are different even as the point estimate is the same as they adjust for different sets of estimates in the FDR corrections (see Appendix E for details). There are several primary items under the component. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Except for the family- and component-level index values, estimates are provided in terms of raw units. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.12: Impact of Guaranteed Income on Duration of Unemployment

	Control Mean	Treatment Effect	N
Duration of Unemployment Index		**-0.07**†† (0.03) [0.033]**	**2928**
Single-item Component: Average length of continuous spells of non-employment in months, over the study duration	7.81 (11.38)	0.77**†† (0.32) [0.016]	2928
Length of longest continuous spell of non-employment in months, over the study duration	8.76 (11.81)	0.82***†† (0.31) [0.040]	2928
Duration of unemployment in months at time of survey	2.87 (8.05)	0.64**†† (0.29) [0.040]	2940
Duration of non-employment in months at time of survey	6.07 (12.21)	0.78**†† (0.36) [0.040]	2938
Number of months of non-employment in the last year	3.38 (4.41)	0.28**† (0.13) [0.050]	2934

This table shows the impacts of an unconditional cash transfer on the duration of non-employment and unemployment of participants. The top-level index, "Duration of Unemployment", declines by about 0.07 standard deviations, representing an increase in duration of unemployment. As there is a single primary item in the component (average length of continuous spells of non-employment), it is "promoted" to act as a component as per appendix E, but it is still presented in raw units. Several items that are italicized represent secondary outcomes for the sake of the FDR corrections. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Except for the family-level index value, estimates are provided in terms of raw units. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.13: Impact of Guaranteed Income on Human Capital

	Control Mean	Treatment Effect	N
Human Capital Index	0.22 (0.32)	**0.01** **(0.01)** **[0.321]**	**2987**
Formal Education Component		0.02 (0.02) [0.791]	2986
Completed a GED or post-secondary degree	0.94 (0.23)	0.00 (0.00) [1.000]	2986
Completed a post-secondary degree (NSC only)	0.35 (0.47)	-0.01 (0.01) [1.000]	2623
Total years of post-secondary education completed post-baseline (NSC only)	0.13 (0.33)	0.01 (0.01) [1.000]	2623
Enrolled in a post-secondary program (NSC only)	0.15 (0.30)	0.01 (0.01) [1.000]	2623
Enrolled in post-secondary program	0.15 (0.29)	0.01 (0.01) [1.000]	2998
Average hours of school per week (full-time, part-time, withdrawn, etc.) in post-secondary program	1.96 (5.28)	0.23 (0.21) [1.000]	2615
Participation in informal education	0.10 (0.21)	0.01 (0.01) [1.000]	2987
Extent of participation in informal education (full-time, part-time, not enrolled)	0.07 (0.18)	0.00 (0.01) [1.000]	2987
Whether the participant plans to receive job training	0.03 (0.14)	0.01** (0.01) [1.000]	2940

This table shows the impacts of an unconditional cash transfer on human capital. The top-level index increases insignificantly by about 0.01 standard deviations. Apart from the component "Formal Education", there is a component "Informal Education" comprised of only secondary items that do not contribute to the index (so the component-level result is not printed). Items that are italicized are secondary outcomes for the sake of the FDR corrections. For each pre-specified outcome, NSC data is preferred if it exists for that outcome. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Except for the family- and component-level index values, estimates are provided in terms of raw units. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.14: Impact of Guaranteed Income on Entrepreneurship

	Control Mean	Treatment Effect	N
Entrepreneurship Index		**0.05*** ††** **(0.02)** **[0.014]**	**2966**
Entrepreneurial Orientation Component		0.07*** ††† (0.02) [0.009]	2959
The respondent's self-reported willingness to take financial risks (1-10 scale)	4.52 (2.09)	0.09 (0.06) [0.104]	2866
Midpoint of the constant relative risk aversion (CRRA) range implied by a participant's coin flip gamble	1.82 (1.55)	-0.16*** †† (0.06) [0.021]	2911
Entrepreneurial Intention Component		0.06** †† (0.02) [0.013]	2911
Whether or not the respondent has an idea for a business	0.58 (0.42)	0.04*** †† (0.01) [0.021]	2910
The respondent's likelihood rating that they will start a business in the next 5 years (1-10 scale)	4.95 (3.05)	0.15* † (0.08) [0.055]	2910
The respondent's interest in starting a business (1-10 scale)	6.21 (2.96)	0.10 (0.09) [0.116]	2911
Entrepreneurial Activity Component		0.01 (0.02) [0.202]	2909
If a family member who started a business lives in the respondent's household	0.06 (0.21)	-0.01** †† (0.01) [0.036]	2908
If the respondent knows someone who started or helped start a business	0.60 (0.41)	0.03*** †† (0.01) [0.021]	2908
If the respondent ever started or helped start a business	0.30 (0.40)	0.00 (0.01) [0.293]	2909

This table shows the impacts of an unconditional cash transfer on entrepreneurship. The top-level index increases significantly by about 0.05 standard deviations. There are three components with estimates in standard deviations (Entrepreneurial Orientation, Entrepreneurial Intention, and Entrepreneurial Activity), two of which are positive and significant. Each component contains more than one primary item under it. The item representing the midpoint of the CRRA range implied by a participant's gamble in an incentive-compatible multiple price list experiment is flipped before combining in the index, since low values represent comfort with risks. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Except for the family- and component-level index values, estimates are provided in terms of raw units. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.15: Impact of Guaranteed Income on Human Capital: Programs and Fields of Study

	Control Mean	Treatment Effect	N
Studied liberal arts in post secondary education	0.10 (0.29)	0.01 (0.01) [1.000]	2931
Studied business in post secondary education	0.04 (0.20)	-0.01 (0.01) [1.000]	2931
Studied education in post secondary education	0.02 (0.14)	-0.01** (0.00) [1.000]	2931
Studied health in post secondary education	0.06 (0.22)	-0.01 (0.01) [1.000]	2931
Studied social sciences in post secondary education	0.08 (0.26)	-0.01 (0.01) [1.000]	2931
Studied STEM in post secondary education	0.06 (0.23)	0.00 (0.01) [1.000]	2931
Studied a vocational major in post secondary education	0.03 (0.17)	0.00 (0.01) [1.000]	2931
Whether the participant has an Associate's degree	0.12 (0.32)	-0.00 (0.01) [1.000]	2593
Whether the participant has a Bachelor's degree	0.23 (0.42)	-0.01 (0.01) [1.000]	2593
Whether the participant has a Master's or Doctoral degree	0.08 (0.26)	-0.01** (0.01) [0.744]	2593
Whether the participant has a Master's degree	0.07 (0.25)	-0.02*** (0.01) [0.259]	2593
Whether the participant has a Doctoral degree	0.02 (0.12)	0.00 (0.00) [1.000]	2593

This table provides exploratory analysis of programs and fields of study that participants pursued, according to the NSC data. These questions were secondary items in the Human Capital family and have been adjusted for multiple hypothesis testing accordingly. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.16: Impact of Guaranteed Income on Barriers to Employment

	Control Mean	Treatment Effect	N
Barriers to Employment Index		**-0.03**	**2941**
		(0.02)	
		[0.306]	
Barriers to Employment Component		-0.03	2941
		(0.02)	
		[0.238]	
Whether the respondent missed work due to lack of childcare in the last month	0.02 (0.13)	0.01	2941
		(0.01)	
		[0.704]	
Whether the respondent missed work due to illness in the last month	0.20 (0.34)	0.01	2940
		(0.01)	
		[0.704]	
Whether the respondent missed work due to lack of transportation in the last month	0.03 (0.13)	0.00	2940
		(0.00)	
		[0.704]	

This table shows the impacts of an unconditional cash transfer on barriers to employment. The top-level index decreases insignificantly by about 0.03 standard deviations, representing an insignificant increase in barriers. The q-values on the component and the top-level family index measures are different even as the point estimate is the same as they adjust for different sets of estimates in the FDR corrections (see Appendix E for details). There are several primary items under the component. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Except for the family- and component-level index values, estimates are provided in terms of raw units. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.17: Impact of Guaranteed Income on Employment Preferences and Job Search

	Control Mean	Treatment Effect	N
Employment Preferences and Job Search Index		**0.02** **(0.02)** **[0.330]**	**2987**
Active Search Component		0.03 (0.02) [0.704]	2987
Dummy for if participant searched for a job	0.60 (0.38)	0.06***††† (0.01) [0.001]	2943
Dummy for if the respondent is seeking a new, additional, or any job	0.39 (0.41)	0.03* (0.01) [0.238]	2939
Number of different actions taken to search for a job	1.69 (1.72)	0.09 (0.05) [0.238]	2942
Whether the participant applied for a job	0.49 (0.39)	0.04***†† (0.01) [0.012]	2942
Number of job applications sent	5.45 (11.83)	-0.84** (0.34) [0.110]	2942
Whether the participant interviewed for a job	0.36 (0.36)	0.01 (0.01) [0.607]	2942
Number of jobs interviewed for	0.73 (1.72)	-0.10* (0.05) [0.238]	2942
Preferences for Employment Component		0.01 (0.02) [0.704]	2940
How many work hours the respondent wants (less, same, more)	2.20 (0.53)	0.03 (0.02) [0.238]	2927
Whether a respondent is employed or, if unemployed, would prefer to be working	0.90 (0.26)	-0.01 (0.01) [0.288]	2939

This table shows the impacts of an unconditional cash transfer on employment preferences and job search. The top-level index increases insignificantly by about 0.02 standard deviations. There are two components in this family of outcomes: Active Search and Preferences for Employment, both presented in standard deviations in order to aggregate primary items beneath them. Several items that are italicized represent secondary outcomes for the sake of the FDR corrections. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Except for the family- and component-level index values, estimates are provided in terms of raw units. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.18: Impact of Guaranteed Income on Employment Preferences and Job Search: Actions Taken to Search for Work

	Control Mean	Treatment Effect	N
Whether participant looked at any job postings in the last 3 months	0.54 (0.39)	0.06***††† (0.01) [0.001]	2942
Whether participant directly contacted any employers for a job in the last 3 months	0.36 (0.38)	0.03** (0.01) [0.160]	2942
Whether participant contacted any job centers in the last 3 months	0.28 (0.35)	0.01 (0.01) [0.441]	2942
Whether participant contacted friends or relatives to find work in the last 3 months	0.36 (0.37)	0.03**† (0.01) [0.085]	2942
Whether participant contacted professional network to find work in the last 3 months	0.22 (0.32)	0.01 (0.01) [0.607]	2942
Whether participant posted a resume online in the last 3 months	0.38 (0.38)	0.02* (0.01) [0.211]	2942
Whether participant took other actions to find work in the last 3 months	0.03 (0.13)	0.01* (0.01) [0.211]	2942

This table provides exploratory analysis of self-reported actions participants took to search for work. As usual, unconditional estimates are presented for the sake of maintaining the causal interpretation of the estimate, so if someone is not searching for work they would be treated as having answered that they did not take that action. These questions were secondary items in the Employment Preferences and Job Search family and have been adjusted for multiple hypothesis testing accordingly. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.19: Impact of Guaranteed Income on Employment Preferences and Job Search: Additional Regressions

	Control Mean	Treatment Effect	N
Dummy for if the respondent is seeking a new, additional, or any job (alternate specification)	0.37 (0.40)	0.02 (0.01) [0.379]	2939
Number of job applications sent (alternate specification)	5.76 (12.92)	-0.41 (0.43) [0.474]	2980
Number of job applications sent, conditional on having applied for a job	11.47 (17.85)	-2.17***††† (0.61) [0.006]	2488
Number of jobs interviewed for, conditional on having interviewed for a job	1.58 (2.63)	-0.25***†† (0.09) [0.041]	2491
Whether the participant applied for a job that they were unqualified for	0.37 (0.42)	-0.01 (0.02) [0.555]	2064
Proportion of jobs the participant applied to that the participant was unqualified for	0.19 (0.29)	-0.01 (0.01) [0.402]	2064

This table provides exploratory analysis of the impact of the transfers on alternative measures of job search and/or the types of jobs that participants applied for. As usual, unconditional estimates are preferred for the sake of maintaining the causal interpretation of the estimate, so if someone did not apply for a job they would be treated as having not applied for any jobs for which they were unqualified. These questions were secondary or exploratory post-pre-analysis plan items in the Employment Preferences and Job Search family and have been adjusted for multiple hypothesis testing accordingly. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.20: Impact of Guaranteed Income on Selectivity of Job Search

	Control Mean	Treatment Effect	N
Selectivity of Job Search Index		**-0.01** **(0.02)** **[0.568]**	**2648**
Perceived likelihood of finding an acceptable job in 6 months (1-4 scale)	3.38 (0.81)	-0.14** (0.05) [1.000]	889
Participant's reservation wage, reported in minimum hourly remuneration	18.30 (8.73)	-0.29 (0.50) [1.000]	1068
Selectivity Component		-0.01 (0.02) [0.964]	2648
Natural log of average income of jobs which the respondent applied to	10.67 (0.34)	-0.00 (0.01) [1.000]	2071
Dummy for if the respondent is willing to take any job offered	0.16 (0.36)	0.01 (0.02) [1.000]	1050
Number of sacrifices participants would be willing to make to secure a job	2.18 (1.06)	0.05 (0.04) [1.000]	2496
If searching for a job, how long respondent is willing to search in months	7.15 (8.64)	0.11 (0.34) [1.000]	2476

This table shows the impacts of an unconditional cash transfer on selectivity of job search. The top-level index decreases insignificantly by about 0.01 standard deviations. There is one component with primary items in it (Selectivity) and two components pre-specified as containing only secondary items regarding participants' expectations and their reservation wage (which do not contribute to the index). Therefore, there is only one component with primary items, whose index value corresponds to the family-level index, though the family-level index is adjusted with a different set of results for multiple hypothesis testing per the description in Appendix E. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Except for the family- and component-level index values, estimates are provided in terms of raw units. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.21: Impact of Guaranteed Income on Selectivity of Job Search: Work Requirements

	Control Mean	Treatment Effect	N
Work requirement: chances for advancement	0.73 (0.43)	0.00 (0.03) [1.000]	967
Work requirement: comfortable workstation or physical environment	0.80 (0.38)	0.02 (0.02) [1.000]	967
Work requirement: flexible hours	0.75 (0.41)	0.04* (0.02) [1.000]	967
Work requirement: high income potential	0.78 (0.39)	-0.01 (0.02) [1.000]	966
Work requirement: interesting or meaningful work	0.70 (0.43)	0.06** (0.03) [1.000]	967
Work requirement: convenient location	0.81 (0.37)	-0.02 (0.02) [1.000]	966
Work requirement: secure, regular earnings	0.89 (0.29)	-0.01 (0.02) [1.000]	967
Work requirement: consistent, predictable schedule	0.81 (0.37)	-0.03 (0.02) [1.000]	967
Participant is not willing to work under any conditions	0.00 (0.04)	0.01 (0.00) [1.000]	1108
Work requirement: other	0.21 (0.38)	-0.01 (0.02) [1.000]	968

This table provides exploratory analysis of self-reported requirements participants stated that a job would have in order for them to be willing to take it. These questions were only asked of those seeking a job and were secondary items in the Selectivity of Job Search family and have been adjusted for multiple hypothesis testing accordingly. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.22: Impact of Guaranteed Income on Quality of Employment: Summary of Top-Level Components

	Control Mean	Treatment Effect	N
Quality of Employment Index		**-0.01**	2550
		(0.01)	
		[0.568]	
Adequacy of Employment Component		0.01	2409
		(0.03)	
		[1.000]	
Employment Quality Component		-0.01	2408
		(0.02)	
		[1.000]	
Single-item Component: Whether the respondent reports working any informal job	0.24 (0.37)	0.00	2404
		(0.01)	
		[1.000]	
Single-item Component: Average hourly income from all jobs, weighted by hours worked at each job	17.26 (9.72)	-0.13	2408
		(0.37)	
		[1.000]	
Stability of Employment Component		0.00	2409
		(0.02)	
		[1.000]	
Quality of Work Life Component		-0.02	2550
		(0.02)	
		[1.000]	

This table shows the impacts of an unconditional cash transfer on quality of employment. The top-level index decreases insignificantly by about 0.01 standard deviations. This table shows summary measures of each component in the family; two are single-primary-item components and are reported in raw units, while the others are reported in terms of standard deviations as they aggregate a number of primary items. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.23: Impact of Guaranteed Income on Quality of Employment: Item-Level Analyses

	Control Mean	Treatment Effect	N
Adequacy of Employment			
The respondent is employed part-time in their main job and would prefer to work full-time	0.24 (0.39)	0.00 (0.02) [1.000]	2336
The respondent would prefer to work more hours in their current main job	0.21 (0.36)	0.01 (0.02) [1.000]	2409
The number of jobs held by the respondent apart from their main job	0.38 (0.70)	-0.03 (0.03) [1.000]	2407
Employment Quality			
Whether training is offered by the respondent's main employer	0.53 (0.45)	0.00 (0.02) [1.000]	2399
Whether training is offered during work hours by the respondent's main employer	0.49 (0.45)	0.01 (0.02) [1.000]	2398
Whether formal training is offered by the respondent's main employer	0.13 (0.29)	-0.00 (0.01) [1.000]	2397
Number of non-wage benefits at respondent's job(s), weighted by hours worked at each job	3.61 (2.90)	-0.11 (0.11) [1.000]	2408
Whether the respondent must work an irregular shift at each job, weighted by hours worked at each job	0.19 (0.34)	0.01 (0.01) [1.000]	2405
Number of non-wage benefits at respondent's job(s), alternate specification	3.96 (2.98)	-0.17 (0.11) [1.000]	2406
Informality of Employment			
Whether the respondent reports any gig economy jobs such as Uber, TaskRabbit, or online surveys	0.09 (0.25)	-0.00 (0.01) [1.000]	2403
Stability of Employment			
How many months the respondent has been employed in the past year	10.70 (2.64)	-0.03 (0.10) [1.000]	2409
How long the respondent has spent at their current main job and other jobs (months), weighted by hours worked at each job	24.88 (34.85)	1.43 (1.15) [1.000]	2403
How many jobs the respondent has held in the past 12 months	1.77 (1.59)	-0.08** (0.04) [1.000]	2403
How many jobs the respondent has held in the past two years	2.33 (3.66)	-0.15* (0.08) [1.000]	2402
Whether the respondent's main job is a temp job	0.10 (0.26)	0.01 (0.01) [1.000]	2404
Whether each of the respondent's jobs is salaried, weighted by hours worked at each job	0.23 (0.39)	-0.00 (0.01) [1.000]	2403
Whether the respondent is performing contract or freelance work at each job, weighted by hours worked at each job	0.25 (0.38)	0.01 (0.01) [1.000]	2402
How many months the respondent expects to remain in their main job (conditional on temp work)	8.97 (6.56)	-0.94 (0.69) [1.000]	341

Quality of Work Life			
Advance notice of schedule provided at the respondent's main job (1-4 scale)	2.52 (1.24)	-0.04 (0.05) [1.000]	2361
The work activities are not boring at the respondent's main job (1-5 scale)	3.11 (1.05)	-0.01 (0.04) [1.000]	2252
Satisfaction with compensation at the respondent's main job (1-5 scale)	3.51 (1.06)	-0.02 (0.04) [1.000]	2405
Whether the respondent faces age discrimination at work	0.06 (0.21)	-0.00 (0.01) [1.000]	2252
Whether the respondent faces sex discrimination at work	0.08 (0.25)	0.00 (0.01) [1.000]	2251
Whether the respondent faces racial or ethnic discrimination at work	0.08 (0.25)	0.00 (0.01) [1.000]	2251
Whether the respondent experienced fair treatment by their supervisor (1-5 scale)	4.05 (0.91)	0.03 (0.04) [1.000]	2255
Whether job demands do not interfere with family life (1-4 scale)	2.91 (0.87)	0.01 (0.03) [1.000]	2405
Whether the job is a good fit with the respondent's experience and skills (1-5 scale)	4.19 (0.92)	-0.04 (0.04) [1.000]	2403
Flexibility of schedule at the respondent's main job (1-4 scale)	1.91 (0.91)	0.01 (0.04) [1.000]	2347
Overall satisfaction with the respondent's main job (1-5 scale)	3.96 (0.96)	0.03 (0.04) [1.000]	2404
Whether the respondent has decision-making input in their job (1-4 scale)	2.67 (0.98)	-0.04 (0.04) [1.000]	2404
Satisfaction with non-wage aspects of respondent's main job (1-5 scale)	3.69 (1.12)	0.03 (0.04) [1.000]	2402
Whether the respondent does not plan to leave their job in the next year (1-3 scale)	2.27 (0.72)	-0.04 (0.03) [1.000]	2403
Opportunities for promotion at the respondent's main job (1-5 scale)	3.41 (1.27)	-0.10* (0.05) [1.000]	2398
Safety and health conditions at the respondent's main job (1-5 scale)	4.22 (0.79)	-0.00 (0.03) [1.000]	2256
Whether a scheduled shift was canceled with less than 24 hours notice in the last month	0.09 (0.26)	0.02** (0.01) [1.000]	2485
Number of stressors in their work environment at respondent's main job	1.25 (1.24)	0.09* (0.05) [1.000]	2246
How hard is it to take time off from the respondent's main job? (1-4 scale)	3.18 (0.87)	-0.05 (0.04) [1.000]	2287

This table shows the impacts of an unconditional cash transfer on items within quality of employment. Under various component headers, the table presents results for primary and secondary items in raw units. Items that are italicized are secondary outcomes in the FDR corrections. Standard errors are provided in parentheses, and FDR-adjusted q-values in square brackets below it. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.24: Impact of Guaranteed Income on Quality of Employment: Specific Benefits

	Control Mean	Treatment Effect	N
Receives health insurance (100% of premium covered by employer)	0.17 (0.33)	0.00 (0.01) [1.000]	2389
Receives health insurance (Less than 100% of premium covered by employer)	0.33 (0.43)	-0.02 (0.02) [1.000]	2385
Receives dental and/or vision insurance	0.48 (0.46)	-0.02 (0.02) [1.000]	2391
Receives traditional pension plan (defined benefit plan)	0.26 (0.40)	-0.01 (0.02) [1.000]	2385
Receives retirement account without employer contribution	0.22 (0.36)	-0.02 (0.01) [1.000]	2386
Receives employer contribution to a retirement account	0.28 (0.41)	0.01 (0.02) [1.000]	2386
Receives health care or dependent care Flexible Spending Account	0.29 (0.42)	-0.02 (0.02) [1.000]	2390
Receives housing or housing subsidy	0.02 (0.14)	-0.01* (0.01) [1.000]	2391
Receives life or disability insurance	0.42 (0.46)	-0.01 (0.02) [1.000]	2391
Receives commuter benefits	0.10 (0.26)	-0.02 (0.01) [1.000]	2393
Receives childcare assistance	0.07 (0.22)	-0.01 (0.01) [1.000]	2392
Receives paid vacation	0.56 (0.46)	-0.01 (0.02) [1.000]	2393
Receives tuition reimbursement	0.26 (0.40)	-0.03* (0.02) [1.000]	2382
Can work from home	0.39 (0.46)	-0.03* (0.02) [1.000]	2402
Receives other non-wage benefit	0.12 (0.29)	-0.00 (0.01) [1.000]	2399

This table provides exploratory analysis of self-reported benefits participants reported receiving as part of their jobs. These questions were secondary items in the Quality of Employment family and have been adjusted for multiple hypothesis testing accordingly. These questions were only asked of people who were employed. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.25: Impact of Guaranteed Income on Labor Market Mobility

	Control Mean	Treatment Effect	N
Labor Market Mobility Index		**0.09***†††**	**2993**
		(0.03)	
		[0.002]	
Single-item Component: Moved labor markets since baseline	0.12 (0.29)	0.02††	2993
		(0.01)	
		[0.036]	
Search New Labor Market Component		0.11***†††	2851
		(0.03)	
		[0.002]	
Any active area-search behaviors	0.10 (0.22)	0.03***†††	2851
		(0.01)	
		[0.004]	
Interested in moving areas	0.23 (0.36)	0.04***†††	2851
		(0.01)	
		[0.004]	
Number of active labor market-search behaviors	0.27 (0.67)	0.08***†††	2851
		(0.03)	
		[0.004]	

This table shows the impacts of an unconditional cash transfer on labor market mobility. The top-level index for labor market mobility increases by about 0.09 standard deviations. A single primary item component and a component with several primary items are listed. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Except for the family-level index value, estimates are provided in terms of raw units. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.26: Impact of Guaranteed Income on Quality of Labor Market

	Control Mean	Treatment Effect	N
Labor Market Quality Index		**0.00**	**3000**
		(0.00)	
		[0.358]	
Labor Market Quality Component		0.01	2995
		(0.00)	
		[1.000]	
Mean wage by education in 2022 (in dollars per hour)	29.70 (12.40)	-0.02	2988
		(0.05)	
		[1.000]	
Employment to population ratio for ages 25 to 64 for respondent's education group	0.76 (0.07)	0.00	2995
		(0.00)	
		[1.000]	
BLS projected job-growth for respondent's education group (percent change)	12.04 (9.02)	0.19*	2961
		(0.11)	
		[1.000]	
Median annual income for respondent's education group (in dollars)	41689.82 (13472.32)	118.47	2995
		(102.00)	
		[1.000]	
Recent population growth for respondent's education group (percent change)	5.23 (11.36)	-0.00	2995
		(0.11)	
		[1.000]	
Mean wage growth 2019-2022 by education (percent change)	12.21 (4.16)	-0.05	2988
		(0.05)	
		[1.000]	
Labor Market Amenities Component		0.00	2993
		(0.01)	
		[1.000]	
Mean percentile household income rank for children whose parents were in the 25th percentile of income	0.40 (0.01)	0.00	2993
		(0.00)	
		[1.000]	
Natural amenities index (ranges from -5 to 9)	-0.38 (1.51)	0.03	2992
		(0.03)	
		[1.000]	
Pollution index (mean PM2.5, RSEI, and AQI z-score)	0.99 (0.33)	0.00	2993
		(0.01)	
		[1.000]	
Consumption amenities index (PCA log scale, ranges from -2 to 2)	0.25 (0.27)	0.00	2993
		(0.00)	
		[1.000]	
Annual violent crime rate (crimes per 100,000 residents)	402.32 (109.49)	-0.90	2967
		(2.07)	
		[1.000]	
Annual property crime rate (crimes per 100,000 residents)	2377.75 (354.83)	-3.13	2967
		(6.12)	
		[1.000]	
Annual per-pupil school spending (in dollars)	13702.10 (3431.41)	-27.10	2993
		(42.16)	
		[1.000]	

This table shows the impacts of an unconditional cash transfer on quality of labor market. The top-level index changes insignificantly by less than 0.01 standard deviations. Two components (Labor Quality and Labor Market Amenities) are both null. All the primary items under them are also null, except for BLS projected job-growth for respondent's education group being significant at $p < 0.1$, though this does not survive the FDR adjustment. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Except for the family- and component-level index values, estimates are provided in terms of raw units. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.27: Impact of Guaranteed Income on Subjective Well-Being

	Control Mean	Treatment Effect	N
Subjective Wellbeing Index		**-0.00**	2989
		(0.02)	
		[1.000]	
Domain Satisfaction Component		0.01	2921
		(0.02)	
		[1.000]	
Single-item Component: Level of satisfaction with life as a whole currently (0-10 scale)	6.89 (1.78)	-0.04	2980
		(0.05)	
		[1.000]	
Single-item Component: Affect balance	5.53 (8.04)	0.01	2913
		(0.21)	
		[1.000]	

This table shows the impacts of an unconditional cash transfer on subjective well-being. The top-level index decreases insignificantly by less than 0.01 standard deviations. There are three components, two of which are single-item components. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Single-item components are presented in terms of raw units, while the other family- and component-level index values are in standard deviations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.28: Impact of Guaranteed Income on Benefits

	Control Mean	Treatment Effect	N
Benefits Index		**-0.01**	**2904**
		(0.02)	
		[0.523]	
Take-Up Benefits Component		-0.01	2904
		(0.02)	
		[1.000]	
Total amount of government benefits received during the previous year	5599.19 (6955.63)	-237.68	2903
		(188.00)	
		[1.000]	
Number of government benefits programs received during the previous year	1.85 (1.57)	0.03	2904
		(0.04)	
		[1.000]	
Number of government benefits programs received during the previous year (excluding educational assistance)	1.74 (1.51)	0.03	2903
		(0.04)	
		[1.000]	

This table shows the impacts of an unconditional cash transfer on monetary and non-monetary benefits. The top-level index decreases insignificantly by about 0.01 standard deviations. There is a single component, with two primary items under it. The q-values on the component and the top-level family index measures are different even as the point estimate is the same as they adjust for different sets of estimates in the FDR corrections (see Appendix E for details). Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Except for the family- and component-level index values, estimates are provided in terms of raw units. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.29: Impact of Guaranteed Income on Relationship Status

	Control Mean	Treatment Effect	N
Relationship Status Index		**-0.01** **(0.02)** **[1.000]**	**2989**
Relationship Stability Component		-0.04 (0.02) [1.000]	2950
How long the respondent has been in their relationship	2.50 (2.48)	0.01 (0.06) [0.988]	2900
Number of times the respondent said they started or ended a relationship in the last year	0.43 (0.72)	0.05** (0.03) [0.924]	2903
Relationship Status Component		0.01 (0.02) [1.000]	2989
Whether the respondent is divorced	0.10 (0.29)	-0.01 (0.01) [0.924]	2943
Whether the respondent has a spouse	0.28 (0.44)	-0.01 (0.01) [0.924]	2945
Whether the respondent is in a romantic relationship	0.58 (0.44)	0.01 (0.01) [0.924]	2989

This table shows the impacts of an unconditional cash transfer on relationship status. The top-level index decreases insignificantly by about 0.01 standard deviations. Both the Relationship Stability and Relationship Status component are null. There are a number of primary items under each component. One item under Relationship Stability is significant at $p < 0.05$ before adjusting for FDR: this item looks at relationships a participant might have, regardless of whether that individual lives in the household. The variable capturing whether or not someone has a spouse includes a few cases in which someone has a spouse who recently left the household. An alternative measure which excludes these cases yields comparable results. Standard errors are provided in parentheses, and the FDR-adjusted q-value in square brackets below it. Except for the family- and component-level index values, estimates are provided in terms of raw units. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.30: Impact of Guaranteed Income on Relationship Status: Reasons for Relationships Ending

	Control Mean	Treatment Effect	N
Whether relationship ended because of abuse	0.031 (0.128)	0.002 (0.005) [1.000]	2903
Whether relationship ended because of distance	0.028 (0.120)	0.009* (0.005) [0.858]	2903
Whether relationship ended because of drugs	0.027 (0.120)	0.003 (0.005) [1.000]	2903
Whether relationship ended because of family	0.019 (0.096)	-0.000 (0.003) [1.000]	2903
Whether relationship ended because of financial issues	0.033 (0.127)	0.006 (0.005) [1.000]	2903
Whether relationship ended because of illness	0.006 (0.057)	0.000 (0.002) [1.000]	2903
Whether relationship ended because of relationship issues	0.123 (0.241)	0.021** (0.010) [0.413]	2903
Whether relationship ended because of religion	0.005 (0.049)	0.004* (0.002) [0.858]	2903
Whether relationship ended because of other reasons	0.019 (0.094)	0.003 (0.004) [1.000]	2903
Participant's relationship was ended by participant	0.078 (0.200)	0.023*** (0.008) [0.171]	2903
Participant's relationship was ended by partner	0.037 (0.137)	0.005 (0.005) [1.000]	2903
Participant's relationship ended mutually	0.045 (0.146)	0.003 (0.006) [1.000]	2903

This table provides exploratory analysis of reasons why relationships ended. These questions were secondary items in the Relationship Status family and have been adjusted for multiple hypothesis testing accordingly. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.31: Impact of Guaranteed Income on Annual Earned and Other Unearned Income: Comparison of Impacts by Income at Baseline

	Control Mean	Entire Sample	Below 100% FPL	Above 100% FPL
Total household income	48.2 (33.9)	-4.3***†††	-2.9*	-4.2***††
		(0.9)	(1.5)	(1.2)
		[0.001]	[0.277]	[0.011]
Total individual income	33.5 (25.2)	-2.4***†††	-3.4***††	-2.1**†
		(0.6)	(0.9)	(0.9)
		[0.009]	[0.013]	[0.086]
Total calculated individual income	36.6 (27.0)	-1.4*	0.7	-2.4**†
		(0.9)	(1.5)	(0.9)
		[0.127]	[1.000]	[0.096]
Individual salaried/wage income	26.0 (26.1)	-1.3	0.1	-1.3
		(0.9)	(1.2)	(0.9)
		[0.360]	[1.000]	[0.514]
Self-employment income	5.9 (13.8)	-0.1	0.6	-0.7
		(0.6)	(0.9)	(0.6)
		[0.642]	[0.977]	[0.800]
Income from supplementary gig work	0.4 (1.2)	-0.1	0.0	-0.1*
		(0.0)	(0.0)	(0.0)
		[0.351]	[1.000]	[0.277]
Passive income	0.0 (0.3)	0.0	0.0	0.0
		(0.0)	(0.0)	(0.0)
		[0.351]	[1.000]	[0.659]
Other income	4.7 (6.0)	-0.1	-0.2	0.0
		(0.3)	(0.3)	(0.3)
		[0.635]	[1.000]	[1.000]
Government transfers	3.6 (4.8)	-0.1	-0.2	-0.1
		(0.0)	(0.3)	(0.3)
		[0.800]	[1.000]	[1.000]

This table compares results for income for participants by whether they were above or below 100% of the FPL at baseline. Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.32: Impact of Guaranteed Income on Annual Earned and Other Unearned Income: Comparison of Impacts for Participants by Baseline Level of Education

	Control Mean	Entire Sample	No Bachelor's Degree	Bachelor's Degree
Total household income	48.2 (33.9)	-4.3***†††	-5.2***†††	-2.6
		(0.9)	(0.9)	(2.4)
		[0.001]	[0.001]	[0.659]
Total individual income	33.5 (25.2)	-2.4***†††	-3.5***†††	-0.4
		(0.6)	(0.9)	(1.5)
		[0.009]	[0.001]	[1.000]
Total calculated individual income	36.6 (27.0)	-1.4*	-2.3**†	1.6
		(0.9)	(0.9)	(1.8)
		[0.127]	[0.086]	[0.865]
Individual salaried/wage income	26.0 (26.1)	-1.3	-1.9**†	1.4
		(0.9)	(0.9)	(1.8)
		[0.360]	[0.091]	[0.961]
Self-employment income	5.9 (13.8)	-0.1	0.3	-1.4
		(0.6)	(0.6)	(0.9)
		[0.642]	[1.000]	[0.514]
Income from supplementary gig work	0.4 (1.2)	-0.1	0.0	-0.2*
		(0.0)	(0.0)	(0.0)
		[0.351]	[1.000]	[0.305]
Passive income	0.0 (0.3)	0.0	0.0	0.0
		(0.0)	(0.0)	(0.0)
		[0.351]	[1.000]	[0.687]
Other income	4.7 (6.0)	-0.1	-0.1	0.1
		(0.3)	(0.3)	(0.3)
		[0.635]	[1.000]	[1.000]
Government transfers	3.6 (4.8)	-0.1	-0.3	0.1
		(0.0)	(0.3)	(0.3)
		[0.800]	[0.366]	[1.000]

This table compares results for income for participants by whether or not they had a bachelor's degree at baseline. Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.33: Impact of Guaranteed Income on Annual Earned and Other Unearned Income: Comparison of Impacts by Sex at Baseline

	Control Mean	Entire Sample	Male	Female/Other
Total household income	48.2 (33.9)	-4.3***†††	-4.3**†	-3.8***††
		(0.9)	(1.8)	(1.2)
		[0.001]	[0.086]	[0.012]
Total individual income	33.5 (25.2)	-2.4***†††	-3.1**†	-1.9**†
		(0.6)	(1.2)	(0.9)
		[0.009]	[0.086]	[0.086]
Total calculated individual income	36.6 (27.0)	-1.4*	-1.1	-1.4
		(0.9)	(1.5)	(0.9)
		[0.127]	[0.977]	[0.505]
Individual salaried/wage income	26.0 (26.1)	-1.3	-1.2	-1.5*
		(0.9)	(1.5)	(0.9)
		[0.360]	[0.961]	[0.358]
Self-employment income	5.9 (13.8)	-0.1	-0.4	0.5
		(0.6)	(0.9)	(0.6)
		[0.642]	[1.000]	[0.961]
Income from supplementary gig work	0.4 (1.2)	-0.1	-0.1	-0.1
		(0.0)	(0.0)	(0.0)
		[0.351]	[0.971]	[0.623]
Passive income	0.0 (0.3)	0.0	0.0	0.0**
		(0.0)	(0.0)	(0.0)
		[0.351]	[1.000]	[0.163]
Other income	4.7 (6.0)	-0.1	-0.2	0.1
		(0.3)	(0.3)	(0.3)
		[0.635]	[0.853]	[1.000]
Government transfers	3.6 (4.8)	-0.1	0.0	-0.1
		(0.0)	(0.3)	(0.3)
		[0.800]	[1.000]	[1.000]

This table compares results for income for participants by sex at baseline. Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.34: Impact of Guaranteed Income on Annual Earned and Other Unearned Income: Comparison of Impacts by State

	Control Mean	Entire Sample	Illinois	Texas
Total household income	48.2 (33.9)	-4.3***†††	-2.8**	-5.0***†††
		(0.9)	(1.2)	(1.5)
		[0.001]	[0.164]	[0.006]
Total individual income	33.5 (25.2)	-2.4***†††	-2.3**†	-2.7***†
		(0.6)	(0.9)	(0.9)
		[0.009]	[0.082]	[0.055]
Total calculated individual income	36.6 (27.0)	-1.4*	-0.7	-1.7
		(0.9)	(1.2)	(1.2)
		[0.127]	[0.960]	[0.390]
Individual salaried/wage income	26.0 (26.1)	-1.3	-1.0	-1.7
		(0.9)	(1.2)	(1.2)
		[0.360]	[0.790]	[0.382]
Self-employment income	5.9 (13.8)	-0.1	0.5	-0.3
		(0.6)	(0.9)	(0.6)
		[0.642]	[0.930]	[1.000]
Income from supplementary gig work	0.4 (1.2)	-0.1	-0.2***††	0.1
		(0.0)	(0.0)	(0.0)
		[0.351]	[0.028]	[0.930]
Passive income	0.0 (0.3)	0.0	0.0	0.0***†
		(0.0)	(0.0)	(0.0)
		[0.351]	[0.537]	[0.050]
Other income	4.7 (6.0)	-0.1	0.0	-0.1
		(0.3)	(0.3)	(0.3)
		[0.635]	[1.000]	[0.960]
Government transfers	3.6 (4.8)	-0.1	-0.1	-0.2
		(0.0)	(0.3)	(0.3)
		[0.800]	[0.960]	[0.701]

This table compares results for income for participants by whether they lived in Illinois or Texas at baseline. Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.35: Impact of Guaranteed Income on Annual Earned and Other Unearned Income: Comparison of Impacts for Participants with and without Children at Baseline

	Control Mean	Entire Sample	No Children in Household	Children in Household
Total household income	48.2 (33.9)	-4.3***†††	-6.3***†††	-3.0**†
		(0.9)	(1.5)	(1.2)
		[0.001]	[0.001]	[0.082]
Total individual income	33.5 (25.2)	-2.4***†††	-2.8***†	-2.2**†
		(0.6)	(1.2)	(0.9)
		[0.009]	[0.082]	[0.082]
Total calculated individual income	36.6 (27.0)	-1.4*	-2.3*	-1.7
		(0.9)	(1.2)	(1.2)
		[0.127]	[0.279]	[0.381]
Individual salaried/wage income	26.0 (26.1)	-1.3	-2.2*	-0.9
		(0.9)	(1.2)	(0.9)
		[0.360]	[0.279]	[0.790]
Self-employment income	5.9 (13.8)	-0.1	-0.8	0.4
		(0.6)	(0.9)	(0.6)
		[0.642]	[0.736]	[0.930]
Income from supplementary gig work	0.4 (1.2)	-0.1	0.0	-0.1
		(0.0)	(0.0)	(0.0)
		[0.351]	[1.000]	[0.510]
Passive income	0.0 (0.3)	0.0	0.0**†	0.0
		(0.0)	(0.0)	(0.0)
		[0.351]	[0.099]	[0.959]
Other income	4.7 (6.0)	-0.1	-0.1	0.0
		(0.3)	(0.3)	(0.3)
		[0.635]	[1.000]	[1.000]
Government transfers	3.6 (4.8)	-0.1	-0.1	0.0
		(0.0)	(0.3)	(0.3)
		[0.800]	[0.960]	[1.000]

This table compares results for income for participants by whether or not they had children in the household at baseline. Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.36: Impact of Guaranteed Income on Labor Supply: Comparison of Impacts by Baseline Level of Education

	Control Mean	Entire Sample	No Bachelor's Degree	Bachelor's Degree
Hours worked per week	30.28 (19.83)	-1.29**††	-2.12***	-0.19
		(0.63)	(0.75)	(1.12)
		[0.042]	[0.127]	[1.000]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.04***	0.01
		(0.01)	(0.01)	(0.02)
		[0.400]	[0.127]	[0.885]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.48***	-3.10***	0.08
		(0.78)	(0.90)	(1.48)
		[0.175]	[0.175]	[1.000]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.39***	-2.13**	-1.61
		(0.92)	(1.07)	(1.72)
		[0.302]	[0.517]	[1.000]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.02	0.12	0.33
		(0.37)	(0.43)	(0.58)
		[1.000]	[1.000]	[1.000]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.20	0.24	0.14
		(0.23)	(0.29)	(0.25)
		[1.000]	[1.000]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.01	0.00	-0.04
		(0.02)	(0.02)	(0.04)
		[1.000]	[1.000]	[1.000]

This table compares results for labor supply for participants by whether or not they had a bachelor's degree at baseline. Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.37: Impact of Guaranteed Income on Labor Supply: Comparison of Impacts by Age at Baseline

	Control Mean	Entire Sample	Under 30	30+
Hours worked per week	30.28 (19.83)	-1.29**††	-2.15**	-1.07
		(0.63)	(0.87)	(0.89)
		[0.042]	[0.243]	[0.732]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.05***	0.01
		(0.01)	(0.02)	(0.02)
		[0.400]	[0.127]	[1.000]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.48***	-3.34***	-0.96
		(0.78)	(1.05)	(1.13)
		[0.175]	[0.175]	[1.000]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.39***	-3.51***	-1.30
		(0.92)	(1.28)	(1.31)
		[0.302]	[0.232]	[1.000]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.02	0.52	-0.47
		(0.37)	(0.58)	(0.38)
		[1.000]	[1.000]	[0.934]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.20	0.19	0.12
		(0.23)	(0.12)	(0.47)
		[1.000]	[0.756]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.01	-0.02	0.00
		(0.02)	(0.03)	(0.03)
		[1.000]	[1.000]	[1.000]

This table compares results for labor supply for participants by age at baseline. Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.38: Impact of Guaranteed Income on Labor Supply: Comparison of Impacts by Sex at Baseline

	Control Mean	Entire Sample	Male	Female/Other
Hours worked per week	30.28 (19.83)	-1.29**††	-1.33	-1.38*
		(0.63)	(1.10)	(0.78)
		[0.042]	[0.732]	[0.475]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.00	-0.03*
		(0.01)	(0.02)	(0.01)
		[0.400]	[1.000]	[0.400]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.48***	-1.32	-2.23**
		(0.78)	(1.30)	(0.97)
		[0.175]	[1.000]	[0.379]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.39***	-2.64*	-1.91*
		(0.92)	(1.54)	(1.13)
		[0.302]	[0.707]	[0.707]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.02	-0.21	0.31
		(0.37)	(0.69)	(0.42)
		[1.000]	[1.000]	[1.000]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.20	-0.35*	0.36
		(0.23)	(0.20)	(0.32)
		[1.000]	[0.707]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.01	-0.00	0.00
		(0.02)	(0.03)	(0.02)
		[1.000]	[1.000]	[1.000]

This table compares results for labor supply for participants by sex at baseline. Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.39: Impact of Guaranteed Income on Labor Supply: Comparison of Impacts by State

	Control Mean	Entire Sample	Illinois	Texas
Hours worked per week	30.28 (19.83)	-1.29**††	-1.13	-1.56*
		(0.63)	(0.83)	(0.92)
		[0.042]	[0.850]	[0.707]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.01	-0.04**
		(0.01)	(0.02)	(0.02)
		[0.400]	[1.000]	[0.380]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.48***	-2.00**	-2.53**
		(0.78)	(1.02)	(1.11)
		[0.175]	[0.531]	[0.380]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.39***	-1.94	-3.23**
		(0.92)	(1.26)	(1.33)
		[0.302]	[0.756]	[0.344]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.02	0.18	-0.18
		(0.37)	(0.51)	(0.54)
		[1.000]	[1.000]	[1.000]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.20	0.41	0.07
		(0.23)	(0.27)	(0.36)
		[1.000]	[0.756]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.01	-0.00	-0.03
		(0.02)	(0.03)	(0.03)
		[1.000]	[1.000]	[1.000]

This table compares results for labor supply for participants by whether they lived in Illinois or Texas at baseline. Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.40: Impact of Guaranteed Income on Labor Supply: Comparison of Impacts by Fresh EBT Recruitment

	Control Mean	Entire Sample	Not Fresh EBT	Recruited by Fresh EBT
Hours worked per week	30.28 (19.83)	-1.29**††	-0.82	0.11
		(0.63)	(0.66)	(1.79)
		[0.042]	[0.818]	[1.000]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.03**	0.03
		(0.01)	(0.01)	(0.04)
		[0.400]	[0.434]	[0.974]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.48***	-2.00**	-1.82
		(0.78)	(0.82)	(2.05)
		[0.175]	[0.344]	[1.000]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.39***	-2.41**	-0.80
		(0.92)	(0.98)	(2.16)
		[0.302]	[0.344]	[1.000]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.02	0.01	0.68
		(0.37)	(0.41)	(0.79)
		[1.000]	[1.000]	[1.000]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.20	0.12	0.94
		(0.23)	(0.23)	(0.81)
		[1.000]	[1.000]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.01	-0.02	0.03
		(0.02)	(0.02)	(0.04)
		[1.000]	[1.000]	[1.000]

This table compares results for labor supply for participants by whether they were recruited over an app to check EBT balances (Fresh EBT). Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.41: Impact of Guaranteed Income on Labor Supply: Comparison of Impacts by Number of Mailers Sent

	Control Mean	Entire Sample	Received <3 mailers	Received >= 3 mailers
Hours worked per week	30.28 (19.83)	-1.29**††	-1.77**	1.16
		(0.63)	(0.74)	(1.61)
		[0.042]	[0.390]	[0.984]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.03**	0.01
		(0.01)	(0.01)	(0.03)
		[0.400]	[0.403]	[1.000]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.48***	-2.34**	0.30
		(0.78)	(0.92)	(1.80)
		[0.175]	[0.344]	[1.000]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.39***	-1.92*	-0.33
		(0.92)	(1.14)	(2.21)
		[0.302]	[0.707]	[1.000]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.02	0.20	-0.37
		(0.37)	(0.48)	(0.78)
		[1.000]	[1.000]	[1.000]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.20	0.37	-0.43
		(0.23)	(0.27)	(0.43)
		[1.000]	[0.850]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.01	-0.00	-0.04
		(0.02)	(0.02)	(0.04)
		[1.000]	[1.000]	[1.000]

This table compares results for labor supply for participants by whether they received three or more mailers. Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.42: Impact of Guaranteed Income on Human Capital Formation: Comparison of Impacts by Age

	Control Mean	Entire Sample	Under 30	30+
Human Capital Index	0.22 (0.32)	**0.01** **(0.01)** **[0.321]**	**0.02** **(0.02)** **[1.000]**	**0.01** **(0.01)** **[1.000]**
Formal Education Component		0.02 (0.02) [0.791]	0.04 (0.03) [1.000]	-0.06** (0.03) [1.000]
Completed a GED or post-secondary degree	0.94 (0.23)	0.00 (0.00) [1.000]	-0.00 (0.00) [1.000]	0.00 (0.00) [1.000]
Completed a post-secondary degree (NSC only)	0.35 (0.47)	-0.01 (0.01) [1.000]	-0.00 (0.02) [1.000]	-0.03* (0.02) [0.747]
Total years of post-secondary education completed post-baseline (NSC only)	0.13 (0.33)	0.01 (0.01) [1.000]	0.03 (0.02) [1.000]	-0.03** (0.01) [1.000]
Enrolled in a post-secondary program (NSC only)	0.15 (0.30)	0.01 (0.01) [1.000]	0.02 (0.02) [1.000]	-0.02* (0.01) [1.000]
Enrolled in post-secondary program	0.15 (0.29)	0.01 (0.01) [1.000]	0.02 (0.02) [1.000]	-0.02* (0.01) [1.000]
Average hours of school per week (full-time, part-time, withdrawn, etc.) in post-secondary program	1.96 (5.28)	0.23 (0.21) [1.000]	0.58* (0.33) [1.000]	-0.47** (0.23) [1.000]
Participation in informal education	0.10 (0.21)	0.01 (0.01) [1.000]	0.01 (0.01) [1.000]	0.00 (0.01) [1.000]
Extent of participation in informal education (full-time, part-time, not enrolled)	0.07 (0.18)	0.00 (0.01) [1.000]	-0.00 (0.01) [1.000]	-0.00 (0.01) [1.000]
Whether the participant plans to receive job training	0.03 (0.14)	0.01** (0.01) [1.000]	0.01* (0.01) [1.000]	0.02** (0.01) [1.000]

This table compares results for income for participants by age at baseline. For each pre-specified outcome, NSC data is preferred if it exists for that outcome. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.43: Impact of Guaranteed Income on Entrepreneurship: Comparison of Impacts by Baseline Level of Education

	Control Mean	Entire Sample	No Bachelor's Degree	Bachelor's Degree
Entrepreneurship Index		**0.05***††**	**0.05***††**	**0.04**
		(0.02)	**(0.02)**	**(0.03)**
		[0.014]	**[0.046]**	**[0.171]**
Entrepreneurial Orientation Component		0.07***†††	0.08***††	0.08*
		(0.02)	(0.03)	(0.05)
		[0.009]	[0.046]	[0.121]
The respondent's self-reported willingness to take financial risks (1-10 scale)	4.52 (2.09)	0.09	0.10	0.14
		(0.06)	(0.08)	(0.11)
		[0.104]	[0.174]	[0.174]
Midpoint of the constant relative risk aversion (CRRA) range implied by a participant's coin flip gamble	1.82 (1.55)	-0.16***††	-0.17**††	-0.14
		(0.06)	(0.07)	(0.12)
		[0.021]	[0.048]	[0.200]
Entrepreneurial Intention Component		0.06**††	0.08***††	-0.02
		(0.02)	(0.03)	(0.04)
		[0.013]	[0.046]	[0.354]
Whether or not the respondent has an idea for a business	0.58 (0.42)	0.04***††	0.04**††	0.01
		(0.01)	(0.02)	(0.03)
		[0.021]	[0.048]	[0.367]
The respondent's likelihood rating that they will start a business in the next 5 years (1-10 scale)	4.95 (3.05)	0.15*†	0.25***††	-0.05
		(0.08)	(0.10)	(0.15)
		[0.055]	[0.048]	[0.367]
The respondent's interest in starting a business (1-10 scale)	6.21 (2.96)	0.10	0.15	-0.20
		(0.09)	(0.10)	(0.14)
		[0.116]	[0.152]	[0.166]
Entrepreneurial Activity Component		0.01	0.00	0.05
		(0.02)	(0.02)	(0.04)
		[0.202]	[0.389]	[0.174]
If a family member who started a business lives in the respondent's household	0.06 (0.21)	-0.01**††	-0.01**†	0.00
		(0.01)	(0.01)	(0.01)
		[0.036]	[0.074]	[0.374]
If the respondent knows someone who started or helped start a business	0.60 (0.41)	0.03***††	0.04**†	0.05**†
		(0.01)	(0.02)	(0.02)
		[0.021]	[0.060]	[0.081]
If the respondent ever started or helped start a business	0.30 (0.40)	0.00	-0.00	0.00
		(0.01)	(0.01)	(0.02)
		[0.293]	[0.384]	[0.402]

This table compares results for entrepreneurship for participants by whether or not they had a bachelor's degree at baseline. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.44: Impact of Guaranteed Income on Entrepreneurship: Comparison of Impacts by Age at Baseline

	Control Mean	Entire Sample	Under 30	30+
Entrepreneurship Index		**0.05***††**	**0.04***	**0.06***††**
		(0.02)	**(0.02)**	**(0.02)**
		[0.014]	**[0.112]**	**[0.046]**
Entrepreneurial Orientation Component		0.07***†††	0.08**†	0.07**†
		(0.02)	(0.03)	(0.04)
		[0.009]	[0.054]	[0.085]
The respondent's self-reported willingness to take financial risks (1-10 scale)	4.52 (2.09)	0.09	0.10	0.12
		(0.06)	(0.08)	(0.09)
		[0.104]	[0.200]	[0.178]
Midpoint of the constant relative risk aversion (CRRA) range implied by a participant's coin flip gamble	1.82 (1.55)	-0.16***††	-0.17**†	-0.14
		(0.06)	(0.08)	(0.09)
		[0.021]	[0.065]	[0.124]
Entrepreneurial Intention Component		0.06**††	0.05	0.09**††
		(0.02)	(0.03)	(0.03)
		[0.013]	[0.146]	[0.048]
Whether or not the respondent has an idea for a business	0.58 (0.42)	0.04***††	0.04*†	0.04**†
		(0.01)	(0.02)	(0.02)
		[0.021]	[0.088]	[0.083]
The respondent's likelihood rating that they will start a business in the next 5 years (1-10 scale)	4.95 (3.05)	0.15*†	0.09	0.30**††
		(0.08)	(0.11)	(0.12)
		[0.055]	[0.296]	[0.048]
The respondent's interest in starting a business (1-10 scale)	6.21 (2.96)	0.10	0.09	0.19
		(0.09)	(0.12)	(0.13)
		[0.116]	[0.296]	[0.152]
Entrepreneurial Activity Component		0.01	-0.02	0.02
		(0.02)	(0.03)	(0.03)
		[0.202]	[0.312]	[0.345]
If a family member who started a business lives in the respondent's household	0.06 (0.21)	-0.01**††	-0.01	-0.02**††
		(0.01)	(0.01)	(0.01)
		[0.036]	[0.296]	[0.048]
If the respondent knows someone who started or helped start a business	0.60 (0.41)	0.03***††	0.02	0.04**†
		(0.01)	(0.02)	(0.02)
		[0.021]	[0.288]	[0.064]
If the respondent ever started or helped start a business	0.30 (0.40)	0.00	-0.02	0.02
		(0.01)	(0.02)	(0.02)
		[0.293]	[0.145]	[0.230]

This table compares results for entrepreneurship for participants by age at baseline. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.45: Impact of Guaranteed Income on Quality of Employment: Comparison of Impacts by Baseline Level of Education, Summary Measures

	Control Mean	Entire Sample	No Bachelor's Degree	Bachelor's Degree
Quality of Employment Index		**-0.01**	**-0.02**	**-0.00**
		(0.01)	**(0.02)**	**(0.03)**
		[0.568]	**[1.000]**	**[1.000]**
Adequacy of Employment Component		0.01	-0.02	0.04
		(0.03)	(0.03)	(0.05)
		[1.000]	[1.000]	[1.000]
Employment Quality Component		-0.01	-0.02	0.01
		(0.02)	(0.03)	(0.04)
		[1.000]	[1.000]	[1.000]
Single-item Component: Whether the respondent reports working any informal job	0.24 (0.37)	0.00	-0.00	0.02
		(0.01)	(0.02)	(0.03)
		[1.000]	[1.000]	[1.000]
Single-item Component: Average hourly income from all jobs, weighted by hours worked at each job	17.26 (9.72)	-0.13	-0.45	-0.42
		(0.37)	(0.38)	(0.79)
		[1.000]	[1.000]	[1.000]
Stability of Employment Component		0.00	-0.00	-0.01
		(0.02)	(0.02)	(0.03)
		[1.000]	[1.000]	[1.000]
Quality of Work Life Component		-0.02	-0.05**	0.05*
		(0.02)	(0.02)	(0.03)
		[1.000]	[1.000]	[1.000]

This table compares summary-level results for quality of employment for participants by whether or not they had a bachelor's degree at baseline. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.46: Impact of Guaranteed Income on Quality of Employment: Comparison of Impacts by Baseline Level of Education, Expanded Measures

	Control Mean	Entire Sample	No Bachelor's Degree	Bachelor's Degree
Adequacy of Employment				
The respondent is employed part-time in their main job and would prefer to work full-time	0.24 (0.39)	0.00	0.01	-0.02
		(0.02)	(0.02)	(0.03)
		[1.000]	[1.000]	[1.000]
The respondent would prefer to work more hours in their current main job	0.21 (0.36)	0.01	0.02	-0.02
		(0.02)	(0.02)	(0.02)
		[1.000]	[1.000]	[1.000]
The number of jobs held by the respondent apart from their main job	0.38 (0.70)	-0.03	-0.03	-0.00
		(0.03)	(0.03)	(0.04)
		[1.000]	[1.000]	[1.000]
Employment Quality				
Whether training is offered by the respondent's main employer	0.53 (0.45)	0.00	-0.00	-0.00
		(0.02)	(0.02)	(0.03)
		[1.000]	[1.000]	[1.000]
Whether training is offered during work hours by the respondent's main employer	0.49 (0.45)	0.01	-0.00	0.04
		(0.02)	(0.02)	(0.03)
		[1.000]	[1.000]	[1.000]
Whether formal training is offered by the respondent's main employer	0.13 (0.29)	-0.00	-0.00	-0.01
		(0.01)	(0.01)	(0.03)
		[1.000]	[1.000]	[1.000]
Number of non-wage benefits at respondent's job(s), weighted by hours worked at each job	3.61 (2.90)	-0.11	-0.17	-0.05
		(0.11)	(0.12)	(0.19)
		[1.000]	[1.000]	[1.000]
Whether the respondent must work an irregular shift at each job, weighted by hours worked at each job	0.19 (0.34)	0.01	0.01	0.01
		(0.01)	(0.02)	(0.02)
		[1.000]	[1.000]	[1.000]
Number of non-wage benefits at respondent's job(s), alternate specification	3.96 (2.98)	-0.17	-0.26**	0.11
		(0.11)	(0.13)	(0.21)
		[1.000]	[1.000]	[1.000]
Informality of Employment				
Whether the respondent reports any gig economy jobs such as Uber, TaskRabbit, or online surveys	0.09 (0.25)	-0.00	-0.01	0.01
		(0.01)	(0.01)	(0.02)
		[1.000]	[1.000]	[1.000]
Stability of Employment				
How many months the respondent has been employed in the past year	10.70 (2.64)	-0.03	-0.00	-0.17
		(0.10)	(0.13)	(0.14)
		[1.000]	[1.000]	[1.000]
How long the respondent has spent at their current main job and other jobs (months), weighted by hours worked at each job	24.88 (34.85)	1.43	1.38	-0.04
		(1.15)	(1.39)	(1.68)
		[1.000]	[1.000]	[1.000]
How many jobs the respondent has held in the past 12 months	1.77 (1.59)	-0.08**	-0.13**	-0.02
		(0.04)	(0.05)	(0.06)
		[1.000]	[1.000]	[1.000]
How many jobs the respondent has held in the past two years	2.33 (3.66)	-0.15*	-0.27**	0.01
		(0.08)	(0.12)	(0.08)
		[1.000]	[1.000]	[1.000]
Whether the respondent's main job is a temp job	0.10 (0.26)	0.01	0.02	0.01
		(0.01)	(0.01)	(0.02)
		[1.000]	[1.000]	[1.000]
Whether each of the respondent's jobs is salaried, weighted by hours worked at each job	0.23 (0.39)	-0.00	-0.02	0.00
		(0.01)	(0.01)	(0.03)
		[1.000]	[1.000]	[1.000]
Whether the respondent is performing contract or freelance work at each job, weighted by hours worked at each job	0.25 (0.38)	0.01	0.01	-0.00
		(0.01)	(0.02)	(0.02)
		[1.000]	[1.000]	[1.000]
How many months the respondent expects to remain in their main job (conditional on temp work)	8.97 (6.56)	-0.94	-0.52	-1.34
		(0.69)	(0.86)	(1.26)

		[1.000]	[1.000]	[1.000]
Quality of Work Life				
Advance notice of schedule provided at the respondent's main job (1-4 scale)	2.52 (1.24)	-0.04	-0.08	-0.03
		(0.05)	(0.06)	(0.09)
		[1.000]	[1.000]	[1.000]
The work activities are not boring at the respondent's main job (1-5 scale)	3.11 (1.05)	-0.01	-0.05	0.12
		(0.04)	(0.05)	(0.07)
		[1.000]	[1.000]	[1.000]
Satisfaction with compensation at the respondent's main job (1-5 scale)	3.51 (1.06)	-0.02	-0.06	0.09
		(0.04)	(0.05)	(0.08)
		[1.000]	[1.000]	[1.000]
Whether the respondent faces age discrimination at work	0.06 (0.21)	-0.00	-0.00	-0.00
		(0.01)	(0.01)	(0.02)
		[1.000]	[1.000]	[1.000]
Whether the respondent faces sex discrimination at work	0.08 (0.25)	0.00	0.01	-0.02
		(0.01)	(0.01)	(0.02)
		[1.000]	[1.000]	[1.000]
Whether the respondent faces racial or ethnic discrimination at work	0.08 (0.25)	0.00	-0.00	0.03
		(0.01)	(0.01)	(0.02)
		[1.000]	[1.000]	[1.000]
Whether the respondent experienced fair treatment by their supervisor (1-5 scale)	4.05 (0.91)	0.03	0.01	0.13**
		(0.04)	(0.05)	(0.06)
		[1.000]	[1.000]	[1.000]
Whether job demands do not interfere with family life (1-4 scale)	2.91 (0.87)	0.01	-0.01	0.05
		(0.03)	(0.04)	(0.06)
		[1.000]	[1.000]	[1.000]
Whether the job is a good fit with the respondent's experience and skills (1-5 scale)	4.19 (0.92)	-0.04	-0.06	0.01
		(0.04)	(0.04)	(0.06)
		[1.000]	[1.000]	[1.000]
Flexibility of schedule at the respondent's main job (1-4 scale)	1.91 (0.91)	0.01	-0.03	0.13*
		(0.04)	(0.04)	(0.07)
		[1.000]	[1.000]	[1.000]
Overall satisfaction with the respondent's main job (1-5 scale)	3.96 (0.96)	0.03	0.00	0.13*
		(0.04)	(0.05)	(0.07)
		[1.000]	[1.000]	[1.000]
Whether the respondent has decision-making input in their job (1-4 scale)	2.67 (0.98)	-0.04	-0.07	0.03
		(0.04)	(0.05)	(0.07)
		[1.000]	[1.000]	[1.000]
Satisfaction with non-wage aspects of respondent's main job (1-5 scale)	3.69 (1.12)	0.03	-0.03	0.10
		(0.04)	(0.05)	(0.08)
		[1.000]	[1.000]	[1.000]
Whether the respondent does not plan to leave their job in the next year (1-3 scale)	2.27 (0.72)	-0.04	-0.08**	0.07
		(0.03)	(0.03)	(0.05)
		[1.000]	[1.000]	[1.000]
Opportunities for promotion at the respondent's main job (1-5 scale)	3.41 (1.27)	-0.10*	-0.18***	0.06
		(0.05)	(0.07)	(0.09)
		[1.000]	[1.000]	[1.000]
Safety and health conditions at the respondent's main job (1-5 scale)	4.22 (0.79)	-0.00	-0.01	0.06
		(0.03)	(0.04)	(0.05)
		[1.000]	[1.000]	[1.000]
Whether a scheduled shift was canceled with less than 24 hours notice in the last month	0.09 (0.26)	0.02**	0.03*	0.00
		(0.01)	(0.01)	(0.01)
		[1.000]	[1.000]	[1.000]
Number of stressors in their work environment at respondent's main job	1.25 (1.24)	0.09*	0.10*	0.05
		(0.05)	(0.06)	(0.09)
		[1.000]	[1.000]	[1.000]
How hard is it to take time off from the respondent's main job? (1-4 scale)	3.18 (0.87)	-0.05	-0.07	-0.01
		(0.04)	(0.04)	(0.06)
		[1.000]	[1.000]	[1.000]

This table compares item-level results for quality of employment by participants' baseline level of education. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.47: Comparison of Administrative and Survey Data

		Illinois			Texas			Aggregate		
		Midline	Endline	Pooled	Midline	Endline	Pooled	Midline	Endline	Pooled
Income (Annual salary/wage income in thousands of dollars)	(a) Survey, entire sample§	0.17	-2.13	-0.99	-1.36	-1.68	-1.68	-0.46	-1.74*	-1.26
		(1.13)	(1.32)	(1.12)	(1.11)	(1.33)	(1.11)	(0.81)	(0.93)	(0.79)
		1411	1390	1454	1413	1393	1466	2824	2783	2920
	(b) Survey, consented§	0.29	-2.08	-0.98	-1.74	-1.26	-1.39	-0.16	-1.47	-0.81
		(1.20)	(1.37)	(1.14)	(1.13)	(1.37)	(1.15)	(0.86)	(0.98)	(0.83)
		1235	1226	1270	1260	1240	1299	2495	2466	2569
	(c) UI, matched	0.47	-1.94	-0.89	-2.06	-3.87**	-3.41**	-0.41	-2.67**†	-1.75*
		(1.07)	(1.37)	(1.16)	(1.47)	(1.76)	(1.62)	(0.87)	(1.08)	(0.94)
		932	932	932	975	975	975	1907	1907	1907
	(d) Pooled: survey, did not consent + UI, matched	0.07	-1.72	-0.12	-2.19	-3.32**	-2.93**	-0.37	-2.45**†	-1.59*
		(1.02)	(1.28)	(1.07)	(1.38)	(1.58)	(1.46)	(0.81)	(1.01)	(0.89)
		1108	1096	1116	1128	1128	1142	2236	2224	2258
Employment (in percentage points)	(a) Survey, entire sample§	0.00	-0.01	-0.01	-0.02	-0.05**	-0.04**	-0.01	-0.03**	-0.02*
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
		1463	1455	1482	1455	1457	1480	2918	2912	2962
	(b) Survey, consented§	-0.00	-0.02	-0.01	-0.02	-0.05**	-0.05**	-0.01	-0.03**	-0.03**
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
		1272	1268	1286	1287	1291	1308	2559	2559	2594
	(c) UI, matched	-0.04	-0.06*	-0.06**	-0.05	-0.08**	-0.07**	-0.04**	-0.07***	-0.07***
		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
		932	932	932	975	975	975	1907	1907	1907
	(d) Pooled: survey, did not consent + UI, matched	-0.03	-0.03	-0.03	-0.04	-0.05*	-0.05**	-0.03	-0.05**	-0.04**††
		(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
		1123	1119	1128	1143	1141	1147	2266	2260	2275

This table compares the estimated impact of the guaranteed income program on income and employment for different data sources. The rows marked (a) show the effects as estimated in SRC-enumerated survey data at midline and endline, for the full sample; (b) shows the effects as estimated in SRC data for those who consented to share administrative data; (c) shows the effects as estimated in the UI data, for those who consented to share these data and could be matched based on provided information; (d) shows the aggregate effects, as combined using fixed-effects meta-analysis. Results are presented separately for Illinois and Texas, as well as aggregated. The aggregation is done across states using fixed-effects meta-analysis, except in the case of the rows indicated with a §, where the regressions (based on survey data only) can be run on the full sample directly. Aggregate results from (a) and (c) were considered secondary and (d) was considered primary for the sake of FDR corrections, while all other estimates are considered subgroup analyses or post-pre-analysis-plan analyses. Standard errors are provided in parentheses, with the sample size below. Income is provided in terms of thousands of dollars, and employment in percentage points. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A.48: Robustness checks for Impact of Guaranteed Income on Annual Earned and Other Unearned Income (in $1,000s)

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Total household income	-4.3***	-4.6***	-3.7***	-4.3***	-4.3***	-5.8***	-4.0***
	(1.0)	(1.3)	(1.0)	(1.0)	(1.0)	(0.9)	(1.0)
Total individual income	-2.4***	-2.5**	-2.0**	-2.7***	-2.4***	-3.5***	-2.0***
	(0.7)	(1.0)	(0.9)	(0.7)	(0.7)	(0.6)	(0.7)
Total calculated individual income	-1.4*	-1.8*	-2.0**	-1.5	-1.4*	-3.0***	-1.1
	(0.9)	(1.1)	(1.0)	(0.9)	(0.9)	(0.8)	(0.9)
Individual salaried/wage income	-1.3	-1.7*	-1.5*	-1.2	-1.3	-2.3***	-1.1
	(0.8)	(1.0)	(0.8)	(0.8)	(0.8)	(0.8)	(0.8)
Self-employment income	-0.1	0.1	-0.0	-0.3	-0.1	-1.2***	-0.0
	(0.5)	(0.6)	(0.0)	(0.5)	(0.5)	(0.4)	(0.5)
Income from supplementary gig work	-0.1	-0.1	N/A	-0.1	-0.1	-0.2***	-0.1
	(0.0)	(0.1)		(0.1)	(0.0)	(0.0)	(0.0)
Passive income	0.0	0.0	N/A	0.0	0.0	-0.0	0.0
	(0.0)	(0.0)		(0.0)	(0.0)	(0.0)	(0.0)
Other income	-0.1	-0.1	N/A	-0.0	-0.1	-0.3*	-0.1
	(0.2)	(0.2)		(0.2)	(0.2)	(0.2)	(0.2)
Government transfers	-0.1	-0.2	-0.1	-0.1	-0.1	-0.3**	-0.1
	(0.1)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)

This table presents robustness checks for the estimates of impact on income, using survey data. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.49: Robustness checks for Impact of Guaranteed Income on Employment

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Hours worked per week	-1.29**	-1.77**	N/A	-1.32**	-1.29**	-1.87***	-1.04
	(0.63)	(0.80)		(0.64)	(0.63)	(0.62)	(0.63)
Whether the respondent is employed	-0.02*	-0.03*	N/A	-0.03*	-0.02*	-0.02**	-0.02
	(0.01)	(0.02)		(0.02)	(0.01)	(0.01)	(0.01)
Total number of hours participant and spouse/partner works per week	-2.48***	-2.42**	N/A	-2.77***	-2.48***	-2.90***	-2.22***
	(0.78)	(1.00)		(0.81)	(0.78)	(0.77)	(0.77)
Total number of hours all household members (including the participant) work per week	-2.39***	-2.94**	N/A	-2.46***	-2.39***	-3.01***	-2.24**
	(0.92)	(1.17)		(0.92)	(0.92)	(0.90)	(0.92)
Total number of hours participant's parents in household work per week	0.02	-0.21	N/A	-0.57	0.02	-0.52	0.03
	(0.37)	(0.46)		(0.42)	(0.37)	(0.33)	(0.37)
Total number of hours participant's adult children in household work per week	0.20	0.30	N/A	0.20	0.20	-0.20	0.20
	(0.23)	(0.29)		(0.23)	(0.23)	(0.19)	(0.23)
Number of other household members which are employed	-0.01	-0.02	N/A	-0.01	-0.01	-0.03	-0.01
	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)	(0.02)

This table presents robustness checks for the estimates of impact on employment outcomes, using survey data. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.50: Robustness checks for Impact of Guaranteed Income on Mobile App-Based Time Use

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Caring for Others Min/Day	-1.29	-2.09	N/A	-1.38	N/A	-4.19***	-1.13
	(1.16)	(1.30)		(1.10)		(0.87)	(1.17)
Child Care Min/Day	-4.01	-4.13	0.00	-2.86	N/A	-15.46***	-3.83
	(4.33)	(6.08)	(0.52)	(4.25)		(3.61)	(4.41)
Community Engagement Min/Day	-1.19	-0.44	0.00	-0.22	N/A	-3.47***	-1.02
	(0.89)	(1.01)	(0.02)	(0.87)		(0.71)	(0.90)
Exercise Min/Day	-0.18	-0.07	0.27	-0.04	N/A	-2.80***	0.16
	(0.86)	(1.04)	(0.25)	(0.88)		(0.64)	(0.88)
Home Production Min/Day	3.62	4.51	7.47**	3.44	N/A	-3.55	6.72**
	(3.43)	(4.10)	(2.99)	(3.43)		(3.06)	(3.42)
Market Work Min/Day	-9.22*	-12.20*	-7.18	-10.43**	N/A	-18.33***	-6.41
	(5.22)	(6.57)	(5.31)	(5.30)		(4.91)	(5.28)
Non-Commuting Transportation Min/Day	5.25***	5.34***	5.29***	4.30***	N/A	0.85	6.10***
	(1.59)	(1.66)	(1.37)	(1.44)		(1.19)	(1.61)
Other Income Min/Day	-2.66**	-2.50**	N/A	-2.95***	N/A	-5.68***	-2.58**
	(1.11)	(1.15)		(1.09)		(0.85)	(1.12)
Other Min/Day	5.90**	5.21*	1.33***	5.83**	N/A	-2.30	6.64**
	(2.58)	(2.97)	(0.50)	(2.64)		(1.90)	(2.62)
Search for a Job Min/Day	-0.31	0.18	N/A	-0.78	N/A	-3.66***	-0.14
	(1.02)	(1.12)		(0.98)		(0.71)	(1.03)
Self-Care Min/Day	1.31	1.21	1.32	1.42	N/A	-1.77*	2.38*
	(1.26)	(1.41)	(1.23)	(1.23)		(1.06)	(1.27)
Self-Improvement Min/Day	0.11	-0.53	0.30	0.55	N/A	-5.96***	0.54
	(2.26)	(2.78)	(0.75)	(2.32)		(1.89)	(2.30)
Sleep Min/Day	-7.27*	-4.52	-6.31*	-11.14***	N/A	-13.16***	-0.25
	(3.97)	(5.05)	(3.62)	(4.13)		(3.83)	(3.72)
Social Leisure Min/Day	7.35*	5.27	7.71*	7.85**	N/A	-0.32	10.76***
	(3.92)	(4.65)	(4.40)	(3.89)		(3.65)	(3.93)
Solo Leisure Min/Day	5.17	4.87	2.38	5.66*	N/A	-1.93	6.92**
	(3.40)	(5.16)	(2.75)	(3.42)		(3.07)	(3.45)
Time with Others Min/Day	1.79	-0.33	7.61	3.65	N/A	-10.62*	5.51

	(6.20)	(8.37)	(5.37)	(6.02)		(5.61)	(6.25)

This table presents robustness checks for the estimates of impact on time use from the mobile app-based time diaries. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to administrative data or data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to administrative data or results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.51: Robustness checks for Impact of Guaranteed Income on Enumerated and Quarterly Time Use

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Finances Hrs/Mo	0.28*	0.44**	0.26**	0.28	0.42**	0.18	0.29*
	(0.17)	(0.20)	(0.11)	(0.17)	(0.20)	(0.16)	(0.17)
Helping Hrs/Mo	0.28	0.42	0.36***	0.30	0.37	0.04	0.30
	(0.26)	(0.28)	(0.13)	(0.26)	(0.31)	(0.24)	(0.26)
Medical Hrs/Mo	0.03	0.82	0.04	-0.09	0.41	-0.37	0.04
	(0.61)	(0.76)	(0.07)	(0.60)	(0.71)	(0.58)	(0.62)
Meetings Hrs/Mo	-0.02	0.03	0.00	-0.03	0.02	-0.08	-0.02
	(0.07)	(0.08)	(0.01)	(0.08)	(0.09)	(0.07)	(0.08)
Religion Hrs/Mo	0.03	0.17	0.00	0.02	0.13	-0.03	0.03
	(0.12)	(0.16)	(0.02)	(0.12)	(0.14)	(0.12)	(0.12)
Childcare Hrs/Wk	-0.74	-0.68	-0.07	-0.77	-0.69	-1.18	-0.72
	(0.88)	(1.41)	(0.27)	(0.87)	(1.00)	(0.86)	(0.88)
Chores Hrs/Wk	-0.20	0.01	0.24	-0.21	-0.10	-0.35	-0.15
	(0.27)	(0.33)	(0.25)	(0.27)	(0.32)	(0.27)	(0.27)
Communicating Hrs/Wk	-0.25	-0.15	-0.03	-0.27	-0.47	-0.50	-0.21
	(0.35)	(0.40)	(0.21)	(0.35)	(0.43)	(0.34)	(0.35)
Commuting Hrs/Wk	-0.07	-0.07	-0.12	-0.11	-0.03	-0.18	-0.06
	(0.14)	(0.16)	(0.12)	(0.14)	(0.16)	(0.13)	(0.14)
Education Hrs/Wk	0.01	0.13	0.02	0.01	-0.02	-0.15	0.01
	(0.22)	(0.25)	(0.10)	(0.22)	(0.26)	(0.21)	(0.22)
Eldercare Hrs/Wk	-0.20	-0.08	-0.02*	-0.21	-0.33	-0.38*	-0.20
	(0.21)	(0.25)	(0.01)	(0.21)	(0.25)	(0.20)	(0.21)
Entertainment Hrs/Wk	0.02	0.13	-0.09	-0.05	0.18	-0.20	0.07
	(0.36)	(0.45)	(0.35)	(0.36)	(0.44)	(0.35)	(0.36)
Family Hrs/Wk	-0.65	-0.79	-0.23	-0.67	-1.20	-1.03	-0.61
	(0.73)	(0.93)	(0.73)	(0.73)	(0.87)	(0.72)	(0.73)
Friends Hrs/Wk	0.04	0.28	0.07	0.01	0.08	-0.14	0.07
	(0.27)	(0.31)	(0.20)	(0.27)	(0.33)	(0.26)	(0.27)
Hobbies Hrs/Wk	-0.06	-0.00	-0.09	-0.06	-0.06	-0.20	-0.05
	(0.15)	(0.17)	(0.09)	(0.15)	(0.18)	(0.14)	(0.15)
Reading Hrs/Wk	-0.11	-0.02	-0.05	-0.12	-0.28	-0.23	-0.10
	(0.19)	(0.22)	(0.15)	(0.19)	(0.23)	(0.18)	(0.19)

Recreation Hrs/Wk	-0.44**	-0.39**	-0.27*	-0.43**	-0.59***	-0.58***	-0.41**
	(0.18)	(0.20)	(0.16)	(0.18)	(0.22)	(0.17)	(0.18)
Sleeping Hrs/Wk	0.18	0.49	-0.00	0.16	-0.29	0.04	0.30
	(0.37)	(0.47)	(0.45)	(0.37)	(0.42)	(0.37)	(0.37)
Working Hrs/Wk	-1.53***	-1.67**	N/A	-1.53***	-1.61***	-1.71***	-1.47***
	(0.51)	(0.66)		(0.51)	(0.59)	(0.50)	(0.51)
Vacation Days/Yr	0.09	0.15	N/A	0.10	-0.08	-0.10	0.12
	(0.31)	(0.37)		(0.30)	(0.32)	(0.29)	(0.31)
Volunteer Hrs/Yr	0.15	1.69	-0.01	0.14	0.62	-1.17	0.16
	(1.47)	(1.73)	(0.21)	(1.48)	(1.77)	(1.37)	(1.48)

This table presents robustness checks for the estimates of impact on time use from the enumerated and quarterly time use surveys. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to administrative data or data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to administrative data or results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.52: Robustness checks for Impact of Guaranteed Income on Disability

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Disability Index	**-0.09******	**-0.10****	**N/A**	**-0.08*****	**-0.09*****	**-0.10*****	**-0.07****
	(0.03)	**(0.04)**		**(0.03)**	**(0.03)**	**(0.03)**	**(0.03)**

This table presents robustness checks for the estimates of impact on disability. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.53: Robustness checks for Impact of Guaranteed Income on Duration of Unemployment

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Duration of Unemployment Index	**-0.07****	**-0.10****	**-0.07*****	**N/A**	**-0.07****	**-0.07****	**-0.05***
	(0.03)	**(0.04)**	**(0.02)**		**(0.03)**	**(0.03)**	**(0.03)**

This table presents robustness checks for the estimates of impact on duration of unemployment. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.54: Robustness checks for Impact of Guaranteed Income on Entrepreneurship

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Entrepreneurship Index	**0.05*** **	**0.07*** **	**0.09*** **	**0.05*** **	**N/A**	**0.03** **	**0.06*** **
	(0.02)	**(0.02)**	**(0.03)**	**(0.01)**		**(0.01)**	**(0.01)**
Entrepreneurial Orientation	0.07***	0.10***	0.08**	0.06***	N/A	0.05**	0.10***
	(0.02)	(0.03)	(0.04)	(0.02)		(0.02)	(0.02)
Entrepreneurial Intention	0.06**	0.08**	0.12**	0.06***	N/A	0.05**	0.06***
	(0.02)	(0.03)	(0.06)	(0.02)		(0.02)	(0.02)
Entrepreneurial Activity	0.01	0.04	0.05*	0.01	N/A	-0.00	0.02
	(0.02)	(0.03)	(0.03)	(0.02)		(0.02)	(0.02)

This table presents robustness checks for the estimates of impact on entrepreneurship. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.55: Robustness checks for Impact of Guaranteed Income on Human Capital

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Human Capital Index	**0.01**	**0.02**	**N/A**	**0.01**	**N/A**	**0.01**	**0.01**
	(0.01)	**(0.01)**		**(0.01)**		**(0.01)**	**(0.01)**
Formal Education	0.02	0.04	N/A	0.01	N/A	-0.03	0.02
	(0.02)	(0.03)		(0.02)		(0.02)	(0.02)

This table presents robustness checks for the estimates of impact on human capital. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.56: Robustness checks for Impact of Guaranteed Income on Barriers to Employment

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Barriers to Employment Index	**-0.03**	**-0.03**	**N/A**	**-0.02**	**-0.03**	**-0.04**	**0.03**
	(0.02)	**(0.03)**		**(0.02)**	**(0.02)**	**(0.02)**	**(0.02)**

This table presents robustness checks for the estimates of impact on barriers to employment. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.57: Robustness checks for Impact of Guaranteed Income on Employment Preferences and Job Search

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Employment Preferences and Job Search	**0.02**	**0.02**	**0.04***	**0.01**	**0.04**	**0.00**	**0.04****
Index	**(0.02)**	**(0.02)**	**(0.03)**	**(0.02)**	**(0.02)**	**(0.02)**	**(0.02)**
Active Search	0.03	0.03	0.12***	-0.01	N/A	0.00	0.03
	(0.02)	(0.03)	(0.04)	(0.03)		(0.02)	(0.02)
Preferences for Employment	0.01	0.01	N/A	0.03	0.01	0.01	0.04*
	(0.02)	(0.03)		(0.02)	(0.02)	(0.02)	(0.02)

This table presents robustness checks for the estimates of impact on employment preferences and job search. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.58: Robustness checks for Impact of Guaranteed Income on Selectivity of Job Search

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Selectivity of Job Search Index	**-0.01**	**-0.02**	**-0.06**	**0.01**	**-0.02**	**-0.10*** **	**0.06*** **
	(0.02)	**(0.03)**	**(0.04)**	**(0.04)**	**(0.06)**	**(0.02)**	**(0.02)**

This table presents robustness checks for the estimates of impact on selectivity of job search. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.59: Robustness checks for Impact of Guaranteed Income on Quality of Employment

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Quality of Employment Index	**-0.01**	**-0.02**	**-0.02**	**0.00**	**-0.01**	**-0.03***	**0.01**
	(0.01)	**(0.02)**	**(0.02)**	**(0.02)**	**(0.01)**	**(0.01)**	**(0.01)**
Adequacy of Employment	0.01	-0.01	0.02	0.03	0.01	-0.03	0.01
	(0.03)	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)
Employment Quality	-0.01	-0.03	-0.05	0.01	-0.01	-0.02	0.01
	(0.02)	(0.03)	(0.06)	(0.03)	(0.02)	(0.02)	(0.02)
Whether the respondent reports working any	0.00	0.01	N/A	0.02	0.00	0.01	-0.00
informal job	(0.01)	(0.02)		(0.02)	(0.01)	(0.01)	(0.01)
Average hourly income from all jobs,	-0.13	-0.18	-0.27	0.07	-0.13	-0.31	0.14
weighted by hours worked at each job	(0.37)	(0.44)	(0.42)	(0.44)	(0.37)	(0.36)	(0.36)
Stability of Employment	0.00	-0.00	-0.00	0.02	0.00	-0.02	0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Quality of Work Life	-0.02	-0.02	-0.03	-0.01	-0.02	-0.04**	-0.00
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)

This table presents robustness checks for the estimates of impact on quality of employment. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.60: Robustness checks for Impact of Guaranteed Income on Consumption

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Total Consumption	**284*****	**329*****	**341*****	**283*****	**N/A**	**198*****	**296*****
	(49)	**(70)**	**(78)**	**(49)**		**(47)**	**(49)**
Human capital expenditures	44***	48**	38**	45***	N/A	25*	45***
	(15)	(20)	(16)	(15)		(14)	(15)
Durable goods expenditures	41***	44***	42**	41***	N/A	28**	44***
	(14)	(16)	(18)	(14)		(13)	(14)
Housing expenditures	32**	52**	34	31*	N/A	25	34**
	(17)	(23)	(29)	(16)		(16)	(17)
Non-durable goods and services	133***	144***	138***	134***	N/A	105***	139***
expenditures	(26)	(35)	(39)	(26)		(25)	(26)
Other expenditures	34***	41***	55***	32***	N/A	15	35***
	(11)	(14)	(12)	(12)		(10)	(11)

This table presents robustness checks for the estimates of impact on consumption, using survey data. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item. For example, we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.61: Robustness checks for Impact of Guaranteed Income on Labor Market Mobility

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Labor Market Mobility Index	**0.09*** **	**0.10*** **	**N/A**	**0.07*** **	**N/A**	**0.04**	**0.10*** **
	(0.03)	**(0.03)**		**(0.02)**		**(0.03)**	**(0.03)**
Moved labor markets since baseline	0.02	0.02**	N/A	N/A	N/A	0.02	0.02
	(0.01)	(0.01)				(0.01)	(0.01)
Search New Labor Market	0.11***	0.12***	N/A	0.07***	N/A	0.02	0.13***
	(0.03)	(0.04)		(0.02)		(0.03)	(0.03)

This table presents robustness checks for the estimates of impact on labor market mobility. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.62: Robustness checks for Impact of Guaranteed Income on Labor Market Quality

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Labor Market Quality Index	**0.003**	**0.012**	**0.005**	**0.015**	**N/A**	**-0.002**	**0.006**
	(0.005)	**(0.012)**	**(0.012)**	**(0.014)**		**(0.005)**	**(0.005)**
Labor Market Quality	0.005	0.025	0.004	0.016	N/A	0.004	0.007
	(0.005)	(0.023)	(0.020)	(0.020)		(0.005)	(0.005)
Labor Market Amenities	0.000	-0.001	-0.001	0.014	N/A	-0.008	0.005
	(0.009)	(0.010)	(0.004)	(0.018)		(0.008)	(0.009)

This table presents robustness checks for the estimates of impact on labor market quality. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.63: Robustness checks for Impact of Guaranteed Income on Benefits

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Benefits Index	**-0.01**	**-0.03**	**-0.01**	**-0.00**	**-0.01**	**-0.04***	**-0.00**
	(0.02)	**(0.04)**	**(0.04)**	**(0.03)**	**(0.02)**	**(0.02)**	**(0.02)**

This table presents robustness checks for the estimates of impact on benefits. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.64: Robustness checks for Impact of Guaranteed Income on Relationship Status

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Relationship Status Index	**-0.01**	**-0.00**	**0.01**	**-0.04***	**0.00**	**-0.02**	**0.01**
	(0.02)	**(0.03)**	**(0.04)**	**(0.02)**	**(0.02)**	**(0.02)**	**(0.02)**
Relationship Stability	-0.04	-0.03	-0.01	-0.08***	N/A	-0.05*	0.01
	(0.02)	(0.03)	(0.03)	(0.03)		(0.03)	(0.02)
Relationship Status	0.01	0.02	0.06	0.01	0.00	0.01	0.01
	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)

This table presents robustness checks for the estimates of impact on relationship status. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.65: Robustness checks for Impact of Guaranteed Income on Subjective Well-Being

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Subjective Wellbeing Index	**-0.00**	**-0.00**	**-0.02**	**-0.00**	**-0.04**	**-0.02**	**0.01**
	(0.02)	**(0.03)**	**(0.04)**	**(0.02)**	**(0.03)**	**(0.02)**	**(0.02)**
Domain Satisfaction	0.01	0.01	0.01	0.01	N/A	-0.01	0.03
	(0.02)	(0.03)	(0.04)	(0.02)		(0.02)	(0.02)
Level of satisfaction with life as a whole currently (0-10 scale)	-0.04	-0.03	-0.05	-0.03	-0.08	-0.04	-0.01
	(0.05)	(0.07)	(0.07)	(0.05)	(0.06)	(0.05)	(0.05)
Affect balance	0.01	0.01	-0.05	-0.01	N/A	-0.11	0.15
	(0.21)	(0.31)	(0.26)	(0.21)		(0.21)	(0.21)

This table presents robustness checks for the estimates of impact on subjective well-being. The columns, in turn, present the main estimate; a version run without any covariates; results from median regression; results from using a difference-in-differences approach; results restricting attention to data from the enumerated surveys; and the lower and upper Lee bound. Not every robustness check can necessarily be run for every item: we cannot restrict attention to results from enumerated surveys ("Midline/Endline") for those questions asked only on web-based surveys. Additionally, median regression will occasionally not converge, and we do not run it in cases in which there is a binary dependent variable.

Table A.66: Comparison of Marginal Propensity to Earn with Other Studies

	Per-adult total post-tax transfer	MPEs based on individual total labor earnings	
		Standard assumptions, 2.5% discount rate	No net savings
	(1)	(2)	(3)
Panel A: Lottery studies			
Golosov et al. (2023)	181200	-0.43	-0.02
Cesarini et al. (2017)	2629	-0.27	-0.01
Imbens et al. (2001)	NA	NA	NA
Panel B: Sustained monthly transfers			
Vivalt et al. (2025)	20118	-2.61	-0.27
Sauval et al. (2024)	7087	-1.92	-0.25

This table calculates MPEs based on individual total labor earnings assuming either a model common in the lottery literature (Column (2)) or no net savings (Column (3)). The difference between these models for the sake of this table is the amount that they imply individuals allocate to spending in a year, which serves as the denominator of the MPE calculations. Appendix J.2 describes in more detail how the numbers in this table are calculated. This table demonstrates that individuals appear to treat large lottery winnings very differently from sustained monthly transfers: the MPE figures in Panel A, Column (2) are more similar to the MPE figures in Panel B, Column (3) than they are to the numbers in Panel B, Column (2), and similarly the numbers in Panel A, Column (3) are not comparable to the numbers in Panel B, Column (3). We do not wish to make strong assumptions about what exactly the savings rate is in each study, but would argue that the numbers on the off-diagonal (Panel A, Column (3) and Panel B, Column (2)) are not reasonable and take this table as evidence that different modeling assumptions may be required for lottery studies as opposed to sustained monthly transfers.

Table A.67: Forecasts of NBER Affiliates and SSPP Forecasters

	NBER Affiliates						SSPP			
	Selected Fields			Labor Studies						
	Median	Mean	N	Median	Mean	N	Median	Mean	N	Result
Employed, in percentage points	-0.5	-1.2	43	-1.0	-2.1	17	-0.6	0.3	94	-2.9
Work hours per week	-0.9	-0.6	42	-1.2	-1.3	16	-1.4	-1.2	94	-1.7
Average hourly wage	1.0	1.2	42	0.5	0.9	16	0.7	0.7	94	-0.2
Duration of non-employment, in weeks	3.7	3.9	41	3.1	3.5	16	2.4	2.6	94	3.5
Participant is searching for work, in percentage points	-2.8	-2.5	42	-4.8	-2.9	16	-	-	-	7.0
Enrollment in a post-secondary program	2.9	3.1	41	2.4	2.6	16	3.5	4.4	94	0.2
Individual salaried income (UI data), in thousands of dollars	-0.7	-0.3	21	-	-	-	0.0	1.1	95	-2.7
Home production, hours per week	-	-	-	-	-	-	0.8	1.8	93	0.7
Sleep, hours per week	-	-	-	-	-	-	0.7	0.6	93	-0.9
Social leisure, hours per week	-	-	-	-	-	-	4.7	5.0	92	0.8
Solitary leisure, hours per week	-	-	-	-	-	-	2.9	3.5	92	0.5

This table shows forecasts of NBER affiliates and users of the Social Science Prediction Platform (SSPP). As described in the text, forecasts were elicited from NBER affiliates in several related Programs, and these forecasts were supplemented by forecasts from the SSPP, including from members of its Superforecaster Panel. SSPP users were not asked the question about job search, to keep the survey short, as they were asked to answer questions on a greater number of topics. Items forecast by fewer than 10 individuals for a subgroup of forecasters are suppressed. All results are from endline data or year 3, as forecasters were asked to predict the effects at the end of the study. Data for the individual salaried income results comes from the administrative records, as this is what forecasters were asked to predict, and data for the enrollment in a post-secondary program result come from the NSC data. All other results come from the survey data.

Table A.68: CE/PCE Correspondence Table

Variable	BLS Variable	CE/PCE
Clothing services such as laundry, dry cleaning, or shoe repair	Comparable services	0.91
Personal care products and services, such as toothpaste, shampoo, hand soap, haircuts and styling, manicures, shaving supplies, or cosmetics	Personal care products	0.37
Gas/electric bills	Household utilities	0.91
Phone bills	Communication	0.96
Cable/internet	Communication	0.96
Other utility bills	Household utilities	0.91
Housekeeping supplies and services, such as cleaning detergents, paper towels, sponges, or a cleaning service	Household cleaning products	0.71
Housekeeping supplies and services, such as cleaning detergents, paper towels, sponges, or a cleaning service	Household paper products	0.33
Baby items (diapers, formula, etc.)	Comparable services	0.91
Child care for under 5	Child care	0.33
School or child care expenses for 5-18	Child care	0.33
Children's extracurricular	Comparable services	0.91
Entertainment for children	Comparable services	0.91
Total mortgage payment	*	1.00
Child support/alimony	*	1.00
Recreation/entertainment	Comparable nondurable goods	0.50
Gambling and lotteries	Gambling	0.05
Taxis and car services	Comparable services	0.91
Health insurance premiums	*	1.00
Health care expenses	Pharmaceutical products	0.18
Pets	Pets and related products	0.57
Pets	Veterinary and other services for pets	0.58
Car payments, insurance, and maintenance	*	1.00
Gas, parking, tolls	Gasoline and other energy goods	0.87
Gas, parking, tolls	Other motor vehicle services	0.74
Debt payments	*	1.00
Clothing, shoes, watches jewelry, etc.	Jewelry and watches	0.22
Clothing, shoes, watches jewelry, etc.	Women's and girls' clothing	0.43
Clothing, shoes, watches jewelry, etc.	Men's and boys' clothing	0.45
Clothing, shoes, watches jewelry, etc.	Shoes and other footwear	0.49
Household furnishings and equipment, including furniture, bed linens, appliances, dishes, or other housewares	Furniture and furnishings	0.53
Household furnishings and equipment, including furniture, bed linens, appliances, dishes, or other housewares	Household appliances	0.95
Household furnishings and equipment, including furniture, bed linens, appliances, dishes, or other housewares	Glassware, tableware, and household utensils	0.21

Household furnishings and equipment, including furniture, bed linens, appliances, dishes, or other housewares	Household maintenance	0.77
TVs, computers, phones, or devices	Televisions	0.44
TVs, computers, phones, or devices	Audio equipment	0.24
TVs, computers, phones, or devices	Personal computers and peripheral equipment	0.35
TVs, computers, phones, or devices	Telephone and facsimile equipment	0.54
Moving and storage	Comparable services	0.91
Health care, specifically payments to providers for visits to the doctor or dentist, hospital stays, therapy, or other services	*	1.00
College or professional or job training, tuition, books, computers, supplies, etc.	Comparable services	0.91
Vacation	Comparable nondurable goods	0.50
Charity	Comparable items	0.71
Food and beverages that you consume at home, including food purchased from stores	Food purchased for off-premises consumption	0.63
Food and beverages that you consume at home, including food purchased from stores	Nonalcoholic beverages purchased for off-premises consumption	0.70
Food that you eat away from home, including eating out in restaurants or buying snacks and drinks	Purchased meals and beverages	0.51
Alcohol	Alcoholic beverages purchased for off-premises consumption	0.18
Alcohol	Purchased meals and beverages	0.51
Cigarettes and tobacco	Tobacco	0.40
Marijuana	Tobacco	0.40
Public transportation	Other motor vehicle services	0.74
Housing	*	1.00
Unexpected car expenses	Motor vehicles and parts	0.74
Unexpected household expenses	Household maintenance	0.77
Unexpected medical emergency expenses	*	1.00
Unexpected healthcare expenses	*	1.00
Unexpected tax expenses	Comparable items	0.71
Unexpected childcare expenses	Child care	0.33
Unexpected travel expenses	Comparable nondurable goods	0.50
Unexpected veterinary expenses	Veterinary and other services for pets	0.58
Unexpected other expenses	Comparable items	0.71
Gifts or loans given to others (excluding charity)	Comparable items	0.71
Other expenses	Comparable items	0.71

This table provides a map between our survey questions and the categories used by the BLS in reporting CE/PCE ratios. Items with a * are those which we assigned a ratio of 1, anticipating participants recalled these accurately. As described in the text, if a category matched to more than one BLS category, we weighted by the share of expenditures in each category in the BLS.

B Appendix Figures

Figure B.1: Illinois Bill SB 1735

Illinois General Assembly

Translate Website

Home Legislation & Laws Senate House My Legislation Site Map

Previous General Assemblies

Bill Status of SB1735 *101st General Assembly*

Full Text **Votes** **Witness Slips** **View All Actions** **Printer-Friendly Version**

Short Description: PUB AID-RESEARCH PROJECT

Senate Sponsors
Sen. Omar Aquino - Kimberly A. Lightford - Jacqueline Y. Collins, Robert Peters, Mattie Hunter and Emil Jones, III

House Sponsors
(Rep. Delia C. Ramirez - Bob Morgan - Mary E. Flowers, Yehiel M. Kalish, Kelly M. Cassidy, Theresa Mah, Justin Slaughter, Jennifer Gong-Gershowitz, Anne Stava-Murray and Will Guzzardi)

Last Action

Date	Chamber	Action
8/16/2019	Senate	Public Act 101-0415

Statutes Amended In Order of Appearance

305 ILCS 5/1-7 from Ch. 23, par. 1-7

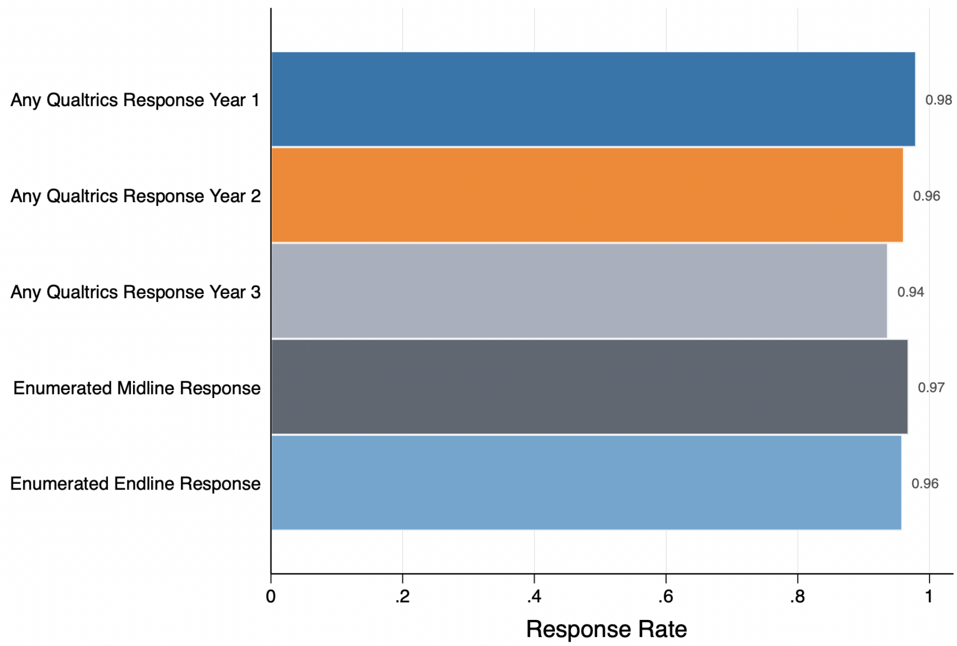
Synopsis As Introduced
Amends the Illinois Public Aid Code. Provides that for purposes of determining eligibility and the amount of assistance under the Code, the Department of Human Services and local governmental units shall exclude from consideration, for a period of no more than 60 months, any financial assistance, including wages, cash transfers, or gifts, that is provided to a person who is enrolled in a program or research project that is not funded with general revenue funds and that is intended to investigate the impacts of policies or programs designed to reduce poverty, promote social mobility, or increase financial stability for Illinois residents if there is an explicit plan to collect data and evaluate the program or initiative that is developed prior to participants in the study being enrolled in the program and if a research team has been identified to oversee the evaluation. Requires the Department to seek all necessary federal approvals or waivers to implement the provisions of the amendatory Act. Effective immediately.

Actions

Date	Chamber	Action
2/15/2019	Senate	Filed with Secretary by Sen. Omar Aquino
2/15/2019	**Senate**	**First Reading**

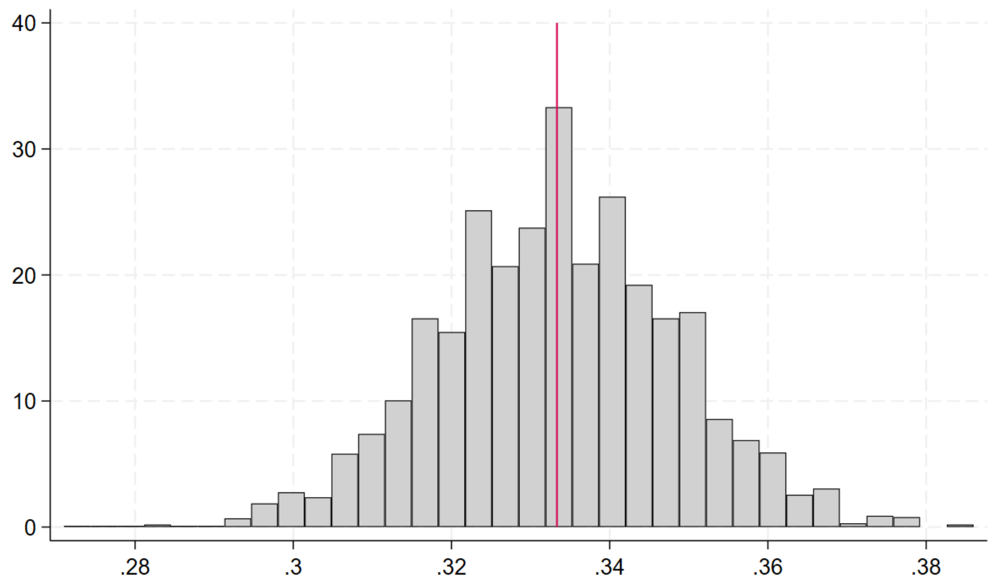
This figure provides a synopsis of the bill that was passed to protect benefits in Illinois.

Figure B.2: Response Rates Over Time



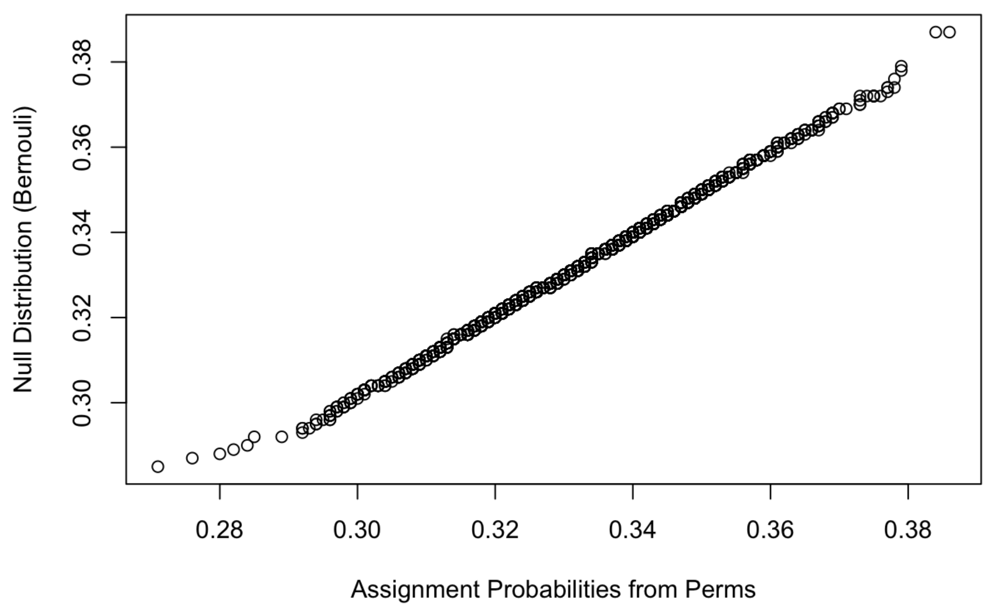
This figure shows response rates for the Qualtrics surveys and enumerated surveys over time.

Figure B.3: Histogram of Treatment Assignment Probabilities



This graph displays a frequency distribution of participants' average treatment assignments, based on 1,000 simulated runs of the assignment process. The vertical line on the graph is positioned at 0.33333, representing the 1 in 3 probability of assignment.

Figure B.4: QQ-plot of Treatment Probability against Bernoulli Distribution

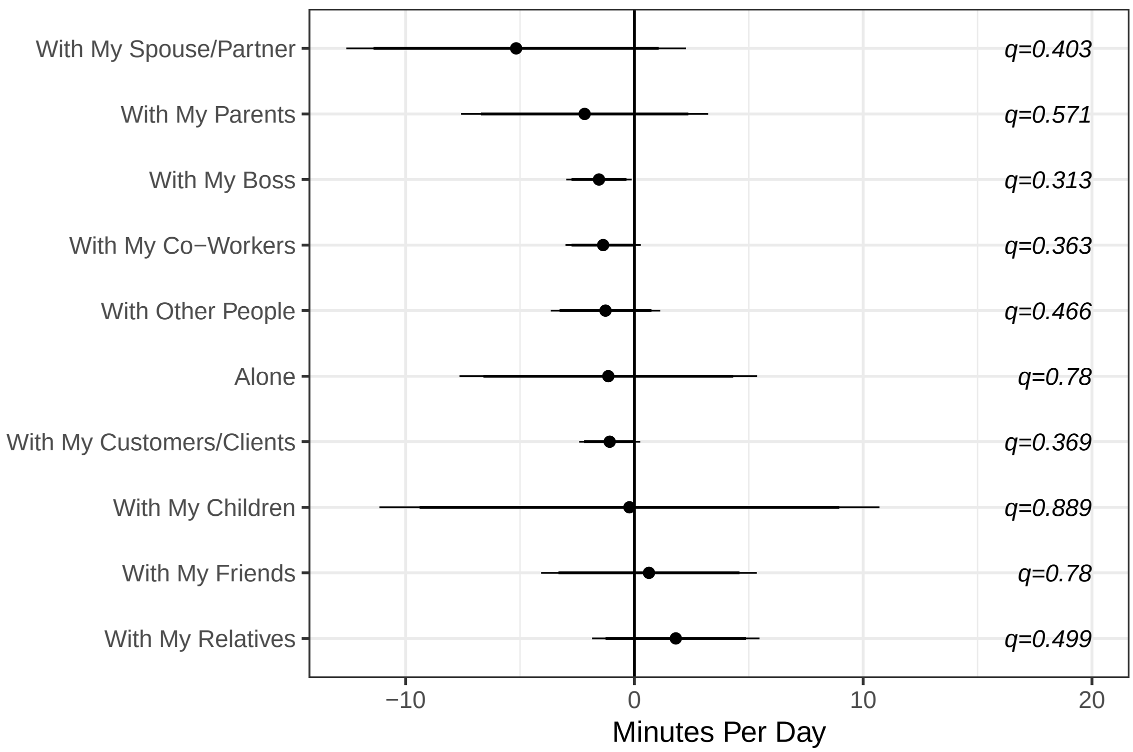


This graph compares the actual distribution of treatment assignments with the theoretical distribution expected from a random assignment process where each participant has a one in three chance of being assigned to the treatment group. The x-axis shows the quantiles of the observed treatment assignments, while the y-axis represents the quantiles of the expected distribution under random assignment. A Kolmogorov-Smirnov test was conducted to compare these distributions. The test result (p=0.5226) indicates that there is not sufficient evidence to conclude that the observed distribution differs significantly from what would be expected by chance.

Figure B.5: Quarterly Results for Income and Employment Over Time

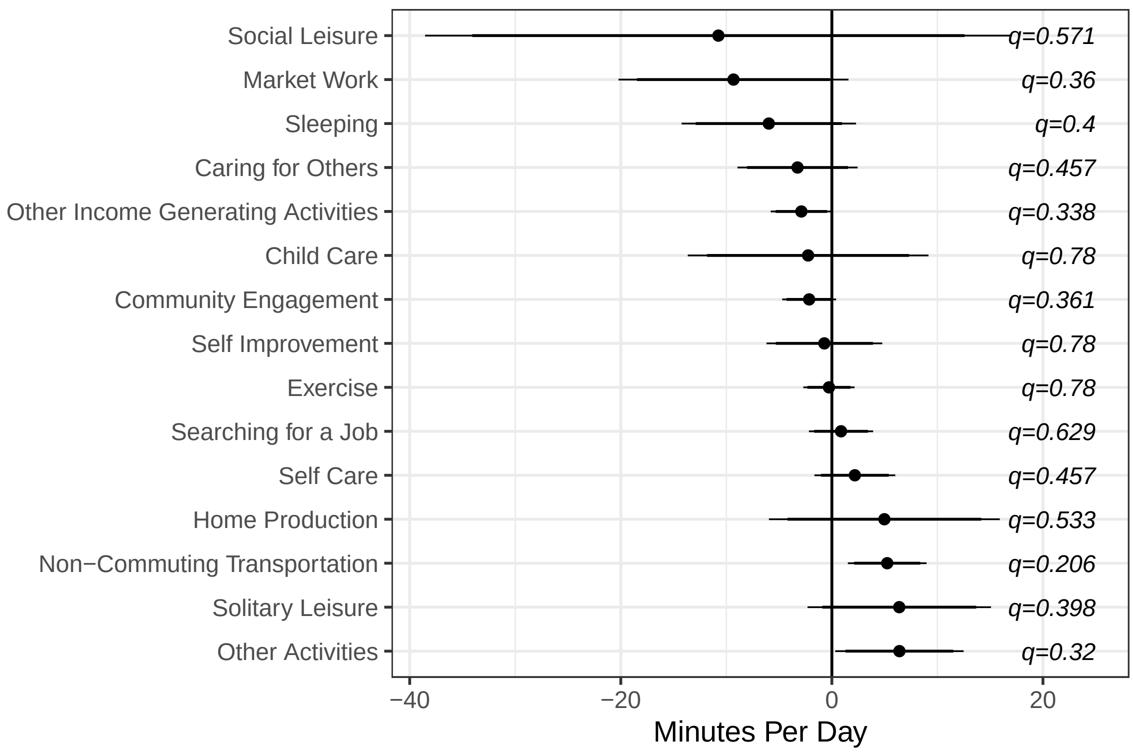
This figure plots the results for income and employment over time. The first two subfigures use data from UI records in each state, pooling results across states as described in Appendix K, while the third uses survey data. The data points in this figure represent estimated effects on individual salaried income or employment for the preceding quarter and are formed via regressions within each quarter (*i.e.*, the value for the treatment group is the estimated treatment effect added to the constant term). Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals. No controls are included in these regressions.

Figure B.6: Time Use Results: Mobile App (Time Spent With Others)



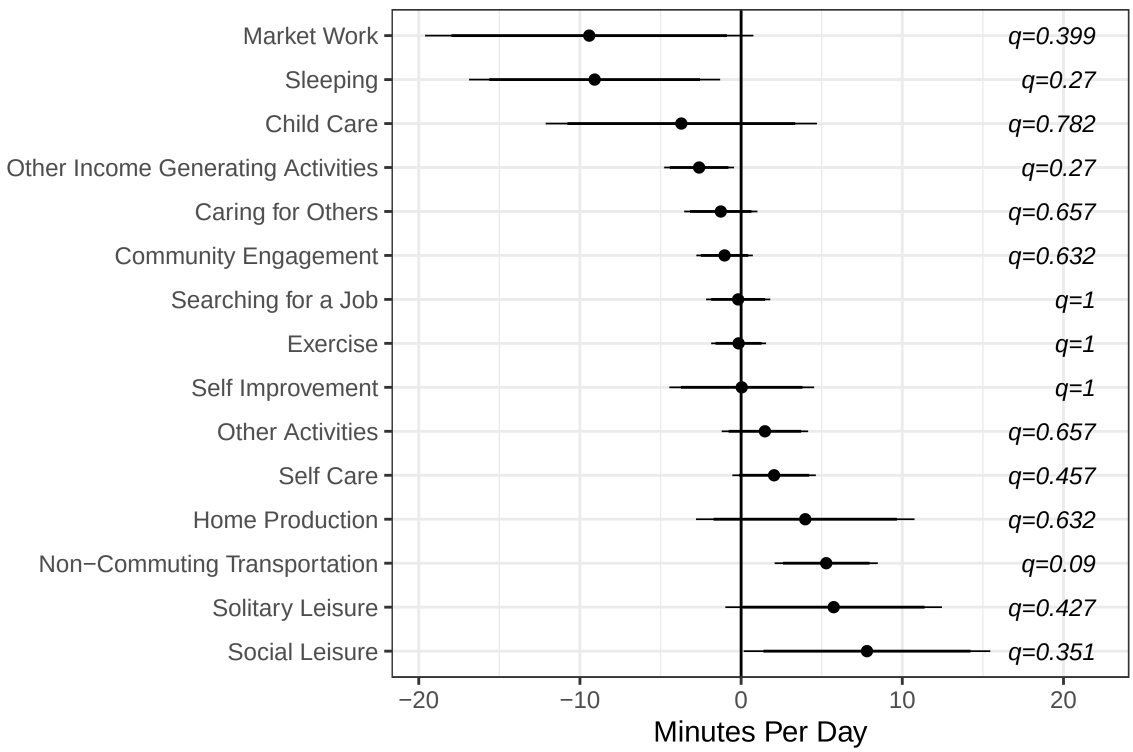
This figure shows the results from the mobile phone app for time spent with others. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.7: Time Use Results: Mobile App (Raw Times)



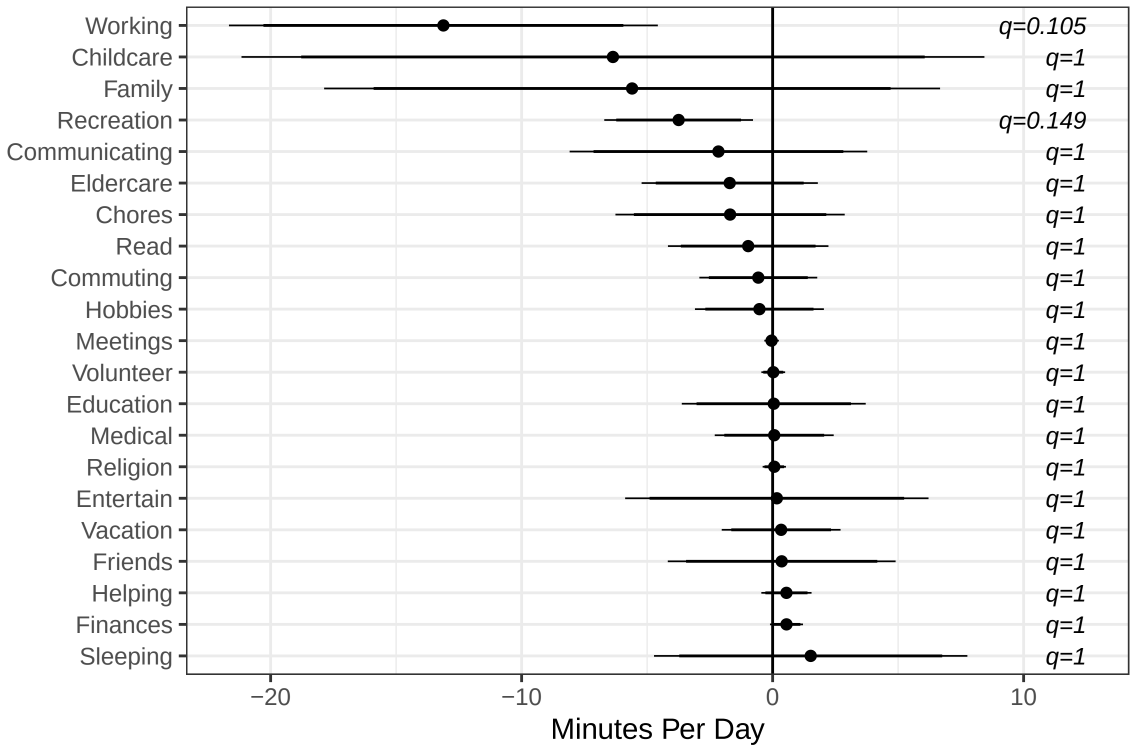
This figure shows the results from the mobile phone app, without adjusting for simultaneous activities. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.8: Time Use Results: Mobile App (ChatGPT-4 Recoded)



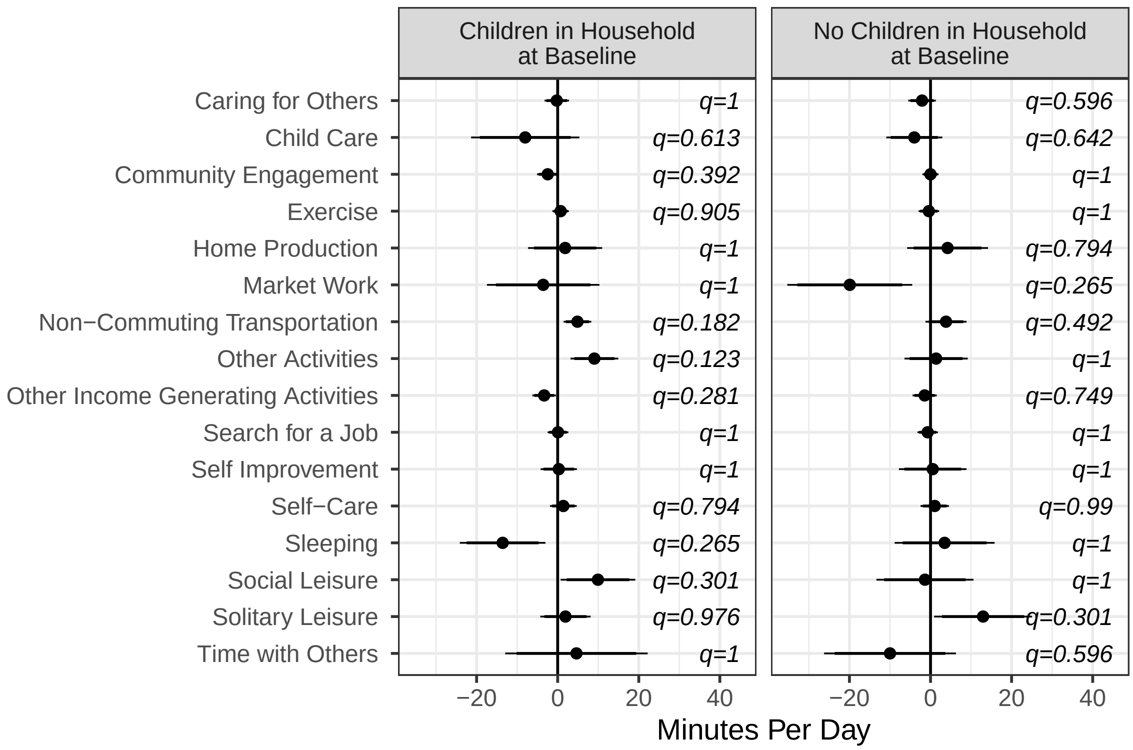
This figure shows the results from the mobile phone app, using GPT to recode open-ended responses. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.9: Time Use Results: Enumerated and Quarterly Surveys



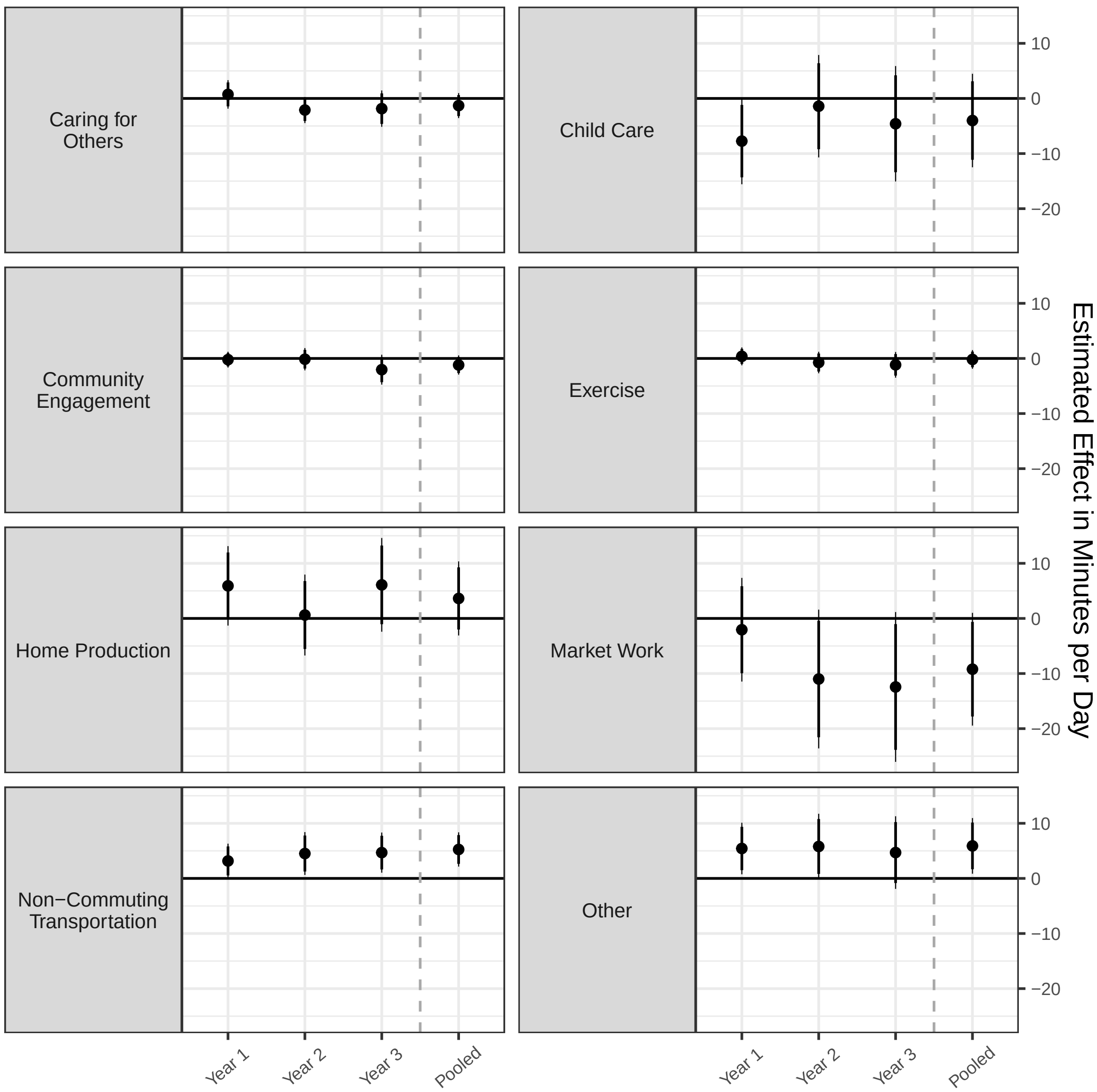
This figure shows the results from the enumerated and quarterly time use surveys. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.10: Time Use Results: Mobile App - By Children in Household at Baseline



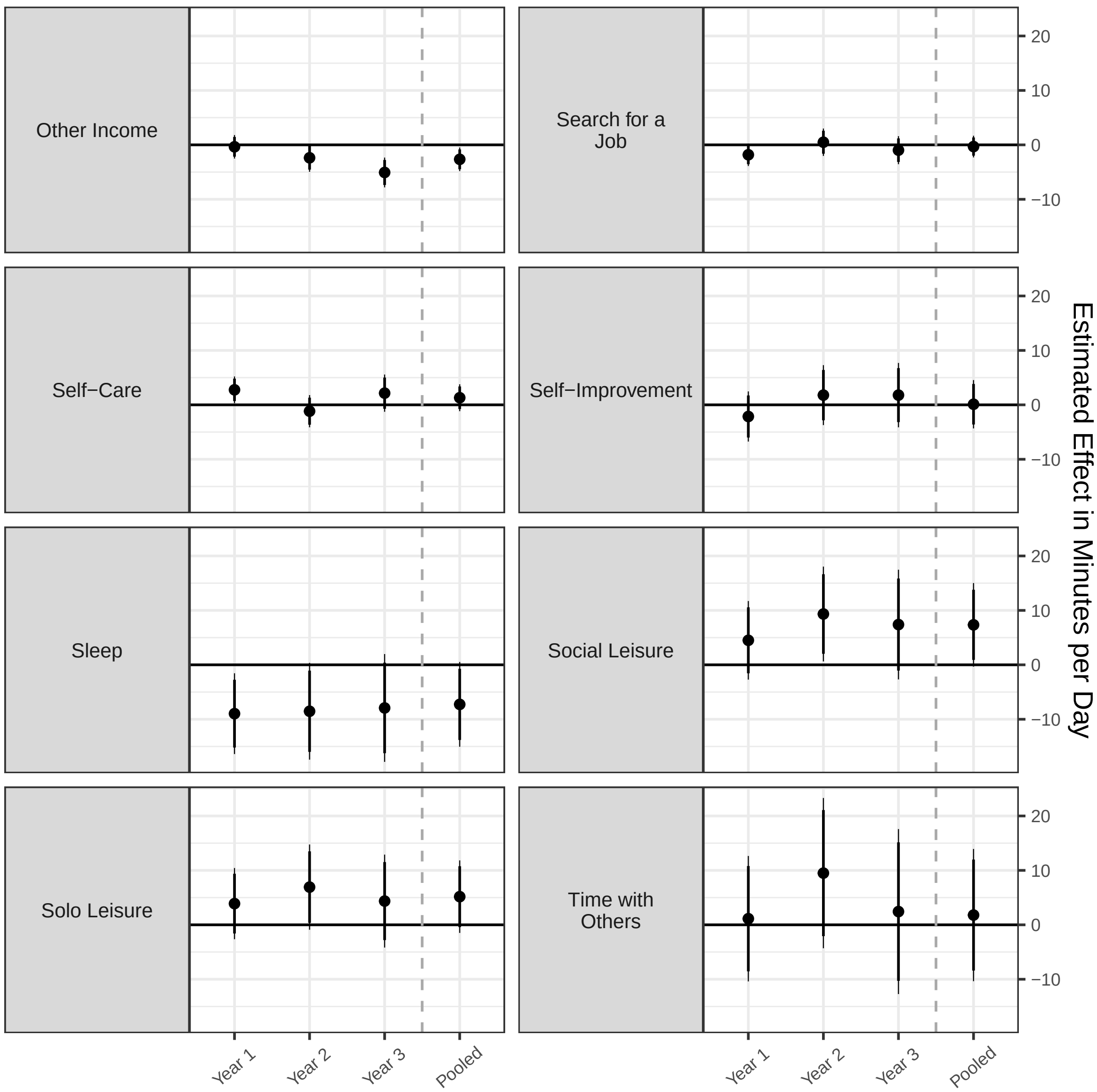
This figure shows the results from the mobile phone app, by whether participants had children in the household at baseline. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.11: Results for Time Use by Time Period: Mobile App (1)



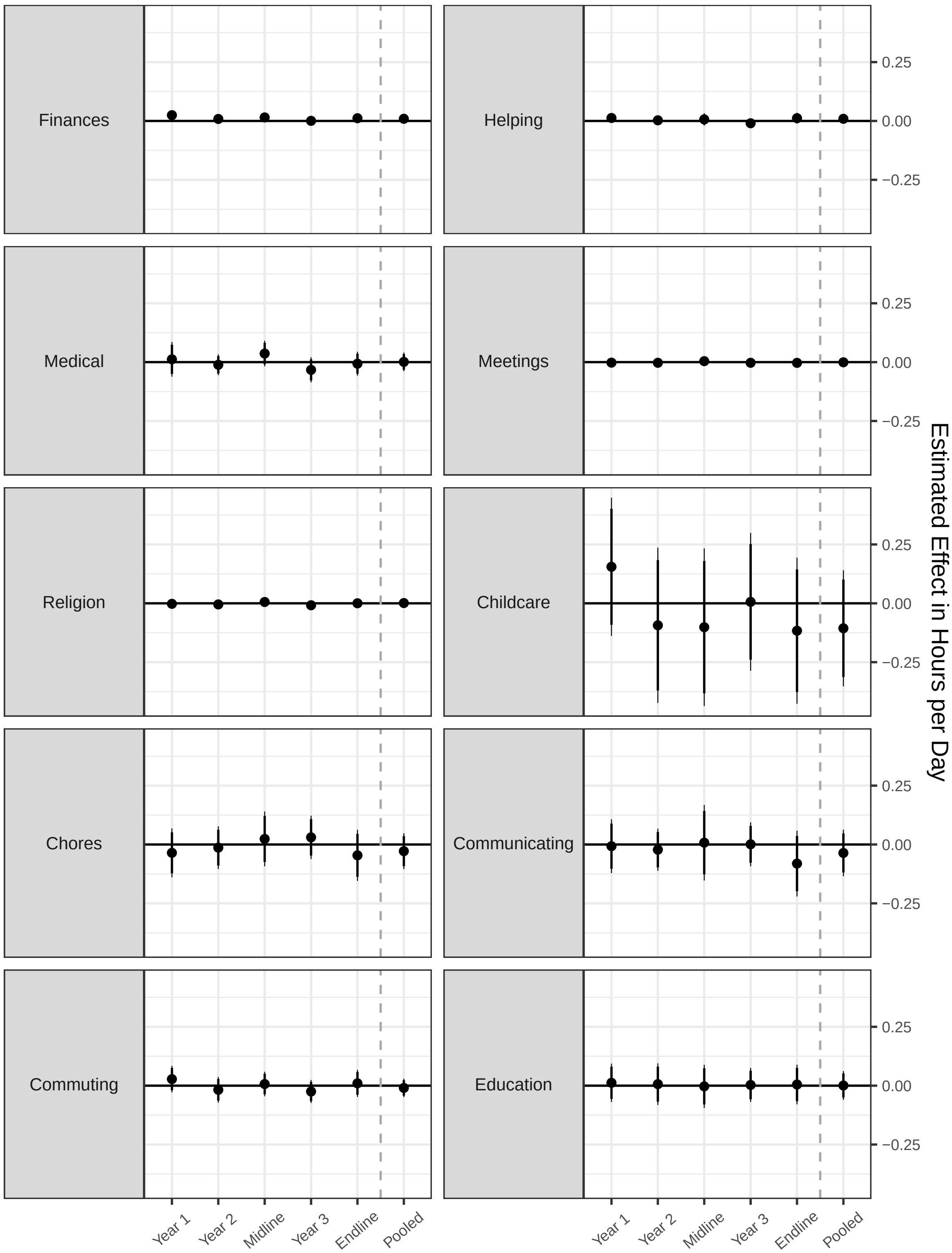
This figure plots the results for time use over time, using data from the mobile app. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.12: Results for Time Use by Time Period: Mobile App (2)



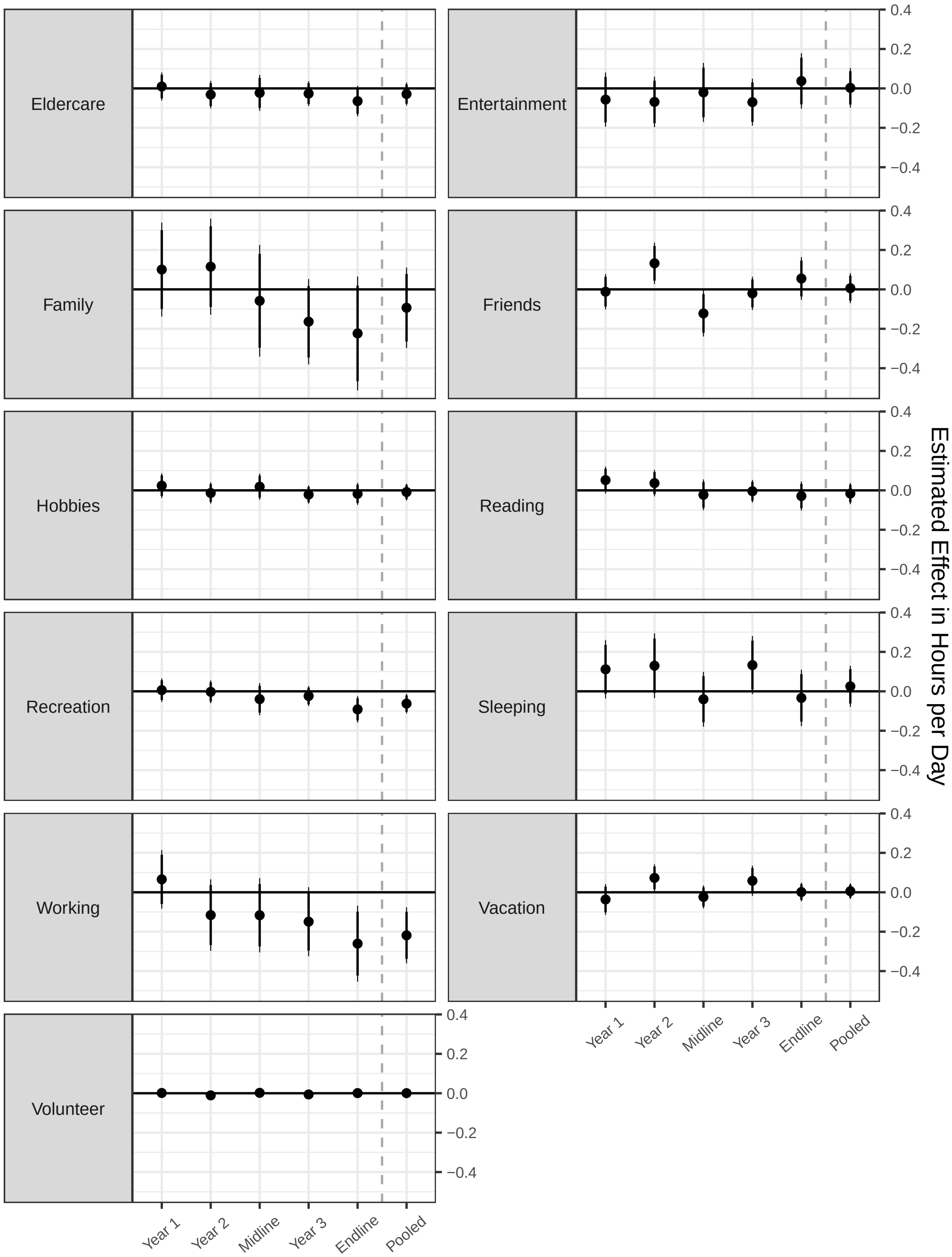
This figure plots the results for time use over time, using data from the mobile app. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.13: Results for Time Use by Time Period: Enumerated and Quarterly Surveys (1)



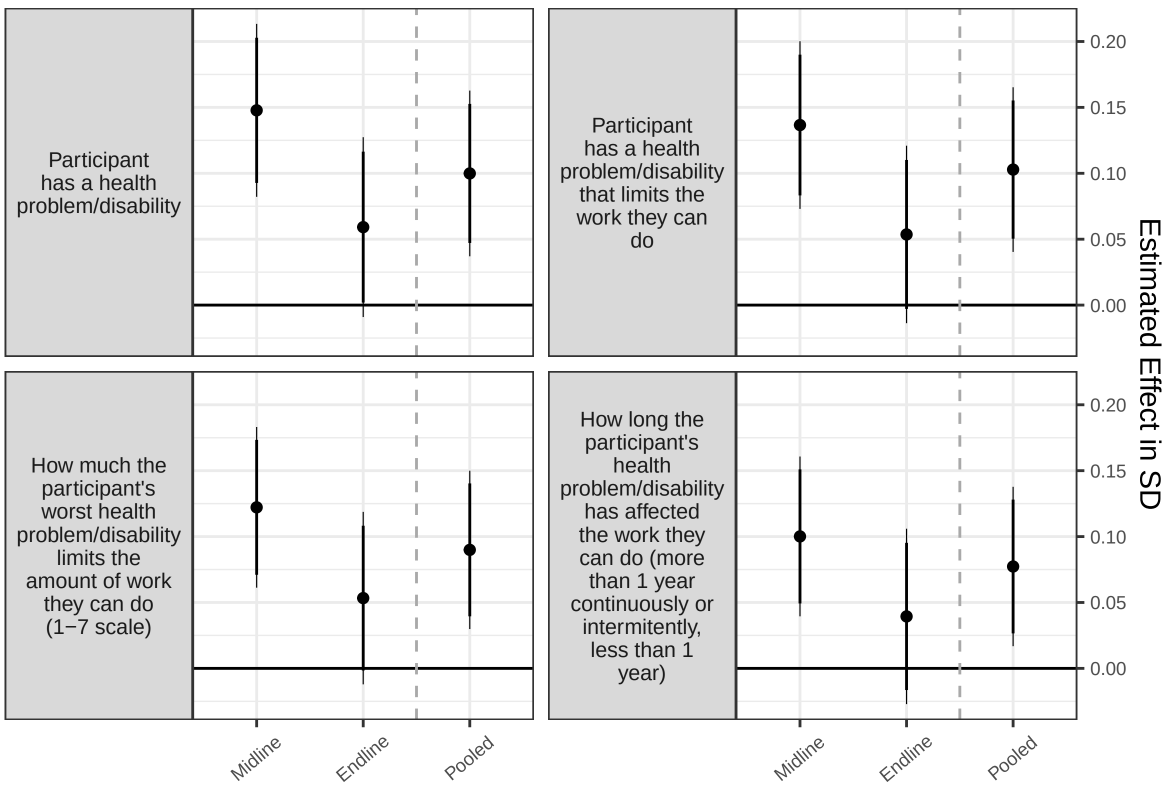
This figure plots the results for time use over time, using data from enumerated and quarterly surveys. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.14: Results for Time Use by Time Period: Enumerated and Quarterly Surveys (2)



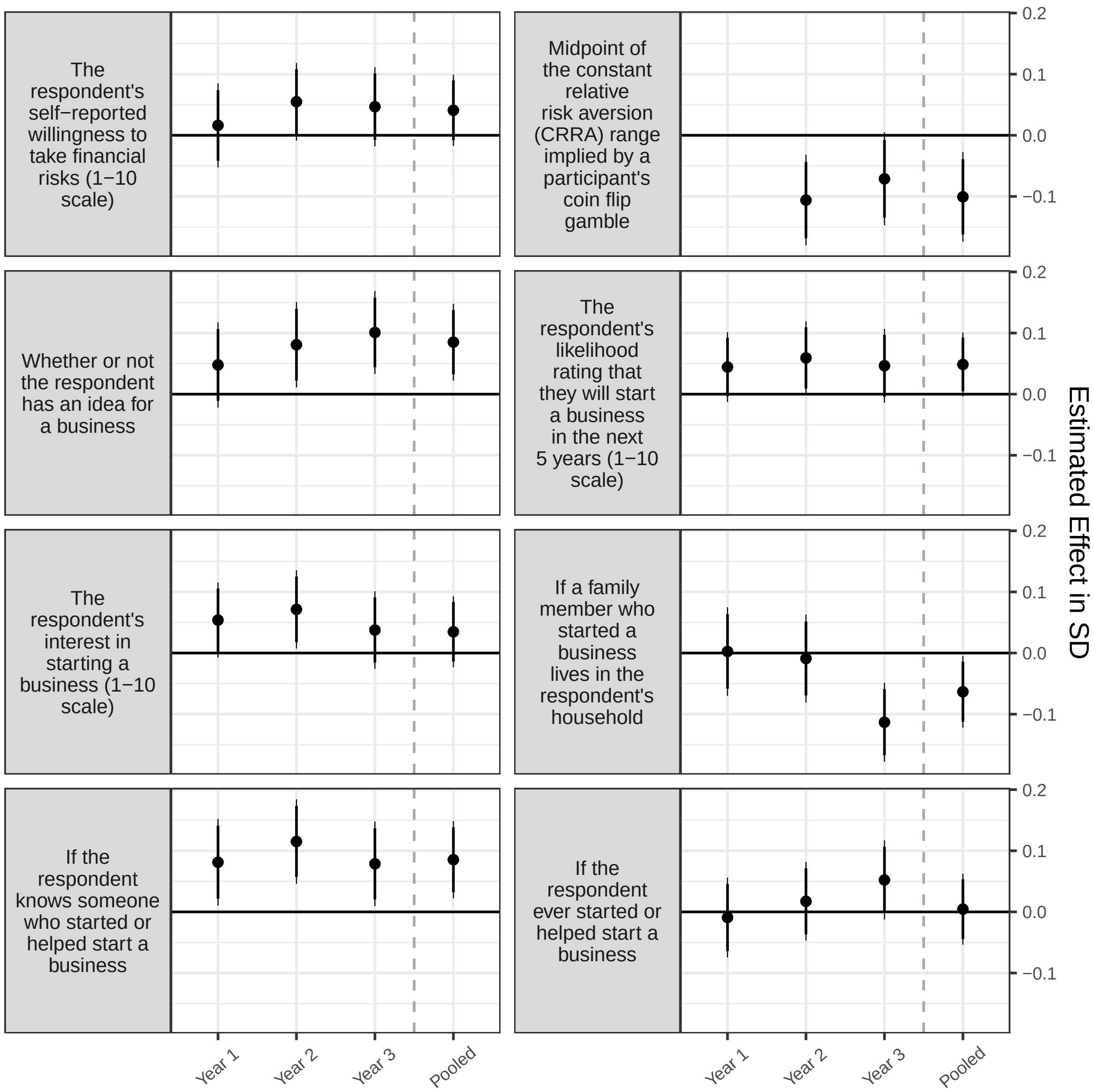
This figure plots the results for time use over time, using data from enumerated and quarterly surveys. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.15: Results for Disability by Time Period



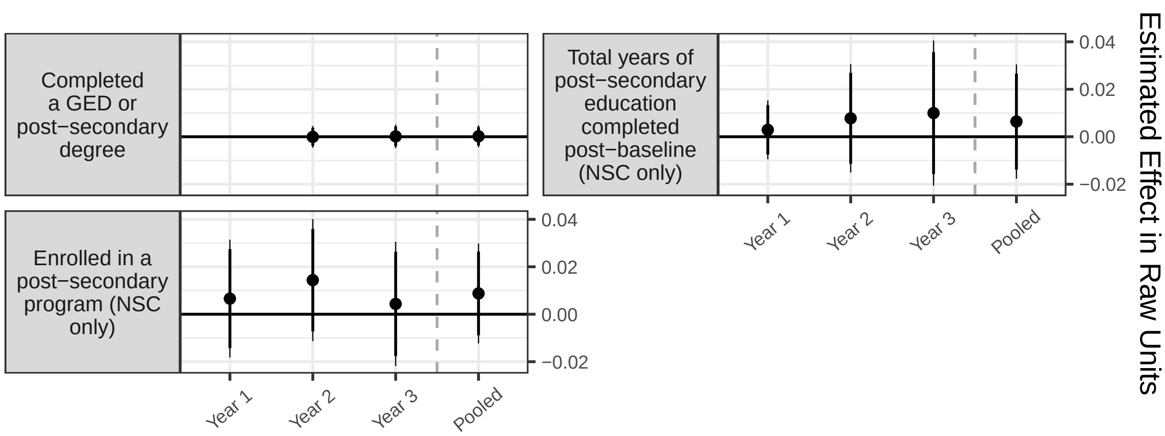
This figure plots the results of the estimates of the transfers on disability over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.16: Results for Entrepreneurship by Time Period



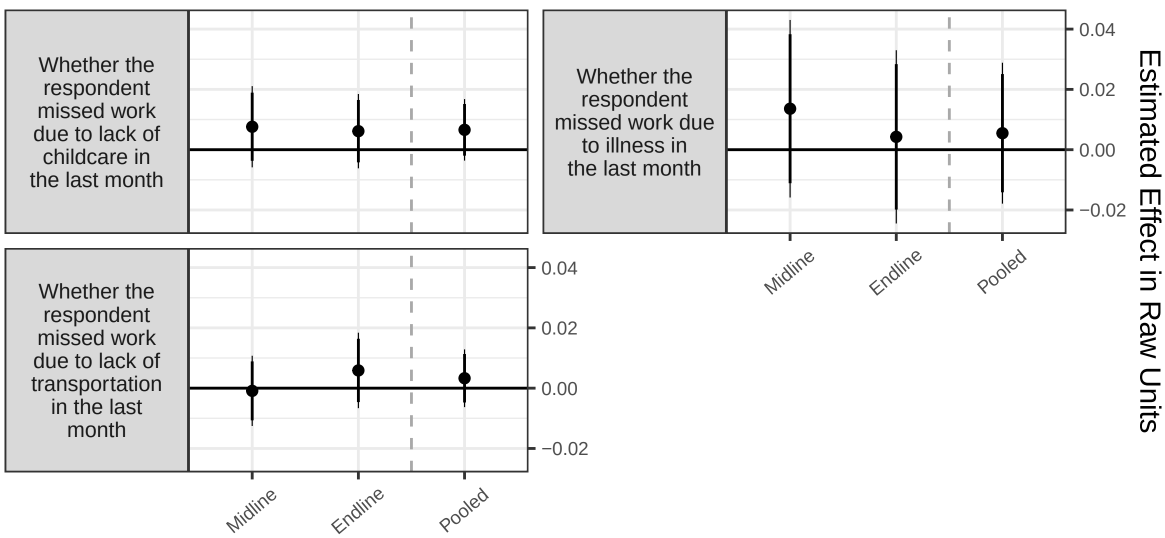
This figure plots the results of the estimates of the transfers on entrepreneurship over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.17: Results for Human Capital by Time Period



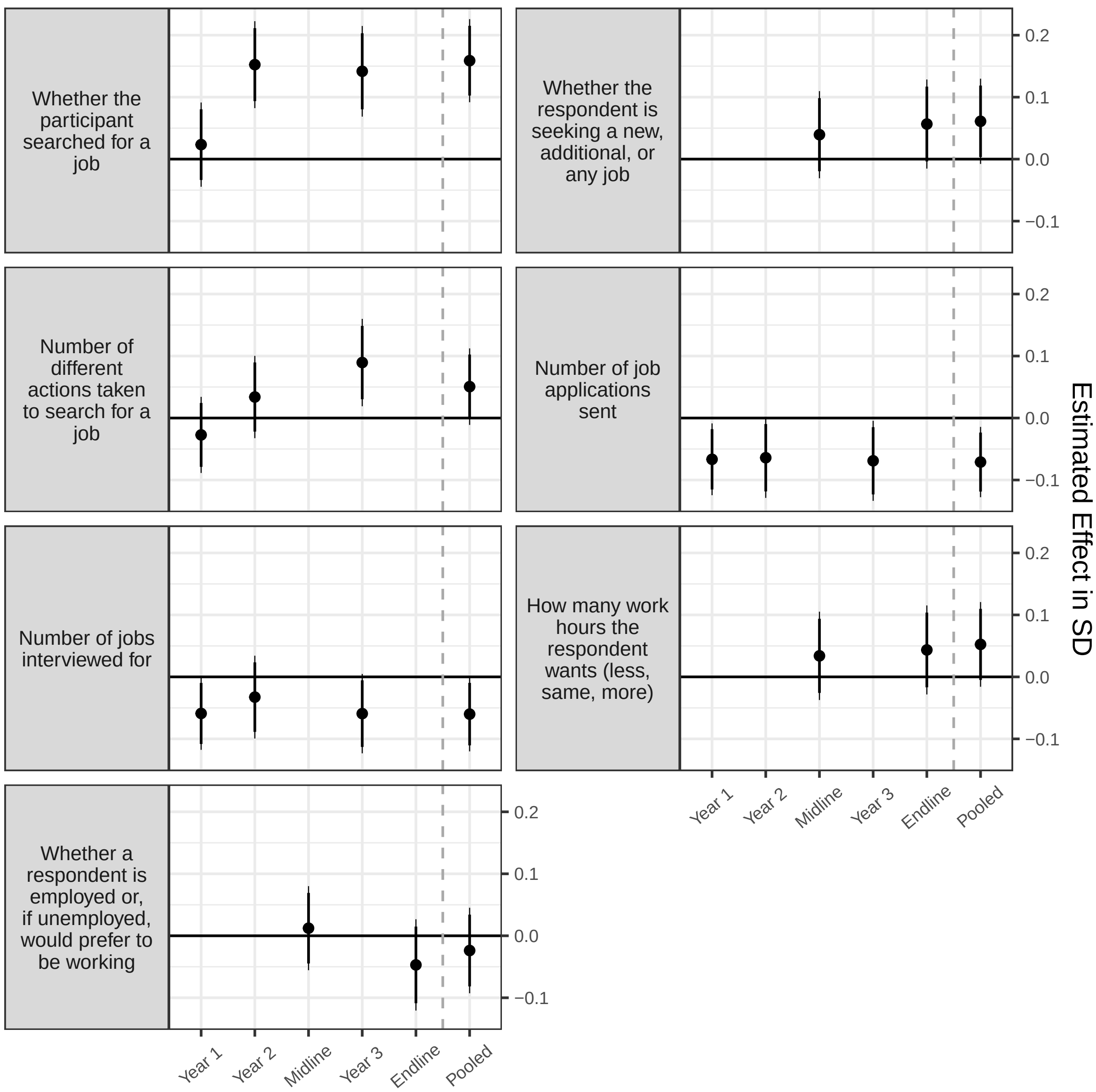
This figure plots the results for human capital over time, showing the point estimates for completion of a GED or post-secondary program trending upwards by the end of the study. There is no value for this variable for Year 1 because participants were only asked about whether they had completed a high school degree or GED in the midline and endline SRC survey. For all outcome variables, data from the NSC were preferred to survey data for those participants that consented to their administrative records being used. For example, for completion of a GED or postsecondary program, GED completion was captured in survey data as it is not in the NSC data, postsecondary program completion was captured in the NSC data for those participants who consented to share these data, and postsecondary program completion was captured in survey data for those participants who did not consent to share NSC data. The other two items in this figure are based on NSC data only. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.18: Results for Barriers to Employment by Time Period



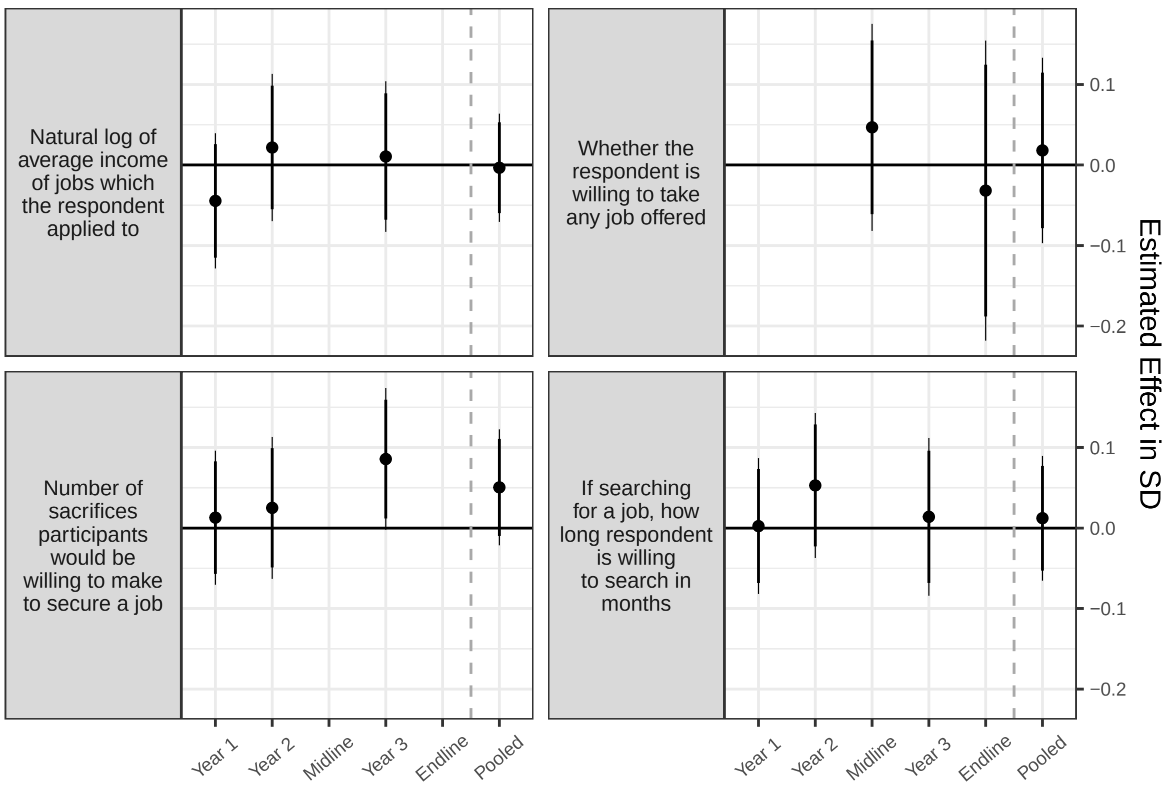
This figure plots the results of the estimates of the transfers on barriers to employment over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.19: Results for Employment Preferences and Job Search by Time Period



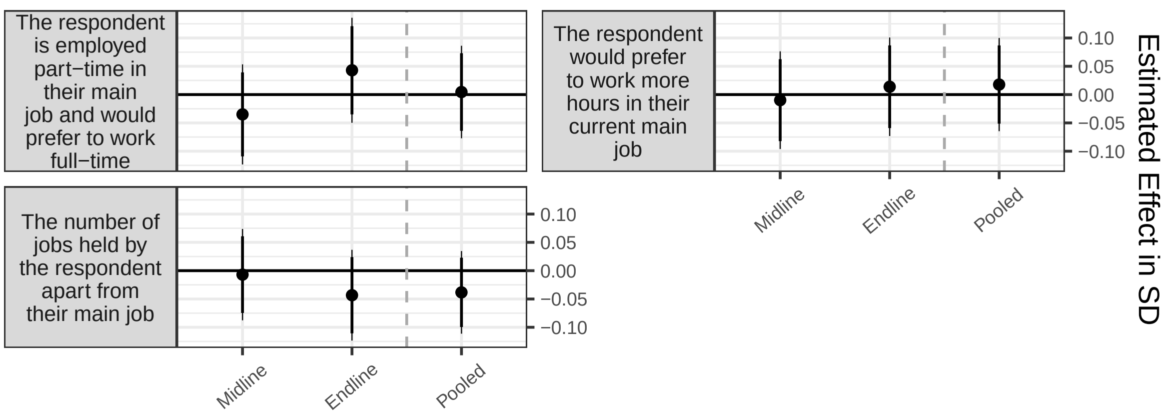
This figure plots the results of the estimates of the transfers on employment preferences and job search over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.20: Results for Selectivity of Job Search by Time Period



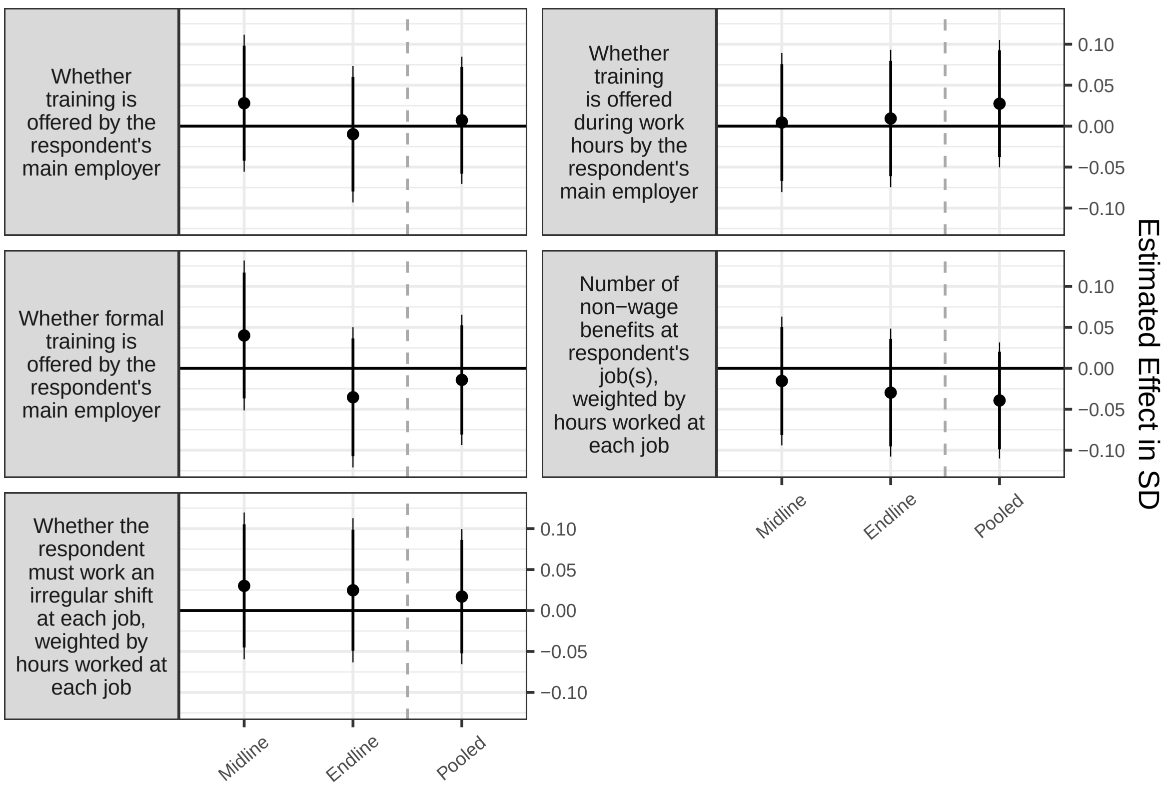
This figure plots the results of the estimates of the transfers on selectivity of job search over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.21: Results for Adequacy of Employment by Time Period



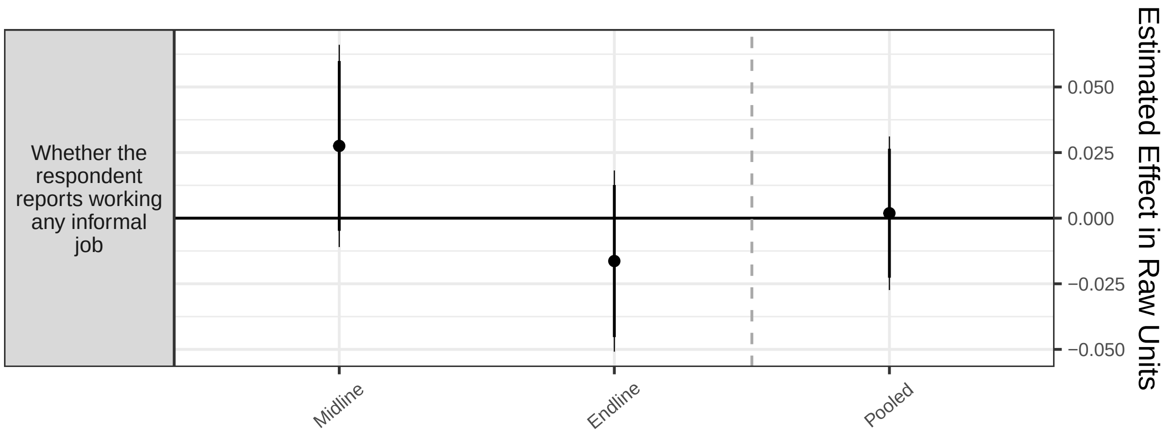
This figure plots the results of the estimates of the transfers on adequacy of employment over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.22: Results for Employment Quality by Time Period



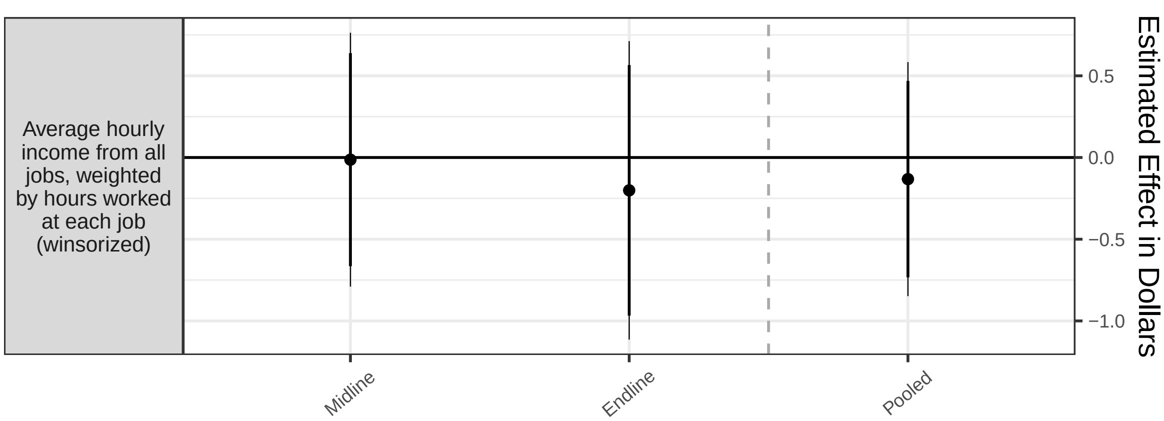
This figure plots the results of the estimates of the transfers on employment quality over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.23: Results for Informality by Time Period



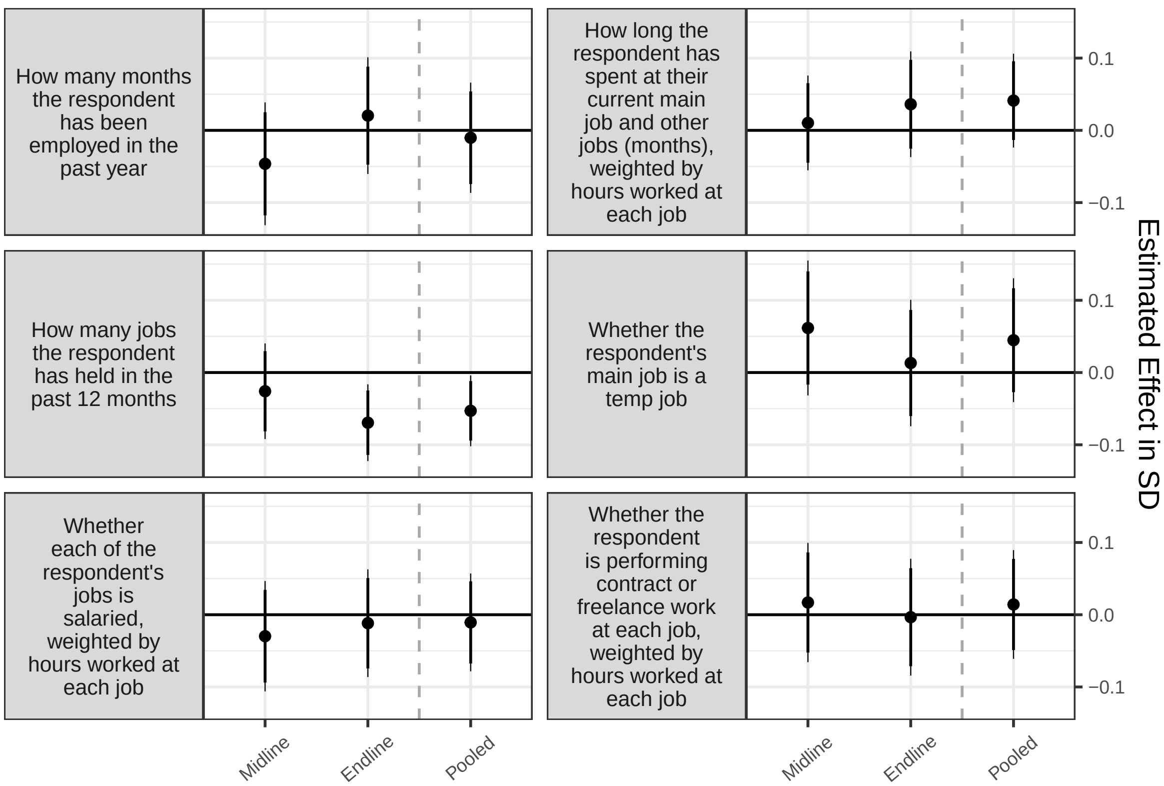
This figure plots the results of the estimates of the transfers on informality over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.24: Results for Hourly Wage by Time Period



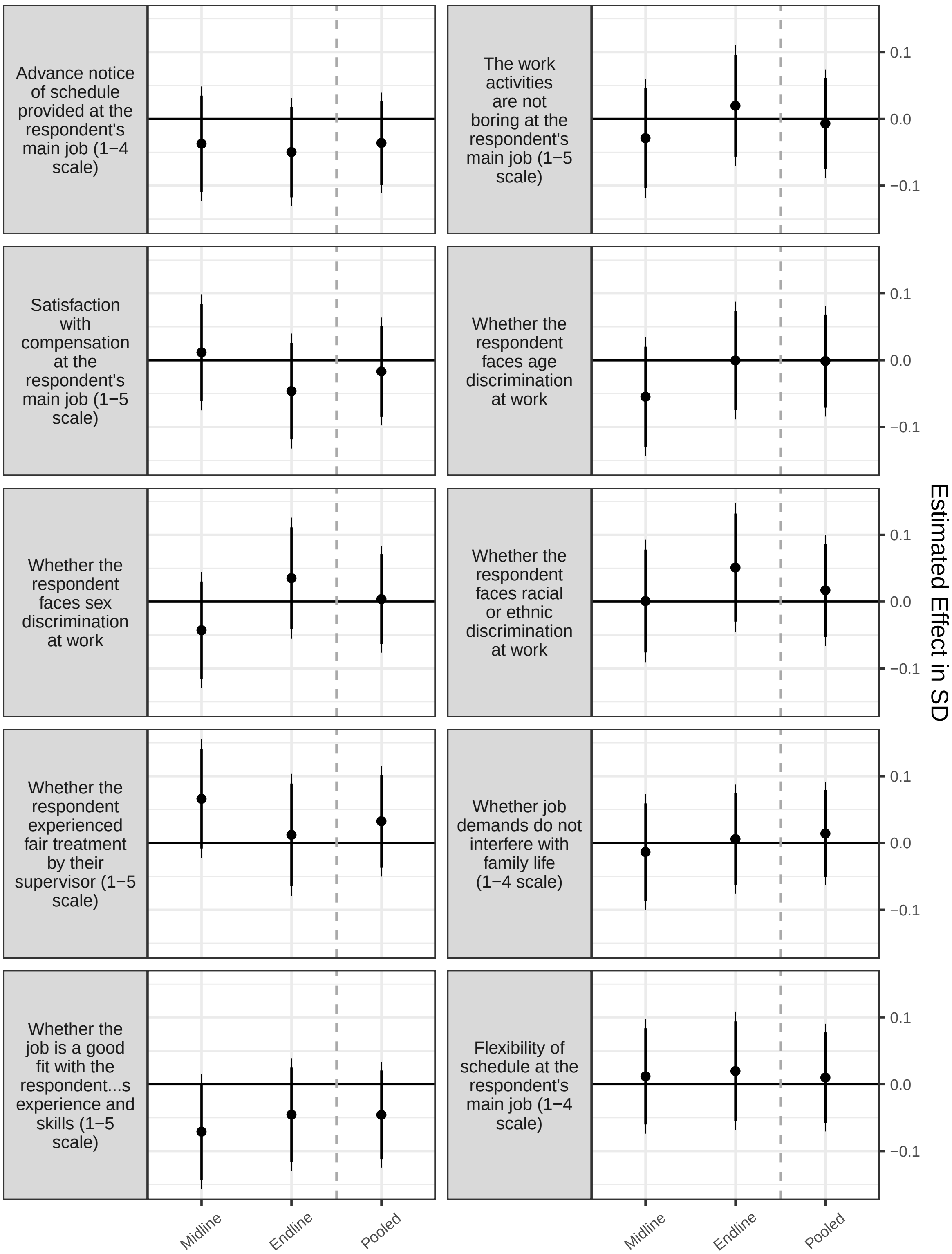
This figure plots the results of the estimates of the transfers on hourly wage over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.25: Results for Stability of Employment by Time Period



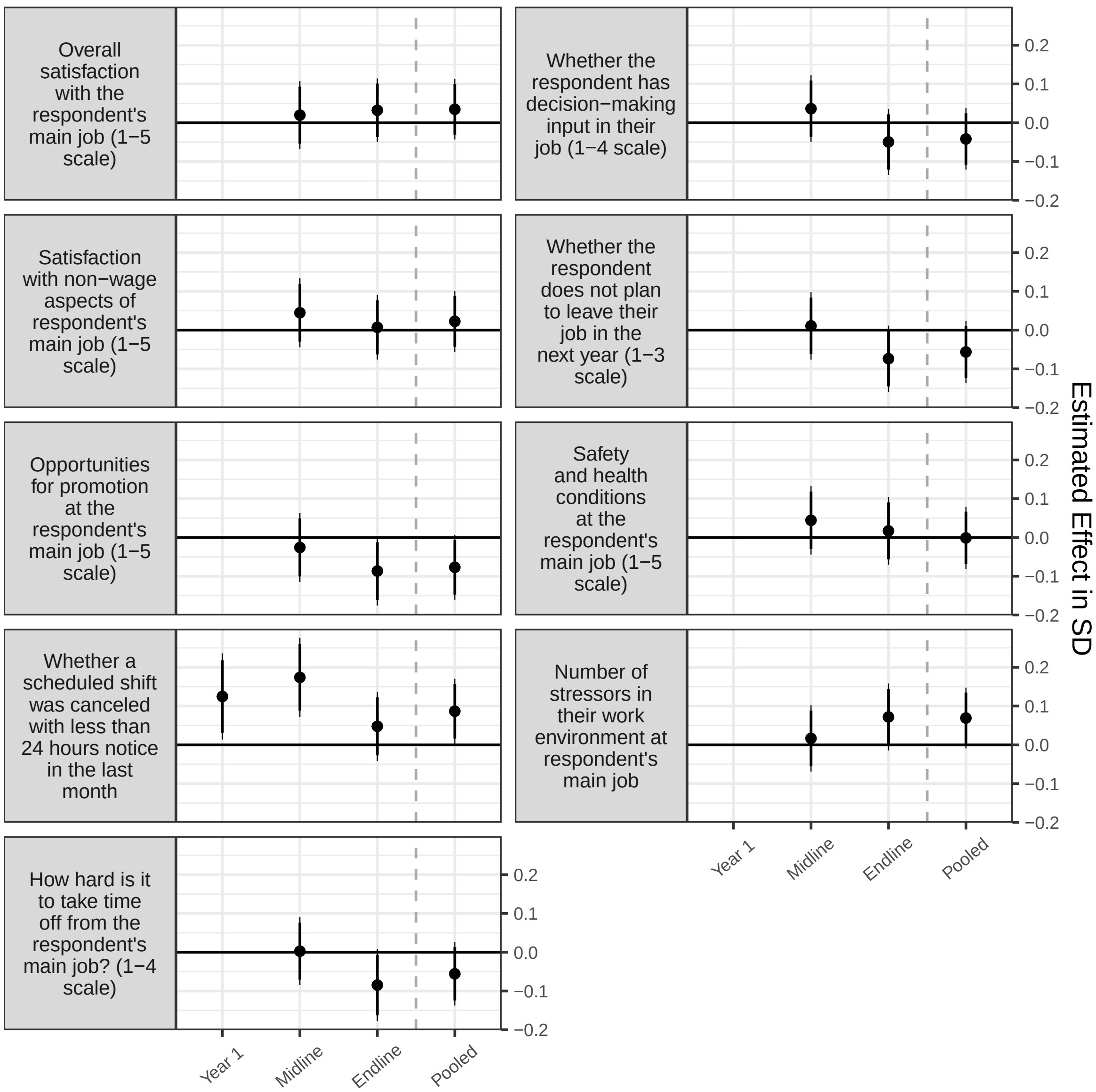
This figure plots the results of the estimates of the transfers on employment stability over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.26: Results for Quality of Work Life by Time Period (1)



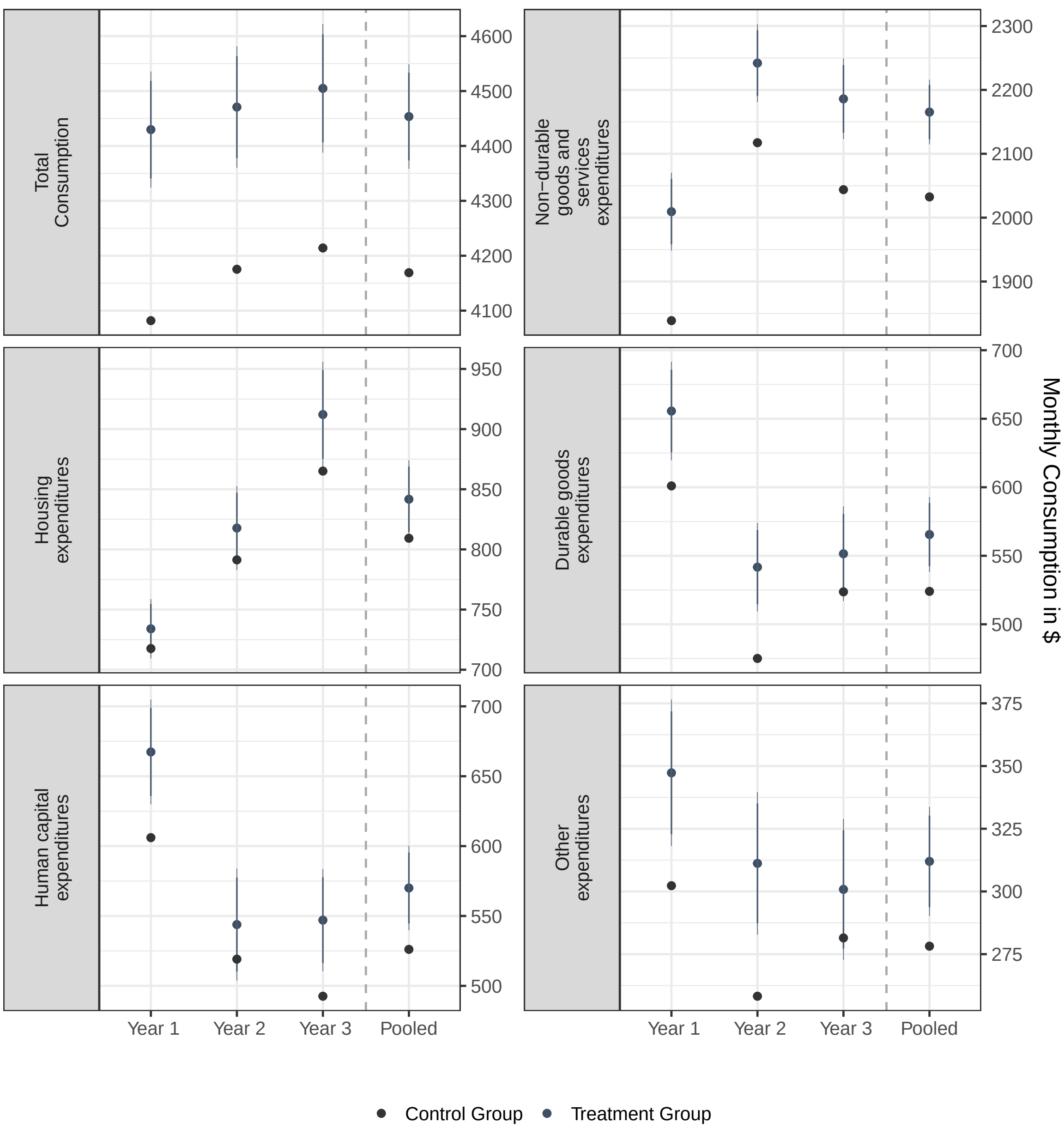
This figure plots the results of the estimates of the transfers on quality of work life over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.27: Results for Quality of Work Life by Time Period (2)



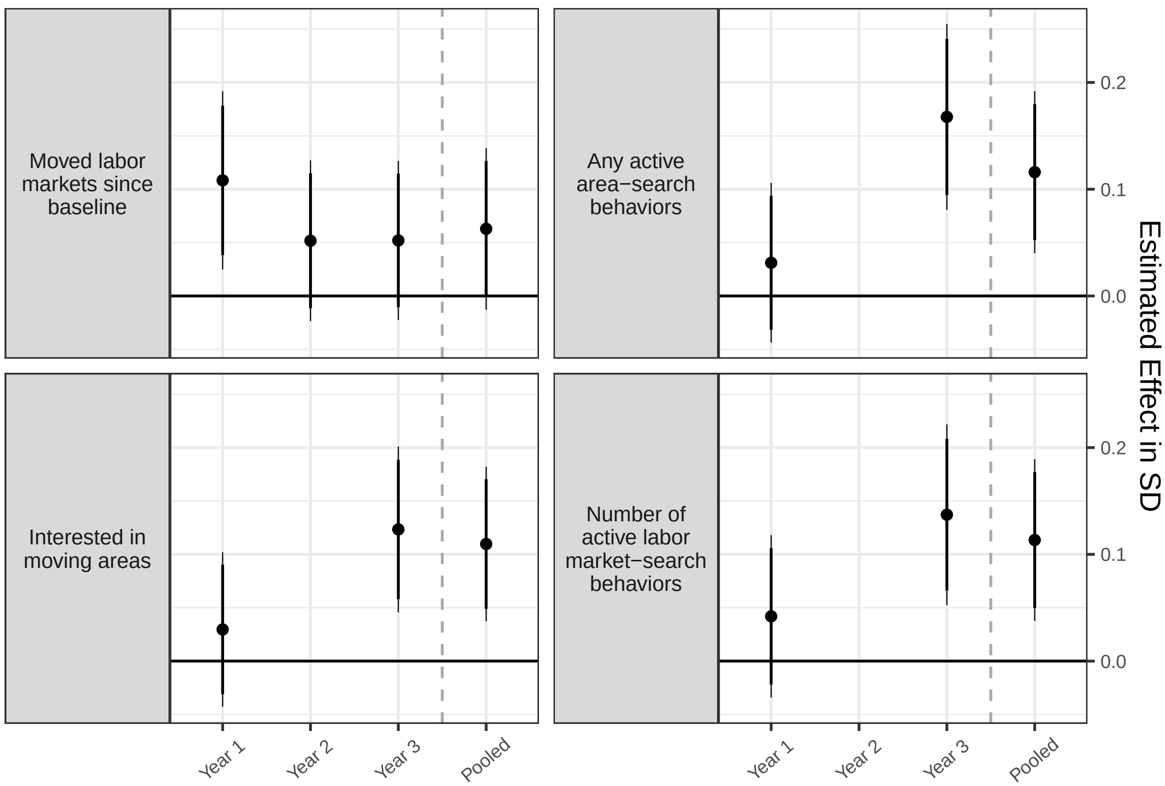
This figure plots the results of the estimates of the transfers on quality of work life over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.28: Results for Consumption by Time Period



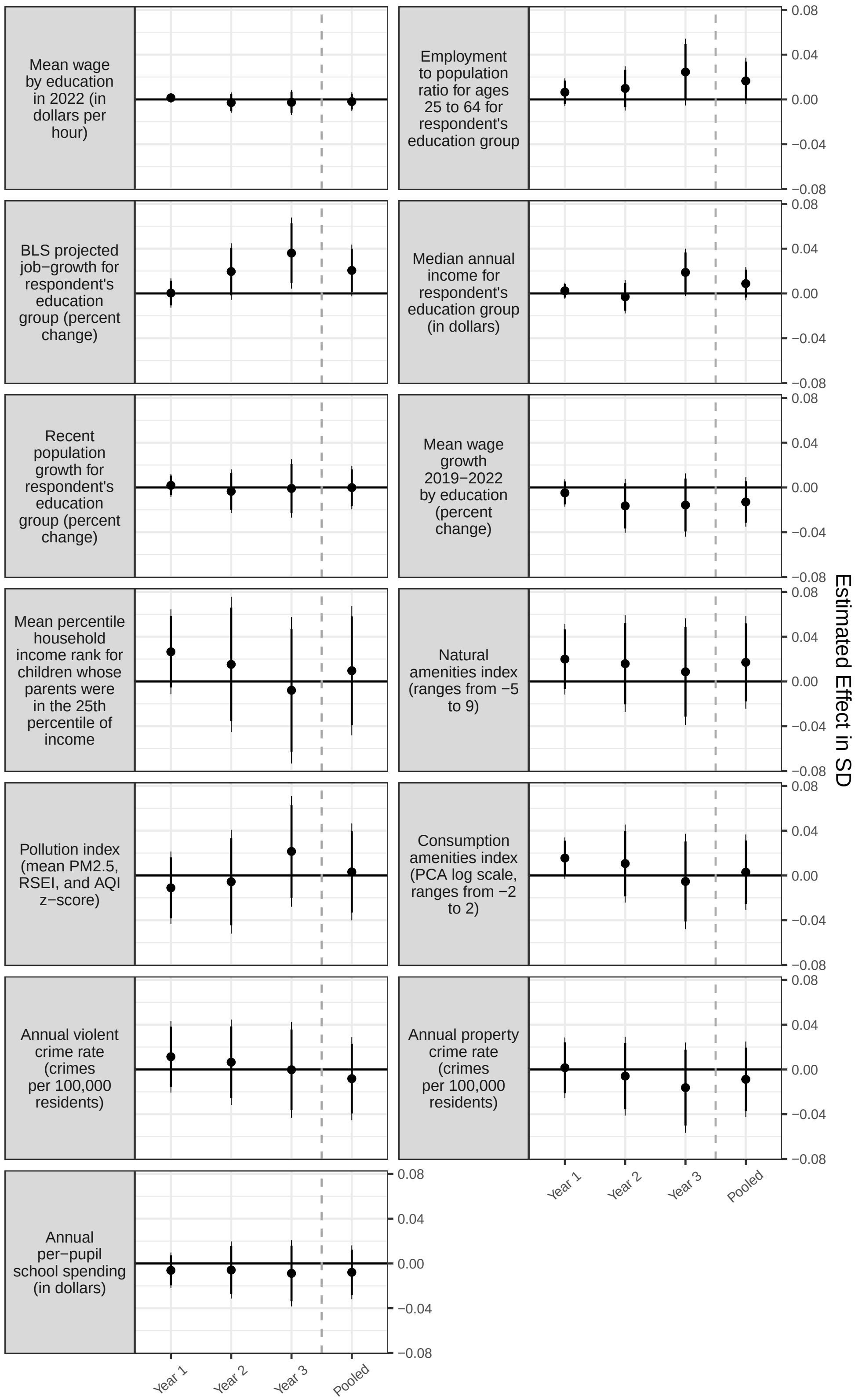
This figure plots the results of the estimates of the transfers on consumption over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.29: Results for Labor Market Mobility by Time Period



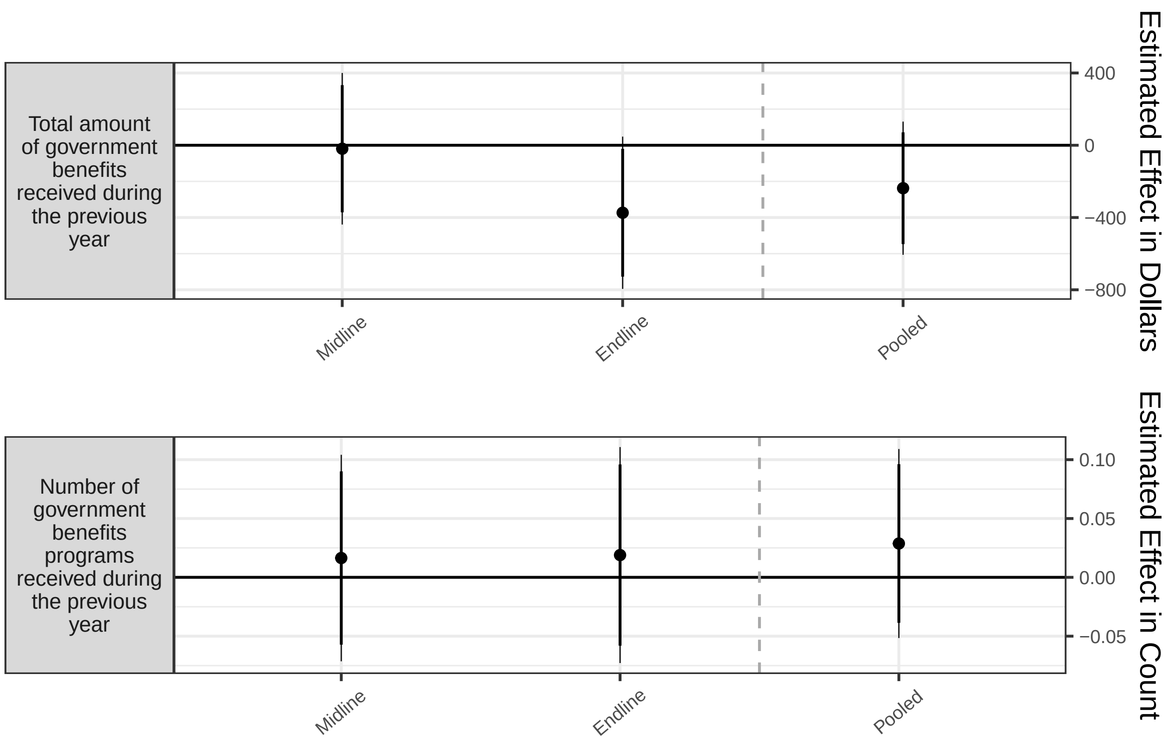
This figure plots the results of the estimates of the transfers on labor market mobility over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.30: Results for Quality of Labor Market by Time Period



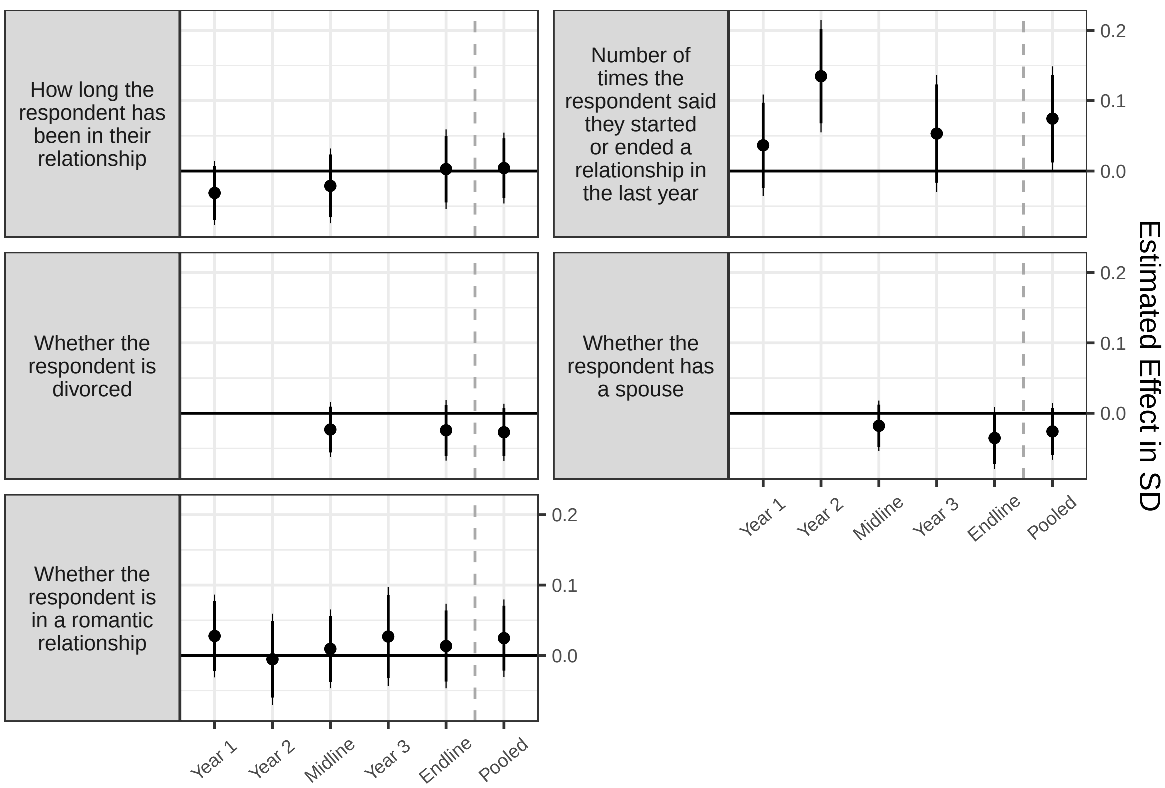
This figure plots the results of the estimates of the transfers on quality of labor markets over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.31: Results for Benefits by Time Period



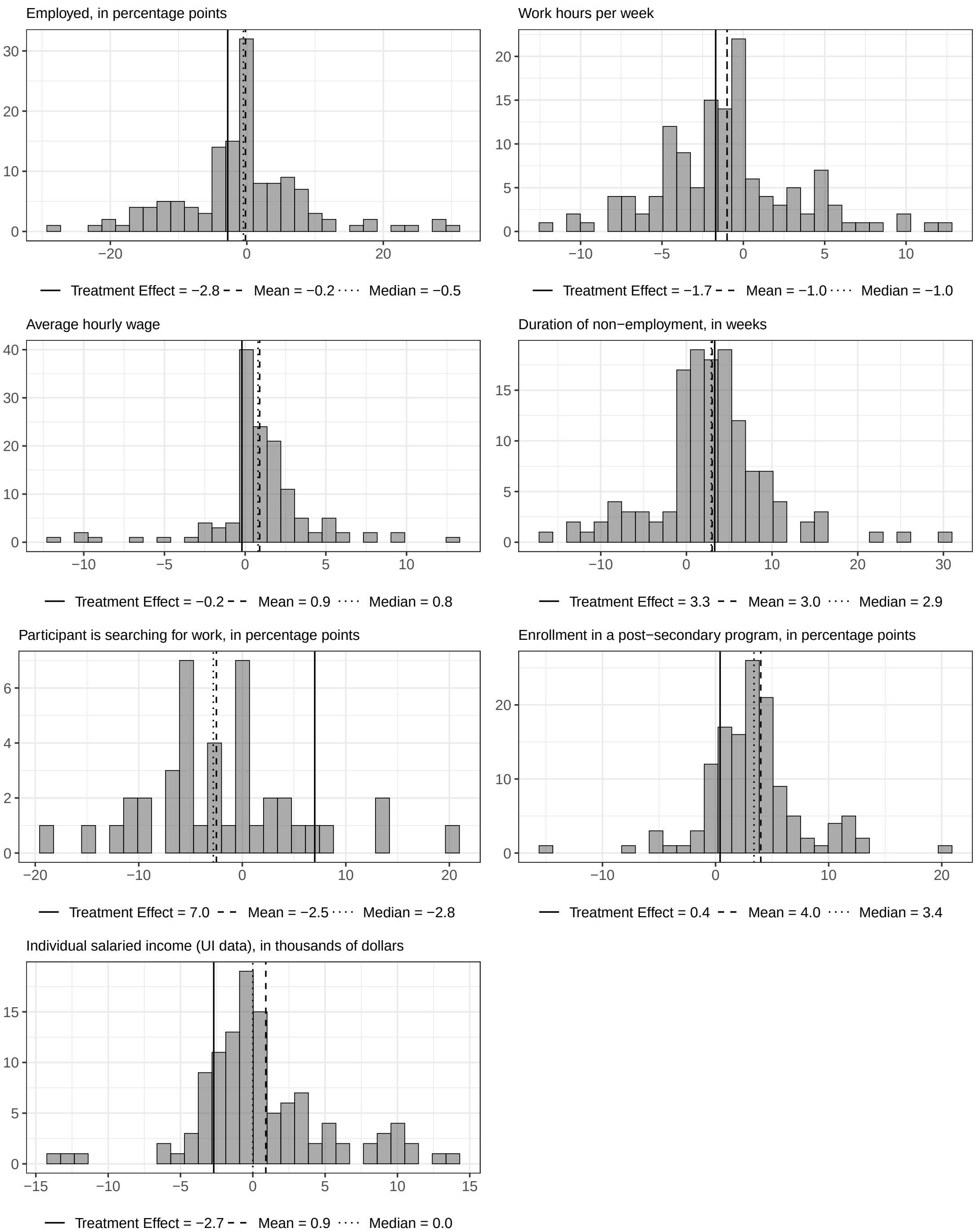
This figure plots the results of the estimates of the transfers on benefits over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.32: Results for Relationship Status by Time Period



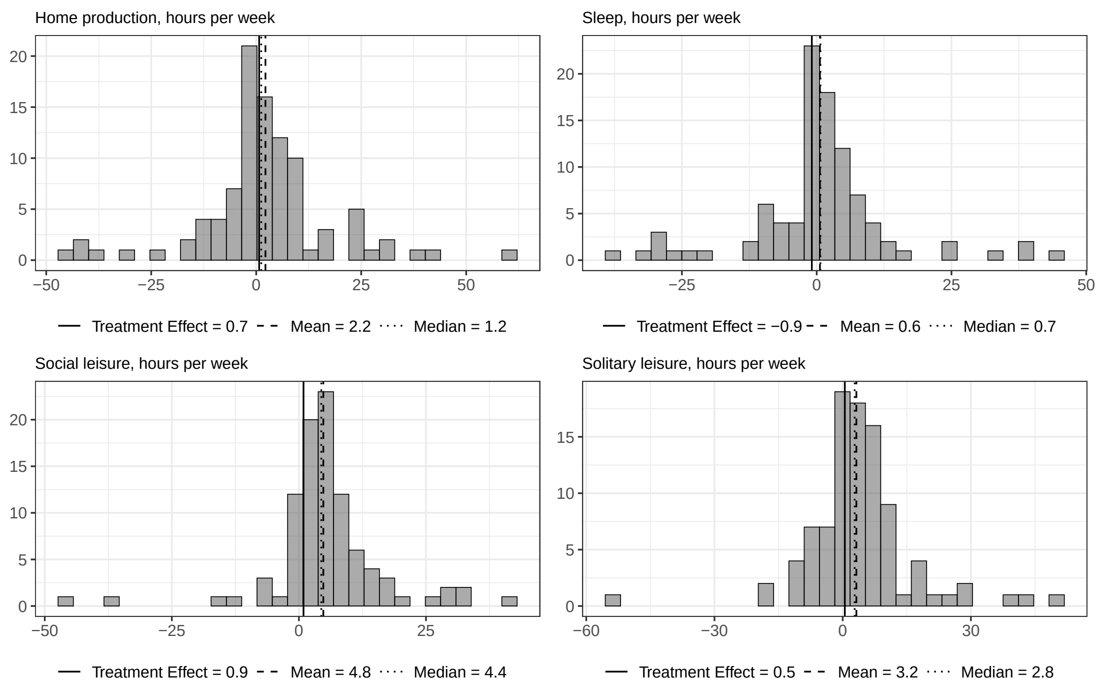
This figure plots the results of the estimates of the transfers on relationship status over time. Thicker line segments plot the 90% confidence intervals, while the full length of the lines plot the 95% confidence intervals.

Figure B.33: Forecasts of Employment Outcomes



These figures show the full distribution of forecasts provided by NBER affiliates and users of the Social Science Prediction Platform. A few rare outliers more than two SD from the mean are omitted from these figures for the sake of legibility.

Figure B.34: Forecasts of Time Use Outcomes



These figures show the full distribution of forecasts provided by NBER affiliates and users of the Social Science Prediction Platform. A few rare outliers more than two SD from the mean are omitted from these figures for the sake of legibility.

C Details on Recruitment and Randomization

When targeting our mailers and ads, we aimed to generate a sample that was diverse along several dimensions. First, we aimed to recruit a sample that was representative by geographic type (large urban, medium-sized urban, rural, and suburban) based on the county of the applicant. We identified 1-5 counties of each type in each state that were demographically representative of this type. Nationally, roughly 19% of households that meet the eligibility criteria for our program live in rural areas, 35% live in suburban areas, 17% live in medium-sized urban areas, and 28% live in large urban areas.[2] Our goal was to recruit a sample that matched these population shares, although we ultimately somewhat oversampled large urban areas to reduce recruitment costs. We also aimed to over-represent low-income participants and to approximately match the eligible population's share of male and female individuals.

We employed a block randomization procedure to recruit eligible individuals to the main study sample from the pool of applicants, with those in some blocks having a higher likelihood of being selected in the first randomization. Table I 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.

For the second randomization, after enrollment and the baseline survey was complete, individuals had an equal 1 in 3 probability of being assigned to treatment or control. Strata were formed according to participants' race/ethnicity (non-Hispanic Black, Hispanic, and non-Black and non-Hispanic), income group (0-100% FPL, 101-200% FPL, 201-300% FPL), and state (IL or TX). A separate strata contained all 20 clusters with more than one individual in them.

Participants were grouped within strata into blocks of three based on similarities across pre-treatment covariates.[3] One cluster per block was selected to be in the treatment group and the other two in the control group.

All participants took up the treatment. A waitlist had been developed, but only one person was enrolled from the waitlist to replace a participant in the treatment group who was removed from the

[2]Less than 1% live in small urban counties so we exclude this group.

[3]After blocking, some clusters were "left over" if the number of clusters in a strata did not divide evenly by three. A second round of blocking was performed for these clusters, again forming blocks based on similarity across pre-treatment covariates.

program for violating program rules regarding a threat of harm to another person. Since we had 99.9% compliance, we analyze the experiment using intent-to-treat, following the original random assignment.

D Balance Tests and Simulations

We assigned a minimum critical p-value for each variable in a set of important baseline covariates, such that any differences between the treatment and control group could not be significant at that level. A randomization which failed to meet the p-value threshold for any baseline covariate was rejected.

We also tested whether any set of baseline covariates within a given outcome area was jointly significant. A randomization in which the p-value of any such F-test was under 0.25 was rejected.

In theory, our strategy could result in some participants being more likely to be assigned to the treatment than others if they have particularly large or small values of some baseline variable. Therefore, we conducted 1,000 simulations to check that our randomization process resulted in every cluster having a 1 in 3 chance of being in the treatment group. A histogram of these simulations is provided in Figure B.3, and Figure B.4 shows a quantile-quantile plot of this distribution against what one would expect from Bernoulli coin flips with a 1 in 3 chance of being assigned to the treatment group. These figures indicate that the observed distribution of treatment assignment probabilities is no different from what we would expect by chance.

E False Discovery Rate

We compute false discovery rate (FDR) q-values within families of outcomes, following Benjamini and Hochberg (1995). Our hypothesis tests are placed into tiers (denoted K0, K1, K2, K3, and K4) as follows, corresponding with our prioritization of the tests:

- K0: Family-level estimates pooled across time. The q-values for these items will be computed using all the K0 items across families in a paper.
- K1: Component-level estimates pooled across time. The q-values for these items are computed using the K0 and K1 items in the outcome's same family.
- K2: Primary item-level estimates pooled across time. The q-values for these items are computed using the K0, K1, and K2 items in the outcome's same family.

- K3: All other estimates ("exploratory" tier). This includes family-level, component-level, and item-level estimates which are computed within each time period, estimates on items pre-specified as secondary or tertiary, and all tests of heterogenous treatment effects, as well as descriptive analyses. The q-values for these items are computed using the K0, K1, K2, and K3 items in the outcome's same family.

- K4: Any post hoc comparisons conducted after filing these pre-analysis plans (*e.g.*, in response to referee comments). The q-values for these items are computed using the K0, K1, K2, K3, and K4 items in the outcome's same family.

In some families, there is only one item pre-specified to be in the index for a given component, or only one component in the family. In these cases, we use one fewer "level" in the FDR adjustment (*e.g.*, if there is only one item in a component, it would not be adjusted with K2, as it would already have been adjusted at the K1 level for that component. If there is only one component in a family, that component is counted as K0, primary items are counted as K1, secondary items are counted as K2, etc.). Secondary and tertiary items both fall into K3, with the exception that in some pre-specified cases we distinguish between secondary and tertiary items; this effectively pushes K3 items to K4 and K4 items to K5, so the tertiary items can be in their own tier. The pre-analysis plan offers further details.[4]

Table A.7 summarizes the FDR tiers of our estimates.

F Relationship to Other Papers

It should be noted that the analyses in this paper come in part from six different pre-analysis plans that focus, alternatively, on employment; income and financial health; time use; intrahousehold outcomes; psychosocial measures; and housing and geographic mobility. While we did not know at the time of registering the pre-analysis plans which outcome variables would be included in which papers, we pre-specified that we would conduct our multiple hypothesis corrections according to how the tests were originally registered. For example, if one family of outcomes from the "income, expenditures and financial health" pre-analysis plan was included in the paper based primarily off results from the "employment" pre-analysis plan, that family of outcomes would be subject to FDR corrections alongside the other tests in the "income and financial health" pre-analysis plan. This measure ensured

[4]Of note, unlike the other outcomes, time use outcomes were pre-specified to not be placed into components or families. Instead, we pre-specified that the item-level estimates pooled across time would be K0 primary hypothesis tests and the item-level estimates at each time period would be K1 hypothesis tests.

that there was no incentive to selectively combine outcomes into papers in such a way as to make results appear more significant.

Readers are also referred to Bartik et al. (2025), Broockman et al. (2024), Krause ⓡ al. (2025) and Miller et al. (2025) for information on household finance, political, children's, and health outcomes.

G Changes from the Pre-Analysis Plan

The pre-specified analyses were closely followed, however, there were a few instances in which we made a small change.

The first set of changes were made prior to receiving midline survey data. At this stage, the following small changes were made:

– We specified a few supplementary tests, outside of the index, relating to considering whether to model the household as following the unitary household model;

– Whether participants were looking for a job in the last 3 months was added as a primary item to the active search component of the Employment Preferences and Job Search family. This was later phrased in the pre-analysis plan as whether someone was looking for a job in the last year (the question always asks about the last 3 months, and the responses are averaged to aggregate up to the year);

– We added more specificity as to how the descriptive conditions under which a respondent would take a job measure would be treated for the purpose of multiple hypothesis testing corrections and specified that a participant's subjective expectations as to when they would find a job would be a secondary outcome;

– We added as a primary measure whether the participant would be willing to take any job and the reservation wage under the Selectivity of Job Search family;

– We specified that the items under the Employment Quality and Stability of Employment components under the Quality of Employment family would all refer to both main and other jobs. Previously, some of the items had referred to the main job and some to any job;

– In the Stability of Employment component in the Quality of Employment family, we look at how many jobs participants have held in the last 12 months, rather than any longer time period, given that the longer time periods asked about could overlap with the pre-treatment time period;

- We added how hard it is to take time off and whether a scheduled shift was canceled with less than 24 hours notice in the last month as primary items under the Quality of Work Life component under the Quality of Employment family;
- The index value for human capital formation was specified to, as an exception, be a binary measure in each time period indicating receipt of any education or job training in the survey or NSC data (the NSC data had not been collected yet, nor any post-treatment survey data relevant to this question);
- We specified that informal educational outcomes would be considered exploratory;
- The Take-Up of Benefits family of outcomes was added (within the income, expenditures and financial health outcomes pre-analysis plan);
- A satellite measure of PM2.5 was added to the Quality of Labor Market family of outcomes (within the mobility and housing pre-analysis plan);
- Participants had been given the option to report "other" expenditures in the baseline survey, with a free text entry field. Based on an examination of this field, we added questions about spending on pets, gambling, and debt payments to future surveys and integrated these items into our existing categories of expenditures;
- We added more specificity to how we would combine outcomes into indices, specifying that primary items would be combined into components using seemingly unrelated regression;
- We specified that we would use the FDR, following Allcott et al. (2020), rather than performing family-wise error rate corrections.

Additional exploratory analyses and robustness checks, including additional subgroup analyses, were also specified.

After receiving the midline survey data, but before receiving the endline survey data, a few additional changes were made:

- We clarified the overall estimation approach that applied to all estimates in the paper, including:
    - We specified that since only one person was enrolled from the waitlist, we would ignore the waitlist in the estimation strategy and analyze the results using an intent-to-treat estimation, given the compliance rate of 99.9%;

- We had previously pre-specified the weights we would place on the different time periods and surveys in how they would be pooled, but we further specified how we would treat missing observations;
- Though the previous version of the pre-analysis plan had specified that the FDR analysis would follow the hierarchical nature of Guess et al. (2023), we more clearly specified the structure of the outcomes with a table;
- We emphasized that the unconditional analyses would be preferred wherever possible. For example, we cannot consider most aspects of quality of employment (such as whether one's manager treats one fairly) for those without jobs, so this family of outcomes is necessarily conditional. However, in other cases we can run an unconditional analysis, such as in the barriers to employment section where we can consider a respondent to miss 0 days of work due to illness if they are not employed.

– Given that the SRC survey version of job search questions were limited to having been asked of those who were employed, and thus could be affected by selection into employment, we specified that we would instead focus on the Qualtrics version of these variables, which would not be subject to this limitation;

– We excluded the reservation wage from the Selectivity of Job Search index given that it would not be available for all individuals;

– There was a potential inconsistency within the Quality of Employment family, where in one place we specified that we would prefer the SRC surveys if there were differential attrition in the Qualtrics surveys and in another place we specified that we would separately present a set of results that were based only on the SRC data as a robustness check. Given that differential attrition looked pretty minor, we kept to the latter rule;

– Under Formality of Employment, the percent of reported income not on W-2s using administrative records for the W-2s and total income from the SRC survey was deemed a robustness check rather than a primary item. No administrative data had been obtained at this time;

– We widened the set of activities considered under informal education;

– We widened the set of measures used to capture pollution under Labor Market Quality;

- We added measures of labor market consumption amenities, the mean hourly wage for respondent's education group and recent wage growth for the respondent's education group to Labor Market Quality;
- We clarified the approach to FDR corrections in the time use topic, given that outcomes were not being combined into components or families;
- We clarified that total individual income, which was "the main measure" of the family in the original pre-analysis plan, would be considered the top-level index value for the sake of FDR adjustments, and that government transfers would be considered descriptive when broken out separately under the Income family of outcomes;
- Questions regarding attitudes towards take-up of benefits were not included in surveys, so this component was removed from the Take-Up of Benefits family.

Other than these changes, we added a few robustness checks and heterogeneity analyses, although these were all pre-specified to be exploratory.

A few other changes were subsequently made based on feasibility/data availability:

- We originally specified an alternative measure of work hours (based off of part-time or full-time employment) that we ultimately did not use as it was only asked once at midline;
- The SRC version of job search questions (under Selectivity of Job Search), which had previously been demoted to a robustness check before midline, were not considered due to their having been only asked to those who were employed and the Qualtrics and SRC versions of one question not being comparable;
- We originally specified an alternative measure of how many work hours the participant wanted, under preferences for employment in the Employment Preferences and Job Search family, that we ultimately did not use as it transpired participants could not indicate that they wanted less work in the specified Qualtrics question;
- Income data for individuals paid per task or with tips was specified as exploratory, as both were subject to error (*e.g.*, if a respondent did not specify the right number of tasks per hour/shift or hours/shifts worked, we would not be able to calculate their total income from tasks). Tasks data appeared more prone to error than tips data, so to avoid under-reporting income for the

few participants paid predominantly in tips, we included tips income in our total calculated individual income measure;

– We could not consider an unemployment-based version of the average duration of non-employment, because we can only clearly distinguish between non-employment and unemployment at the time of the SRC surveys, and the average duration of non-employment variable was pre-specified to be based on both SRC and Qualtrics survey data. As the next best alternative, we created a variable that captured unemployment at the time of the survey, as well as a variable that captured non-employment at the time of the survey, for comparison;

– One item in the Quality of Employment family was only asked to people who were pursuing temp work. As this was answered by very few people, we decided it should be considered a secondary rather than primary item;

– We did not consider descriptive reasons why some participants held more than one job given that few people held more than one job;

– We planned to conduct some exploratory analyses around the unitary household model. However, we determined that participants and their partners may not be identical in terms of baseline characteristics, so we cannot formally test this model. We had also thought that perhaps we could leverage who the mailer was addressed to as an instrument for who applied, but ultimately this was not feasible;

– We had initially pre-specified that we would consider consumption primarily through the enumerated baseline/midline/endline surveys. Given that the response rates to the Qualtrics surveys were higher than anticipated, we decided to use these data alongside the SRC survey data, using the merged variables as our preferred measures. This involved converting the variables to survey year 1, survey year 2, and survey year 3 measures, where the midline survey was considered as part of survey year 2 and the endline was considered as part of survey year 3. Combining the Qualtrics and enumerated survey items required some rescaling and other adjustments to make the measures comparable, *e.g.*, given different lookback periods, a process described in more detail in Bartik et al. (2025). We also allocate unexpected expenses (elicited in other survey questions) to existing expenditure categories and create an "other" component to more fully capture total expenditures;

– Within the Consumption family, housekeeping expenditures were added to non-durables and, rather than focusing on net help given or received, we focus on help given to better conduct the accounting exercise of measuring flows in and out of the household;

– The family-level index for the Labor Supply Elasticity family was originally comprised of two items (employment status and work hours per week) within one component. Given the interest in employment outcomes, we thought it more interpretable to have employment status be our preferred measure for the family, similar to how in the Income family we pre-specified that we would use total individual income as the family-level index. With this promotion of employment status to represent the family-level estimate, only work hours per week remains as a primary item within the component. We had also pre-specified that we would prefer administrative data if available. Our preferred measure in this paper prioritizes the administrative data but also uses survey data for those who did not consent to be matched, which we take to be in the spirit of preferring adminsitrative data when available. The point estimate on UI-based employment measures can be seen in Table IV;

– For the Income family, we pre-specified that we would prefer measures using administrative data where possible, for those categories we expected it to capture well. In the UI data, this would be salaried and wage income. Similar to our approach for employment, we merge administrative data for those who consented to share it with survey data for those who did not, which we take to be in the spirit of using administrative data where it exists, though the pre-analysis plan was not explicit about this;

– For the sake of FDR corrections for Income and Labor Supply Elasticity, once we had obtained administrative data to construct our preferred combined administrative and survey data outcomes, the survey data-only and UI data-only versions of that outcome were considered secondary, following the original logic of having one outcome per construct in this family be primary;

– We could not compare the *ex ante* forecasts we gathered with experimental results for all items forecast due to mismatches between the questions forecast and the data ultimately available;

– For time use outcomes, we had specified that without family- or component-level analyses, "item-level estimates pooled across time will be K0 primary hypothesis tests, and the item-level estimates at each time period will be K1 hypothesis tests." At the same time, a distinction was made between the main tests and the robustness checks of not adjusting for simultaneous activities in the mobile app diaries and considering time with others. We decided that treating these latter tests as at the K2 level when pooled and at the K3 level by time period would be most consistent with the pre-analysis plan.

– We added some post-pre-analysis plan analyses of heterogeneous treatment effects. Given that heterogeneous treatment effects were de-prioritized in our pre-specified hierarchy for the multiple hypothesis test corrections (Appendix Table A.7), there was some ambiguity as to whether the post-pre-analysis plan examinations of these effects should fall into a new tier (K5). To be conservative, we consider them simply as "post-pre-analysis plan" regressions at the K4 level, thereby making all tests at that level stricter, but as with other heterogeneous treatment effects we do not calculate them by period.

H Time Use

H.1 Mobile App Robustness Checks

The mobile app's time diary allowed participants to record if they were engaged in two activities simultaneously (*e.g.*, watching television while cooking dinner). Following the pre-analysis plan, the estimates in the main text split this time equally between overlapping activities. For example, if someone recorded cooking dinner from 6:00 - 6:30 and watching television from 6:00 - 7:00, this would be counted as 15 minutes of home production (half of the 30 minutes from 6:00 - 6:30) and 45 minutes of leisure (half of the 30 minutes from 6:00 - 6:30, and the entire 30 minutes from 6:30 - 7:00). Figure V in the main text uses this equal allocation method. Figure B.7 shows that the results are similar when we measure time use by the raw sum of all time and do not discount activities by the number of simultaneous activities that occur.

Participants were able to select an "Other" category and write an open-ended description of how they spent a particular block of time if they did not find any of the pre-existing categories suitable. Figure V in the main text reported an estimated 6 minutes/day increase in time spent on these "Other" activities. We used ChatGPT-4 to recode these open-ended responses into one of our pre-existing categories when possible. Figure B.8 shows the results on this version of the measures.

H.2 Results from Enumerated and Quarterly Surveys

The enumerated midline and endline as well as the quarterly surveys also asked participants to report the typical number of hours per week, hours per month, hours per year, or days per year, depending on the activity[5] that they engaged in certain activities. Figure B.9 shows the estimates on these outcomes.

I CE/PCE Weighting of Consumption Outcomes

As discussed in the main text, consumption surveys generally will not capture all consumption. The Consumer Expenditure Surveys (CE) by the Bureau of Labor Statistics (BLS) are a good example of this: they routinely capture only about 70% of the consumption estimated from the Personal Consumption Expenditures (PCE) data from the Bureau of Economic Analysis (BEA). Therefore, when considering the share of the transfers that treated participants allocate to various categories of spending (Table VI) we re-weight the estimates from the consumption surveys to account for this.[6]

To conduct this re-weighting, we use the most recent data made available by the BLS (Bureau of Labor Statistics 2023) and match the survey items to the CE/PCE ratios at the lowest possible level of disaggregation. For example, the BLS estimates that in 2022 the CE/PCE ratio is 0.50 for "comparable nondurable goods", but 0.87 for "gasoline and other energy goods", which are better reported in survey data. For our survey question about spending on gasoline, we use 0.87 as the CE/PCE ratio, rather than the overall non-durables ratio of 0.50.

A full correspondence between survey questions and categories in the BLS data is provided in Appendix Table A.68. Where one question in our survey data maps onto more than one category in the BLS data, we use the weighted average of the BLS CE/PCE ratios, weighted by the total amount of the expenditures in each category; where more than one survey question corresponds to a single BLS category, the BLS category is assigned to multiple survey questions.

There are a few exceptions to the coding rule above, where the BLS data does not have categories that nicely map onto our survey questions. We assume our participants accurately report mortgage payments, rent, alimony, health insurance/healthcare, medical emergencies and debt, and therefore assign these categories a ratio of 1. For a few survey questions where there is not an obvious correspondence in the BLS data, we use the more aggregate ratio (*e.g.*, for a non-durable item in the survey

[5] We rescale the estimates that are in terms of hours per month and days per year variables to be in terms of minutes per day to match the scale used in the mobile app data.

[6] This re-weighting is only applied to this comparison table and not to the main results in Table V.

that is not present in the BLS data, we use the 0.50 ratio for comparable non-durables).

J Labor Supply and MPEs

J.1 Elasticity Calculations

As described in the main text, we translate our results into labor supply elasticities according to $\eta_e = \frac{NY}{\partial v}\frac{\partial p}{p}$ and $\eta_i = \frac{NY}{\partial v}\frac{\partial h}{h}$, where p denotes labor force participation, h denotes total hours worked (including zeros), ∂v is the change in virtual income (the transfer), and NY is net-of-tax income. All calculations consider changes compared to baseline p, h and NY.

We observe values for most of these variables, however, we must make assumptions about net-of-tax income, as we do not observe it directly. To impute it, we leverage what we know about income and household structure from the survey data (*e.g.*, whether participants are married, have children in the household and would qualify as household heads, etc.) and estimate net-of-tax income using the NBER TAXSIM model, as described in Appendix J.3.

J.2 Details on MPE Comparisons

To construct Table A.66, we start with the assumption that individuals in each study follow the standard model used in (Imbens, Rubin and Sacerdote 2001; Cesarini et al. 2017; Golosov et al. 2024), a permanent income hypothesis model with Stone-Geary utility, and apply the assumptions in the most recent of these papers, Golosov et al. (2024), namely that individuals have a 2.5% discount rate. We consider the impacts of a post-tax transfer on individual total labor earnings. Golosov et al. (2024) and Cesarini et al. (2017) provide post-tax estimates of transfer size but Imbens, Rubin and Sacerdote (2001) does not so is dropped at this stage. We calculate the per-adult total post-tax transfer as \$181,200 in Golosov et al. (2024) per their Table A.1 and the per-adult total post-tax transfer as \$2,629 in Cesarini et al. (2017) per dividing their total prize amount, \$650 million USD (4,662 million SEK), by the 247,275 winners reported in their Table 2. The values for the MPE of -0.43 and -0.27 for these two papers, respectively, are as in Golosov et al. (2024). If one were to instead assume that there is no net savings, and participants see the total amount available to them in a year as the total amount they have to potentially spend (*i.e.*, the denominator in the MPE calculation), one could then calculate the values in Column (3) for the lottery studies simply as the decrease in earned income per adult over the per-adult total post-tax transfer.

For the monthly transfers, our own per-adult total post-tax transfers (recalling that the transfers are tax-free) are \$20,118, calculated as the difference between the treatment and control group per month (\$950) multiplied by 36 months and divided by the average number of adults per household, 1.7. We calculate Sauval et al. (2024)'s total post-tax transfers as \$7,087 given that the difference in what their treatment and control groups receive is \$313/month, participants receive the transfers for 48 months, and there are an average of 2.12 adults per household (Noble et al. 2021). To generate the MPE under the same assumptions as Golosov et al. (2024), we calculate the share of this amount that participants would be expected to spend in the first year using the equation in footnote 45, with the assumed 2.5% discount rate and life expectancy of T=80, and with our participants having an average age of 30 at baseline and the participants in Sauval et al. (2024) having an average age of 27 at baseline. Since our participants, and those in Sauval et al. (2024), are younger than lottery winners on average, they are expected to spend a slightly smaller share of their total post-tax transfers in the first year: approximately 3% rather than the 4-5% in Golosov et al. (2024) and Cesarini et al. (2017). The resultant estimated MPEs are shown in Column (2). Without any net savings, of course, they would spend the full amount available to them in the first year, *i.e.*, \$6,706 per adult in our study and \$1,772 per adult in Sauval et al. (2024), and the MPEs that result with this denominator are shown in Column (3).

J.3 Estimating Taxes

Two parts of this paper require estimating participants' tax burden.

First, the estimation of elasticities in Tables III-IV requires participants' net-of-tax baseline household income. Second, the assessment of the MVPF also requires an estimate of how much tax participants may pay given their observed income. The fiscal cost of the program may vary from year to year. The transfers are not treated as taxable, however, to the extent to which they change participants' other earnings, they can change the taxes paid.

We estimate federal and state tax liabilities using the NBER TAXSIM model (v35) (Feenberg and Coutts 1993). Taxes are computed at the level of the statutory tax unit. For unmarried respondents, this corresponds to the individual. For married respondents, this corresponds to the household filing jointly. A head of household status is assigned to any unmarried respondent with dependents. As a result, taxes payable reflect total household labor earnings (respondent plus spouse, where applicable). Total tax payable is defined as federal income taxes plus payroll (FICA) and state income taxes, net of refundable credits such as the EITC and CTC. We leverage our survey data for inputs to TAXSIM's model, though we abstract from several items not captured in the data such as capital gains or qualified dividends. Overall, we expect the impact of these categories to be relatively minor. Spousal wage income is approximated as the total household income minus the total individual calculated income, including benefits, for those who are married.

K Pooling and Comparison of Administrative and Survey Data

K.1 Pooling Approach

Table A.47 provides results comparing and aggregating survey and administrative data.

For each outcome, the first two rows show results from survey data on either the entire sample or the subgroup that consented to share administrative data. The "Illinois" and "Texas" columns disaggregate these data by state. Unlike the administrative records, which are held in two siloed data environments, we can create the "Aggregate" results for the survey data by simply running the regressions over the full data sets.

Row (c) for each outcome is based solely on administrative data. We present results for midline, endline, and pooled values for Illinois and Texas separately. Here, the "Aggregate" columns must be constructed differently, through a fixed-effect meta-analysis of the corresponding Illinois and Texas results.

To construct row (d), we similarly aggregate results based on UI data for those who consented to share administrative records with results based on survey data for those who did not, using fixed-effect meta-analyses. Even when we get to the "Aggregate" columns, we prefer to use the same approach to pooling rather than pooling "horizontally" across the columns of results for Illinois and Texas. This approach is taken because we had such a small sample of non-consenting individuals that the sample would be cut very finely if we further cut the survey data for those non-consenting individuals by state. We do not think there is as much signal within the Illinois and Texas subgroups for those who did not consent to share these records than there is among the full sample of those who did not provide consent, so the results for the "Aggregate" columns are more robust than the results in either the Illinois or Texas columns.

Different measures have different strengths and weaknesses, and we hope that presenting disaggregated administrative and survey results and aggregating them transparently helps the reader understand and assess these different measures and their advantages and disadvantages.

K.2 Comparison of UI and Survey Data

In general, results from the survey data line up very well with results in the UI data. Table A.47 shows a full breakdown across the different data sources. In both sources of data, individual income and labor supply appears to fall in the treatment group, the difference between the treatment and control group increases over time, and the difference between the treatment and control group is larger in Texas. The administrative and survey data even show similar patterns in that both data sources show a growing gap between the treatment and control group over time which somewhat rebounds towards the end of the treatment period (Figures VI-VII and B.5). However, the magnitude of the treatment effect is meaningfully larger in the administrative records than in the survey data.

The difference between the results in the administrative and survey data could in part be due to treated participants switching out of jobs that are captured in UI records into less formal work. However, as we saw when considering information on the types of jobs people hold and survey questions about whether they do "gig" or "temp" work, there appears to be limited substitution into informal work or work that may not be well-captured in UI records, so this does not seem to be able to explain the difference. It is possible that the survey data may be somewhat noisier, at least for categories of income and employment that the UI data captures well. Finally, the survey data may somewhat understate the declines in employment given that we observe that treated participants appear to value work more and express more negative perceptions towards those who do not work (Broockman et al. 2024). Despite these discrepancies, the administrative and survey data tell similar stories about the trajectory of the effects over time and heterogeneity in treatment effects by state, increasing our confidence in these results.

L Robustness Checks

While differential attrition was very low over the study period, we nonetheless perform a number of pre-specified robustness checks. In particular, we conduct a difference-in-differences analysis; restrict attention to administrative data from which individuals cannot attrit or data collected at midline or endline in the enumerated surveys, to which we expected high response rates; and estimate a set of results with Lee bounds. In addition, given that some variables are more likely to contain outliers, we conduct median regression for these outcomes. We also present results from a set of regressions which do not include any covariates.

Overall, the results of these robustness checks appear broadly consistent with the estimates from the main analyses (Appendix Tables A.48-A.64). With only a few exceptions, the family-level indices which were significant in the main regressions are significant in all the robustness checks, and no family-level index which was insignificant in the main regression is significant in any robustness check.[7] The component-level estimates that were significant (insignificant) in the main regressions are also generally significant (insignificant) in the robustness checks. We show the income and labor supply estimates by item, as we do for time use, and at the item level there is a bit more variation, but results from the robustness checks are still broadly in line with the main estimates. The regressions on whether the respondent is employed based on survey data and hours worked per week are significant in the robustness checks without covariates, in the difference-in-differences estimates, and when restricting attention to data from the enumerated midline and endline surveys, but not in the bounding analyses. The magnitudes of the point estimates remain broadly comparable.

M Exploratory Heterogeneity Analyses

Two additional heterogeneity analyses were pre-specified as secondary to heterogeneity analyses discussed in the main text. First we compare those recruited through the Fresh EBT app - who were generally lower-income than those who received mailers or were recruited via Facebook ads - to those recruited through other means. Those recruited through the Fresh EBT app did not significantly reduce their labor supply (Appendix Table A.40). This supports the negative effects on income being smaller for groups that had lower household income at baseline. Second, for those recruited by mailer, we randomized how many mailers they received. Those who received one or two mailers, who we might think of as being easier to recruit to a study all else equal, seemed to reduce their labor supply by more than those who had been randomized into receiving three or more mailers (Appendix Table A.41). However, we do not wish to lean too hard on these results given the relatively small size of the subgroups involved.

We did not pre-specify heterogeneity by state, but *ex post* we observe substantial differences in income and labor supply effects between states in both the UI and survey data (Tables A.34, A.39, and A.47). A number of site-level differences may help explain the observed patterns; though we cannot attribute the heterogeneity to any single factor, several differences are worth highlighting. First, the cost of living was lower in the Texas site than the Illinois site. This means that the transfers could

[7] The exceptions are: the total calculated individual income measure becomes insignificant in the difference-in-differences estimation and has an insignificant upper Lee bound, employment preferences and job search family becomes significant with median regression, and the relationship status index becomes significant with a differences-in-differences estimation, both at $p < 0.1$, and the labor market mobility index has an insignificant lower Lee bound.

in principle go farther and thus have larger effects on earned income and labor supply. Second, Illinois has a more generous existing social safety net. In Texas, where public benefits are smaller and eligibility is more restricted, the transfers may fill a larger gap in basic needs and thus potentially induce larger changes in recipients' financial and work decisions. Third, employment growth was much higher in the Texas site than in the Illinois site over the course of this study. In a high growth environment, participants may be more likely to believe that if they left a job they would be able to find one again quickly if needed. Finally, Texas has a lower minimum wage than Illinois. This means that there are some jobs that pay very poorly and participants may not be interested in them if they are in the treatment group. However, we did not see significant changes in participants' reservation wage (Table A.20), employed participants' wage rate (Table A.22), or the weight participants placed on higher income potential in job search (Table A.21). Further, baseline wages were actually insignificantly higher in the Texas sample, so this explanation may be less likely.[8]

Another potential source of heterogeneity is the presence of children in the household. We did not pre-specify this analysis but observe substantially larger negative effects on income for those who did not have children at baseline (Table A.35). This difference could be consistent with households with children having a greater need for income. Alternatively, it could reflect heterogeneity by age—participants without children are more likely to be younger. Appendix Figure B.10 presents the effects on time use as measured in the mobile app separately by whether participants had children living in the household at baseline. Those without children in the household reduced their market work by more than those with children in the household, consistent with the income results. Interestingly, we do not detect changes in time spent on childcare for those with children in the household. As with other heterogeneity analyses, these results are not causal; baseline characteristics may proxy for other factors that drive differential responses to transfers, rather than directly causing those differences themselves.

N Comparison to Permanent Income Hypothesis Model

Per Golosov et al. (2024), a k-year old household with remaining lifetime of $T - k$ years, interest rate r, and discount rate d allocates share λ of a lump-sum transfer to the first t years:

[8] We may also expect minor differences across states due to the more limited set of controls available in the Texas administrative data environment, though given random assignment and the inclusion of 56 baseline covariates, this is unlikely to drive results.

$$\lambda(r,d) = \Sigma_t \left(\frac{1+r}{1+d}\right)^t \frac{d}{1+d}\left(1-\left(\frac{1}{1+d}\right)^{T-k+1}\right)^{-1} \tag{2}$$

With their $r = 0.025$, $d = 0.025$, and life expectancy $T = 80$, our average 30-year-old participant should "spend" (in either leisure or consumption of goods and services) only about 10% of the transfer amount over the first 3 years and save 90% for use after the transfers end. Even if savings were understated, the share saved would not be close to 90%. We observe more than 10% of the transfer being spent in each of several categories (*e.g.*, leisure and non-durable goods and services), and it is not plausible that our treated participants receiving $1,000 per month are spending only $95 more per month, on average, than the control participants receiving $50 per month. If participants had a very high discount rate, the model would not require them to save as much, but the discount rate required would be much higher than what is typically assumed.

O Comparison to Forecasts from NBER Affiliates

We elicited forecasts from a subset of researchers affiliated with at least one of several NBER Programs.[9] The survey was designed such that each person was encouraged to answer a small set of questions relating to their main field of expertise, but they were allowed to take other survey modules if they wished. In total, we sent 795 researchers an email with an individualized link to take the forecasting survey, and 136 (17.1%) completed it, of whom 43 completed the employment module, primarily affiliates of Labor Studies, Public Economics, and Economics of Health. While this response rate is relatively low, it is commensurate with what one might expect for researchers at this level of seniority. Researchers were not compensated, and the survey was unincentivized. Given the researchers' level of seniority, this approach is appropriate as those taking the survey would tend to be taking it out of personal interest and not be swayed by small cash incentives (Ferguson et al. 2023).

We supplemented the sample by eliciting forecasts from users of the SSPP, including its Superforecaster Panel. The Superforecaster Panel is a panel of researchers interested in forecasting who take nearly every survey posted on the platform. Panelists are paid a flat fee every quarter for their services and receive other benefits. For the version of the survey posted on the platform, participants were offered accuracy-based incentives.

Table A.67 presents results. Interestingly, NBER Labor Studies affiliates and SSPP users perform

[9] Children, Development Economics, Development of the American Economy, Health Care and Health Economics (now merged into Economics of Health), Labor Studies, Political Economy, and Public Economics.

fairly comparably, with the exception of the question about individual salaried income, where SSPP users predicted substantially more positive effects. NBER program affiliates and SSPP users were asked overlapping but non-identical sets of questions, as we wanted to maximize the attention paid by NBER domain experts to particular topics, but for the Superforecaster Panel we wanted respondents to answer as many questions - independent of field - as possible.

We observe that the NBER affiliates had fairly accurate assessments of the effects of the transfers on the intensive and extensive margin of labor supply, the duration of non-employment in weeks, and on individual salaried income as measured in the administrative data, as judged by their mean and median responses. These forecasts somewhat understated the observed effects for employment and income, but are generally within their confidence intervals. For effects on employment, the median and mean forecast fell within the confidence interval for the results from survey data (the question asked respondents to predict the survey data result) but would not have fallen within the confidence interval for the results using UI or pooled UI and survey data. For effects on individual salaried income, the median but not the mean forecast fell within the estimate's confidence interval. There was also great heterogeneity in beliefs. Figure B.33 shows the distribution of responses. While the group as a whole may be reasonably accurate in their responses about labor supply, any one given individual is likely to be off by a large margin.

NBER affiliates also predicted increases in the hourly wage, whereas the estimated effects on hourly wage were -$0.20 at endline. The mean and median NBER affiliate's forecast are outside of the confidence interval associated with this point estimate, as is the mean but not the median forecast from NBER affiliates in Labor Studies. NBER affiliates also believed that participants would search for work less, whereas we observed participants searching for work 7.0 percentage points more towards the end of the study, and all mean and median forecasts are far outside the confidence intervals associated with this result. It is possible that forecasters were not thinking about how, if participants reduce labor supply as a result of the transfers, they may also seek employment more, particularly as the end of the transfers approaches. It is also true that the point estimate on the number of jobs applied to is negative, *i.e.*, they were searching less intensively. Finally, NBER affiliates expected enrollment in a formal post-secondary program to increase slightly (2.4-3.1 percentage points), while our point estimate for the final year of the program was 0.4. The confidence interval on the point estimate contains the median but not the mean forecast of NBER affiliates, though the median and mean of those in Labor Studies are within the confidence interval.[10]

Overall, this analysis suggests that economists expected somewhat more positive outcomes than we observed. They may also have more of a sense of effects on labor supply than they do other important employment outcomes such as hourly wages, human capital investments, and job search, underscoring the benefits of the diverse array of outcome variables considered in this study.

[10] Six NBER affiliates also answered questions about time use, however, this sample is too small to draw inferences from. SSPP forecasters who answered these questions tended to overestimate the amount of time spent on social and solitary leisure.

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