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

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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 3.9 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 younger participants may pursue more formal education. 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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1 Introduction

The design and success of public poverty alleviation programs depend crucially on how cash transfers affect beneficiaries' labor supply and other employment-related outcomes. Means-tested cash transfer programs distort returns to work, causing beneficiaries to cut back on their work hours or earnings in order to preserve benefits. As a result, there has been growing interest in unconditional cash transfer programs, including guaranteed income programs, that would not generate such distortions. However, even such unconditional programs can result in labor supply reductions, harm participants' long-term labor market prospects, and increase the fiscal costs of public programs. On the other hand, cash transfers may help recipients overcome credit or liquidity constraints, allowing them to search longer and potentially find higher-quality or better-fitting jobs, reduce barriers to employment, support entrepreneurship or human capital formation, or lead to productive non-work activities like caregiving. In this case, the benefits of cash transfers to the beneficiaries and society may be large even if they generate some reductions in labor supply. We provide evidence on the effects of a large unconditional cash transfer program leveraging a combination of administrative data, enumerated and online surveys, and data from a custom mobile phone app. The depth and breadth of the outcomes considered offers 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, where much of the literature reports no effect or a weak negative effect (summarized in Krueger and Meyer 2002). Much less is known about the impact of unearned income on other significant aspects of the labor market, such as quality of employment, entrepreneurial activities, and human capital formation. We also have limited understanding of how income affects other uses of a recipient's 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, which are difficult to measure using the administrative and survey data sets employed in existing research, 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 (Jones and Marinescu, 2018), this paper also offers timely evidence on potential effects based on micro data.

We investigate the causal effects of income on employment and other related outcomes by analyzing a program 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. 2,000 participants were 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 we might expect from the past literature (Imbens, Rubin and Sacerdote, 2001; Cesarini et al., 2017; Golosov et al., 2023), 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 choose to spend their time is also informative of their revealed preferences. Moreover, if the transfers enable unemployed participants to search longer for work, does that translate to 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 studied is particularly relevant to policymakers as it is targeted at lower-income individuals, who are the target 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 the bulk of the sample targeted to fall below 100% or 200% of the FPL. Participants reported an average household income of about $29,900 in 2019, so the transfers represented about a 40% increase in household income. The sample approximated the broader U.S. population among those who satisfied the income and age eligibility criteria, and we ensured balance between the treatment and control group on a long list of variables. The study's experimental approach 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. These decreases should be viewed in the context of increasing income in both the treatment and control group over the study period. The program caused a 3.9 percent-

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

age 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 an approximately 5-6% decline relative to the control group mean. This is a moderate effect: compared to results from studies of lottery winners, these effects are arguably larger than seen in Imbens, Rubin and Sacerdote (2001) or Cesarini et al. (2017), but smaller than those in Golosov et al. (2023).[2] Interestingly, partners and other adults in the household seem to change their labor supply by about as much as participants. For every one dollar received, total household income excluding the transfers fell by around 29 cents, and total individual income fell by around 16 cents. Estimates are broadly similar using administrative data alone and survey data alone, though some outcomes show treatment effects of larger magnitudes in administrative data. We also conducted exploratory analysis of the effect of the transfer on a small number of pre-specified subgroups. Estimated labor supply effects are weak and even positive for some subgroups, although for the most part these subgroup estimates are not precise enough to reject an effect equal to the one derived from the full sample.

We captured time use using a combination of survey questions adapted from the American Time Use Survey (ATUS) and 24-hour time diaries delivered through a mobile phone app on a randomly-selected weekday and a randomly-selected weekend day each month. The time diaries and survey questions support the findings for employment. Treated participants primarily use the time gained through working less to increase leisure, also increasing time spent on driving or other transportation and finances, though the effects are modest in magnitude. 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 time spent on 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 improvements in the index of more than 0.022 standard deviations and increases in wages of more than 60 cents. We find that those in the treatment group have more interest in entrepreneurial activities and are willing to take more financial risks, but the coefficient on whether a participant started a business is close to 0 and not statistically significant. Using data from the National Student Clearinghouse on post-secondary education, we see no significant impacts overall but some suggestive evidence that younger individuals may pursue more education as a result of the transfers, which could potentially

[2] Though results are not directly comparable given the differences in the transfer size, payment frequency, and samples.

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 getting more medical care. We also saw significant impacts on duration of non-employment and unemployment. Over the three years of the transfers, the duration of the average spell of non-employment in the control group was 7.8 months; the treatment had the effect of increasing this by 0.8 months. Compared to evaluations of the impacts of UI benefit extensions, this is a relatively small number (Cohen and Ganong, 2024). Those in the treatment group 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 are allocated to consumption expenditures, led by non-durable goods and services, though we also see small increases in spending across the board, most notably on human capital and durable goods.[3] Treated participants are more likely to move labor markets, though labor market quality does not significantly improve. We see no significant changes in marriage or divorce. Finally, we see temporary improvements in subjective well-being across various types of measures in year one, but these revert in years two and three, closely mirroring the patterns in the effects of this intervention on stress and mental health documented in Miller et al. (2024) and in self-reported measures of financial health in Bartik et al. (2024).

The study has a number of strengths compared to existing literature. To examine the effects of a negative income tax (NIT) on the labor supply of recipients, the U.S. government conducted four randomized experiments between 1968 and 1980 (e.g., Ashenfelter and Plant 1990). While these studies were pathbreaking and are still often referred to today, these experiments were plagued by nonrandom selection, errors in randomization protocols, differential attrition, nonparticipation, and systematic income misreporting (Hausman and Wise 1979; Greenberg and Halsey 1983). Further, these experiments were begun in a very different economic and political context, so their results may not generalize to the present day, and we are able to collect much more detailed data on a much broader range of outcome variables, including through the use of a mobile phone app. A related strand of the literature utilizes the exogenous increase in income created by the introduction of the Earned Income Tax Credit (EITC) and subsequent expansions to examine labor market effects (Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Eissa and Hoynes 2004; Nichols and Rothstein 2016). However, there has been debate about these estimates due to simultaneous reforms and a strong economy (Kleven 2024). Further, this literature necessarily focuses on subgroups potentially affected by the expansions, par-

[3] These increases are small in absolute magnitude, but relatively large in proportion to how much was spent on these items at baseline.

ticularly married couples and families with children, and these subgroups could respond differently than the broader population.

Unlike unconditional cash transfers, programs like the Earned Income Tax Credit (EITC) affect beneficiaries' labor market incentives because the amount of the benefit is linked to the amount of earned income. To address this limitation, 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 in some way, such as being generally higher-income and perhaps more risk-loving than the individuals a public guaranteed income program might target (Golosov et al. 2023). 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 2018), 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).[4]

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; Krueger and Meyer 2002; 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 states' unemployment insurance agencies from the two states from which participants were recruited, as well as National Student Clearinghouse 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 Research Center at the University of Michigan as well as frequent web-based surveys and a mobile phone app. We had very high responses to these surveys.

[4]There is also an important literature on cash transfers in a developing country context. Most of this work focuses on conditional cash transfers and children's outcomes (reviewed, for example, in Fiszbein et al. 2009). However, some studies leverage unconditional cash transfers and consider employment outcomes (Mostert and Castello 2020). Banerjee et al. (2017) review seven government-run cash transfer programs and find no systematic effect on labor supply on either the intensive or extensive margin. In a study of three-generation households in South Africa, Bertrand, Mullainathan and Miller (2003) find a sharp decline in both the extensive and intensive margin in working-age individuals' labor supply when an individual in the household receives a pension. These results are important but may not generalize to the U.S., given the significant contextual differences.

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 (Bartik et al., 2024), we can show that the standard model does not describe our participants' behavior 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 for these kinds of transfers.

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, with Centeno (2004), Caliendo, Tatsiramos and Uhlendorff (2012), and Nekoei and Weber (2017) finding that more generous benefits enable better jobs, while another strand of the literature finds no such effects (e.g. Card, Chetty and Weber, 2007, Lalive, 2007, van Ours and Vodopivec, 2008). In addition to drawing from other countries with more generous social safety nets, past papers in this literature have often had limited information on job quality, inferring job quality from income or the duration that the post-unemployment job was held. In contrast, we 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 for 9 months to 1,067 treated participants, with a group of 730 residents serving as the control. The transfers ran from November 2020 to 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 400 low-income new mothers in a "high" cash arm with $333/month for 72 months, starting in May 2018-July 2019, with an additional 600 in a "low" cash arm receiving \$20/month. These transfers were provided on a debit card labelled "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. (2023) examine a U.S. program which randomized 699 individuals to receive a one-time transfer of \$2,000, 1,374 individuals to receive a one-time transfer of \$500, and 3,170 individuals to receive 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 to 695 low-income, mostly Hispanic households, with a control group of 1,402 households (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, likely due to the control group receiving smaller transfers and extensive outreach and tracking efforts by the project team. We also 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, including data from a custom mobile phone application.

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. For a policymaker interested in cash transfers, the main benefits 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 opportunity for any one given pursuit such as obtaining a post-secondary degree or starting a business.

In the following sections, we describe the sample and approach in more detail. After presenting results, we explore heterogeneity and compare our results to the existing literature.

2 The OpenResearch Unconditional income Study (ORUS)

2.1 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 1.

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 Federal Poverty Level (FPL) in 2019 were eligible to participate. The organizations implementing the program excluded individuals from households where at least one person receives Supplemental Security Income (SSI) or Social Security Disability Insurance (SSDI), as well as those in publicly-subsidized housing, so that they would not lose important benefits. Extensive effort was taken to protect eligibility for public assistance programs, with collaboration between some of the research team, implementing partners, and state representatives to pass Bill SB 1735, which protected many government-provided benefits in Illinois.[5] Only Medicaid and energy assistance were protected in Texas, but benefits are less generous and eligibility criteria are more restrictive in Texas. A table of specific benefits and their protection status is provided in Appendix Table A1. The transfers were not conditioned on research participation and were considered gifts from non-profit organizations and not taxable income.

The non-profit implementers recruited potential participants in three ways. Most participants (87%) were recruited via a mailer that asked if they were interested in participating in a cash assistance demonstration program and stated that they would receive "$50 or more" each month if they were chosen to participate. Addresses within program counties were selected to receive mailers based on information from TargetSmart, which provides address data and demographic details about residents at each address. Approximately 69% of mailers were sent to individuals who appeared to be eligible for the program on the basis of their age and income, but 31% of mailers were sent without any targeting, to avoid systematically excluding individuals who were eligible but who would not have appeared to be so based on the commercial data (e.g., through having missing data). The mailers

[5] Specifically, this bill protected SNAP, TANF, child care assistance, Medicaid, and energy assistance. Further details on the bill are provided in Appendix Figure A1.

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 that allowed them to complete a simple intake survey to determine eligibility. Recipients were also offered randomized incentives of \$0 to \$20 to complete the survey questionnaire. Upon survey completion, online gift cards were immediately sent via email to increase trust. Follow-up letters were sent to those who did not respond, with each individual randomized to receive between 0 and 4 follow-ups. A flowchart of the recruitment process is provided in Figure 2.

A smaller number of participants were recruited by alternative methods. First, advertisements were displayed through Facebook and Instagram to all individuals who appeared to be eligible for the program based on their age and county. Approximately 1 percent of the sample was recruited through this approach. Second, advertisements were posted on "Fresh EBT", a free mobile application that is used by over 4 million recipients of the Supplemental Nutrition Assistance Program (SNAP) nationwide to check their balance and manage their benefits.[6] Advertisements were limited to the eligible zip codes. Approximately 12% of the sample was recruited through this app.

2.2 Randomizations

There were two randomizations, described in more detail below. The first randomized individuals to be in the main study sample (receiving either \$50/month or \$1,000/month for three years) or out of the main study sample. The second randomization occurred after all individuals in the main study sample were enrolled. It randomized people into receiving either a high or low transfer amount.

2.2.1 Randomization to the Main Study Sample

The first randomization took the eligible applicants and randomized 3,000 individuals into being part of the study sample. Beyond receiving further surveys, this group received a minimum of \$50 per month unconditionally. This randomization was designed so that the study sample met certain criteria. There were three desiderata: 1) that the study sample included a minimum of 20% non-Hispanic White, 20% Black, and 20% Hispanic participants; 2) that it included a minimum of 30% individuals 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; and 3) that in terms of gender representation it broadly reflected the distribution of men and women in the eligible population according to data from the American

[6] The application, developed by Propel, is now called Providers.

Community Survey (ACS).[7] To achieve the desired sample, we blocked participants on demographic characteristics and randomized a larger share from some blocks to the study sample.

2.2.2 Enrollment

After the first randomization, the contact information of the sample of potential participants was provided to the Survey Research Center (SRC) at the University of Michigan on a rolling basis. Participants were first enrolled in the cash transfer program before being invited to participate in the research. Consenting participants then completed a comprehensive baseline survey and were asked if they wished to provide consent for the research team to analyze their administrative data. As part of the enrollment procedures, participants also provided bank account information so that funds could be transferred to them via direct deposit. 348 individuals did not have a bank account at enrollment, and an online bank account was created for them to receive their transfers. Enrollment was conducted in person from October 2019 to March 2020, when it switched to being conducted over the phone until all 3,000 individuals were enrolled by October 2020.

The long baseline period was intentional. First, it enabled us to obtain a large amount of baseline data on participants, since during the baseline period we sent participants monthly surveys. Second, we believed that attrition might be highest in the first few months of the study, and by having a long baseline period, we could balance on baseline attrition when conducting the second randomization to the $50/month or $1,000/month transfers. Participants were paid $10 per survey during the baseline period ($50 for the enrollment survey, which was much longer), and received $50/month unconditionally as a gift during this 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 is 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 1). The participants look comparable to the broader population on all measures except for being slightly more likely to rent, slightly more likely to have a college degree, and slightly more likely to be female.

[7] This last criterion was loosely implemented to help us meet the FPL-group by state targets.

2.2.3 Randomization to Treatment or Control

After enrollment, we conducted the second randomization to assign participants to either receive \$50/month ("control") or \$1,000/month ("treatment") unconditionally for three years. The differences between these two groups will be of primary interest in our analyses.

For this second randomization, all participants had an equal 1 in 3 probability of being assigned to the treatment group.[8] We implemented a blocked random assignment process to ensure balance over key strata as well as imposing 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 2. About 58% of participants were employed at baseline, with a total household income in the year before enrollment of about \$29,900. 17% of participants had a second job. 57% had children living with them in the household, and 33% were living with a romantic partner. The average household had 3.0 people in it, including the participant. About 20% of the sample had a bachelor's degree.

During enrollment, we identified a handful of participants who knew each other. Out of an abundance of caution, we grouped these individuals and anyone at the same address (such as a large apartment building) into a "cluster", and each cluster was assigned to either treatment or control together.[9] Given the random assignment of clusters to treatment or control, the standard errors in our analyses are also clustered at this level.[10] We conducted simulations to confirm that every cluster had a 1 in 3 chance of assignment to the treatment group. Further details are provided in Appendix C.

2.3 Cash Transfers

After the second randomization, members of the treatment group were notified of the increased transfer amounts. Both the treatment and control group were reminded of the transfer timeline, and the implementing partners reminded them repeatedly about this in the final year of the program.[11] The

[8] A waitlist for the \$1,000 payments was also developed, however, it was not meaningfully used as we had excellent take-up of the \$1,000 payments. Appendix B provides more details.

[9] In total, this approach yielded 18 clusters which had 2 people in them and 2 which had 3 people. All other clusters included 1 person.

[10] 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.

[11] One might wonder how long participants thought the transfers would last. An advantage of our study is that since most participants had been enrolled for several months prior to the start of the treatment period, receiving \$50/month during the baseline enrollment period, participants already had extensive interaction with and trust in the program by the time it began. This makes it likely that they believed what they were told about the transfers and indeed, in qualitative interviews, we did not observe any indication that participants did not believe when the transfers would end. Participants were told the three years would start as soon as everyone was enrolled, and all participants were contacted at that point and told when

cash transfers were unconditional, and participants in the treatment and control arms continued to receive them even if they did not participate in the research.

Enrollment in ORUS was completed by October 2020. Randomization into treatment and control took place immediately thereafter, and treatment began in November 2020 and ran through October 2023. This timing means that the first year of the treatment period overlapped with an era that included COVID-related disruptions, but that years 2 and 3 were from a period when vaccines were widely available. Since our analysis relies primarily on data from 2022 and 2023, with 2023 weighted particularly heavily, our results are predominantly based on the post-COVID-19 era, particularly compared to other cash transfer pilots.

3 Data Collection and Outcome Measures

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

3.1 Administrative Data

We leveraged data on income and employment from Illinois and Texas Unemployment Insurance (UI) agency records. Employers are required to report quarterly employment and earnings for all employees in UI-covered positions to state agencies. These data were then made available via a data use agreement. While some jobs are excluded from the UI system–for example, independent contractors and those who are self-employed but not incorporated, including "gig" workers such as drivers for ride share companies–we expect these records to be fairly comprehensive.[12] 87.5% of participants consented for us to link their administrative records. 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. In total, among those who consented to share administrative records but not conditioning on provision of SSN we obtained a 71% match

the last payment would be sent. Reminders were sent to both the treatment and control group at 1 year, 6 months, 3 months, 2 months, and 1 month before the transfers ended. Given the close communication with participants and the relationship of trust, we think that participants were well aware of the transfer duration.

[12] Graham et al. (2022) estimates that 95 percent of employment nationally is covered by UI. Looking at Californian tax filers, Bernhardt et al. (2022) find 5.9% have exclusively independent contractor or self-employment earnings, while an additional 6.2% have some independent contractor or self-employment earnings supplementing a W-2 income. Katz and Krueger (2019) find 15.8% of US workers are in alternative work arrangements, including independent contractors, on-call workers, temporary help agency workers, and contract workers; W-2 employees of a temp-agency or contract agency would be represented in the UI data, as would on-call workers who receive a W-2.

rate in Illinois and a 73% match rate in Texas. In Illinois, we were able to analyze survey data alongside the administrative records.[13,14] Given that the ultimate match rate is lower than in Golosov et al. (2023) and Cesarini et al. (2017), as a robustness check we present results using Lee bounds in the appendix (Appendix Table A8). The match rates were very similar in the treatment and control group, so the results of the Lee bounds analysis are consistent with the main results.[15]

We also linked participants to administrative data from the National Student Clearinghouse (NSC) on post-secondary educational enrollment and completion. The NSC provides excellent coverage of post-secondary institutions in the U.S., covering 97% of institutions over the transfer period.[16] These data include information on degree attainment, enrollments, and progress in the degree, as well as descriptive details about the fields of study pursued. We supplement these data with survey data for those who did not consent to linkage.

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

3.2 Enumerated Survey Data

Trained enumerators from SRC conducted interviews 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 transfer payments ("endline"). The midline ran from April 3 - August 2, 2022, while the endline ran from March 30 - August 15, 2023. The endline surveys were planned to take place a few months prior to the end of the program so as not to capture changes in behavior that may arise from the anticipation of no longer receiving monthly transfers. A timeline of the main study events is included in Figure 3.

To avoid burdening respondents with overly long surveys, we partitioned some of the survey

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

[14] 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.

[15] The balance between treatment and control groups on consent to share administrative data is due to our asking participants to provide consent and share full SSNs early on, as early as the online screener and again at enrollment. First, the initial online screener asked potential participants whether they would consent to share their administrative records. 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, with no significant differences between the treatment and control group. The early requests for consent to examine administrative data were essential in ensuring balance across the treatment and control groups. Participants were asked prospectively for consent to analyze outcomes in administrative data for 30 years.

[16] https://nscresearchcenter.org/workingwithourdata/.

questions we wanted to ask about and asked them in separate online surveys following the corresponding SRC survey. Participants were provided with $50-$100 for answering the SRC surveys and $15-$30 for answering each of the follow-up online surveys.[17]

We obtained very high response rates to the midline and endline survey. 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%.[18]

3.3 Web-based Survey Data

We measured many of the outcomes using data from monthly surveys administered using the Qualtrics web-based survey platform. These surveys included questions on time use with different lookback periods as a complement to mobile app-based time diaries, as well as questions on job search, quality of employment, job satisfaction, hours worked, income changes, intrahousehold employment outcomes, housing search and mobility, and participation in formal and informal education and training, among other outcomes. Participants were compensated $10 for every survey completed.

This frequent contact with participants enabled us to keep up-to-date on any address or contact information changes. The questions that were asked on each survey varied by survey, but generally each module of questions was asked multiple times per year. This gave us multiple chances to collect information that might have been missed in any one survey. In our analyses, we collapse participant responses within a year.

Response rates to the monthly web-based surveys were high: 98% completed at least one web-based survey in the first year, 96% in year 2, and 94% in year 3. Appendix Figure A2 shows response rates by survey year.

3.4 Mobile Application Data

Participants in ORUS used a mobile phone application created for the program by Avicenna Research. We used this mobile app for both passive and active data collection for the proposed study. Daily time diaries are widely regarded as the gold standard of time use surveys, and the app provides a user-

[17] At baseline, we offered a $50 kept appointment bonus at the very end of the recruitment period, on top of the $50 base incentive, and at midline and endline people were randomly assigned to receive a kept appointment bonus of $0, $25, or $50 in addition to the base incentive. For the mobile endline, total incentives were increased to $30 in the final weeks of the endline period.

[18] For the three online surveys that followed the midline and were associated with it, we obtained response rates of 93.7%, 91.0% and 89.2%, and for the four online surveys that followed the endline, we had response rates of response rates of 95.2%, 93.2%, 91.1% and 88.6%.

friendly calendar interface that allows respondents to report all of their activities in a 24-hour period by dragging activities into time slots. This interface also has the advantage of enabling us to collect information on both primary and secondary activities (e.g., participants may say they were cooking but also watching television alone at the same time). We asked respondents to complete time diaries on a randomly-selected weekday and weekend day each month. Participants were compensated with $5 for every time diary completed. A screenshot of the interface is provided in Figure 4.

The time diaries had a high response rate and were elicited very frequently, so we have a large number of repeated measures in these data. The web-based surveys achieved higher response rates, but were less frequent. Results for both modalities will be presented.

3.5 Response Rates and Attrition

We proactively curbed attrition and non-response through several means. First, we think that it was likely important that the control group received a small ($50) cash transfer each month. We note that other unconditional monthly transfer programs with high response rates, like Baby's First Years, have also compared a high-cash to a low-cash arm (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 provided another opportunity for participants to update their contact information, and these surveys were kept short and engaging to sustain interest.[19] Participants were encouraged to respond to surveys through email and text reminders, as well as higher-effort measures such as calling non-responders by phone and sending postcards that appeared to be handwritten. Other measures were also taken to build trust: for example, as soon as recruited individuals finished the baseline survey, they immediately received their first incentive payment to their bank account.

We observed extremely limited differential attrition given the length of time over which we stayed in contact with participants. At the time of the midline survey, we observed differential attrition of only 1.7%, and at endline, only 3.2%. For the monthly online surveys, we did not observe significant differential attrition at all in year 1 and year 2 after pooling across surveys within the year, with 4.3% differential attrition observed in year 3. Differential attrition in the app-based time diaries was 6.0%

[19] Given that some questions were easier or more interesting to answer than others, each survey was intentional about which questions it included so that no survey would contain too many particularly taxing questions. The survey flow was similarly designed to be as engaging as possible. Participants were able to provide comments about the surveys they took, which helped to inform subsequent survey rounds. Response times and attrition was closely monitored throughout.

on average across the three years of the study.[20]

Despite this very low overall attrition, we take several measures to mitigate concerns that differential attrition might affect results. First, we prioritize outcomes in the administrative data, where we do not observe differential attrition. Second, we check that respondents and non-respondents appear similar to one another on a long list of baseline covariates (Appendix Tables A2-A6). Third, we provide Lee bounds estimates conservatively correcting for this (at the expense of less precision). Fourth, we present a set of results restricting attention to the midline and endline surveys, to which we had particularly high response rates. Finally, we implement a differences-in-differences approach as a further robustness check, for those outcomes for which we have baseline values. This approach does not require respondents in the treatment and control group to be balanced for identification, but rather only requires these groups to have parallel trends. All robustness checks are included in the appendix.

4 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.[21]

Given that we have outcomes data from multiple time periods, we had to pre-specify how we would treat them. Our preferred specification pools results across time periods, leveraging the extra power that multiple measures gives us, yielding a single aggregate measure capturing changes over the study period, though we also show disaggregated results for completeness. We pre-specified that we would we would place more weight on the endline outcomes (70%) than the midline outcomes (30%), and that we would similarly place more weight on online survey responses in year 3 (50%)

[20] 4.4% in year 1, 7.5% in year 2, and 6.5% in year 3.

[21] Recall, we are unable to bring the full set of covariates into the Texas UI data environment, but we do bring in a set of 56 baseline covariates from survey data, focusing on demographic variables, employment, income, household composition, relationship status, and county type, and we can additionally leverage baseline covariates in the administrative data. We run a Lasso within the administrative data environment on this more limited set of covariates to generate the main estimates based on UI data. We are able to bring all baseline covariates into the Illinois UI data environment.

than in year 2 (30%) or year 1 (20%). Placing more weight on the results from later years has the advantage that if the transfers have effects that accrue over time, this approach would better capture them. Another reason we preferred to place more weight on later time periods is that one of the unique features of our study is the relatively long duration of the transfers it studies, and we are primarily interested in changes that might occur over longer periods of time. Further, by focusing on this timeframe, 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 many cash transfers around that period. We also pre-specified that we would place more weight on the SRC survey data (70%) than the online survey data (30%), given that these data may be higher-quality and have less non-response bias. We further present a set of estimates that rely only on SRC data and administrative data, from which individuals cannot attrit. 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 time 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, so that we ultimately have one observation for each 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 the outcome during the treatment period; otherwise, their outcome is missing and they are not included.

Since we have multiple outcome measures, we must correct for the fact we are conducting multiple hypothesis tests. Here, we take two approaches. First, we generate summary index measures as a way of reducing 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 that someone might care about is their day-to-day experience at work. This could include such factors as whether they face discrimination at work, whether their boss treats them fairly, etc. Questions asking about these factors ("primary items") could 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 in order for a positive treatment effect to represent a positive impact. We also present all item-level test results in raw units, unadjusted, for the sake of interpretability. Sometimes a family may also contain one or more secondary items, which are pre-specified to not be included in the index.

Our second approach to reduce the risk of "false positives" is to present false discovery rate (FDR) adjusted q-values for our estimates in the main results. We put our estimates into tiers for the sake of conducting multiple comparison adjustments, following Guess et al. (2023). The logic of this approach is that some estimates may be higher-priority than others, and so long as this is pre-specified we can also conduct secondary analyses that are clearly denoted as such without penalizing the higher-priority tests. The family-level estimates are considered to be in the top tier, and all family-level estimates are pooled when constructing the q-values. Component-level estimates occupy the next tier, and these are pooled with the family-level and other component-level tests within the family. Primary items are pooled with all the family-level, component-level and item-level tests within the family. The last level of the hierarchy includes exploratory analyses, including any secondary items, subgroup analyses, or estimates by time period. Further details are provided in Appendix D.

5 Results

5.1 Income, Labor Supply, and Time Use

The transfers led to substantial reductions in earned income. In rows (1) and (2) of Table 3, we see a decline in total household income excluding the transfers of about $4,200 per year (with a standard error of $1,000) and a decline in total individual income excluding the transfers of $2,300 (s.e. $700) per year. These numbers are based on survey questions that asked participants to provide one number for their household income and individual income.[22] Additionally, participants were asked about their earnings from each specific job they held as well as other sources of income. We add up income in these categories and obtain an estimated effect on total calculated individual income of $1,500 (s.e. $900) per year in row (3). This estimate is slightly smaller than the estimate obtained in row (2), but

[22]Specifically, the question on household income asked about total household income, while the question about individual income asked for one number representing total earned income, 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.

the calculated measure does seem to have a control mean with a slightly larger magnitude, suggesting that individuals did think of more income when prompted to think about more finely-grained 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 categories of income according to the control mean, and we do not observe a quantitatively large or significant decline in this category (row 5). Income from supplementary gig work contributes only $400 per year on average in the control group, and we do not see large or significant changes here, either (row 6). It is possible that this category is more meaningful for some participants, but on net we do not see substantial impacts. Participants have essentially no passive income, and there does not appear to be any effect on passive income (row 7). "Other" income (row 8) consists of gifts from family and friends as well as monetary government transfers.[23] These contribute a meaningful share of participants' total income, but the point estimate suggests these may only decrease by about $100 (s.e. $200) per year on net, and this difference is not statistically significant. Breaking government transfers out separately (row 9), we see a decline in benefits of about $200 (s.e. $100) per year, but this is also not statistically significant.

Turning to consider administrative data from UI records, we observe a $1,700 (s.e. $900) per year decline in individual salaried/wage income in row (10). This estimate is larger than the corresponding survey measure, alone, which had a point estimate of $1,200 (s.e. $800) per year in row (4). Pooling UI data for those who consented to share these data and could be matched with those who did not consent to share administrative data, we obtain an estimate of about $1,600 (s.e. $900) per year (row 11).[24] The pooled results in row (11) differ from the results based on UI data alone in row (10) by only about $160, and they are not limited to those who were matched, so we will treat the pooled results as our preferred measure. Appendix Table A46 contains a more detailed comparison of administrative and survey data, and the approach to pooling across data sources is described in Appendix J.

Given that we have several measures of income, it may be informative 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 data estimate in row (4). If we prefer the pooled measure to the survey data only measure, the calculated total individual

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

[24] All measures in Table 3 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 6.

income measure in row (3) may be understated by at least a similar amount.[25] This would suggest a decrease in total individual income of about $1,800/year, aggregating across rows (11) and rows (5)-(8).[26] Similarly, if we think that the calculated measure in row (3) is more accurate than the measure in row (2) which almost entirely depends on an answer to a single survey question, it suggests the total household income number 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 $3,300 may be a more reasonable magnitude for this estimate.[27]

These estimates can be used to calculate participants' MPE. We define the marginal propensity to earn 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. This means that participants MPE depends on two decisions: 1) the decision of how much of the transfer to allocate for consumption today (whether consumption of leisure or other expenditures) vs. how much to allocate for consumption in the future; and 2) the decision of how much to consume today in leisure out of the total amount reserved for today.[28] The first of these decisions can be gauged by considering how much net worth participants are accumulating. However, we have a range of estimates for effects on net worth, as these estimates depend on whether one includes types of assets like real estate that only a few people possess and how one treats the uncertainty inherent in the estimates. To be conservative, we use a range of different plausible numbers for effects on net worth in calculating the MPE. The point estimate of the preferred estimate for changes in net worth from Bartik et al. (2024) is -$1,000 (*i.e.*, treated participants end up with $1,000 more net debt than the control group over the course of the program), but an estimate of -$2,000 is possible if excluding real estate, which is imprecisely estimated. We also include an optimistic estimate of the impact of the transfers on net worth of $5,000, based on the confidence intervals around the main result. Thus, Table 3 shows a range of possible values for each type of income.

[25] An important note is that the UI data does not capture contract, gig, or informal work, and some participants incorrectly listed this type of work as their main "salaried/wage" job in survey data. In including these other types of work, survey data may capture more than the UI data. Since we observe if anything a negative point estimate on supplemental gig work and other types of work in response to the transfers, it implies the difference between the UI data and survey data for salaried/wage income, specifically, may be slightly larger than implied by the difference between rows (11) and (4).

[26] The estimates in rows (5)-(8) sum to $213, to be precise, and aggregating with a point estimate of $1,590 in row (11) yields a preferred total individual income measure of $1,803.

[27] Specifically, if we deflate the total household income estimate ($4,240) by the ratio of our preferred $1,803 estimate for total individual income to the value in row (2) ($2,327), we get $3,285. 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).

[28] Explicitly, the MPE can then be calculated as the ratio of the change in income over the total amount allocated for consumption today. We do not use after-tax income here, as we do not directly observe it, but tax rates would be low for participants and not affect these estimates by much.

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 η_e and η_i are the extensive and intensive margins, respectively, NY is net-of-tax income, v is virtual income (the transfers), p is participation and h is hours.[29] We estimate η_e for participants as -0.16 and η_i for participants as -0.15.

Turning to consider effects on employment, we observe effects on both the intensive and extensive margin (Table 4). In survey data, we observe that hours worked per week decline by about -1.4 (s.e. 0.6) hours/week as a result of the transfers (row 1).[30] Treated respondents appear 2.2 (s.e. 1.2) percentage points less likely to be employed in the survey data (row 2) but, similarly to how we saw larger impacts on income in the pooled UI and survey data than we did in survey data alone, we observe a 6.6 (s.e. 2.0) percentage point decrease in employment using the UI data alone (row 3) and a 3.9 (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 be a result of contract work, temp work, or gig work not being included in the UI data, as we observe lower rates of employment in the UI data in both the treatment group and control group overall. However, we do not observe substitution into these types of work as a result of the transfers in the survey data, as we will see in the section on quality of employment. As with the income estimates, a detailed comparison of administrative and survey data is available in Appendix Table A46 and the pooling approach is described in Appendix J. The pooled measure will again be our preferred measure.

Turning to consider the employment responses of other individuals in the household, we estimate that there is a decrease in labor supply for participants and their partners of 2.4 (s.e. 0.8) hours/week (row 5 of Table 4), or 2.3 (s.e. 0.9) hours/week for all members of the household (row 6). These estimates represent a 4.8-5.9% decrease compared to the control mean. This is not too far off from the extent to which total household income declined in our preferred estimate (6.8%, using our adjusted estimate of $3,300). The difference between this decrease and the percent implied by the estimates for total household income could be partially due to a small, imprecisely-estimated decline in other transfers received, but it could also reflect some slight substitution into employment not captured by the UI data and some slight underestimation of the decline in total work hours. The estimates for the work hours of others in the household were not pre-specified and so are subjected to a large penalty in

[29] There is a subtlety here: since the control group gets $50/month, the changes we observe in p or h are due to changes in unearned income of $950/month, and the elasticities are calculated accordingly. Further details of this calculation are provided in Appendix I.1.

[30] This estimate is unconditional, i.e., inclusive of both employed and non-employed participants, and could partially reflect both employed participants cutting back on work hours as well as non-employed participants not beginning new employment.

the false discovery rate corrections but are significant before those corrections are applied. Though the effect on partners' hours worked may be slightly understated, it is of a roughly comparable magnitude to the effect we observe on the participants' own work hours.[31]

As described in section 3, participants recorded their time use on a mobile app. These time diaries are asked on a frequent (bi-monthly) basis and elicited in 15-minute increments. Figure 5 shows the estimated effects on time use as measured in the mobile app. Between reductions in "market work" and "other income", they show a reduction in about 1.4 hour/week of work, consistent with the employment module survey questions about hours worked at each job. Appendix G 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, as well as further results, including for time spent with others (e.g., time spent with friends, children, or alone) and results from the enumerated and quarterly surveys. Of note, in our enumerated and quarterly time use surveys, we observe a 1.5 hours/week reduction in work hours (Appendix Figure A4), which corresponds closely to what we observe in the mobile time diaries. The extra time participants have from reduced work is used largely for leisure,[32] non-commuting transportation, and other activities (Figure 5).[33]

Appendix Figure A5 shows 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 who did not, consistent with the earlier results for income. Interestingly, we do not observe those with children spending more or less time on childcare as a result of the transfers. More generally, the mobile app also asked participants who they were conducting activities with. Appendix Figure A6 shows the effects on the amount of time spent with various people. These effects are all small and insignificant after adjusting for the false discovery rate, though the closest category to being significant before adjustment is a reduction in time "with

[31]On the face of it, this seems consistent with the unitary household model, in which participants and their partners make joint decisions over how much to work. However, a subtlety is that participants and their partners are not necessarily identical in terms of baseline characteristics. First, recall that the program was targeted at those in lower bins of the Federal Poverty Level (FPL). Females are more likely to be represented in these bins, both due to having lower incomes on average as well as being more likely to have children in the household. Second, even among those living with partners at baseline, in which both the participant and their partner would fall in the same FPL bin, there is a slight over-representation of females. This could be due to the age criterion for program eligibility (participants had to be age 21-40 at baseline), as females tend to be slightly younger than their male partners on average and more people live with partners in their 30s than in their late teens. Given this, while results look suggestive we cannot formally test the unitary household model.

[32]Social and solo leisure are not individually significant, but they represent the largest category if pooled.

[33]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 "solo" 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 false discovery rate (Figure A4).

my boss".

Comparing the estimates of the effects on income, labor supply, and time use, we can observe they are consistent with each other. A $1,800 reduction in annual income represents an approximate 4.9% reduction relative to the control mean for total individual income, while a 1.4 hour/week reduction in work (from the labor supply survey modules or the time diaries) or a 1.5 hour/week reduction (from the quarterly ATUS-style surveys) represents an approximately 4.5-5.5% reduction relative to these variables' control means. As we will see when discussing the labor market quality family, average wages were unaffected by the treatment, so any small difference here 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. We may also think that work hours/week could be somewhat understated in survey data similarly to what we see for income.

Overall, the effect seems to be driven by the extensive margin.[34] This would be in line with the suggestion that many low-income jobs do not come with a great amount of flexibility in work hours (Lachowska et al., 2023). Indeed, at baseline, conditional on working, 44% of participants reported that they would prefer to work more or fewer hours, directly suggesting some inflexibility in work hours. While gig work may be more flexible (Garin et al., 2024), this work represents a relatively small share of participants' total income. Further exploring the data, it seems that the effects on employment and work hours are primarily driven by those leaving their primary job rather than leaving second, third, or fourth jobs (Appendix Table A9). This would go against a story in which, for example, those individuals who work several jobs quit a second job and suggests a somewhat greater share of the reduction of work hours coming from the extensive margin than in much of the literature.[35]

We see a time trend both for effects on income and employment (Figures 6 and 7) and effects

[34]Specifically, in our preferred estimate, the treatment causes labor supply to fall by 3.9 percentage points. If the average person worked about 40 hours a week, 50 weeks a year, that would translate into a reduction of 78 hours over the course of a year. If hours worked per week declined by 1.5 due to the treatment, representing about 5-6% of the control group mean, the entire labor supply effect would be driven by the extensive margin. If hours worked per week declined by 1.7-1.8, representing about 6-7% of the control group mean, the extensive margin would represent about 5/6ths of the labor supply effect. Some of the robustness checks for hours/week from the survey questions and time diaries resulted in estimates of 1.7-1.8 hours/week, aggregating across time spent on "market work" and "other income" (Tables A49-A50).

[35]In Golosov et al. (2023), roughly half the response is through the extensive margin, but they note that the extensive margin response is greater among low-income households, and our sample is much lower income than theirs. Cesarini et al. (2017) estimate that the extensive margin represents 40% of the five-year labor supply response, with the relative share of the response explained by the extensive margin decreasing over time. We study a shorter time period and, again, our sample is relatively low-income, so we might expect a higher extensive-margin response. Imbens, Rubin and Sacerdote (2001) does not distinguish between the extensive vs. intensive margin response. In Sauval et al. (2024), which focused on the impacts of a cash transfer to new mothers, the extensive and intensive margin effects appear roughly balanced, though it is somewhat difficult to say as their pooled analyses range from a -5.6 to +2.9 percentage point effect on employment and a -3.4 to +0.1 hours/week effect on total labor hours.

on time use (Figure A11-A14): the treatment effect grows over the course of the study. Restricting attention to the quarterly UI data, this is evident both considering event study results in Figure 7 and looking quarter-by-quarter (Appendix Figure A3). This effect would be consistent with increasing separation from the labor market, but it would also be consistent with individuals taking some time to switch into non-employment activities such as pursuing education. We will return to consider this hypothesis when presenting results for human capital investment, but the bottom line is that it does not appear that pursuing higher education explains most of the reduction in labor supply that we observe.

One interesting note, however, is that it appears in quarterly administrative and survey data that treated participants may start to "catch up" towards the end of the study (Figure 7 and A3): at least, in these figures we cannot reject null effects for some quarters close to the end, while we can in the second or third year of the program. This could be due to the end of the transfers drawing near, 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 between the treatment and control group in terms of the magnitudes of the point estimates after the end of the program, but this difference is smaller than it was during the program and no longer significant for any of the measures.[36]

5.2 Other Employment Outcomes

Figure 8a summarizes other effects on employment outcomes at the index level, in standard deviations.[37] For ease of interpretation, even when an increase represents a potentially "negative" outcome, such as increased reports of disability or unemployment, these measures are reported as-is in this table (i.e., not reversed) but in red.

The indices for disability and duration of unemployment increase by the most. While one might expect disabilities to remain fairly constant throughout the course of the program, this is not necessarily the case if people are able to leverage the transfers to improve their health or, conversely, if people in the sample get more care and therefore get diagnosed more. It is also possible that if individuals are out of the labor force more, they may be more likely to think of themselves as disabled as a form of self-signalling (i.e., to mitigate any stigma associated with non-employment). We find a significant increase in the likelihood that a respondent has a self-reported disability (an increase of 4.3 percent-

[36]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 A8. Results are broadly comparable.

[37]Results for specific items under each index are provided in Appendix Tables A12-A27.

age points on a base of 31 percentage points in the control group) and in the likelihood they report a health problem or disability that limits the work they can do (an increase of 4.3 percentage points on a base of 28 percentage points in the control group) (Appendix Table A11). Participants also report slightly worse disabilities or health problems that have persisted for slightly longer periods of time. As a result of these consistent responses, the index for the family is significant. Somewhat reassuringly, none of these measures was significant at endline (Appendix Figure A15), which might support the hypothesis that participants received diagnoses early into the program and perhaps were able to partially treat them or have them otherwise be 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 immediately take up a new job upon leaving one or the transfers increase participants' reservation wage. These impacts translate to the duration of the average spell of non-employment causally increasing by 0.8 months relative to the control group mean of 7.8 months, with treated participants' longest spell of non-employment increasing by 0.9 months relative to the control group mean of 8.8 months (Appendix Table A12).[38] This is a relatively small number compared to evaluations of impacts of UI benefit extensions.[39]

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 8, which is perhaps notable despite its insignificance since the sample includes many older adults who one might expect to be less likely to go back to

[38]We construct two types of variables to examine impacts in this domain: 1) considering the average and longest duration of non-employment over the entire study, using an employment history timeline that we made that captures when participants left or started *any* job, including second, third, or fourth jobs, and 2) considering cross-sectional measures of how long participants were non-employed or unemployed at the point in time at which they answered a survey. The cross-sectional measures had slightly smaller estimated effects on lower control group means: the effect on the point-in-time measure of the duration of non-employment was 0.8 months, on an aggregate control group mean of 6.1 months, and the corresponding effect on the duration of unemployment at the time of being surveyed was 0.6 months on a control group mean of 2.9 months. The number of months of non-employment in the last year increased by 0.3 months, but this item (which was not pre-specified) is from a survey question in which participants were asked to identify which months they were employed in the last year, and people can be unemployed for far longer than a year. If the effect of the transfer on duration of non-employment is particularly large among those who have not been employed for more than a year, that would be consistent with the duration of non-employment increasing by less when participants provide their employment status for the last 12 months than when they report how long they have not been employed over an unrestricted period of time.

[39]There is a large literature on the impacts of extensions of UI benefits. In a meta-analysis of 52 studies, after accounting for publication bias, Cohen and Ganong (2024) find an elasticity of unemployment with respect to extending the potential benefit duration of 0.34, and an elasticity with respect to increasing the weekly benefit amount of 0.31. Someone who had been earning $21,000 in the baseline period (approximately the average individual income) might receive about $200/week in UI benefits for up to 26 weeks in Illinois and Texas normally, though benefits were extended during the pandemic. This weekly benefit is very close to what the treatment group would have received relative to the control group if the transfers we evaluate had been disbursed weekly. Yet the magnitude of the change in unemployment that we see is clearly much lower than the Cohen and Ganong (2024) meta-analysis result would imply (though within the large range of results from individual studies). It is important to note that the effects of UI benefits may be expected to differ from an unconditional cash transfer because they combine both income effects and substitution effects (i.e., one stops receiving UI benefits upon re-employment).

school. Full results are presented in Appendix Table A13. We pre-specified that we would conduct heterogeneity analysis for human capital outcomes based on the age of the participant at baseline since younger people tend to have higher rates of return to investment in education and may be more likely to embark on post-secondary education as a result of the transfers. When we conduct this heterogeneity analysis, we observe that those participants who were in their 20s at the time of the baseline survey qualitatively appear to have obtained more years of post-secondary education (Appendix Table A41). However, this response is not significant and is offset by older participants obtaining if anything less education.

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 lead to increased productivity and they would not show up in the UI data. This index is comprised of three components: the "entrepreneurial orientation" component captures willingness to take financial risks and includes both a survey measure and risk preferences from an incentive-compatible multiple price list experiment; the "entrepreneurial intention" component was based on questions such as whether or not the respondent has an idea for a business and the respondent's self-reported likelihood that they would start a business in the next five years; the "entrepreneurial activity" component captured whether participants actually started a job or is close to someone who started a job. We saw significant increases in entrepreneurial orientation and intention, but this did not translate into significantly more entrepreneurial activity (Appendix Table A15). The point estimate is positive, but very small, and it is possible that very few people have the inclination to become entrepreneurs in general. We pre-specified that we would consider entrepreneurial orientation and intentions 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 2.6 percentage points. There also appears to be a time trend, with this treatment effect growing over time and the point estimate only marginally insignificant in year three (Appendix Figure A17). It should be noted that the businesses being started are not very large.[40]

Participants were also asked about barriers to employment. One theoretical motivation for the provision of cash transfers has been that it might help individuals overcome challenges preventing them from working, such as a lack of transportation or childcare. However, we do not find significant

[40]To give some examples from open-ended questions, some participants started screenprinting t-shirts or bought a vending machine for their apartment block.

impacts on self-reported barriers to employment (Appendix Table A16).

On the whole, we do not see movement in the indices for job search or 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 A17). When asked about specific actions taken to search for a job, such as looking at job postings or contacting friends or relatives to find work, treated participants were more likely to report having taken several specific actions (Appendix Table A18), though the number of actions taken overall does not significantly change. However, while treated participants were more likely to apply for a job, they perhaps applied to on average about 0.8 fewer jobs in the last 3 months (compared to a control mean of 5.5 applications in that time period) and interviewed for fewer as well.[41] These results suggest that while treated participants are more likely to search for work, they either search a little less intensively or more selectively.

To disambiguate between searching less intensively or more selectively, we consider if there are changes in the types of jobs participants applied for. As the index value in Figure 8 shows, overall, we do not see much in the way of differences in the types of jobs participants applied for. This is true even at the item level (Appendix Table A20). In exploratory analysis of self-reported requirements for them to take a job, treated participants are more likely to say that interesting or meaningful work or work with flexible hours is a requirement, but these results do not survive the false discovery rate correction (Appendix Table A21).

As described earlier, there is debate in the literature as to whether quality of employment should go up or down in response to a cash transfer. In order to address this question, we included a large number of questions relating to quality of employment, divided into several components. Unlike the other families, this family of outcomes focuses exclusively on those who are employed, as it makes little sense to ask about quality of employment for those who are not employed. Note that since employment changed in the treatment group relative to the control group, there is some selection into our ability to observe these outcomes. However, since the extensive margin effects on labor supply were fairly small (an impact of 3.9 percentage points in the pooled administrative and survey data), we believe these quality estimates are still largely directly interpretable.

This index leveraged 35 primary items across six different components.[42] Despite the very de-

[41] These items are significant before the multiple hypothesis testing correction, but not after it.

[42] This count includes two single-item components. The six components include adequacy of employment, employment quality, informal work, the hourly wage, stability of employment, and quality of work life. The first component, adequacy of

tailed questions, the results do not support any changes in quality of employment, and for most items we can reject even small changes (see Appendix Table A22 for the component index measures and Appendix Table A23 for the raw item measures). For example, we observe a 11 cent decline in wages and can reject increases of more than 60 cents per hour and, overall, we can reject changes of more than 0.028 standard deviations in the family-level index or improvements of more than 0.022 standard deviations. There were two main clusters of variables that showed some significance. First, in the stability of employment component, a variable capturing how many jobs the respondent held in the past 12 months (or, descriptively, in the past two years) was significant before the false discovery rate adjustment. This could simply be a function of participants reducing their labor supply, rather than being a measure of quality of employment. Second, under quality of work life, the treatment effects appeared slightly negative for opportunities for promotion, treated participants were slightly more likely to say a scheduled shift was canceled with less than 24 hours notice in the last month and report a larger number of stressors in their work environment. None of these changes survived the false discovery rate corrections. Apart from results not being broadly significant, point estimates were generally small across the board.[43]

5.3 Other Outcomes

The largely - though not universally - negative effects on employment outcomes contrast with more positive impacts on other outcomes. Figure 8b shows a summary of index-level values for other families of outcomes, in standard deviations. Notably, there are relatively large changes in consumption and geographic mobility. The following subsections describe the different outcomes in more detail.

5.3.1 Consumption

Most papers that estimate MPEs do not have data on consumption and instead infer it based on assumptions about how much people might smooth consumption over time or to impute consumption

employment, includes whether participants are part-time in their main job and would prefer to work full-time, whether they would prefer to work more hours in their main job, and the number of jobs they hold. The employment quality component captures benefits that are provided, including whether training is provided by the employer and related survey questions, as well as whether the respondent must work irregular shifts. Third, we consider whether the respondent reports working any informal job and, in exploratory analysis, whether they report supplemental income from any gig economy jobs such as Uber, TaskRabbit, etc. Fourth, we elicit participants' hourly wage. Fifth, we consider stability of employment, including questions like how many months the respondent has been employed in the past year, how many months the respondent expects to remain in their main job, and whether their jobs are salaried or whether they are performing contract or freelance work. The last component, quality of work life, aims to capture the day-to-day experience at work, including questions such as whether the participant faces discrimination at work, how satisfied they are with the compensation and non-wage aspects of their main job, whether job demands interfere with family life, and the number of stressors in their work environment.

[43]Table A24 provides further exploratory analyses within this family of outcomes, including a more detailed breakdown of which specific benefits are offered by participants' employers.

using a combination of data on earnings and assets or asset returns.[44,45] We collect detailed survey data on consumption from enumerated surveys at baseline/midline/endline and roughly quarterly online survey modules.[46] To expand on the results in Figure 8, Table 5 presents the main results from these surveys, including estimates for total monthly expenditures as well as broad categories of expenses.[47] The largest absolute increases in spending are in non-durable goods and services, which also had the largest control group mean. Expenditures on human capital, durable goods, housing, and other expenditures are much smaller and all 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 a relatively small base, as indicated by the control group means.

These results would imply that roughly 32% of the transfers go to monthly consumption.[48] However, given that we observe very limited asset accumulation (on the order of \$0 to \$2000) and increases in debt (of around \$1000 to \$2000) in the treatment group relative to the control over the course of the study (Bartik et al., 2024), we expect that our consumption estimates in Table 5 understate the true amount spent.

There are several possible reasons for the consumption results being understated. First, this may in part be due to the fact the main regressions use data that are winsorized at 99%, following the pre-analysis plan, which was concerned about the possible impact of outliers. If treated participants have different needs and use the transfers to buy different things (one of the theoretical advantages of unconditional cash transfers), then this winsorization would by itself lead to underestimating the effect. We therefore also conduct median regression on the unwinsorized data, which indeed shows slightly larger impacts of about \$352 or 37% of the transfers (Table A59). Second, there is a large literature that suggests that effects on consumption are likely to be understated in surveys (e.g., Bee,

[44] A common approach is to assume the permanent income hypothesis holds and assume a certain discount rate (e.g., Golosov et al. (2023), who also use asset returns to calibrate savings).

[45] 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.

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

[47] 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.

[48] Recalling that the denominator is \$950 to account for control group payments.

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, 2023).[49]

In Table 6, we consider how great a share of the transfers might go to consumption (as opposed to income or net worth) under different assumptions. Column (1) shows our unadjusted estimates. Column (2) rescales consumption by the ratio of the PCE/CE.[50] Column (3) assumes that the effect on net worth is $5000 (approximately the upper bound of its estimated confidence interval in Bartik et al. 2024) and assigns the remainder to consumption, proportionately. Finally, Column (4) assumes all under-reporting is in consumption. The true share spent on goods and services likely falls between the values in Columns (3) and (4).

5.3.2 Labor Market Mobility

Apart from changes in consumption, we observe particularly large changes in where participants live over the course of the study, which can affect labor market outcomes (e.g., Chetty and Hendren, 2018). On average, 43% of those in the control group move housing units since baseline, and 4.1 percentage points more people moved in the treatment group, with the vast majority of these moves being to different neighborhoods, defined as a different Census tract (Bartik et al., 2024). Fewer moves were to different labor markets, which we define as moves to a different commuting zone (Appendix Table A25). In particular, by the end of the study, 12% of control households had moved labor markets since baseline, and 1.9 percentage points more people in the treatment group moved labor markets. The treatment group also reported more active labor market search behaviors and participants indicated significantly greater interest in moving labor markets, such that the overall index for moving labor markets is highly significant with an effect size of 0.09.

Perhaps because most moves are within the same labor market, we do not see significant changes in the quality of the labor markets participants reside in (Figure 8). The only exception at the item level is that treated participants are perhaps more likely to live in areas where the BLS projects more job growth for the respondent's education group, though this is only marginally significant and does not survive the false discovery rate adjustments (Appendix Table A26). Further, all differences are

[49]These are the most recent estimates put out by the BLS. The CEX and PCE capture different items, but this statistic was formed on comparable categories, and in general the PCE is typically taken to be more accurate and less subject to underreporting as it is based in part on sales data from retailers.

[50]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 H.

relatively small. 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, by revealed preference participants moved; this suggests it was welfare-enhancing for them even if it did not improve their employment prospects. Second, it is possible that even moves within a commuting zone could affect proximity to certain labor markets.

5.3.3 Subjective Well-Being

It would be natural to think that if treated participants work less and spend more, they might be happier as a result of the transfers. Some past work on lottery winners does indeed find lasting effects on well-being (Lindqvist, Ostling and Cesarini, 2020). We instead find temporary increases 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). Despite asking participants about subjective well-being in several different ways, including eliciting measures of life satisfaction, satisfaction across various domains such as satisfaction with standard of living, satisfaction with health, and satisfaction with time for enjoyable things, and measures of affect balance,[51] the index and all components are insignificant, and the point estimate for the index and two of the three components are negative (Table 7).

Figure 9, which plots results over time, demonstrates the dynamic nature of the estimates: in year 1, we observe a positive effect of the transfers on all components of subjective well-being, but by year 2 this treatment effects becomes smaller and insignificant, 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: if participants had only been followed for one year, we might have come to very different conclusions about the effects on well-being. In companion papers, we show that these types of dynamics–in which meaningful beneficial effects fade out quickly–also appear for outcomes related to mental health and stress and food security (Miller et al., 2024), as well as subjective perceptions of financial health (Bartik et al., 2024).

5.3.4 Relationship Status

In order to interpret the effects on employment and income that we observe, we need to also consider potential changes in household composition. The initial analyses of the NIT experiments in the 1970s

[51] We use the Scale of Positive and Negative Experience (SPANE).

showed an effect on marriage dissolution and if the cash transfers in our study caused people to leave the household, that could mediate the effects we observe on total household income. However, the treatment did not cause any significant changes in relationships between participants and others in their household (Figure 8). A relatively small share of people in our sample were married at baseline, and other types of relationships were more common, so we asked about relationships in several different ways. We observed no effect on whether the respondent is divorced, whether the respondent has a spouse/partner, or whether the respondent is in a romantic relationship (Appendix Table A28). If anything, it is possible that participants had more changes in relationships with romantic partners outside the household, but not with partners within the household.[52]

5.3.5 Take-Up of Benefits

Finally, we consider effects of the transfers on receipt of benefits. The effects are theoretically ambiguous. On the one hand, some literature suggests that low-income individuals may be particularly bandwidth constrained and so may be less likely to take up certain benefits that could have onerous application processes. On the other hand, we might expect that the transfers could partially substitute for other benefits and make participants feel like they have less need to apply for them. Further, we expect a small mechanical effect on benefits since while almost all benefits were preserved, some participants may have become temporarily ineligible for food stamps in Texas, as described earlier. Overall, we do not observe statistically significant effects on benefits take-up (Figure 8). Benefits decrease by about $260 per year, but this is a very noisy estimate (Appendix Table A27).[53] To the extent that benefits were reduced by the transfers, however, the elasticity estimates may slightly understate the income effects of the transfers.

6 Discussion

6.1 Heterogeneity in Treatment Effects

We pre-specified several heterogeneity analyses that consider impacts by various attributes participants held at baseline. These subgroup analyses all are adjusted for multiple hypothesis corrections as discussed in Section 4 and Appendix D. Due to these tests being pre-specified as exploratory, they

[52] In exploratory analysis, we consider self-reported reasons why relationships ended (Table A29), but there are no clear trends. If anything, participants may be more likely to report they themselves ended the relationship, rather than it being ended by their partner or by mutual agreement, but this result does not survive the false discovery rate adjustment.

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

are unlikely to be significant once the false discovery rate correction is applied unless the estimated effects are quite large, however, it can still be informative to consider the past estimates and broad trends observed across different measures.

The treatment effects on income appear stronger among those who were above the Federal Poverty Level at baseline (Table A30). This is consistent with what we would theoretically expect with decreasing returns to income. It is also in line with other lottery studies, in which higher-income individuals are seen to adjust their labor supply by more than lower-income individuals (Golosov et al., 2023); we confirm this pattern holds even at lower absolute income levels. The fact that income effects are different for relatively low-income individuals has implications for redistribution: the income effects of redistribution would not "cancel out" when redistributing from the rich to the poor, but rather we observe smaller income effects for those who are lower-income at baseline.

We observe interesting heterogeneity in treatment effects by education. Treated participants who did not have a bachelor's degree at baseline seemingly reduced their income and employment by more than those who did (Tables A31 and A35). In fact, those with a bachelor's degree had insignificant increases in individual salaried/wage income, while potentially reducing supplemental income from gig work. While these subgroup analyses are only exploratory, they align with heterogeneity tests by age: negative labor supply effects are larger for participants in their 20s at baseline (Table A36), and we also observe qualitatively larger effects on formal education among those in this younger age group (Table A41), though these latter estimates are not significant. This suggests a story in which younger participants may be more likely to use the money to enroll in post-secondary education and do not work as much while they do so. However, this is only suggestive, and it remains possible that we observe larger negative labor supply effects on participants in their 20s without a college degree for other reasons. The quality of employment measures are broadly comparable between those who had a bachelor's degree at baseline and those who did not, with potentially slightly more negative impacts among those without a bachelor's degree at baseline (Tables A44-A45). Again, caution should be taken in interpretation given the large number of items tested.

We also pre-specified a close look at outcomes by sex, since the literature often finds large empirical differences along this dimension (e.g., Eissa and Hoynes, 2004). In the survey data, differences in impacts on income and employment by sex are mixed. Males in the treatment group may have had slightly larger reductions in income relative to the control group according to one self-reported measure (Table A32), while females appear to have slightly larger treatment effects on labor supply

(Table A37). In general, we cannot observe significant differences by subgroup here.

Finally, we pre-specified two heterogeneity analyses for the entrepreneurship family of outcomes, looking at differences by age and education at baseline. These results are relatively noisy, but there may be larger effects on entrepreneurial intention for those who do not have a bachelor's degree at baseline and who are in their 30s at baseline (Appendix Tables A42-A43).[54]

One potential source of heterogeneity in treatment effects that we did not pre-specify that we would consider is heterogeneity by state. We observed substantial differences in the income and labor supply effects by state in both the UI and survey data (Tables A33, A38, and A46). While the sites differ along several dimensions and so we cannot attribute the observed differences to any one factor, several differences between the sites are worth highlighting. First, the Texas site had a lower cost of living than the Illinois site. This means that the transfers could in principle go farther, and thus we may anticipate them to have a larger effect on earned income and labor supply. Second, the Illinois site has a more generous set of existing social safety net programs. In Texas, given that there are few public benefits, the transfers may be filling a more substantial gap in basic needs, potentially triggering 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 expect 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 any significant changes in either participants' reservation wage (Table A20), employed participants' wage rate (Table A22), or the weight participants placed on potential income in job search (Table A21), and in fact baseline wages were insignificantly higher in our sample in Texas, so this explanation may be less likely.[55]

[54] Additionally, we pre-specified two more exploratory heterogeneity analyses. These tests were designed to focus more on attributes of participants that relate to how they were recruited to the sample. In particular, 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. This supports the negative effects on income being smaller for groups that had lower household income at baseline (Appendix Table A39). 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 A40). However, we do not wish to lean too hard on these results given the relatively small size of the subgroups involved.

[55] We may more generally anticipate some minor differences in results given that we are unable to include the full set of Lasso-selected controls in the Texas administrative data environment, though given the randomized nature of the treatment we do not expect this to drive results. As described in the methods section, we included a subset of 56 baseline covariates, focusing on variables capturing demographic information, employment, income, household composition, relationship status, and county type.

Another potential source of heterogeneity in treatment effects that we did not pre-specify is whether or not the participant had children in the household at baseline. We observe substantially larger negative effects on income for those who did not have children at baseline (Table A34). This could be consistent with households with children having a greater need for income. Alternatively, it is possible that this relates to the earlier observation that effects tend to be larger for younger participants. Ultimately, it should be remembered that these heterogeneity analyses are not causal and the variables considered could merely be correlated with other variables that mediate income effects, without mediating them directly themselves.

6.2 Comparison of UI and Survey Data

In general, results from the survey data line up very well with results in the UI data. Table A46 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 6-7 and A3). 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 remarkably similar stories about the effects and the trajectory of those effects over time, increasing our confidence in these results.

6.3 Robustness Checks

While differential attrition was very low over the study period, we nonetheless performed a number of pre-specified robustness checks. In particular, we conducted a difference-in-differences analysis; restricted 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 estimated a set of results with Lee bounds. In addition, given that some variables are more likely to contain outliers, we conducted median regression for these outcomes. We also provide 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 A47-A63). 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.[56] This is also generally true for component-level estimates. 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 are still broadly in line with the main estimates. The regression on whether the respondent is employed based on survey data is significant in the robustness check without covariates and when restricting attention to data from the enumerated midline and endline surveys, but it is not in the difference-in-differences or bounding analysis. The magnitudes of the point estimates remain broadly comparable.

6.4 Comparison to Other Transfers

Much of the evidence on cash transfers in high-income contexts has come from studies of lottery winners where, like in our setting, a pure income effect can be observed. However, lottery winnings are generally disbursed by lump sum, or else by long-term annuities, and it is not straightforward to compare these transfers to our monthly sustained cash transfers. Most studies assume that lottery winners follow the permanent income hypothesis and save a large share of their winnings for future time periods, only slowly spending it down (Golosov et al., 2023; Cesarini et al., 2016; Imbens, Rubin and Sacerdote, 2001). In this way, 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. In

[56] The exceptions are: the 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 move labor markets index has an insignificant lower Lee bound.

particular, even under the most optimistic estimates, it cannot be the case that our participants save anywhere near the share of the transfers that would be implied by this model.[57]

Instead, our estimates of the MPE line up more with estimates from the lottery literature if we assume that rather than save per the standard assumptions in the lottery literature, individuals view the monthly transfers as spending money. Appendix Table A64 compares estimates of the MPE from lottery studies, assuming the permanent income hypothesis holds, and the estimates in our paper assuming that individuals do not, on net, save the transfers for future time periods.[58] We can observe that the MPEs from the lottery studies, calculated per Golosov et al. (2023), line up pretty well with the MPEs we estimate assuming participants do *not* save any of the transfers for future periods. Further, 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: 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, we would calculate a similar MPE to what we obtained in this study.

There may be something about receiving a lump sum that encourages people to think about saving a large share for the future, or conversely something about receiving monthly transfers that encourages participants to think of the transfers as spending money for that month. People who play the lottery may also be differently selected from our sample. In general, compared to lottery winners, our participants are younger and lower-income, so one might imagine they are more constrained and may spend more of the transfers to meet their basic needs. The lottery studies typically have not been able to directly observe savings or consumption, and it is possible that if they had observed these out-

[57]Specifically, per Golosov et al. (2023), 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:

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

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, they would not be close to 90%, and we observe more than 10% being spent on each of several categories of spending, such as leisure or non-durable goods and services. It does not seem plausible that our treatment participants receiving $1,000/month are only spending $95 more per month, on average, than the control participants receiving $50/month. Nor could a higher discount rate make the model fit: taking the most optimistic estimate, that the transfers lead to a causal change in net worth of $5,000 (the upper bound of the wide confidence interval on net worth in Bartik et al. 2024), treated participants consume about 85% of the transfers over the 3 year period. But the right-hand side of equation 2 goes to 0 at both small and large values of d and peaks at around $d = 0.58$ with a share consumed during the three-year period of just under 50%; even under the heroic assumption of $d = 0.58$, we observe participants spend much more than 50% in expenditures and reduced income in the data.

[58]Participants may still save for the future through purchases of durable goods or through investments in human capital, but for the sake of exploring how the studies compare, this assumption may still be illustrative.

comes and were to restrict attention to those who are socio-economically similar to our participants, perhaps those recipients would be seen to also save less. While our participants do not appear to save much, in other respects our results support the literature from lottery studies. For example, consistent with Golosov et al. (2021), we observe that those who at baseline had higher income levels were also likely to reduce their labor supply more (Table A30).

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 programs provide low-income workers who file taxes with a refundable credit; Paycheck Plus was designed as a supplement to the EITC, particularly for those without dependent children, as the EITC is largely geared towards those with dependent children. Unlike lottery winnings or monthly unconditional cash transfers, these transfers directly incentivize work. Their effects on incentives to earn are ambiguous, however, and may depend on how much a worker would have earned in the absence of the credit: because these programs both phase-in and phase-out at different earned income levels, they may incentivize earning additional income among low-earning workers but disincentivize earning additional income among those close to the phase-out income levels.

Evaluations of the EITC have focused, like the EITC itself, on households with children. Eissa and Liebman (1996) find that the EITC increased employment among single women with children,[59] while Eissa and Hoynes (2004) caution that the same program may decrease incentives to work for married women with children given that they are likely to be secondary earners. The recent evaluations of Paycheck Plus provide supporting evidence that this type of program can boost employment. In the New York evaluation, the treatment group was 2-3 percentage points more likely to be employed in years 2 and 3 of the program (Miller et al., 2018), and in Atlanta the treatment group was insignificantly about 1 percentage point more likely to be employed in years 2 and 3 of the program (Yang et al., 2022).[60] The programs had no significant effect on earned income in either site in any year.[61] The different effects of these programs on incentives may help to explain their different impacts, though the samples and structures of the payments also differed.

[59] Though a recent re-analysis by Kleven (2024) suggests the effects may be modest.

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

[61] 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.

6.5 Comparison to Forecasts from NBER Affiliates

We can also compare our estimates, more generally, to the current received wisdom about cash transfers by surveying experts in economics as to what they think we will find. As described in DellaVigna, Pope and Vivalt (2019), expert forecasts can be a valuable tool for judging the novelty of research findings. We elicited forecasts from a subset of researchers affiliated with the National Bureau of Economic Research (NBER). These researchers were affiliated with at least one of several NBER Programs.[62] 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 normally expect for researchers at this level of seniority.[63] Researchers were not compensated, and the survey was unincentivized.[64]

We supplemented the sample by eliciting forecasts from users of the Social Science Prediction Platform (SSPP), including its Superforecaster Panel.[65] 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 A65 presents results. Interestingly, NBER Labor Studies affiliates and SSPP users perform 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

[62] 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.

[63] Ferguson et al. (2023) suggest a 10-24% rate is typical.

[64] Given the researchers' level of seniority, this 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. See Ferguson et al. (2023), who randomize $75 and $100 incentives to faculty.

[65] `https://www.socialscienceprediction.org/`.

median responses. These forecasts somewhat understated the observed effects for employment and income, but are generally within their confidence intervals; for effects on individual salaried income, the median but not the mean fell within the estimate's confidence interval. However, there was great heterogeneity in beliefs. Figure A32 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 -\$0.18 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 (2.5-4.4 percentage points), while our point estimate for the final year of the program was 0.2. Again, the confidence interval on the point estimate excludes the mean and median of any subgroup's forecasts.[66]

Overall, this analysis suggests that economists have more of a sense for effects on labor supply than they do for 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.

7 Conclusion

After decades of shifting welfare assistance from direct cash payments to in-kind benefits, cash transfers have increasingly been proposed as a way to alleviate poverty and provide beneficiaries the flexibility to purchase what they need. At the same time, some policymakers have raised concerns that such transfers may lead beneficiaries to pull back from the labor market, which may in turn increase

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

the need for and reliance on future transfers and dampen beneficiaries long-term job prospects, while raising the fiscal cost of the transfers themselves. Alternatively, if cash transfers help beneficiaries search for higher quality or better fitting jobs, start new businesses, or invest in their future earnings via human capital, a reduction in labor supply may ultimately be productive.

Our results provide support for both sides of this debate. On the one hand, we do find that the transfer we study generated significant reductions in individual and household market earnings. The reductions in individual labor supply we observe are smaller than what has been documented in some settings (e.g., Golosov et al., 2023), but larger than what has been observed in others (e.g., Imbens, Rubin and Sacerdote, 2001; Cesarini et al., 2017). The spillovers onto other household members–who also reduced their labor supply in response to the transfer–implies the total amount of work withdrawn from the market is fairly substantial. Further, we do not find evidence of the type of job quality or human capital improvements that advocates have hoped might accompany the provision of greater resources, and our confidence intervals allow us to rule out even very small effects of the transfer on these outcomes. On the other hand, we find that participants showed more interest in entrepreneurial activities and willingness to take risks due to the transfers, which could improve future earnings and lead to additional economic benefits over time. And, exploratory analysis of subgroups suggests that not all responses to the transfer were identical: older participants experienced very little change in either labor supply or human capital, while younger participants reduced time spent working but appeared to pursue more 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 continued work or, equivalently, that the marginal value of leisure rises more rapidly than the marginal value of consumption as income rises, at least given the kind of work that is available to our participants.

While the our data collection was extremely comprehensive, future work would improve our understanding of the long-term impacts of income on employment. In particular, follow-ups could consider to what extent labor market effects persist after the end of the transfer period and shed light on effects on participants' children as they grow up, which may be particularly important in policy decisions. Additional work would be needed to understand the potential general equilibrium effects that might arise should such a program be scaled up.

Other outcomes may also be relevant for policy. Regarding health impacts, in Miller et al. (2024) 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, while we can rule out even small improvements in physical health, including as captured through biomarkers from blood draws. In Broockman et al. (2024), we find no effect on political preferences or participation, though we see some signs of mood misattribution. Bartik et al. (2024) finds transfers had a short-term impact on self-reported financial health but did not appear to substantially affect net worth or financial behaviors such as delinquencies during the course of the program. Krause ⓡ et al. (2025) finds limited impacts on children and parenting overall. Parents spend more on children but report that their children have more developmental difficulties and stress, potentially due to a monitoring effect. There are also no substantial improvements in educational attainment or changes in fertility.

Our analysis demonstrates that even a fully unconditional cash transfer results in moderate labor supply reductions for recipients. Virtually all existing large-scale cash transfer programs in the U.S. are means-tested, which provides additional disincentives to work. Rather than being driven by such program features, participants in our study reduced their labor supply because, as their incomes rise, the marginal value of leisure became 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.

Table 1: 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	0.58	0.58	0.62	0.57	0.59	0.57
HH Size	3.35	3.23	3.30	3.14	3.20	2.98
Age < 30	0.52	0.53	0.53	0.54	0.54	0.54
White (non-hispanic)	0.58	0.44	0.38	0.48	0.46	0.47
Black (non-hispanic)	0.16	0.25	0.30	0.25	0.26	0.30
Hispanic	0.18	0.23	0.26	0.22	0.22	0.22
Female	0.57	0.60	0.62	0.68	0.69	0.67
HH Income	37,003	30,611	31,210	32,327	29,245	29,942
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

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 and county type distribution. Our sample is very similar along most dimensions, though our participants are a little more likely to be renters. It should be noted that columns (4) and (5) use data from the online screener while column (6) uses baseline survey data, so the numbers may differ slightly.

Table 2: Descriptive Statistics: Baseline Covariate Balance

	Treatment	Control	p-value
Demographic			
Age	30.169	30.035	0.542
Female/Other	0.672	0.681	0.627
Non-Hispanic Black	0.294	0.305	0.536
Hispanic	0.220	0.214	0.709
Non-Hispanic White/Other	0.486	0.481	0.798
Household Size	2.943	2.996	0.435
Number of Other Adults in the Household	0.684	0.716	0.347
Any Children	0.568	0.571	0.897
Has Disability	0.338	0.311	0.130
Bachelor's Degree	0.202	0.205	0.866
Employed	0.578	0.586	0.675
Income and Employment			
Total Household Income ($1000s)	29.950	29.895	0.942
Total Individual Income ($1000s)	21.255	21.172	0.917
Work Hours/Week	21.734	22.140	0.631
Has a Second Job	0.168	0.173	0.712
Months Employed in the Past Year	7.215	7.268	0.778
Number of Jobs in the Past 1 Year	1.403	1.439	0.457
Number of Jobs in the Past 3 Years	2.684	2.620	0.485
Searching for Work	0.495	0.510	0.429
Started or Helped to Start a Business	0.315	0.296	0.268
Housing			
Lived Temporarily with Family or Friends	0.262	0.281	0.286
Stayed in Non-Permanent Housing	0.086	0.084	0.811
Housing Search Actions in Last 3 Months	0.255	0.242	0.447
Number of Times Moved in the Past 5 Years	1.328	1.358	0.468
Relationships			
Is in a Romantic Relationship	0.627	0.621	0.749
Lives with a Partner	0.331	0.324	0.681
Married	0.221	0.222	0.951
Divorced	0.077	0.081	0.706
Monthly Consumption ($1000s)			
Total Consumption	3.348	3.309	0.529
Non-durable Goods and Services	1.836	1.830	0.873
Housing Expenditures	0.678	0.652	0.222
Human Capital Expenditures	0.411	0.392	0.429
Durable Goods Expenditures	0.304	0.321	0.155
Other Expenditures	0.119	0.114	0.538

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.

Table 3: 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)	-4.2***††† (1.0) [0.001]	-0.35 - -0.44	-0.33	2898
(2) Total individual income	33.6 (25.1)	-2.3***††† (0.7) [0.008]	-0.19 - -0.24	-0.26	2855
(3) Total calculated individual income	36.6 (27.0)	-1.5*† (0.9) [0.096]	-0.12 - -0.15	-0.16	2881
(4) Individual salaried/wage income	26.0 (26.2)	-1.2 (0.8) [0.405]	-0.10 - -0.12	-0.21	2920
(5) Self-employment income	5.9 (13.7)	-0.1 (0.5) [0.642]	-0.01 - -0.01	-0.06	2902
(6) Income from supplementary gig work	0.4 (1.3)	-0.1 (0.0) [0.317]	-0.00 - -0.01	-1.67	2925
(7) Passive income	0.0 (0.2)	0.0 (0.0) [0.317]	0.00 - 0.00	0.59	2923
(8) Other income	4.7 (6.1)	-0.1 (0.2) [0.642]	-0.01 - -0.01	-0.04	2935
(9) Government transfers	3.6 (4.9)	-0.2 (0.1) [0.650]	-0.01 - -0.02	-0.10	2962
Panel B: UI Data					
(10) Individual salaried/wage income	21.2 (23.6)	-1.7* (0.9) [0.284]	-0.14 - -0.18	-0.30	1907
Panel C: Pooled UI and Survey Data					
(11) Individual salaried/wage income	22.0 (24.2)	-1.6* (0.9) [0.237]	-0.13 - -0.16	-0.27	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 -$3,300 and -$1,800, respectively. All measures are survey-based except for the pooled UI and survey data estimate and the UI data estimate. Appendix J describes the approach to pooling. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table 4: 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)	-1.37**†† (0.63) [0.032]	-0.15	2940
(2) Whether the respondent is employed	0.74 (0.39)	-0.02* (0.01) [0.622]	-0.09	2962
UI Data				
(3) Whether the respondent is employed	0.61 (0.44)	-0.07*** (0.02) [0.115]	-0.27	1907
Pooled UI and Survey Data				
(4) Whether the respondent is employed	0.63 (0.43)	-0.04**†† (0.02) [0.040]	-0.16	2275
Panel B: Other Household Members				
Survey Data				
(5) Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.42*** (0.78) [0.210]	-0.19	2945
(6) Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.31** (0.91) [0.291]	-0.15	2945
(7) Total number of hours participant's parents in household work per week	3.22 (12.07)	0.12 (0.36) [1.000]	0.08	2941
(8) Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.21 (0.23) [1.000]	1.27	2945
(9) Number of other household members which are employed	0.47 (0.61)	-0.01 (0.02) [1.000]	-0.06	2943

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 J describes the approach to pooling. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table 5: Impact of Guaranteed Income on Consumption

	Control Mean	Treatment Effect	N
Total Consumption	4139 (1851)	**306***†††**	2988
		(48)	
		[0.001]	
Non-durable goods and services expenditures	2032 (946)	133***†††	2987
		(26)	
		[0.001]	
Housing expenditures	809 (592)	34**†††	2977
		(17)	
		[0.007]	
Human capital expenditures	505 (462)	50***†††	2988
		(15)	
		[0.001]	
Durable goods expenditures	517 (400)	52***†††	2987
		(14)	
		[0.001]	
Other expenditures	276 (361)	37***†††	2987
		(11)	
		[0.001]	

This table shows the impacts of an unconditional cash transfer on 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 6: 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	0.06	0.08	0.09	0.11
Human capital	0.05	0.11	0.12	0.15
Non-durables (excluding housing)	0.14	0.25	0.28	0.35
Housing services	0.04	0.04	0.04	0.05
Other	0.04	0.06	0.06	0.08
Income				
Household income	-0.29	-0.29	-0.29	-0.29
Household balance sheet				
Net worth	-0.02	-0.02	0.15	-0.02
Unexplained				
Residual	0.41	0.21	-	-

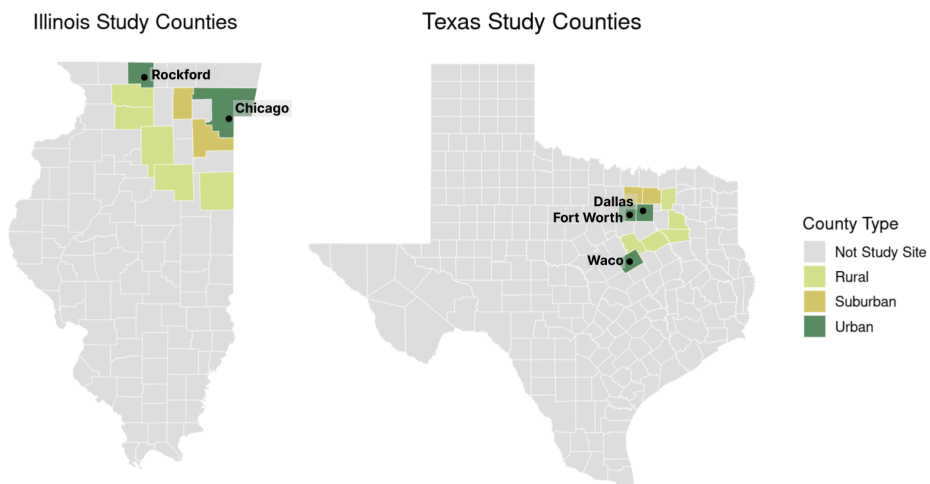
This table shows how participants allocate the transfers across different categories of spending, in particular to expenditures on goods and services, leisure (reduced income), and net saving. Per Bartik et al. (2024), there is a slight imprecisely-estimated decrease in net worth. Column (1) is calculated 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 H. Column (3) assumes net worth increases by $5,000 over the course of the study (approximately the upper bound of the confidence interval on net worth in Bartik et al. (2024)) 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.

Table 7: Impact of Guaranteed Income on Subjective Well-Being

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

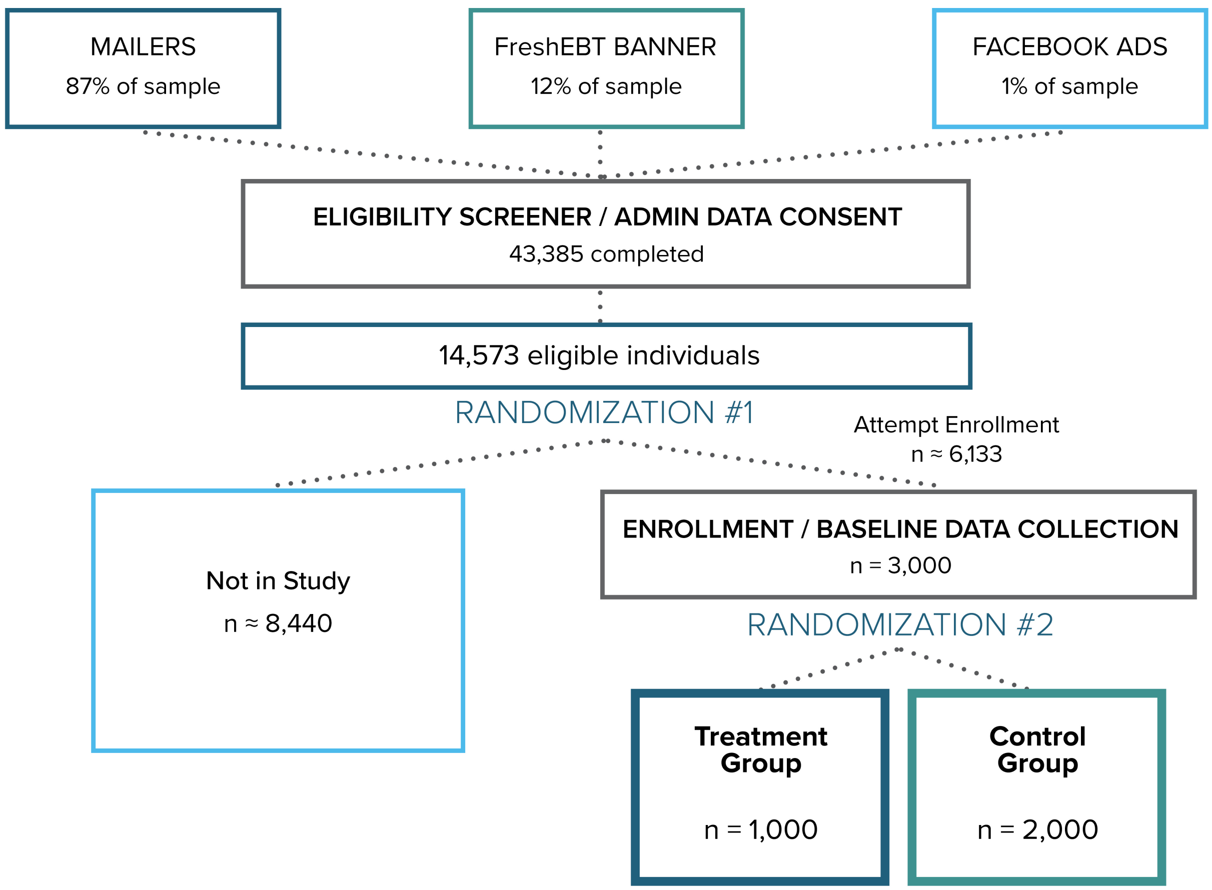
This table shows the impacts of an unconditional cash transfer on subjective well-being. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Figure 1: Location of Study



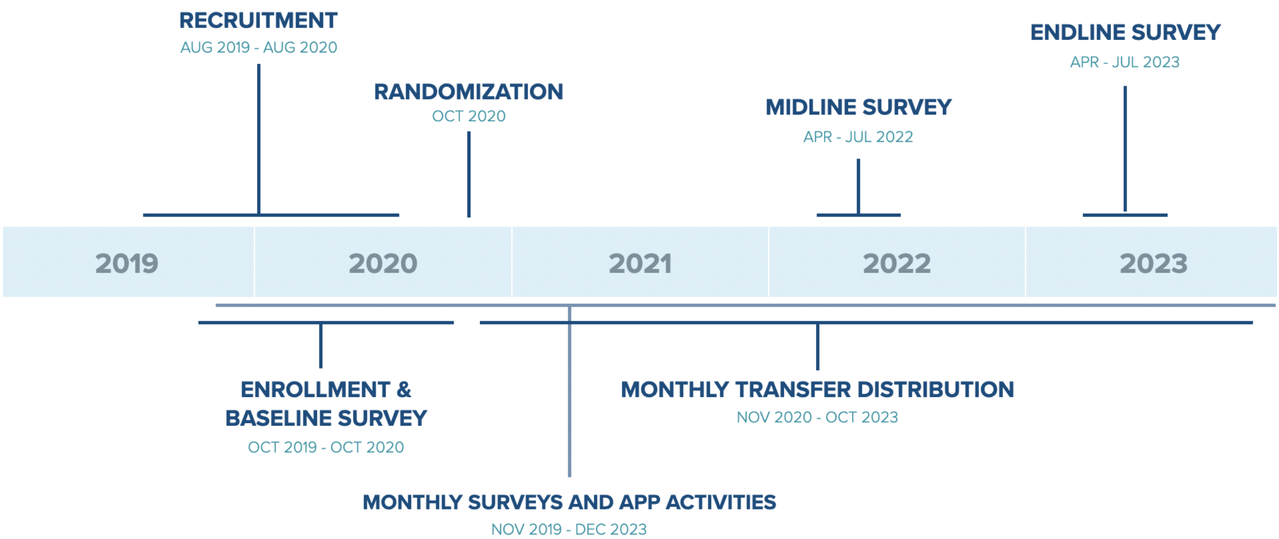
This figure plots the location of the sites in the study. Reproduced from Bartik et al. (2024).

Figure 2: Flowchart of Recruitment Process



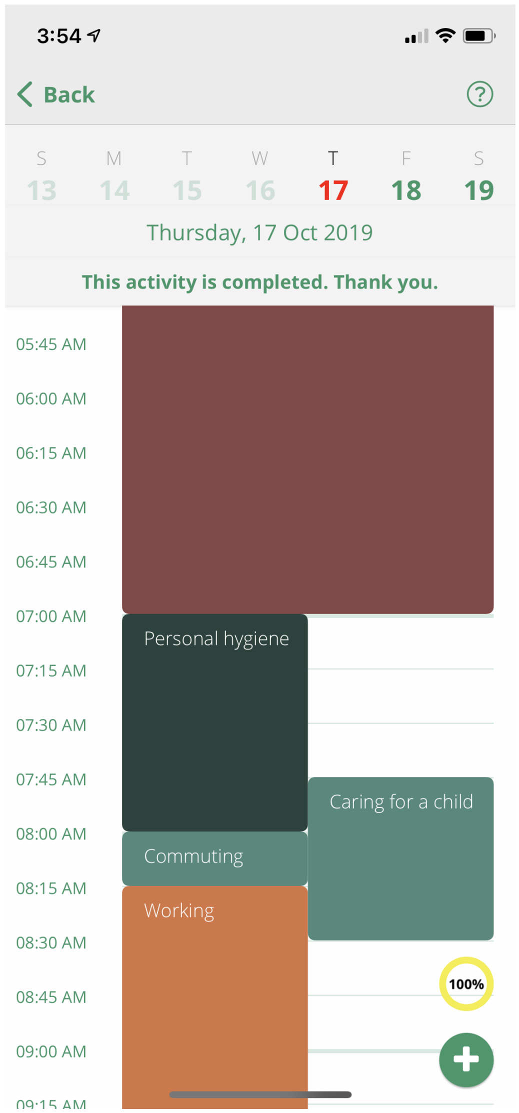
This figure shows a representation of the recruitment process.

Figure 3: Timeline of Study



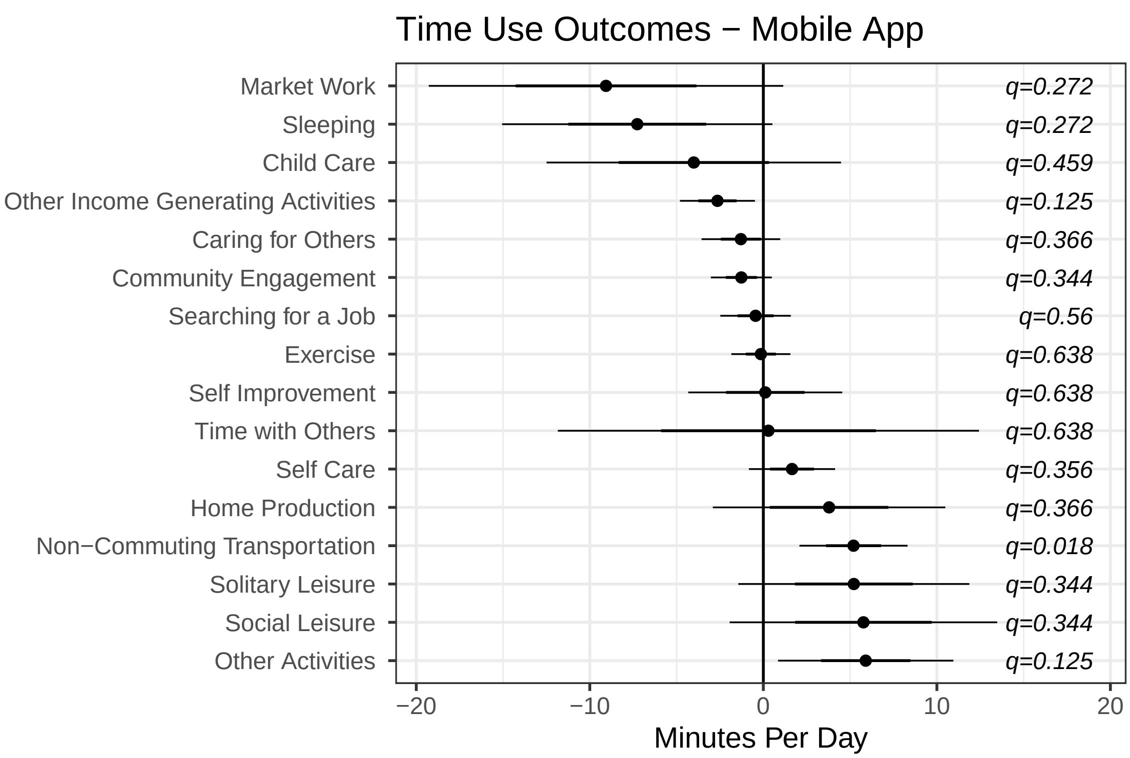
This figure shows a timeline of the program and study.

Figure 4: Time Use Mobile App



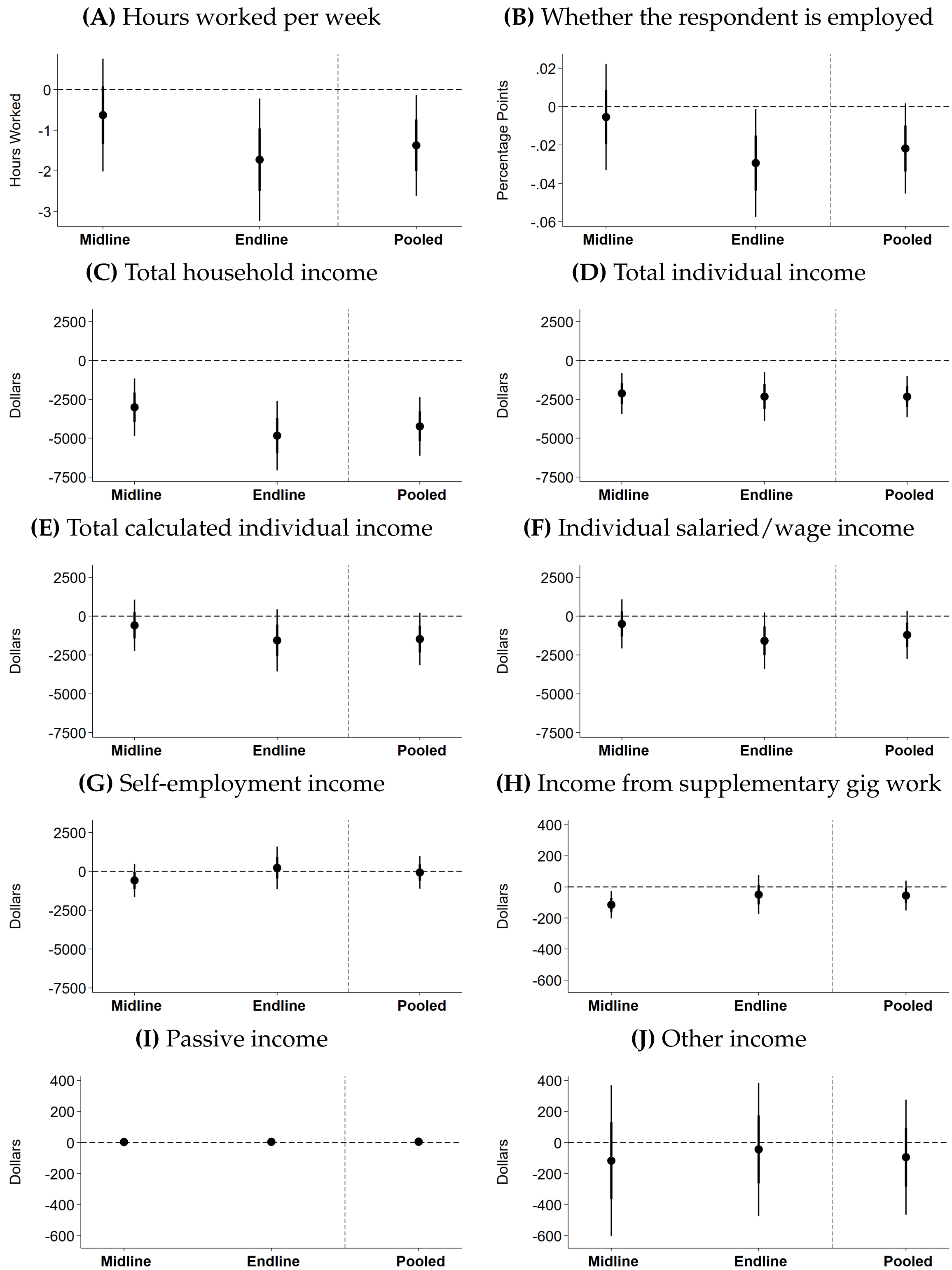
This figure shows a screenshot of the mobile phone application participants used to fill in time diaries on a randomly-selected weekday and weekend day each month.

Figure 5: Time Use Results: Mobile App



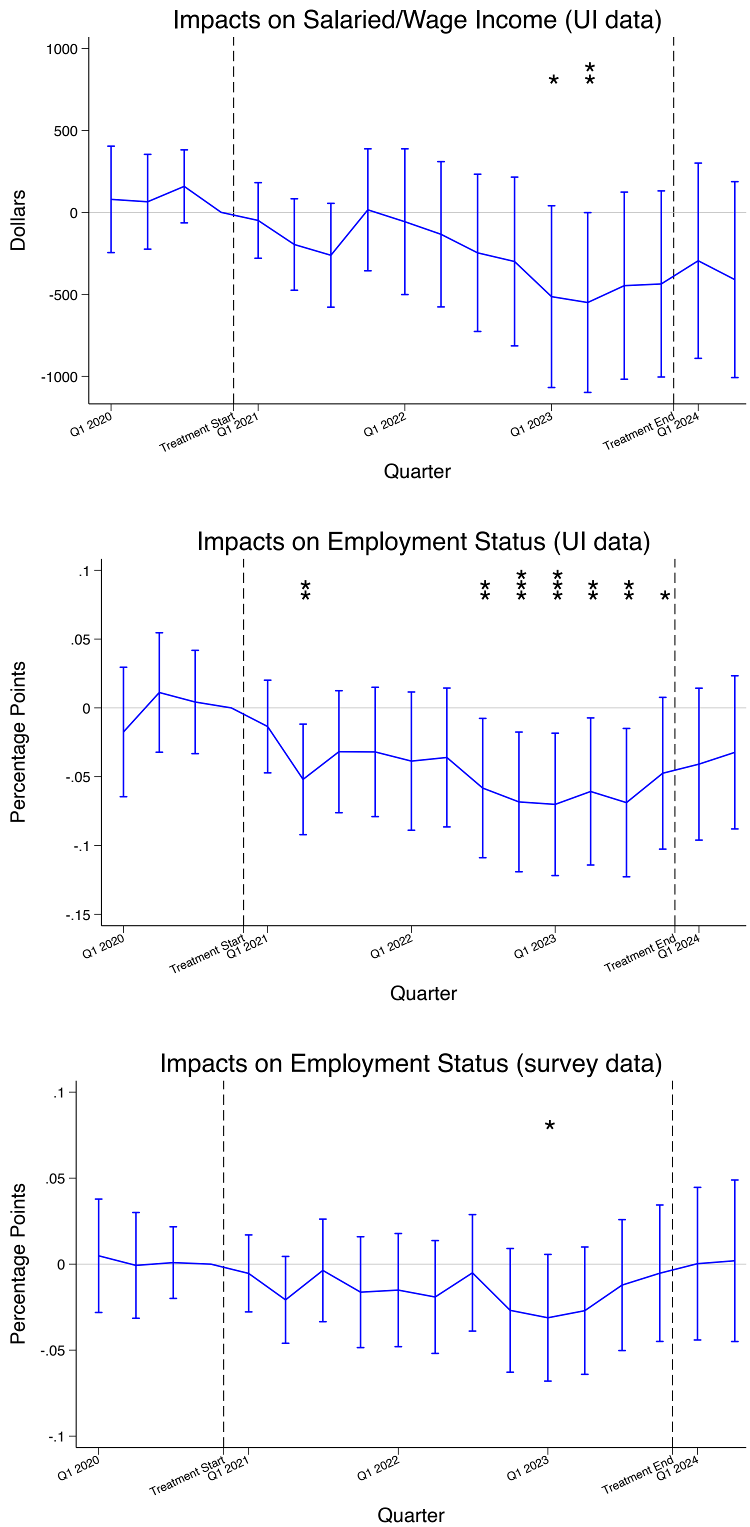
This figure shows the main results from the time diaries. Treatment effects and confidence intervals are plotted, while q-values are provided alongside.

Figure 6: Estimated Effects on Income and Employment Measures, Enumerated Survey Data



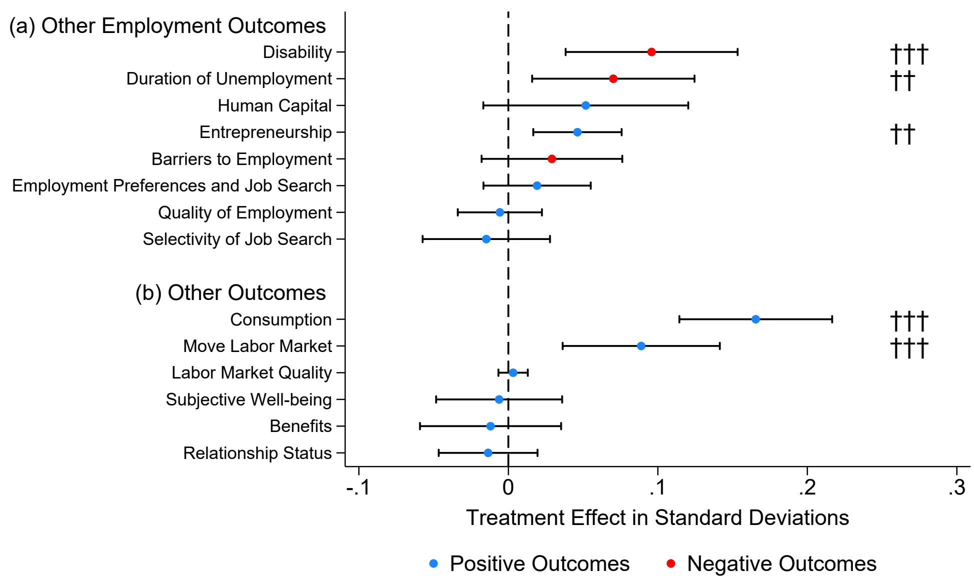
This figure plots the results for treatment effects on income and employment over time from the enumerated survey data, showing a clear time trend in the major categories of income and that treatment effects on employment are trending more negative towards the end of the study. 95% confidence intervals are provided.

Figure 7: Event Study Results for Income and Employment



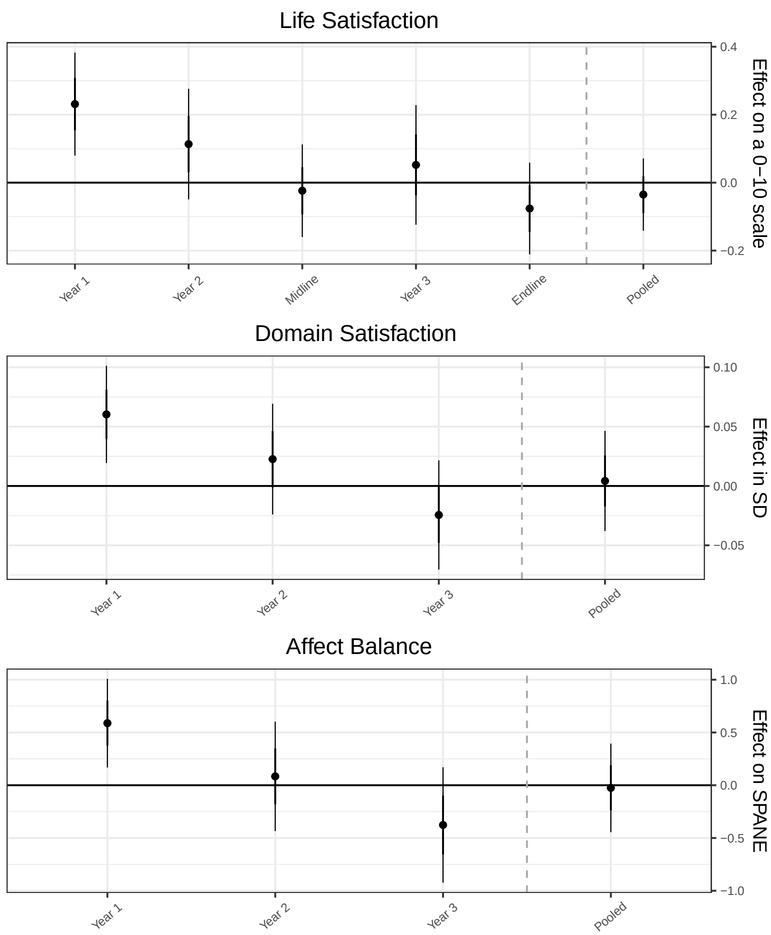
This figure plots the results for income and employment over time, leveraging an event study analysis. The data points represent estimated effects for the preceding quarter, while 95% confidence intervals are shown. Results from Illinois and Texas are pooled in these figures, following Appendix J. Q3 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 8: Family-Level Index Results for Other Index Measures



This figure plots the results for family-level indices, with significance per q-values denoted by daggers. Those families in red (disability, duration of unemployment, and barriers to employment) represent "negative" outcomes (e.g., a positive value as shown in the figure for duration of unemployment represents an increase in duration of unemployment), while families in blue may be more likely to be thought of as positive outcomes. When results are broken down by time period in the appendix, the sign of the "negative" outcomes is reversed per our standard protocol.

Figure 9: 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.

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

Appendix

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

A Details on Recruitment

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. In the end, 13% of program participants lived in rural counties, 18% in suburban, 16% in medium urban and 53% in large urban areas.

We also aimed to over-represent low-income participants and to approximately match the eligible population's share of male and female individuals.

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

B Detailed Blocking and Randomization Procedures

Strata were formed according to participants' race/ethnicity (non-Hispanic White, Black, and Hispanic), income group (0-100% FPL, 101-200% FPL, 201-300% FPL), and state (IL or TX). A separate

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

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. Only one person was enrolled from the waitlist in order to replace a participant in the treatment group who was removed from the 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.

C Detailed 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 over 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 A9, and Figure A10 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.

D 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:

[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.

- 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.). For some families, we also distinguish between secondary and tertiary items; this effectively pushes K3 items to K4 and K4 items to K5, so the distinct tertiary items can be in their own K3 tier. These cases were flagged in the pre-analysis plan, which offers further details.[4]

Table A7 summarizes the FDR tiers of our estimates.

E Relationship with Other Papers

It should be noted that the analyses in this paper come in part from five different pre-analysis plans that focus, alternatively, on employment; income and financial health; time use; psychosocial measures; and housing and geographic mobility. While we did not know at the time of registering the

[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.

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 false discovery rate corrections alongside the other tests in the "income and financial health" pre-analysis plan. This measure ensured 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. (2024), Broockman et al. (2024), Miller et al. (2024), and Krause ⓡ et al. (2025) for information on household finance, political, health, and children's outcomes.

F 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;
- If 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, but this may be misleading as the question always asks about over the last 3 months, and the responses are merely 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 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 cancelled 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 indicating receipt of any education or job training in the survey or National Student Clearinghouse data (the National Student Clearinghouse 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 false discovery rate (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 unemployed.
- 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 mobile 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 W-2 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 specified that total individual income 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 had initially pre-specified that we would consider consumption primarily through the enumerated baseline/midline/endline surveys. Given that the response rates to the mobile 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 mobile 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. (2024). We also allocate unexpected expenses (elicited in other survey questions) to existing expenditure categories and create an "other" component in order 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 4;

– 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, 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;

– 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 A7), 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.

G Time Use

G.1 Robustness Check: Secondary Activities

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 5 in the main text uses this equal allocation method. Figure A7 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.

G.2 Robustness Check: Recoding of "Other" Activities

Next, 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 5 in the main text reported an imprecisely estimated 5 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 A8 shows the results on this version of the measures.

G.3 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 A4 shows the estimates on these outcomes.

H 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

[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.

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 6) 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 the table below (Table A66). 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 that is not present in the BLS data, we use the 0.50 ratio for comparable non-durables).

I Labor Supply and MPEs

I.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 η_e and η_i are the extensive and intensive margins, respectively, NY is net-of-tax income, v is virtual income (the transfers), p is participation and h is hours.

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

[6] This re-weighting is only applied to this comparison table and not to the main results in Table 5.

in the household and would qualify as household heads, etc.) and apply standard tax brackets in Illinois and Texas.

I.2 Details on MPE Comparisons

To construct Table A64, 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., 2023), a permanent income hypothesis model with Stone-Geary utility, and apply the assumptions in the most recent of these papers, Golosov et al. (2023), 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. (2023) 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. (2023) 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. (2023). 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. (2023), 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. (2023) 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 Details on Pooling of Administrative and Survey Data

Table A46 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 in the usual way.

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 is 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.

Table A1: 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 A2: 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-Hispanic White/Other	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	0.256	0.352	0.427
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)	29.927	29.916	0.989	28.518	32.144	0.607
Total Individual Income ($1000s)	21.190	21.218	0.973	20.401	23.704	0.483
Work Hours/Week	22.181	21.491	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	7.875	8.200	0.785
Number of Jobs in the Past 1 Year	1.433	1.395	0.437	1.705	1.933	0.579
Number of Jobs in the Past 3 Years	2.613	2.647	0.713	2.905	5.133	0.065
Searching for Work	0.508	0.495	0.504	0.587	0.467	0.424
Started or Helped to Start a Business	0.296	0.316	0.264	0.303	0.301	0.986
Housing						
Lived Temporarily with Family or Friends	0.285	0.263	0.202	0.113	0.255	0.194
Stayed in Non-Permanent Housing	0.085	0.085	0.964	0.036	0.150	0.212
Housing Search Actions in Last 3 Months	0.241	0.251	0.582	0.276	0.532	0.052
Number of Times Moved in the Past 5 Years	1.363	1.321	0.316	1.147	1.759	0.097
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	3.350	0.532	3.249	3.246	0.994
Non-durable Goods and Services	1.828	1.835	0.846	1.904	1.868	0.881
Housing Expenditures	0.653	0.679	0.229	0.581	0.579	0.992
Human Capital Expenditures	0.392	0.412	0.424	0.385	0.369	0.918
Durable Goods Expenditures	0.322	0.303	0.116	0.278	0.363	0.405
Other Expenditures	0.114	0.120	0.503	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 A3: 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-Hispanic White/Other	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	0.337	0.154	0.281	0.375	0.302
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)	29.991	29.991	1.000	28.350	29.109	0.867
Total Individual Income ($1000s)	21.223	21.099	0.879	20.177	26.045	0.175
Work Hours/Week	22.173	21.258	0.285	21.570	36.467	0.005
Has a Second Job	0.173	0.162	0.430	0.174	0.333	0.100
Months Employed in the Past Year	7.249	7.164	0.659	7.698	8.700	0.284
Number of Jobs in the Past 1 Year	1.420	1.376	0.356	1.872	2.267	0.241
Number of Jobs in the Past 3 Years	2.576	2.652	0.399	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	0.269	0.504	0.013
Housing						
Lived Temporarily with Family or Friends	0.284	0.266	0.290	0.215	0.170	0.550
Stayed in Non-Permanent Housing	0.082	0.088	0.606	0.102	0.045	0.211
Housing Search Actions in Last 3 Months	0.242	0.252	0.552	0.235	0.358	0.175
Number of Times Moved in the Past 5 Years	1.360	1.323	0.377	1.343	1.425	0.719
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	3.314	3.342	0.654	3.239	3.581	0.386
Non-durable Goods and Services	1.833	1.827	0.875	1.782	2.141	0.098
Housing Expenditures	0.658	0.680	0.314	0.525	0.596	0.543
Human Capital Expenditures	0.386	0.413	0.307	0.505	0.376	0.301
Durable Goods Expenditures	0.323	0.303	0.106	0.295	0.347	0.505
Other Expenditures	0.114	0.119	0.485	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 A4: 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-Hispanic White/Other	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	0.271	0.287	0.854
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)	29.864	29.903	0.961	30.710	34.722	0.288
Total Individual Income ($1000s)	21.233	21.136	0.905	20.653	26.676	0.108
Work Hours/Week	22.194	21.370	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	1.377	0.243	1.514	2.333	0.004
Number of Jobs in the Past 3 Years	2.598	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	0.327	0.494	0.071
Housing						
Lived Temporarily with Family or Friends	0.289	0.264	0.162	0.187	0.227	0.613
Stayed in Non-Permanent Housing	0.082	0.087	0.634	0.099	0.075	0.652
Housing Search Actions in Last 3 Months	0.241	0.255	0.404	0.242	0.241	0.987
Number of Times Moved in the Past 5 Years	1.367	1.317	0.234	1.268	1.613	0.104
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	3.301	3.347	0.475	3.419	3.578	0.633
Non-durable Goods and Services	1.828	1.829	0.967	1.881	2.078	0.344
Housing Expenditures	0.656	0.678	0.300	0.615	0.713	0.330
Human Capital Expenditures	0.382	0.416	0.192	0.487	0.318	0.103
Durable Goods Expenditures	0.322	0.304	0.139	0.320	0.363	0.595
Other Expenditures	0.114	0.120	0.460	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 A5: 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-Hispanic White/Other	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	0.283	0.416	0.269
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)	29.939	30.128	0.805	29.537	21.969	0.059
Total Individual Income ($1000s)	21.184	21.378	0.809	21.009	14.859	0.137
Work Hours/Week	22.207	21.826	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	1.401	0.385	1.347	1.500	0.665
Number of Jobs in the Past 3 Years	2.619	2.678	0.533	2.640	2.982	0.550
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	0.282	0.445	0.160
Housing						
Lived Temporarily with Family or Friends	0.282	0.262	0.235	0.255	0.314	0.603
Stayed in Non-Permanent Housing	0.081	0.086	0.628	0.138	0.104	0.668
Housing Search Actions in Last 3 Months	0.241	0.256	0.370	0.263	0.212	0.620
Number of Times Moved in the Past 5 Years	1.364	1.325	0.347	1.237	1.385	0.525
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	3.313	3.352	0.538	3.251	3.214	0.924
Non-durable Goods and Services	1.831	1.835	0.924	1.823	1.941	0.569
Housing Expenditures	0.653	0.676	0.287	0.639	0.773	0.449
Human Capital Expenditures	0.390	0.416	0.314	0.424	0.180	0.001
Durable Goods Expenditures	0.324	0.306	0.128	0.255	0.243	0.848
Other Expenditures	0.115	0.120	0.495	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 A6: 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-Hispanic White/Other	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	0.206	0.278	0.450	0.153
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	30.129	0.793	29.864	25.926	0.337
Total Individual Income ($1000s)	21.201	21.347	0.856	20.763	18.532	0.654
Work Hours/Week	22.119	21.828	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	0.529	1.548	1.450	0.755
Number of Jobs in the Past 3 Years	2.586	2.676	0.325	3.226	3.150	0.920
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	0.300	0.351	0.480	0.269
Housing						
Lived Temporarily with Family or Friends	0.286	0.262	0.170	0.195	0.314	0.278
Stayed in Non-Permanent Housing	0.082	0.086	0.749	0.100	0.154	0.528
Housing Search Actions in Last 3 Months	0.242	0.257	0.376	0.226	0.150	0.402
Number of Times Moved in the Past 5 Years	1.362	1.322	0.335	1.308	1.567	0.284
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	3.300	3.351	0.424	3.503	3.577	0.848
Non-durable Goods and Services	1.825	1.832	0.837	1.952	2.093	0.556
Housing Expenditures	0.654	0.675	0.313	0.627	0.870	0.098
Human Capital Expenditures	0.383	0.419	0.168	0.531	0.162	0.000
Durable Goods Expenditures	0.324	0.306	0.149	0.283	0.289	0.946
Other Expenditures	0.114	0.118	0.612	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 A7: 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

Table A8: 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	-0.40	-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 Unemployment Insurance 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 A9: 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) [0.765]	2939
Hours per week worked at 1st job	27.27 (17.98)	-1.49*** (0.57) [0.257]	2939
Hours per week worked at 2nd job	2.41 (5.69)	-0.09 (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 per week worked at 1st job (conditional on having 1st job)	36.39 (12.95)	-1.00* (0.53) [0.520]	2404
Hours per week worked at 2nd job (conditional on having 2nd job)	12.88 (11.48)	-0.17 (0.81) [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)	-1.47** (0.60) [0.253]	2984
Minimum number of hours worked in a typical week	21.84 (15.29)	-0.88* (0.47) [0.576]	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 A10: 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) [0.789]	2941
Not working due to attending school	0.04 (0.15)	0.01 (0.01) [0.900]	2941
Not working due to caring for elderly	0.02 (0.13)	0.01 (0.01) [0.900]	2941
Not working due to have given up looking for work	0.04 (0.17)	-0.01 (0.01) [0.691]	2941
Not working due to illness	0.07 (0.23)	0.01 (0.01) [0.691]	2941
Not working due to lack in necessary skills	0.08 (0.24)	0.01 (0.01) [1.000]	2941
Not working due to other reasons	0.06 (0.19)	0.01 (0.01) [0.900]	2941
Not working due to personal or family responsibilities	0.13 (0.29)	0.01 (0.01) [0.948]	2941
Not working due to preferring to stay at home	0.09 (0.26)	0.00 (0.01) [1.000]	2941
Not working due to lack in transportation to/from work	0.06 (0.20)	0.01 (0.01) [0.789]	2941
Not working due to suitable work being unavailable	0.13 (0.29)	0.01 (0.01) [1.000]	2941

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 A11: Impact of Guaranteed Income on Disability

	Control Mean	Treatment Effect	N
Disability Index		**-0.10***†††** **(0.03)** **[0.010]**	**2875**
Disability Component		-0.10***††† (0.03) [0.002]	2875
Whether the participant has a health problem/disability	0.31 (0.42)	0.04***††† (0.01) [0.003]	2875
Whether the participant has a health problem/disability that limits the work they can do	0.28 (0.41)	0.04***††† (0.01) [0.003]	2873
How much the participant's worst health problem/disability limits the amount of work they can do (1-7 scale)	1.11 (1.71)	0.17***††† (0.05) [0.003]	2873
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**††† (0.03) [0.003]	2874

This table shows the impacts of an unconditional cash transfer on disability. The top-level index decreases significantly by about 0.10 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 D 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 A12: 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)	0.80**†† (0.32) [0.013]	2928
Length of longest continuous spell of non-employment in months, over the study duration	8.76 (11.81)	0.85***†† (0.31) [0.036]	2928
Duration of unemployment in months at time of survey	2.87 (8.05)	0.64**†† (0.29) [0.036]	2940
Duration of non-employment in months at time of survey	6.07 (12.21)	0.81**†† (0.36) [0.036]	2938
Number of months of non-employment in the last year	3.38 (4.41)	0.28**†† (0.13) [0.045]	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 D, 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 A13: Impact of Guaranteed Income on Human Capital

	Control Mean	Treatment Effect	N
Human Capital Index	0.22 (0.32)	**0.02** **(0.01)** **[0.248]**	**2987**
Formal Education Component		0.02 (0.02) [0.406]	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)	0.21 (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)	0.02** (0.01) [0.493]	2940

This table shows the impacts of an unconditional cash transfer on human capital. The top-level index increases insignificantly by about 0.02 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, National Student Clearinghouse 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 A14: 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)	-0.00 (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) [0.718]	2593
Whether the participant has a Master's degree	0.07 (0.25)	-0.02*** (0.01) [0.280]	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 National Student Clearinghouse 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 A15: Impact of Guaranteed Income on Entrepreneurship

	Control Mean	Treatment Effect	N
Entrepreneurship Index		**0.05***††** **(0.02)** **[0.011]**	**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) [0.079]	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) [0.023]	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)	0.03**†† (0.01) [0.023]	2910
The respondent's likelihood rating that they will start a business in the next 5 years (1-10 scale)	4.95 (3.05)	0.16**†† (0.08) [0.039]	2910
The respondent's interest in starting a business (1-10 scale)	6.21 (2.96)	0.10 (0.09) [0.114]	2911
Entrepreneurial Activity Component		0.01 (0.02) [0.178]	2909
If a family member who started a business lives in the respondent's household	0.06 (0.21)	-0.01**†† (0.01) [0.039]	2908
If the respondent knows someone who started or helped start a business	0.60 (0.41)	0.04***†† (0.01) [0.023]	2908
If the respondent ever started or helped start a business	0.30 (0.40)	0.00 (0.01) [0.279]	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 A16: Impact of Guaranteed Income on Barriers to Employment

	Control Mean	Treatment Effect	N
Barriers to Employment Index		**-0.03** **(0.02)** **[0.329]**	**2941**
Barriers to Employment Component		-0.03 (0.02) [0.291]	2941
Whether the respondent missed work due to lack of childcare in the last month	0.02 (0.13)	0.01 (0.01) [1.000]	2941
Whether the respondent missed work due to illness in the last month	0.20 (0.34)	0.01 (0.01) [1.000]	2940
Whether the respondent missed work due to lack of transportation in the last month	0.03 (0.13)	0.00 (0.00) [1.000]	2940

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 D 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 A17: 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)** **[0.416]**	**2988**
Active Search Component		0.03 (0.02) [0.845]	2987
Whether the participant searched for a job	0.60 (0.38)	0.06***††† (0.01) [0.001]	2943
Whether the respondent is seeking a new, additional, or any job	0.39 (0.41)	0.03* (0.01) [0.248]	2939
Number of different actions taken to search for a job	1.69 (1.72)	0.09 (0.05) [0.297]	2942
Whether the participant applied for a job	0.49 (0.39)	0.04***††† (0.01) [0.010]	2942
Number of job applications sent	5.45 (11.83)	-0.84** (0.34) [0.114]	2942
Whether the participant interviewed for a job	0.36 (0.36)	0.01 (0.01) [0.657]	2942
Number of jobs interviewed for	0.73 (1.72)	-0.10* (0.05) [0.248]	2942
Preferences for Employment Component		0.01 (0.02) [0.845]	2943
How many work hours the respondent wants (less, same, more)	2.18 (0.52)	0.02 (0.02) [0.358]	2927
Whether a respondent is employed or, if unemployed, would prefer to be working	0.90 (0.26)	-0.01 (0.01) [0.524]	2942

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 A18: 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)	0.02* (0.01) [0.174]	2942
Whether participant contacted any job centers in the last 3 months	0.28 (0.35)	0.01 (0.01) [0.493]	2942
Whether participant contacted friends or relatives to find work in the last 3 months	0.36 (0.37)	0.03**† (0.01) [0.095]	2942
Whether participant contacted professional network to find work in the last 3 months	0.22 (0.32)	0.00 (0.01) [0.699]	2942
Whether participant posted a resume online in the last 3 months	0.38 (0.38)	0.02* (0.01) [0.200]	2942
Whether participant took other actions to find work in the last 3 months	0.03 (0.13)	0.01* (0.01) [0.222]	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 A19: Impact of Guaranteed Income on Employment Preferences and Job Search: Additional Regressions

	Control Mean	Treatment Effect	N
Whether the respondent is seeking a new, additional, or any job (alternate specification)	0.37 (0.40)	0.02 (0.01) [0.347]	2939
Number of job applications sent (alternate specification)	5.76 (12.92)	-0.40 (0.43) [0.514]	2980
Number of job applications sent, conditional on having applied for a job	11.47 (17.85)	-2.11***††† (0.61) [0.009]	2488
Number of jobs interviewed for, conditional on having interviewed for a job	1.58 (2.63)	-0.25***†† (0.09) [0.044]	2491
Whether the participant applied for a job that they were unqualified for	0.37 (0.42)	-0.01 (0.02) [0.564]	2064
Proportion of jobs the participant applied to that the participant was unqualified for	0.19 (0.29)	-0.01 (0.01) [0.448]	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 A20: Impact of Guaranteed Income on Selectivity of Job Search

	Control Mean	Treatment Effect	N
Selectivity of Job Search Index		**-0.01** **(0.02)** **[0.595]**	**2648**
Perceived likelihood of finding an acceptable job in 6 months (1 - 4 scale)	3.38 (0.81)	-0.15*** (0.06) [0.785]	889
Participant's reservation wage, reported in minimum hourly remuneration	18.30 (8.73)	-0.28 (0.51) [1.000]	1068
Selectivity Component		-0.01 (0.02) [0.989]	2648
Natural log of average income of jobs which the respondent applied to	10.67 (0.34)	-0.00 (0.01) [1.000]	2071
Whether 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)	0.07 (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 less than 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 D. 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 A21: 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]	965
Work requirement: comfortable workstation or physical environment	0.80 (0.38)	0.02 (0.02) [1.000]	965
Work requirement: flexible hours	0.74 (0.41)	0.05** (0.02) [0.785]	965
Work requirement: high income potential	0.78 (0.39)	-0.01 (0.02) [1.000]	964
Work requirement: interesting or meaningful work	0.70 (0.43)	0.06** (0.03) [0.785]	965
Work requirement: convenient location	0.81 (0.37)	-0.02 (0.02) [1.000]	964
Work requirement: secure, regular earnings	0.89 (0.29)	-0.01 (0.02) [1.000]	965
Work requirement: consistent, predictable schedule	0.81 (0.37)	-0.03 (0.02) [1.000]	965
Participant is not willing to work under any conditions	0.00 (0.04)	0.01 (0.00) [1.000]	1106
Work requirement: other	0.21 (0.38)	-0.01 (0.02) [1.000]	966

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 A22: 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)	
		[0.619]	
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)	-0.11	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 A23: 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	3.62 (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	4.53 (2.97)	-0.18 (0.11) [1.000]	2342
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	10.69 (2.66)	-0.02 (0.10) [1.000]	2396
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)	1.55 (1.15) [1.000]	2403
How many jobs the respondent has held in the past 12 months	1.76 (1.60)	-0.08** (0.04) [1.000]	2390
How many jobs the respondent has held in the past two years	2.33 (3.67)	-0.15* (0.08) [1.000]	2389
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)	-0.00 (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)	-0.05 (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)	0.02 (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 easy 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 A24: Impact of Guaranteed Income on Quality of Employment: Specific Benefits

	Control Mean	Treatment Effect	N
Receives health insurance (100% of premium covered by employer)	0.20 (0.37)	-0.01 (0.02) [1.000]	2065
Receives health insurance (Less than 100% of premium covered by employer)	0.39 (0.46)	-0.02 (0.02) [1.000]	2123
Receives dental and/or vision insurance	0.55 (0.47)	-0.02 (0.02) [1.000]	2165
Receives traditional pension plan (defined benefit plan)	0.31 (0.43)	-0.02 (0.02) [1.000]	2092
Receives retirement account without employer contribution	0.27 (0.41)	-0.03* (0.02) [1.000]	2100
Receives employer contribution to a retirement account	0.34 (0.44)	0.02 (0.02) [1.000]	2100
Receives health care or dependent care Flexible Spending Account	0.34 (0.45)	-0.02 (0.02) [1.000]	2108
Receives housing or housing subsidy	0.03 (0.16)	-0.01 (0.01) [1.000]	2017
Receives life or disability insurance	0.48 (0.47)	-0.01 (0.02) [1.000]	2153
Receives commuter benefits	0.12 (0.31)	-0.02 (0.01) [1.000]	2065
Receives childcare assistance	0.09 (0.26)	-0.01 (0.01) [1.000]	2030
Receives paid vacation	0.63 (0.45)	-0.00 (0.02) [1.000]	2193
Receives tuition reimbursement	0.31 (0.43)	-0.03 (0.02) [1.000]	2099
Can work from home	0.45 (0.48)	-0.03 (0.02) [1.000]	2200
Receives other non-wage benefit	0.15 (0.33)	0.00 (0.01) [1.000]	2071

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 A25: Impact of Guaranteed Income on Moving Labor Markets

	Control Mean	Treatment Effect	N
Move Labor Market 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)	
		[0.031]	
Search New Labor Market Component		0.11***†††	2851
		(0.03)	
		[0.002]	
Any active area-search behaviors	0.10 (0.22)	0.02***†††	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 moving labor markets. The top-level index for moving labor markets 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 A26: Impact of Guaranteed Income on Quality of Labor Market

	Control Mean	Treatment Effect	N
Labor Market Quality Index		**0.00** **(0.01)** **[0.353]**	**3000**
Labor Market Quality Component		0.01 (0.00) [1.000]	2995
Mean wage by education in 2022 (in dollars per hour)	29.70 (12.40)	-0.02 (0.05) [1.000]	2988
Employment to population ratio for ages 25 to 64 for respondent's education group	0.76 (0.07)	0.00 (0.00) [1.000]	2995
BLS projected job-growth for respondent's education group (percent change)	12.04 (9.02)	0.19* (0.11) [1.000]	2961
Median annual income for respondent's education group (in dollars)	41689.82 (13472.32)	118.47 (102.00) [1.000]	2995
Recent population growth for respondent's education group (percent change)	5.23 (11.36)	-0.00 (0.11) [1.000]	2995
Mean wage growth 2019-2022 by education (percent change)	12.21 (4.16)	-0.05 (0.05) [1.000]	2988
Labor Market Amenities Component		0.00 (0.01) [1.000]	2993
Mean percentile household income rank for children whose parents were in the 25th percentile of income	0.40 (0.01)	0.00 (0.00) [1.000]	2993
Natural amenities index (ranges from -5 to 9)	-0.38 (1.51)	0.03 (0.03) [1.000]	2992
Pollution index (mean PM2.5, RSEI, and AQI z-score)	0.99 (0.33)	0.00 (0.01) [1.000]	2993
Consumption amenities index (PCA log scale, ranges from -2 to 2)	0.25 (0.27)	0.00 (0.00) [1.000]	2993
Annual violent crime rate (crimes per 100,000 residents)	402.32 (109.49)	-0.51 (2.07) [1.000]	2967
Annual property crime rate (crimes per 100,000 residents)	2377.75 (354.83)	-2.76 (6.14) [1.000]	2967
Annual per-pupil school spending (in dollars)	13702.10 (3431.41)	-27.58 (42.12) [1.000]	2993

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 A27: Impact of Guaranteed Income on Benefits

	Control Mean	Treatment Effect	N
Benefits Index		**-0.01** **(0.02)** **[0.394]**	**2912**
Take-Up Benefits Component		-0.01 (0.02) [1.000]	2912
Total amount of government benefits received during the previous year	5623.44 (6918.62)	-258.12 (189.00) [1.000]	2911
Number of government benefits programs received during the previous year	1.85 (1.56)	0.02 (0.04) [1.000]	2912
Number of government benefits programs received during the previous year (excluding educational assistance)	1.75 (1.50)	0.02 (0.04) [1.000]	2911

This table shows the impacts of an unconditional cash transfer on 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 D 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 A28: Impact of Guaranteed Income on Relationship Status

	Control Mean	Treatment Effect	N
Relationship Status Index		**-0.01**	**2989**
		(0.02)	
		[1.000]	
Relationship Stability Component		-0.03	2950
		(0.02)	
		[1.000]	
How long the respondent has been in their relationship	2.50 (2.48)	0.01	2900
		(0.06)	
		[1.000]	
Number of times the respondent said they started or ended a relationship in the last year	0.43 (0.72)	0.05*	2903
		(0.03)	
		[0.997]	
Relationship Status Component		0.01	2989
		(0.02)	
		[1.000]	
Whether the respondent is divorced	0.10 (0.29)	-0.01	2943
		(0.01)	
		[0.997]	
Whether the respondent has a spouse	0.28 (0.44)	-0.01	2945
		(0.01)	
		[0.997]	
Whether the respondent is in a romantic relationship	0.58 (0.44)	0.01	2989
		(0.01)	
		[0.997]	

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 $p < 0.1$ before adjusting for FDR: this item looks at relationships a participant might have, regardless of whether that individual lives in the household. 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 A29: 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)	0.004 (0.005) [1.000]	2903
Whether relationship ended because of distance	0.028 (0.120)	0.009* (0.005) [1.000]	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)	0.007 (0.005) [1.000]	2903
Whether relationship ended because of illness	0.006 (0.057)	0.001 (0.002) [1.000]	2903
Whether relationship ended because of relationship issues	0.123 (0.241)	0.021** (0.010) [0.450]	2903
Whether relationship ended because of religion	0.005 (0.049)	0.004* (0.002) [0.976]	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) [0.166]	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 A30: 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)	-4.2***†††	-2.9*	-4.5***†††
		(1.0)	(1.6)	(1.2)
		[0.001]	[0.287]	[0.004]
Total individual income	33.6 (25.1)	-2.3***†††	-3.4***††	-2.1**†
		(0.7)	(1.0)	(0.9)
		[0.008]	[0.013]	[0.086]
Total calculated individual income	36.6 (27.0)	-1.5*†	0.7	-2.6**†
		(0.9)	(1.5)	(1.0)
		[0.096]	[1.000]	[0.086]
Individual salaried/wage income	26.0 (26.2)	-1.2	0.1	-1.2
		(0.8)	(1.2)	(1.0)
		[0.405]	[1.000]	[0.609]
Self-employment income	5.9 (13.7)	-0.1	0.6	-0.8
		(0.5)	(0.9)	(0.7)
		[0.642]	[1.000]	[0.609]
Income from supplementary gig work	0.4 (1.3)	-0.1	-0.0	-0.1
		(0.0)	(0.1)	(0.1)
		[0.317]	[1.000]	[0.405]
Passive income	0.0 (0.2)	0.0	0.0	0.0
		(0.0)	(0.0)	(0.0)
		[0.317]	[1.000]	[0.609]
Other income	4.7 (6.1)	-0.1	-0.3	0.0
		(0.2)	(0.3)	(0.2)
		[0.642]	[0.718]	[1.000]
Government transfers	3.6 (4.9)	-0.2	-0.2	-0.1
		(0.1)	(0.3)	(0.2)
		[0.650]	[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 A31: 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)	-4.2***†††	-5.2***†††	-2.6
		(1.0)	(1.0)	(2.2)
		[0.001]	[0.001]	[0.609]
Total individual income	33.6 (25.1)	-2.3***†††	-3.5***†††	-0.9
		(0.7)	(0.8)	(1.6)
		[0.008]	[0.001]	[1.000]
Total calculated individual income	36.6 (27.0)	-1.5*†	-2.3**†	0.8
		(0.9)	(0.9)	(1.8)
		[0.096]	[0.086]	[1.000]
Individual salaried/wage income	26.0 (26.2)	-1.2	-2.0**†	0.8
		(0.8)	(0.8)	(1.7)
		[0.405]	[0.086]	[1.000]
Self-employment income	5.9 (13.7)	-0.1	0.4	-1.2
		(0.5)	(0.6)	(1.0)
		[0.642]	[1.000]	[0.609]
Income from supplementary gig work	0.4 (1.3)	-0.1	-0.0	-0.2*
		(0.0)	(0.1)	(0.1)
		[0.317]	[1.000]	[0.330]
Passive income	0.0 (0.2)	0.0	0.0	0.0
		(0.0)	(0.0)	(0.0)
		[0.317]	[1.000]	[0.613]
Other income	4.7 (6.1)	-0.1	-0.1	0.1
		(0.2)	(0.2)	(0.4)
		[0.642]	[1.000]	[1.000]
Government transfers	3.6 (4.9)	-0.2	-0.3*	0.1
		(0.1)	(0.2)	(0.2)
		[0.650]	[0.349]	[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 A32: 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)	-4.2***†††	-4.0**	-4.1***†††
		(1.0)	(1.7)	(1.1)
		[0.001]	[0.104]	[0.004]
Total individual income	33.6 (25.1)	-2.3***†††	-3.5***†	-1.9**†
		(0.7)	(1.3)	(0.8)
		[0.008]	[0.062]	[0.090]
Total calculated individual income	36.6 (27.0)	-1.5*†	-1.0	-1.3
		(0.9)	(1.6)	(1.0)
		[0.096]	[1.000]	[0.516]
Individual salaried/wage income	26.0 (26.2)	-1.2	-1.0	-1.5
		(0.8)	(1.5)	(0.9)
		[0.405]	[1.000]	[0.370]
Self-employment income	5.9 (13.7)	-0.1	-0.3	0.5
		(0.5)	(1.0)	(0.6)
		[0.642]	[1.000]	[0.902]
Income from supplementary gig work	0.4 (1.3)	-0.1	-0.1	-0.1
		(0.0)	(0.1)	(0.1)
		[0.317]	[1.000]	[0.609]
Passive income	0.0 (0.2)	0.0	-0.0	0.0**
		(0.0)	(0.0)	(0.0)
		[0.317]	[1.000]	[0.164]
Other income	4.7 (6.1)	-0.1	-0.3	0.1
		(0.2)	(0.2)	(0.2)
		[0.642]	[0.609]	[1.000]
Government transfers	3.6 (4.9)	-0.2	-0.1	-0.1
		(0.1)	(0.2)	(0.2)
		[0.650]	[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 A33: 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)	-4.2***†††	-3.1**	-4.9***†††
		(1.0)	(1.4)	(1.4)
		[0.001]	[0.107]	[0.006]
Total individual income	33.6 (25.1)	-2.3***†††	-2.2**	-2.7***††
		(0.7)	(0.9)	(1.0)
		[0.008]	[0.107]	[0.046]
Total calculated individual income	36.6 (27.0)	-1.5*†	-0.6	-1.7
		(0.9)	(1.3)	(1.1)
		[0.096]	[1.000]	[0.423]
Individual salaried/wage income	26.0 (26.2)	-1.2	-0.8	-1.7
		(0.8)	(1.1)	(1.1)
		[0.405]	[0.939]	[0.402]
Self-employment income	5.9 (13.7)	-0.1	0.5	-0.2
		(0.5)	(0.8)	(0.7)
		[0.642]	[0.972]	[1.000]
Income from supplementary gig work	0.4 (1.3)	-0.1	-0.2***††	0.1
		(0.0)	(0.1)	(0.1)
		[0.317]	[0.034]	[0.958]
Passive income	0.0 (0.2)	0.0	-0.0	0.0***††
		(0.0)	(0.0)	(0.0)
		[0.317]	[0.604]	[0.046]
Other income	4.7 (6.1)	-0.1	0.1	-0.1
		(0.2)	(0.3)	(0.3)
		[0.642]	[1.000]	[1.000]
Government transfers	3.6 (4.9)	-0.2	-0.1	-0.2
		(0.1)	(0.2)	(0.2)
		[0.650]	[0.972]	[0.646]

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 A34: 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)	-4.2***†††	-6.3***†††	-3.0**†
		(1.0)	(1.5)	(1.2)
		[0.001]	[0.001]	[0.096]
Total individual income	33.6 (25.1)	-2.3***†††	-3.1***††	-1.9**
		(0.7)	(1.1)	(0.9)
		[0.008]	[0.046]	[0.128]
Total calculated individual income	36.6 (27.0)	-1.5*†	-2.3*	-1.6
		(0.9)	(1.3)	(1.1)
		[0.096]	[0.292]	[0.423]
Individual salaried/wage income	26.0 (26.2)	-1.2	-2.2*	-0.8
		(0.8)	(1.3)	(1.0)
		[0.405]	[0.292]	[0.806]
Self-employment income	5.9 (13.7)	-0.1	-0.8	0.5
		(0.5)	(0.8)	(0.7)
		[0.642]	[0.676]	[0.958]
Income from supplementary gig work	0.4 (1.3)	-0.1	-0.0	-0.1
		(0.0)	(0.1)	(0.1)
		[0.317]	[1.000]	[0.604]
Passive income	0.0 (0.2)	0.0	0.0**	-0.0
		(0.0)	(0.0)	(0.0)
		[0.317]	[0.107]	[0.972]
Other income	4.7 (6.1)	-0.1	-0.1	-0.0
		(0.2)	(0.2)	(0.3)
		[0.642]	[1.000]	[1.000]
Government transfers	3.6 (4.9)	-0.2	-0.1	-0.0
		(0.1)	(0.2)	(0.2)
		[0.650]	[0.958]	[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 A35: 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)	-1.37**††	-2.09***	-0.16
		(0.63)	(0.75)	(1.12)
		[0.032]	[0.121]	[1.000]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.04***	0.02
		(0.01)	(0.01)	(0.02)
		[0.622]	[0.115]	[0.952]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.42***	-3.15***	-0.69
		(0.78)	(0.90)	(1.44)
		[0.210]	[0.115]	[1.000]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.31**	-2.11*	-2.61
		(0.91)	(1.08)	(1.61)
		[0.291]	[0.536]	[0.691]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.12	0.10	0.34
		(0.36)	(0.42)	(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)	0.21	0.28	0.14
		(0.23)	(0.29)	(0.25)
		[1.000]	[0.905]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.01	0.00	-0.03
		(0.02)	(0.02)	(0.04)
		[1.000]	[1.000]	[0.908]

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 A36: 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)	-1.37**††	-2.14**	-1.09
		(0.63)	(0.87)	(0.89)
		[0.032]	[0.249]	[0.900]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.04**	0.01
		(0.01)	(0.02)	(0.02)
		[0.622]	[0.236]	[1.000]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.42***	-3.27***	-0.97
		(0.78)	(1.05)	(1.13)
		[0.210]	[0.115]	[0.925]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.31**	-3.54***	-1.21
		(0.91)	(1.28)	(1.33)
		[0.291]	[0.121]	[0.908]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.12	0.59	-0.55
		(0.36)	(0.59)	(0.37)
		[1.000]	[0.900]	[0.691]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.21	0.19	0.14
		(0.23)	(0.12)	(0.47)
		[1.000]	[0.691]	[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 A37: 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)	-1.37**††	-1.32	-1.43*
		(0.63)	(1.10)	(0.78)
		[0.032]	[0.900]	[0.622]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.00	-0.03**
		(0.01)	(0.02)	(0.01)
		[0.622]	[1.000]	[0.489]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.42***	-1.66	-2.41**
		(0.78)	(1.30)	(0.97)
		[0.210]	[0.875]	[0.236]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.31**	-2.53*	-1.99*
		(0.91)	(1.52)	(1.13)
		[0.291]	[0.691]	[0.664]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.12	-0.18	0.39
		(0.36)	(0.69)	(0.42)
		[1.000]	[1.000]	[0.905]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.21	-0.35*	0.38
		(0.23)	(0.20)	(0.32)
		[1.000]	[0.691]	[0.900]
Number of other household members which are employed	0.47 (0.61)	-0.01	-0.00	-0.01
		(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 A38: 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)	-1.37**††	-1.36	-1.66*
		(0.63)	(0.84)	(0.92)
		[0.032]	[0.737]	[0.598]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.01	-0.04**
		(0.01)	(0.02)	(0.02)
		[0.622]	[1.000]	[0.312]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.42***	-2.14**	-2.62**
		(0.78)	(1.00)	(1.12)
		[0.210]	[0.383]	[0.312]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.31**	-2.08*	-3.04**
		(0.91)	(1.26)	(1.33)
		[0.291]	[0.705]	[0.323]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.12	0.24	-0.32
		(0.36)	(0.50)	(0.51)
		[1.000]	[1.000]	[1.000]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.21	0.35	0.07
		(0.23)	(0.26)	(0.36)
		[1.000]	[0.881]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.01	-0.00	-0.02
		(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 A39: Impact of Guaranteed Income on Labor Supply: Comparison of Impacts by Fresh EBT Recruitment

	Control Mean	Entire Sample	Not Through Fresh EBT	Through Fresh EBT
Hours worked per week	30.28 (19.83)	-1.37**††	-0.87	0.41
		(0.63)	(0.66)	(1.85)
		[0.032]	[0.981]	[1.000]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.03**	0.01
		(0.01)	(0.01)	(0.04)
		[0.622]	[0.391]	[1.000]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.42***	-1.96**	-2.54
		(0.78)	(0.83)	(2.06)
		[0.210]	[0.339]	[1.000]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.31**	-2.46**	-0.80
		(0.91)	(0.98)	(2.23)
		[0.291]	[0.267]	[1.000]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.12	0.00	0.68
		(0.36)	(0.40)	(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)	0.21	0.16	0.91
		(0.23)	(0.23)	(0.79)
		[1.000]	[1.000]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.01	-0.01	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. Survey data are used in this table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A40: 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)	-1.37**††	-1.72**	1.32
		(0.63)	(0.74)	(1.61)
		[0.032]	[0.339]	[1.000]
Whether the respondent is employed	0.74 (0.39)	-0.02*	-0.03**	0.01
		(0.01)	(0.01)	(0.03)
		[0.622]	[0.267]	[1.000]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.42***	-2.41***	0.40
		(0.78)	(0.92)	(1.86)
		[0.210]	[0.231]	[1.000]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.31**	-2.25**	0.29
		(0.91)	(1.08)	(2.23)
		[0.291]	[0.523]	[1.000]
Total number of hours participant's parents in household work per week	3.22 (12.07)	0.12	0.31	-0.37
		(0.36)	(0.47)	(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)	0.21	0.35	-0.43
		(0.23)	(0.27)	(0.43)
		[1.000]	[0.981]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.01	0.00	-0.05
		(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 A41: 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)	**0.02**	**0.02**	**0.01**
		(0.01)	**(0.02)**	**(0.02)**
		[0.248]	**[1.000]**	**[1.000]**
Formal Education Component		0.02	0.04	-0.07**
		(0.02)	(0.03)	(0.03)
		[0.406]	[1.000]	[0.493]
Completed a GED or post-secondary degree	0.94 (0.23)	0.00	-0.00	-0.00
		(0.00)	(0.00)	(0.00)
		[1.000]	[1.000]	[1.000]
Completed a post-secondary degree (NSC only)	0.35 (0.47)	-0.01	-0.00	-0.03*
		(0.01)	(0.02)	(0.02)
		[1.000]	[1.000]	[1.000]
Total years of post-secondary education completed post-baseline (NSC only)	0.13 (0.33)	0.01	0.03	-0.03**
		(0.01)	(0.02)	(0.01)
		[1.000]	[1.000]	[0.702]
Enrolled in a post-secondary program (NSC only)	0.15 (0.30)	0.01	0.02	-0.03**
		(0.01)	(0.02)	(0.01)
		[1.000]	[1.000]	[0.712]
Enrolled in post-secondary program	0.15 (0.29)	0.01	0.02	-0.02*
		(0.01)	(0.02)	(0.01)
		[1.000]	[1.000]	[1.000]
Average hours of school per week (full-time, part-time, withdrawn, etc.) in post-secondary program	1.96 (5.28)	0.21	0.58*	-0.47**
		(0.21)	(0.33)	(0.23)
		[1.000]	[1.000]	[1.000]
Participation in informal education	0.10 (0.21)	0.01	0.01	-0.00
		(0.01)	(0.01)	(0.01)
		[1.000]	[1.000]	[1.000]
Extent of participation in informal education (full-time, part-time, not enrolled)	0.07 (0.18)	-0.00	-0.00	-0.01
		(0.01)	(0.01)	(0.01)
		[1.000]	[1.000]	[1.000]
Whether the participant plans to receive job training	0.03 (0.14)	0.02**	0.01*	0.02**
		(0.01)	(0.01)	(0.01)
		[0.493]	[1.000]	[1.000]

This table compares results for income for participants by age at baseline. For each pre-specified outcome, National Student Clearinghouse 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 A42: 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***†**	**0.03**
		(0.02)	**(0.02)**	**(0.03)**
		[0.011]	**[0.055]**	**[0.215]**
Entrepreneurial Orientation Component		0.07***†††	0.08***†	0.09*
		(0.02)	(0.03)	(0.05)
		[0.009]	[0.056]	[0.126]
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)
		[0.079]	[0.208]	[0.208]
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**†	-0.16
		(0.06)	(0.07)	(0.12)
		[0.023]	[0.060]	[0.203]
Entrepreneurial Intention Component		0.06**††	0.07***†	-0.03
		(0.02)	(0.03)	(0.04)
		[0.013]	[0.056]	[0.368]
Whether or not the respondent has an idea for a business	0.58 (0.42)	0.03**††	0.04**†	0.00
		(0.01)	(0.02)	(0.03)
		[0.023]	[0.058]	[0.476]
The respondent's likelihood rating that they will start a business in the next 5 years (1-10 scale)	4.95 (3.05)	0.16**††	0.23**†	-0.05
		(0.08)	(0.10)	(0.15)
		[0.039]	[0.068]	[0.446]
The respondent's interest in starting a business (1-10 scale)	6.21 (2.96)	0.10	0.15	-0.22
		(0.09)	(0.10)	(0.14)
		[0.114]	[0.182]	[0.171]
Entrepreneurial Activity Component		0.01	0.01	0.04
		(0.02)	(0.02)	(0.04)
		[0.178]	[0.468]	[0.255]
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)
		[0.039]	[0.099]	[0.487]
If the respondent knows someone who started or helped start a business	0.60 (0.41)	0.04***††	0.04**†	0.05**†
		(0.01)	(0.02)	(0.02)
		[0.023]	[0.060]	[0.099]
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)
		[0.279]	[0.476]	[0.492]

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 A43: 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)**
		[0.011]	**[0.131]**	**[0.056]**
Entrepreneurial Orientation Component		0.07***†††	0.08**†	0.07*
		(0.02)	(0.03)	(0.04)
		[0.009]	[0.067]	[0.126]
The respondent's self-reported willingness to take financial risks (1-10 scale)	4.52 (2.09)	0.09†	0.10	0.11
		(0.06)	(0.08)	(0.09)
		[0.079]	[0.237]	[0.237]
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**†	-0.13
		(0.06)	(0.08)	(0.09)
		[0.023]	[0.085]	[0.175]
Entrepreneurial Intention Component		0.06**††	0.05	0.08**†
		(0.02)	(0.03)	(0.03)
		[0.013]	[0.168]	[0.065]
Whether or not the respondent has an idea for a business	0.58 (0.42)	0.03**††	0.04**†	0.03*
		(0.01)	(0.02)	(0.02)
		[0.023]	[0.087]	[0.140]
The respondent's likelihood rating that they will start a business in the next 5 years (1-10 scale)	4.95 (3.05)	0.16**††	0.08	0.31***†
		(0.08)	(0.11)	(0.12)
		[0.039]	[0.336]	[0.056]
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)
		[0.114]	[0.331]	[0.171]
Entrepreneurial Activity Component		0.01	-0.02	0.02
		(0.02)	(0.03)	(0.03)
		[0.178]	[0.346]	[0.336]
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)
		[0.039]	[0.326]	[0.082]
If the respondent knows someone who started or helped start a business	0.60 (0.41)	0.04***††	0.02	0.04**†
		(0.01)	(0.02)	(0.02)
		[0.023]	[0.319]	[0.087]
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)
		[0.279]	[0.171]	[0.219]

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 A44: 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)**
		[0.619]	**[1.000]**	**[1.000]**
Adequacy of Employment Component		0.01	-0.01	0.04
		(0.03)	(0.03)	(0.05)
		[1.000]	[1.000]	[1.000]
Employment Quality Component		-0.01	-0.02	0.00
		(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)	-0.11	-0.45	-0.41
		(0.37)	(0.38)	(0.78)
		[1.000]	[1.000]	[1.000]
Stability of Employment Component		0.00	-0.00	-0.02
		(0.02)	(0.02)	(0.03)
		[1.000]	[1.000]	[1.000]
Quality of Work Life Component		-0.02	-0.05**	0.04
		(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 A45: 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 (0.02) [1.000]	0.01 (0.02) [1.000]	-0.02 (0.03) [1.000]
The respondent would prefer to work more hours in their current main job	0.21 (0.36)	0.01 (0.02) [1.000]	0.02 (0.02) [1.000]	-0.02 (0.02) [1.000]
The number of jobs held by the respondent apart from their main job	0.38 (0.70)	-0.03 (0.03) [1.000]	-0.03 (0.03) [1.000]	-0.01 (0.04) [1.000]
Employment Quality				
Whether training is offered by the respondent's main employer	0.53 (0.45)	0.00 (0.02) [1.000]	-0.01 (0.02) [1.000]	-0.00 (0.03) [1.000]
Whether training is offered during work hours by the respondent's main employer	0.49 (0.45)	0.01 (0.02) [1.000]	-0.00 (0.02) [1.000]	0.04 (0.03) [1.000]
Whether formal training is offered by the respondent's main employer	0.13 (0.29)	-0.00 (0.01) [1.000]	-0.00 (0.01) [1.000]	-0.00 (0.03) [1.000]
Number of non-wage benefits at respondent's job(s), weighted by hours worked at each job	3.62 (2.90)	-0.11 (0.11) [1.000]	-0.17 (0.12) [1.000]	-0.07 (0.19) [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) [1.000]	0.01 (0.02) [1.000]	0.01 (0.02) [1.000]
Number of non-wage benefits at respondent's job(s), alternate specification	4.53 (2.97)	-0.18 (0.11) [1.000]	-0.32** (0.13) [1.000]	0.02 (0.21) [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) [1.000]	-0.01 (0.01) [1.000]	0.01 (0.02) [1.000]
Stability of Employment				
How many months the respondent has been employed in the past year	10.69 (2.66)	-0.02 (0.10) [1.000]	0.03 (0.13) [1.000]	-0.20 (0.14) [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)	1.55 (1.15) [1.000]	1.78 (1.39) [1.000]	-0.54 (1.66) [1.000]
How many jobs the respondent has held in the past 12 months	1.76 (1.60)	-0.08** (0.04) [1.000]	-0.14** (0.06) [1.000]	-0.00 (0.06) [1.000]
How many jobs the respondent has held in the past two years	2.33 (3.67)	-0.15* (0.08) [1.000]	-0.25** (0.11) [1.000]	0.03 (0.08) [1.000]
Whether the respondent's main job is a temp job	0.10 (0.26)	0.01 (0.01) [1.000]	0.02 (0.01) [1.000]	0.01 (0.02) [1.000]
Whether each of the respondent's jobs is salaried, weighted by hours worked at each job	0.23 (0.39)	-0.00 (0.01) [1.000]	-0.02 (0.01) [1.000]	0.01 (0.03) [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) [1.000]	0.01 (0.02) [1.000]	-0.00 (0.02) [1.000]
How many months the respondent expects to remain in their main job (conditional on temp work)	8.97 (6.56)	-0.94 (0.69)	-0.52 (0.86)	-1.34 (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	-0.09	-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)	-0.00	-0.05	0.13*
		(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	-0.05	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)	-0.05	-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	0.12*
		(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)	0.02	-0.02	0.09
		(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	-0.07**	0.02
		(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]	[0.675]	[1.000]
Safety and health conditions at the respondent's main job (1-5 scale)	4.22 (0.79)	-0.00	-0.01	0.05
		(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*	0.09	0.05
		(0.05)	(0.06)	(0.09)
		[1.000]	[1.000]	[1.000]
How easy 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 A46: 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§	-0.08	-1.68	-0.84	-1.44	-1.62	-1.67	-0.50	-1.59*	-1.21
		(1.13)	(1.32)	(1.11)	(1.10)	(1.33)	(1.11)	(0.81)	(0.93)	(0.79)
		1411	1390	1454	1413	1393	1466	2824	2783	2920
	(b) Survey, consented§	0.24	-1.99	-1.16	-1.78	-1.57	-1.41	-0.07	-1.51	-0.85
		(1.20)	(1.35)	(1.15)	(1.13)	(1.38)	(1.15)	(0.86)	(0.98)	(0.82)
		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 and UI, matched	0.01	-1.80	-0.24	-2.19	-3.32**	-2.93**	-0.37	-2.20**	-1.59*
		(1.01)	(1.26)	(1.07)	(1.38)	(1.58)	(1.46)	(0.81)	(1.00)	(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	-0.01	-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	-0.01	-0.06**	-0.05***	-0.01	-0.03**	-0.03**
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(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 and UI, matched	-0.03	-0.03	-0.02	-0.04	-0.05*	-0.05**	-0.03	-0.04**	-0.04**††
		(0.02)	(0.02)	(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 Unemployment Insurance 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 A47: 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	-4.2***	-4.6***	-3.8***	-4.2***	-4.2***	-5.6***	-3.8***
	(1.0)	(1.3)	(1.2)	(1.0)	(1.0)	(0.9)	(1.0)
Total individual income	-2.3***	-2.5**	-2.0**	-2.6***	-2.3***	-3.4***	-1.9***
	(0.7)	(1.0)	(0.8)	(0.7)	(0.7)	(0.6)	(0.7)
Total calculated individual income	-1.5*	-1.8*	-2.4**	-1.6*	-1.5*	-3.1***	-1.2
	(0.9)	(1.1)	(1.0)	(0.9)	(0.9)	(0.8)	(0.9)
Individual salaried/wage income	-1.2	-1.7*	N/A	-1.1	-1.2	-2.2***	-0.9
	(0.8)	(1.0)		(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	-0.2	-0.1	-0.1	-0.1	-0.3*	-0.1
	(0.2)	(0.2)	(0.1)	(0.2)	(0.2)	(0.2)	(0.2)
Government transfers	-0.2	-0.2	-0.1	-0.2	-0.2	-0.3**	-0.1
	(0.1)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.2)

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 A48: 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	-1.37**	-1.77**	-1.07	-1.40**	-1.37**	-1.95***	-1.13*
	(0.63)	(0.80)	(0.81)	(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	-2.42***	-2.42**	-2.34***	-2.50***	-2.42***	-2.85***	-2.23***
spouse/partner works per week	(0.78)	(1.00)	(0.90)	(0.81)	(0.78)	(0.77)	(0.78)
Total number of hours all household members	-2.31**	-2.94**	-2.23*	-2.37***	-2.31**	-2.92***	-2.13**
(including the participant) work per week	(0.91)	(1.17)	(1.25)	(0.91)	(0.91)	(0.89)	(0.92)
Total number of hours participant's parents in	0.12	-0.21	N/A	-0.54	0.12	-0.43	0.12
household work per week	(0.36)	(0.46)		(0.41)	(0.36)	(0.32)	(0.37)
Total number of hours participant's adult children	0.21	0.30	N/A	0.21	0.21	-0.18	0.21
in household work per week	(0.23)	(0.29)		(0.23)	(0.23)	(0.18)	(0.23)
Number of other household members which are	-0.01	-0.02	N/A	-0.01	-0.01	-0.03	-0.01
employed	(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 A49: 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***	-1.20
	(1.16)	(1.30)		(1.10)		(0.87)	(1.18)
Childcare Min/Day	-4.01	-4.13	-0.00	-2.86	N/A	-15.46***	-3.55
	(4.33)	(6.08)	(0.52)	(4.25)		(3.61)	(4.41)
Community Engagement Min/Day	-1.27	-0.44	0.00	-0.28	N/A	-3.54***	-1.18
	(0.90)	(1.01)	(0.02)	(0.87)		(0.73)	(0.91)
Exercise Min/Day	-0.14	-0.07	0.21	-0.01	N/A	-2.77***	0.04
	(0.87)	(1.04)	(0.24)	(0.89)		(0.64)	(0.88)
Home Production Min/Day	3.79	4.51	8.02**	3.55	N/A	-3.41	6.86**
	(3.42)	(4.10)	(3.21)	(3.43)		(3.05)	(3.42)
Market Work Min/Day	-9.07*	-12.20*	-7.92	-10.41**	N/A	-18.22***	-5.94
	(5.21)	(6.57)	(5.43)	(5.29)		(4.90)	(5.28)
Non-Commuting Transportation Min/Day	5.20***	5.34***	5.16***	4.23***	N/A	0.76	6.13***
	(1.59)	(1.66)	(1.38)	(1.44)		(1.19)	(1.62)
Other Income Min/Day	-2.64**	-2.50**	N/A	-2.94***	N/A	-5.66***	-2.41**
	(1.10)	(1.15)		(1.09)		(0.85)	(1.11)
Other Min/Day	5.90**	5.21*	1.33***	5.83**	N/A	-2.30	6.58**
	(2.58)	(2.97)	(0.50)	(2.64)		(1.90)	(2.62)
Search for a job Min/Day	-0.45	0.18	N/A	-0.95	N/A	-3.80***	-0.29
	(1.04)	(1.12)		(1.00)		(0.74)	(1.06)
Self-Improvement Min/Day	0.11	-0.53	0.30	0.55	N/A	-5.96***	1.06
	(2.26)	(2.78)	(0.75)	(2.32)		(1.89)	(2.29)
Self-care Min/Day	1.65	1.21	1.43	1.81	N/A	-1.48	2.62**
	(1.27)	(1.41)	(1.25)	(1.24)		(1.06)	(1.28)
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	5.77	5.27	3.99	6.33	N/A	-1.71	9.14**
	(3.93)	(4.65)	(3.94)	(3.92)		(3.68)	(3.95)
Solo Leisure Min/Day	5.22	4.87	2.55	5.71*	N/A	-1.90	6.96**
	(3.40)	(5.16)	(2.86)	(3.42)		(3.07)	(3.45)
Time with Others Min/Day	0.29	-0.33	5.53	2.13	N/A	-11.71**	4.02

	(6.19)	(8.37)	(5.49)	(5.99)		(5.64)	(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 A50: 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	0.29*	0.44**	0.30**	0.29*	0.42**	0.19	0.31*
	(0.17)	(0.20)	(0.12)	(0.17)	(0.20)	(0.16)	(0.17)
Helping Hrs/Mo	0.28	0.42	0.38***	0.30	0.36	0.05	0.29
	(0.26)	(0.28)	(0.13)	(0.26)	(0.31)	(0.24)	(0.26)
Medical Hrs/Mo	0.19	0.82	0.07	0.07	0.42	-0.21	0.21
	(0.62)	(0.76)	(0.07)	(0.61)	(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)	(0.07)	(0.09)	(0.07)	(0.08)
Religion Hrs/Mo	0.04	0.17	0.00	0.02	0.13	-0.03	0.04
	(0.12)	(0.16)	(0.02)	(0.12)	(0.15)	(0.12)	(0.12)
Childcare Hrs/Wk	-0.67	-0.68	-0.00	-0.69	-0.70	-1.12	-0.60
	(0.88)	(1.41)	(0.26)	(0.87)	(1.00)	(0.86)	(0.88)
Chores Hrs/Wk	-0.19	0.01	0.23	-0.21	-0.06	-0.34	-0.14
	(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	-0.36	-0.50	-0.21
	(0.35)	(0.40)	(0.21)	(0.35)	(0.43)	(0.34)	(0.35)
Commuting Hrs/Wk	-0.04	-0.00	-0.10	-0.09	0.09	-0.15	-0.02
	(0.14)	(0.17)	(0.13)	(0.14)	(0.18)	(0.14)	(0.14)
Education Hrs/Wk	0.01	0.13	0.02	0.02	-0.01	-0.14	0.03
	(0.22)	(0.25)	(0.10)	(0.22)	(0.26)	(0.21)	(0.22)
Eldercare Hrs/Wk	-0.18	-0.07	-0.01	-0.19	-0.25	-0.36*	-0.17
	(0.21)	(0.25)	(0.01)	(0.21)	(0.25)	(0.20)	(0.21)
Entertainment Hrs/Wk	0.06	0.13	0.07	-0.00	0.15	-0.15	0.11
	(0.36)	(0.45)	(0.34)	(0.36)	(0.43)	(0.35)	(0.36)
Family Hrs/Wk	-0.63	-0.79	-0.46	-0.65	-1.23	-1.00	-0.59
	(0.73)	(0.93)	(0.73)	(0.73)	(0.87)	(0.72)	(0.73)
Friends Hrs/Wk	-0.00	0.28	0.02	-0.04	0.04	-0.17	0.02
	(0.27)	(0.31)	(0.21)	(0.27)	(0.33)	(0.26)	(0.27)
Hobbies Hrs/Wk	-0.06	-0.00	-0.08	-0.07	-0.06	-0.20	-0.06
	(0.15)	(0.17)	(0.09)	(0.15)	(0.18)	(0.14)	(0.15)
Reading Hrs/Wk	-0.13	-0.02	-0.03	-0.14	-0.28	-0.24	-0.11
	(0.19)	(0.22)	(0.14)	(0.19)	(0.22)	(0.18)	(0.19)

Recreation Hrs/Wk	-0.44**	-0.39**	-0.25*	-0.44**	-0.63***	-0.59***	-0.43**
	(0.18)	(0.20)	(0.15)	(0.18)	(0.22)	(0.17)	(0.18)
Sleeping Hrs/Wk	0.21	0.49	-0.18	0.19	-0.13	0.07	0.32
	(0.37)	(0.47)	(0.47)	(0.37)	(0.43)	(0.37)	(0.37)
Working Hrs/Wk	-1.53***	-1.67**	-1.41**	-1.53***	-1.60***	-1.70***	-1.46***
	(0.51)	(0.66)	(0.63)	(0.51)	(0.58)	(0.50)	(0.51)
Vacation Days/Yr	0.09	0.15	-0.11	0.09	-0.06	-0.09	0.12
	(0.31)	(0.37)	(0.25)	(0.30)	(0.32)	(0.29)	(0.31)
Volunteer Hrs/Yr	0.30	1.70	-0.00	0.27	1.02	-1.07	0.33
	(1.47)	(1.73)	(0.20)	(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 A51: 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	**-0.10*** **	**-0.10** **	**N/A**	**-0.09*** **	**-0.10*** **	**-0.11*** **	**-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 A52: 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 A53: 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	**0.02**	**0.02**	**N/A**	**0.02**	**N/A**	**0.01**	**0.02**
	(0.01)	**(0.01)**		**(0.01)**		**(0.01)**	**(0.01)**
Formal Education	0.02	0.04	N/A	0.00	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 A54: 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)**		**(0.02)**	**(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**	0.07***	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*	0.02	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 A55: 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**	**-0.03**	**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 A56: 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***	**-0.00**	**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	0.01	0.01	-0.00	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 A57: 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**	**0.00**	**-0.01**	**-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 A58: 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)**	**(0.01)**	**(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	0.02	-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	0.00	0.01	0.00	0.01	-0.00
informal job	(0.01)	(0.02)	(0.00)	(0.02)	(0.01)	(0.01)	(0.01)
Average hourly income from all jobs,	-0.11	-0.18	-0.17	0.03	-0.11	-0.28	0.16
weighted by hours worked at each job	(0.37)	(0.44)	(0.40)	(0.44)	(0.37)	(0.37)	(0.36)
Stability of Employment	0.00	-0.00	0.00	0.02	0.00	-0.02	0.01
	(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 A59: 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	**306*** **	**338*** **	**352*** **	**307*** **	**N/A**	**222*** **	**316*** **
	(48)	**(69)**	**(76)**	**(49)**		**(46)**	**(48)**
Human capital expenditures	50***	51***	37**	51***	N/A	31**	51***
	(15)	(19)	(15)	(15)		(14)	(15)
Durable goods expenditures	52***	48***	46**	52***	N/A	39***	54***
	(14)	(16)	(19)	(14)		(13)	(14)
Housing expenditures	34**	52**	34	32*	N/A	27	35**
	(17)	(23)	(29)	(16)		(16)	(17)
Non-durable goods and services expenditures	133***	144***	138***	134***	N/A	106***	139***
	(26)	(35)	(39)	(26)		(25)	(26)
Other expenditures	37***	43***	57***	37***	N/A	19*	38***
	(11)	(14)	(12)	(11)		(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: 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 A60: Robustness checks for Impact of Guaranteed Income on Moving Labor Markets

	Main Estimate	No Covariates	Median Regression	Diff-in-Diff	Midline/ Endline	Lower Lee Bound	Upper Lee Bound
Move Labor Market 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 moving labor markets. 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 A61: 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**	**0.016**	**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	0.017	N/A	0.005	0.007
	(0.005)	(0.023)	(0.020)	(0.020)		(0.005)	(0.005)
Labor Market Amenities	0.001	-0.001	-0.001	0.015	N/A	-0.008	0.006
	(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 A62: 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**	**-0.01**	**-0.01**	**-0.04***	**-0.01**
	(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 A63: 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	-0.03	-0.03	-0.01	-0.09***	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 A64: 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			
Our estimate	20118	-2.63	-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 I.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 A65: 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 employment and total household income results come from survey data, as this is what was specified in the questions that forecasters saw. 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 A66: 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.

Figure A1: 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

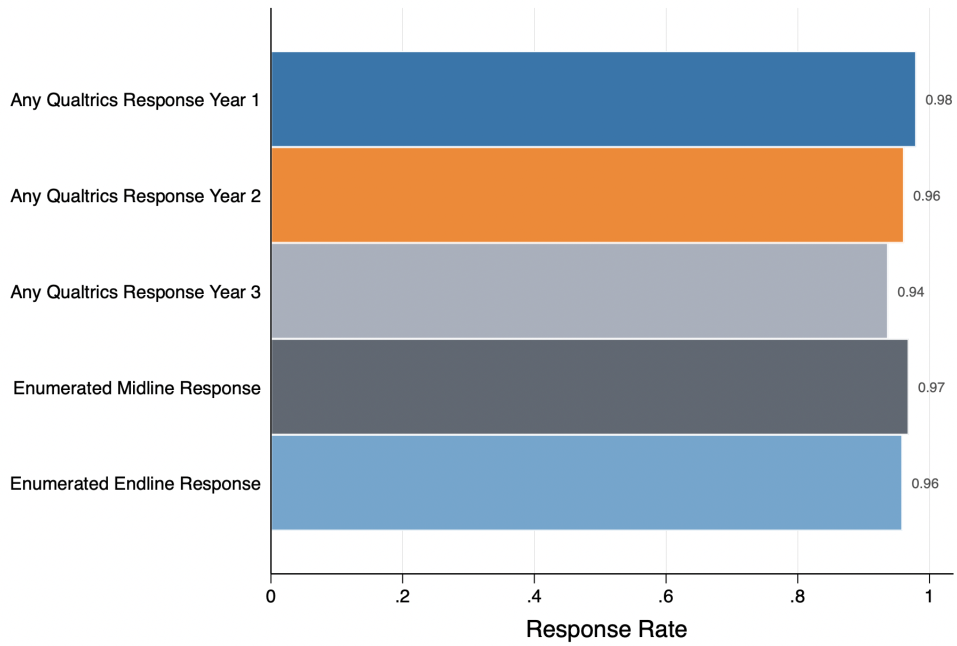
Synopsis As Introduced
Amends the Illinois Public Aid Code. Provides that for purposes of determining eligibility and the amount of assistance under the Code, the Department of Human Services and local governmental units shall exclude from consideration, for a period of no more than 60 months, any financial 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**

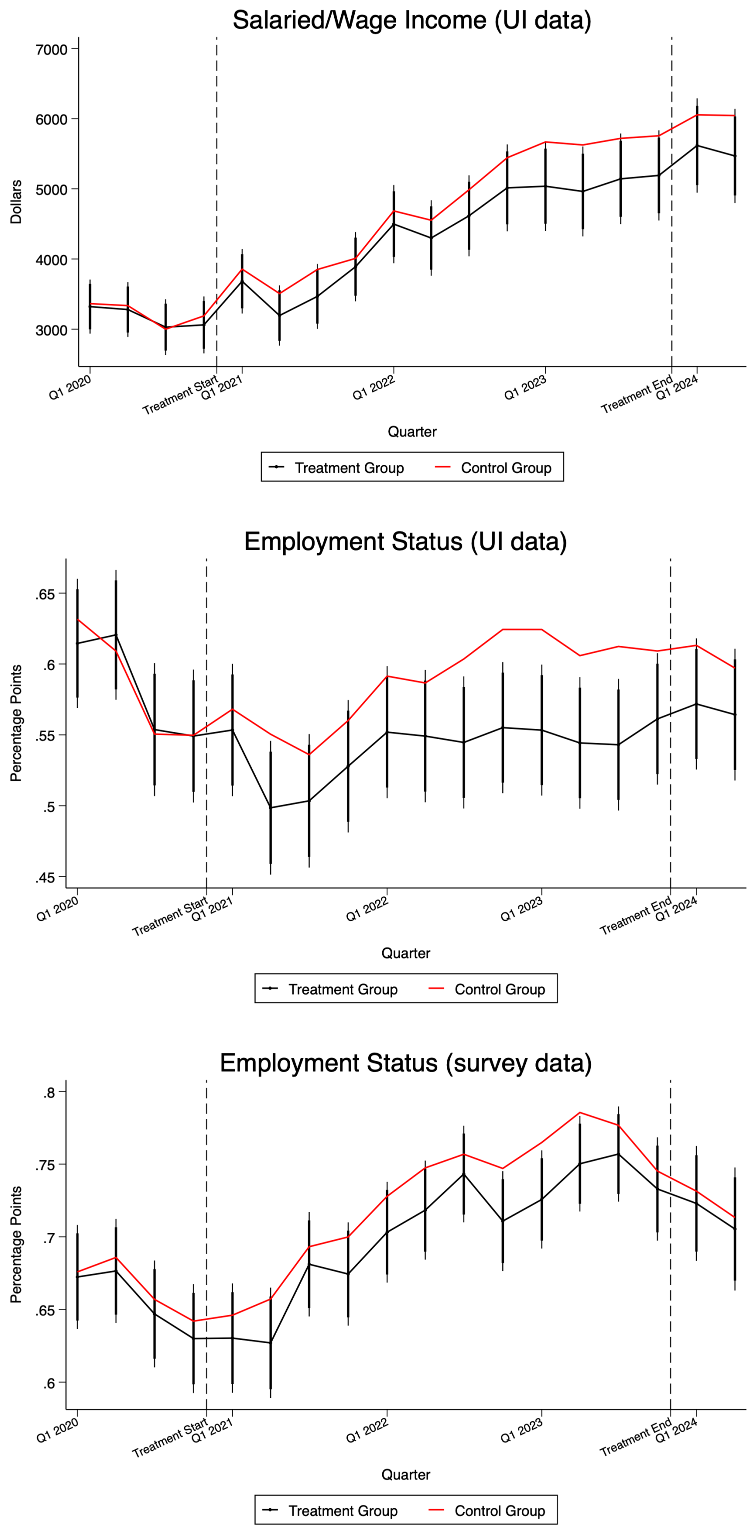
This figure provides a synopsis of the bill that was passed to protect benefits in Illinois.

Figure A2: Response Rates Over Time



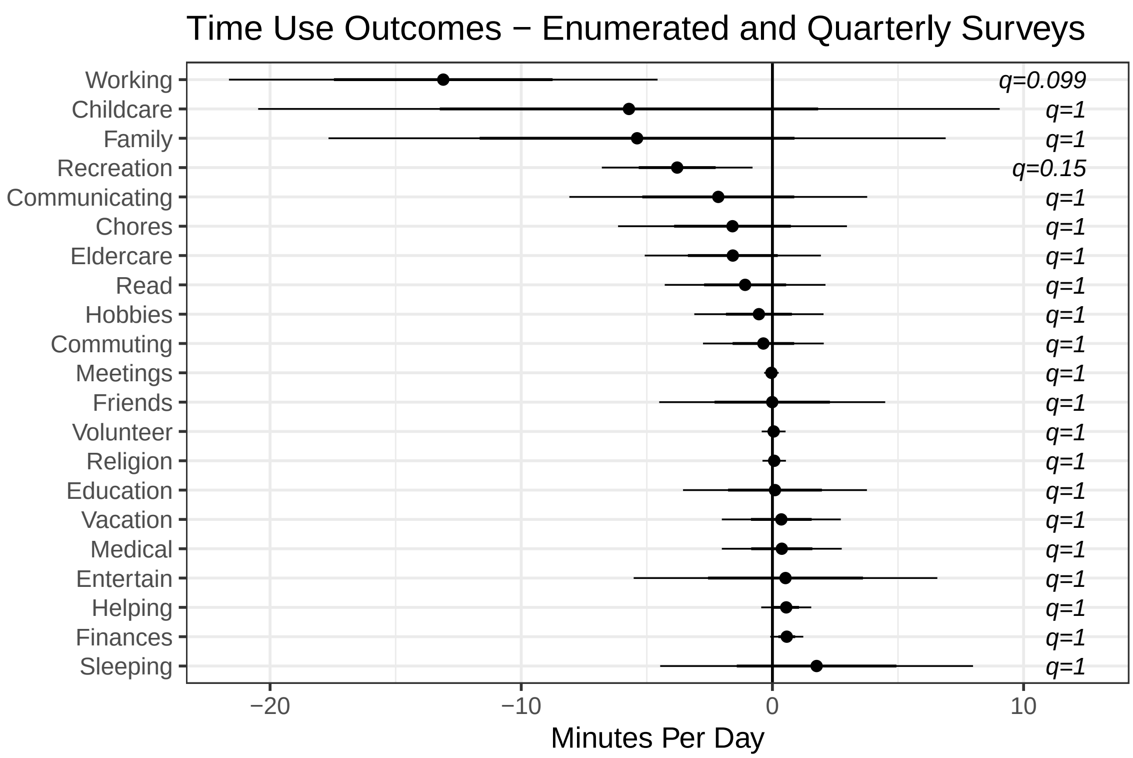
This figure shows response rates for the Qualtrics surveys and enumerated surveys over time.

Figure A3: Quarterly Results for Income and Employment Over Time



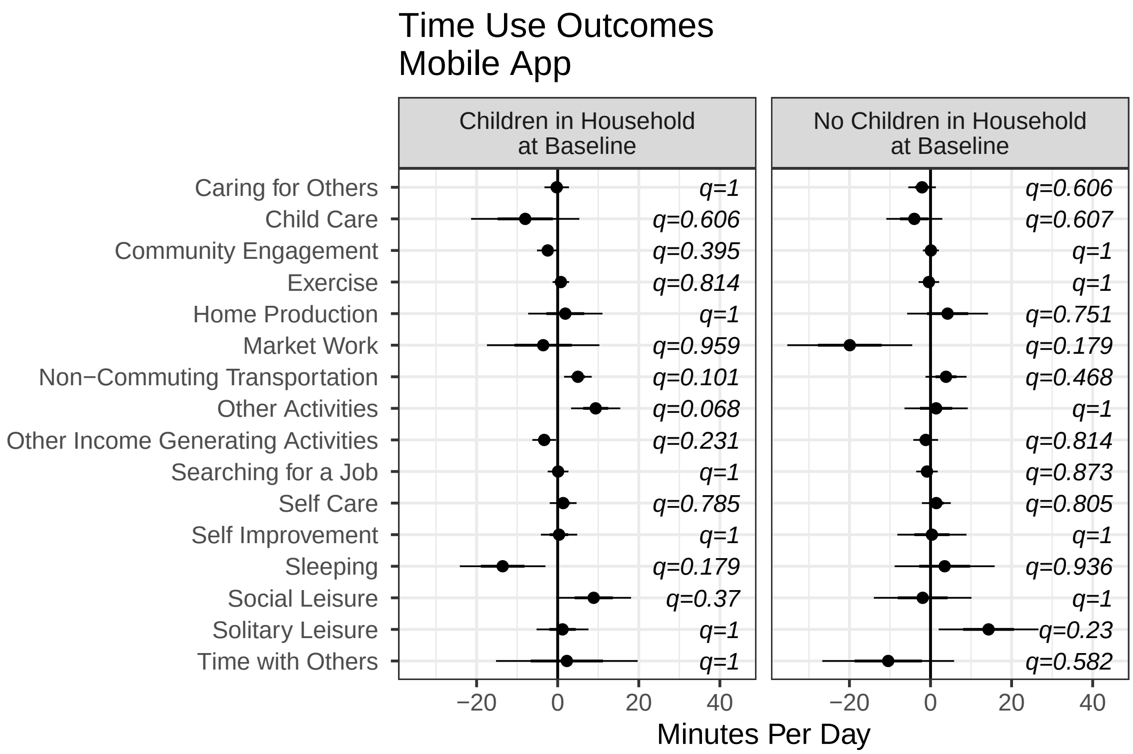
This figure plots the results for income and employment over time. The first two subfigures use data from UI records in each state, pooling results across states as described in Appendix J, while the third uses survey data. The data points in this figure represent estimated effects on individual salaried 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). 95% and 90% confidence intervals are depicted through the thickness of the interval lines. No controls are included in these regressions.

Figure A4: Time Use Results: Enumerated and Quarterly Surveys



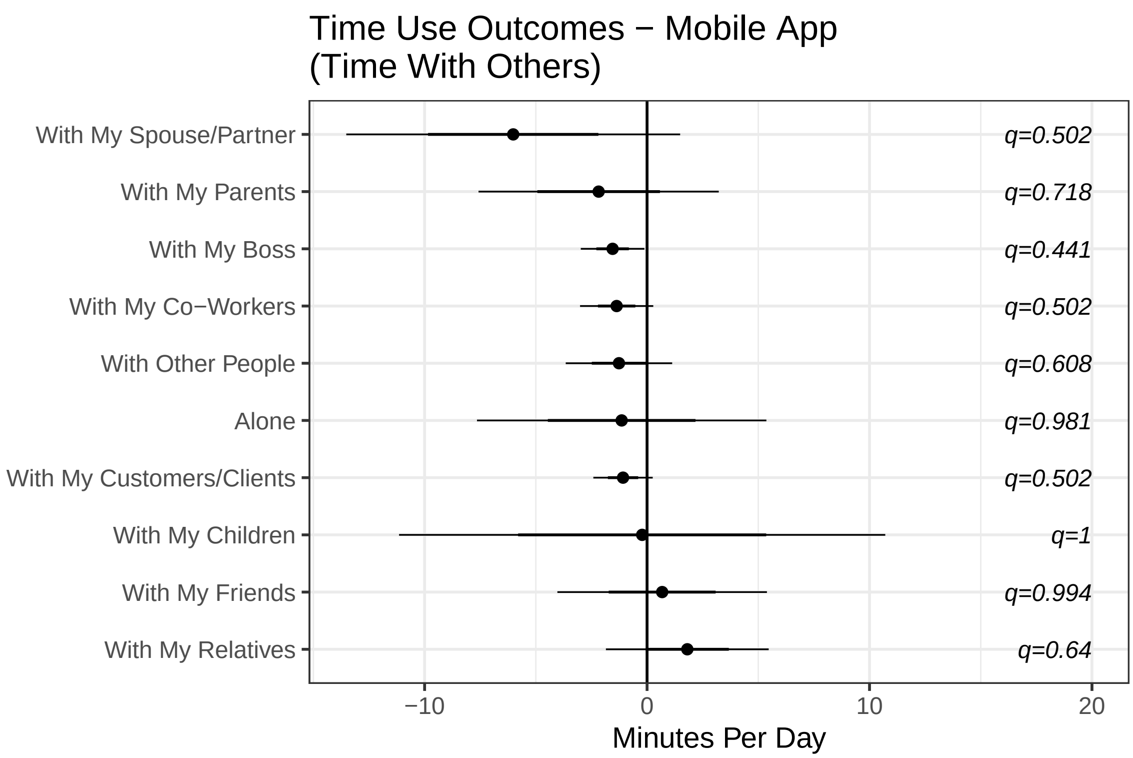
This figure shows the results from the enumerated and quarterly time use surveys.

Figure A5: Time Use Results: Mobile App - By Children in Household at Baseline



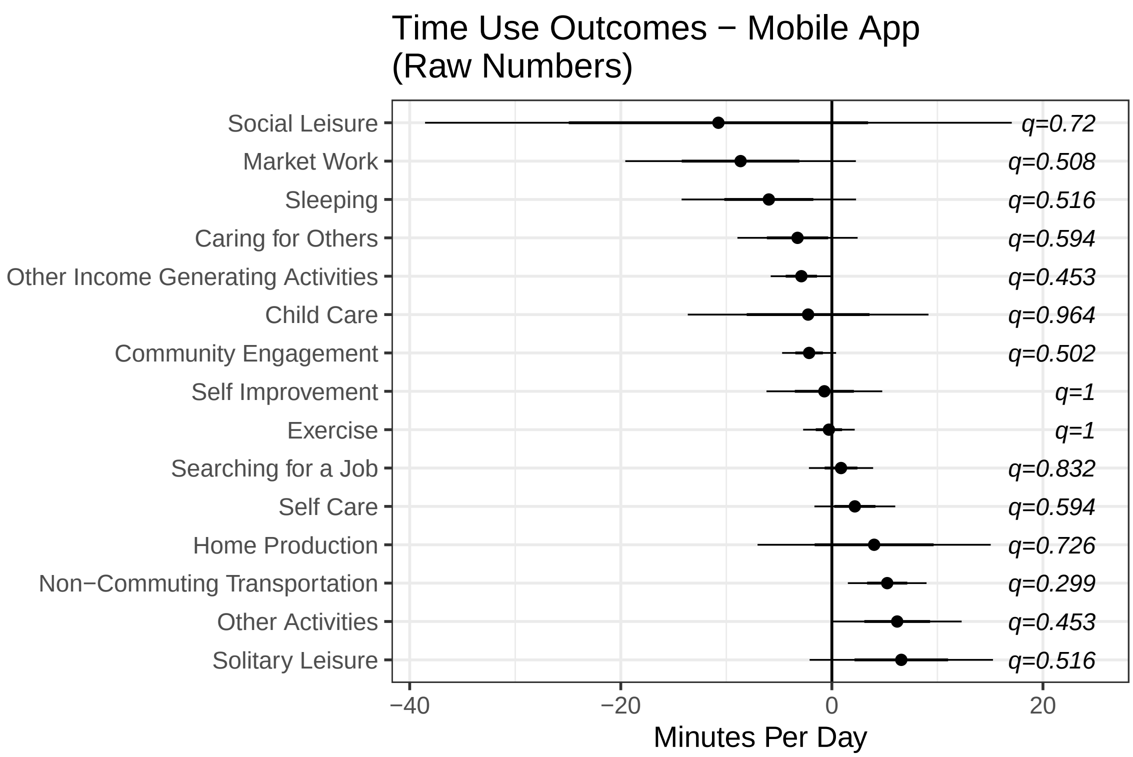
This figure shows the results from the mobile phone app, by whether participants had children in the household at baseline.

Figure A6: Time Use Results: Mobile App (Time Spent With Others)



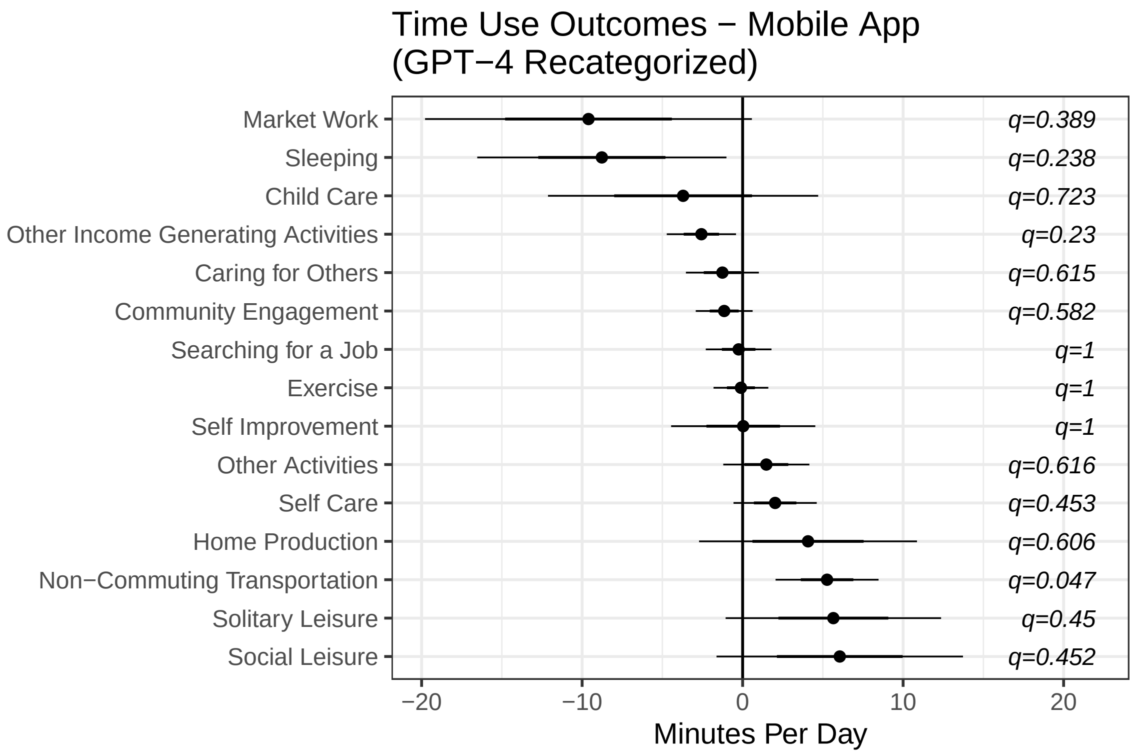
This figure shows the results from the mobile phone app for time spent with others.

Figure A7: Time Use Results: Mobile App (Raw Times)



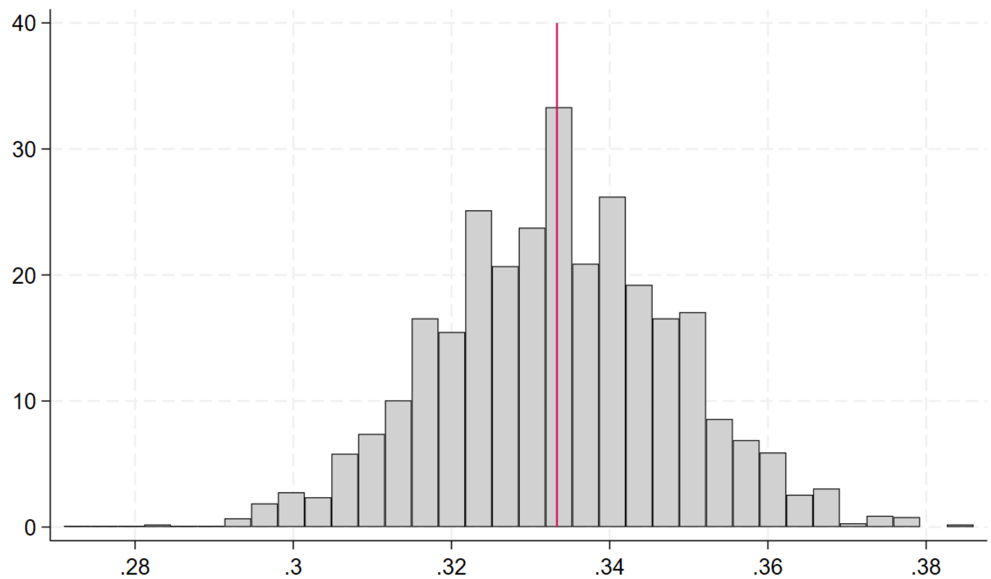
This figure shows the results from the mobile phone app, without adjusting for simultaneous activities.

Figure A8: Time Use Results: Mobile App (ChatGPT-4 Recoded)



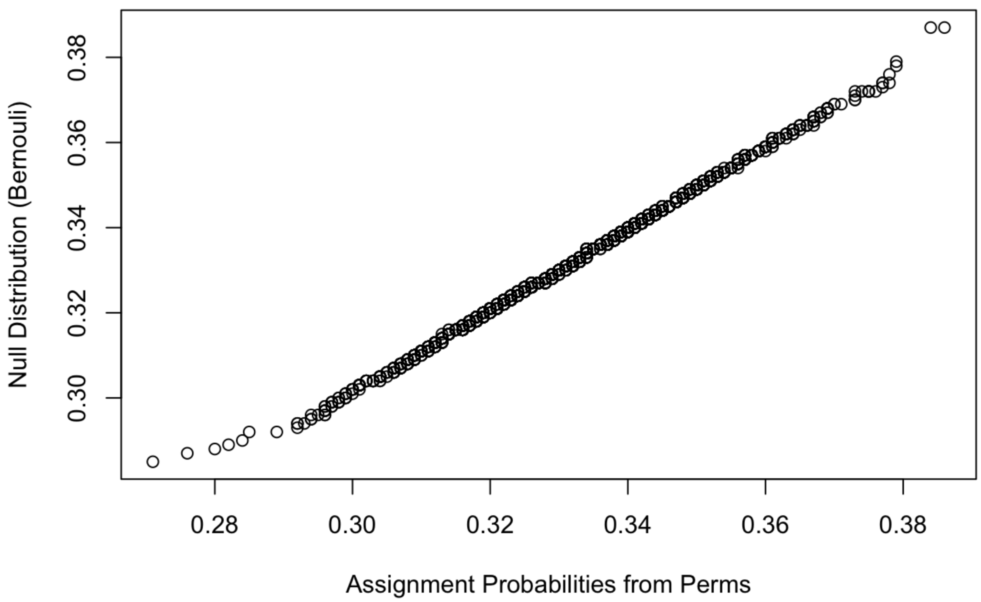
This figure shows the results from the mobile phone app, using GPT to recode open-ended responses.

Figure A9: Histogram of Treatment Assignment Probabilities



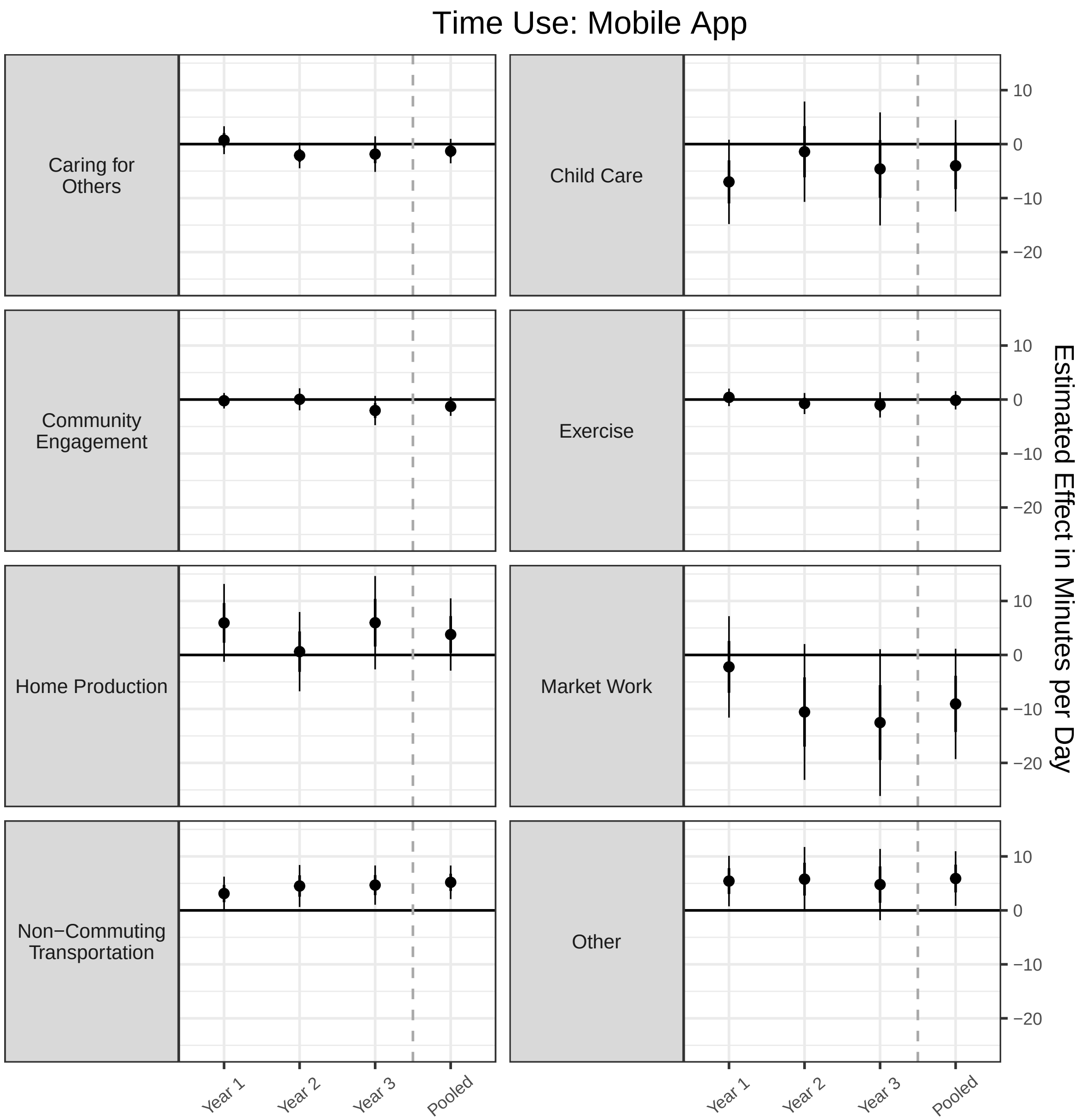
This graph displays a frequency distribution of participants' average treatment assignments, based on 1,000 simulated runs of the assignment process. The vertical line on the graph is positioned at 0.33333, representing the 1 in 3 probability of assignment.

Figure A10: QQ-plot of Treatment Probability against Bernoulli Distribution



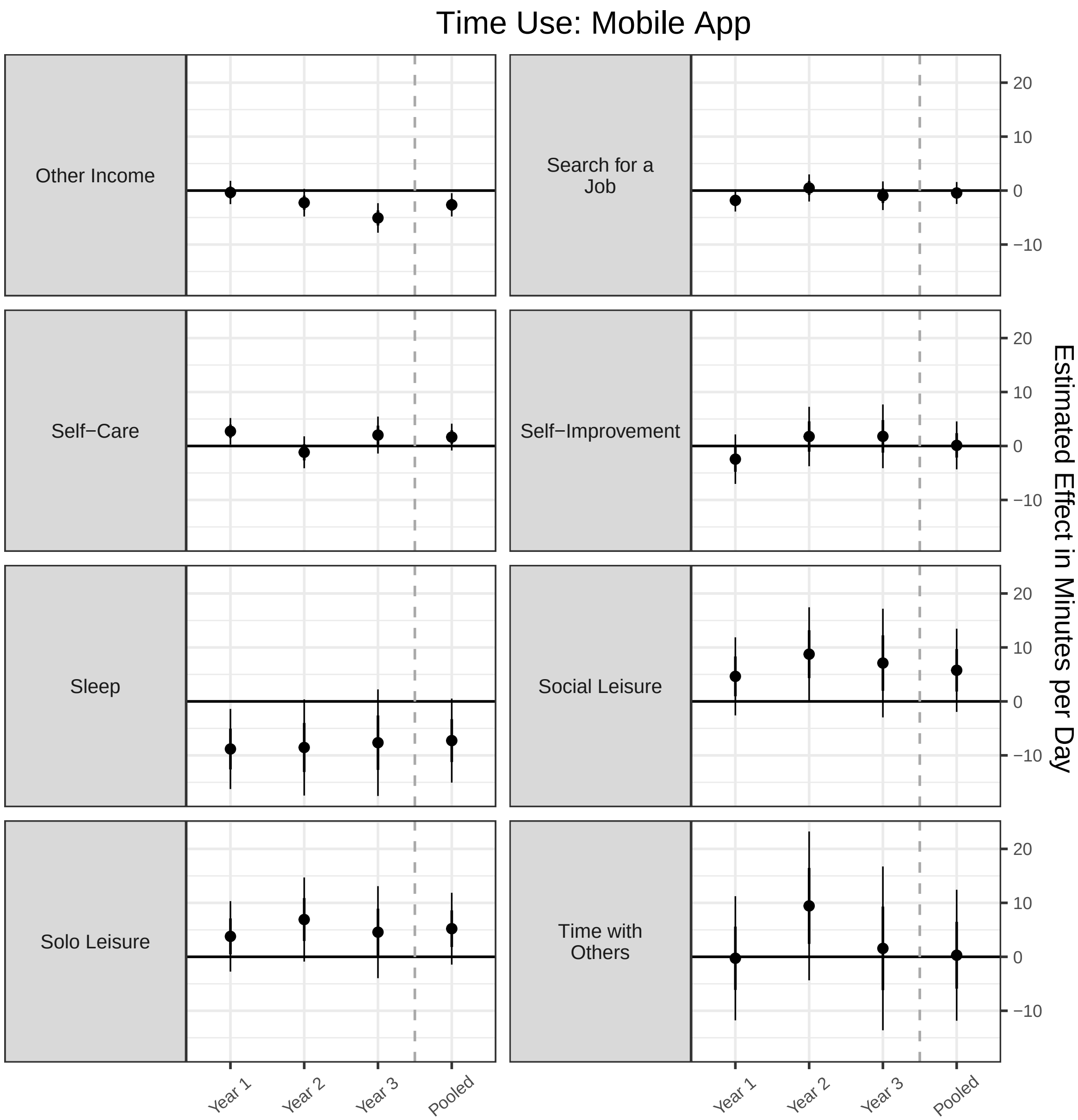
Note: This graph compares the actual distribution of treatment assignments with the theoretical distribution expected from a random assignment process where each participant has a one in three chance of being assigned to the treatment group. The x-axis shows the quantiles of the observed treatment assignments, 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 A11: Results for Time Use by Time Period: Mobile App (1)



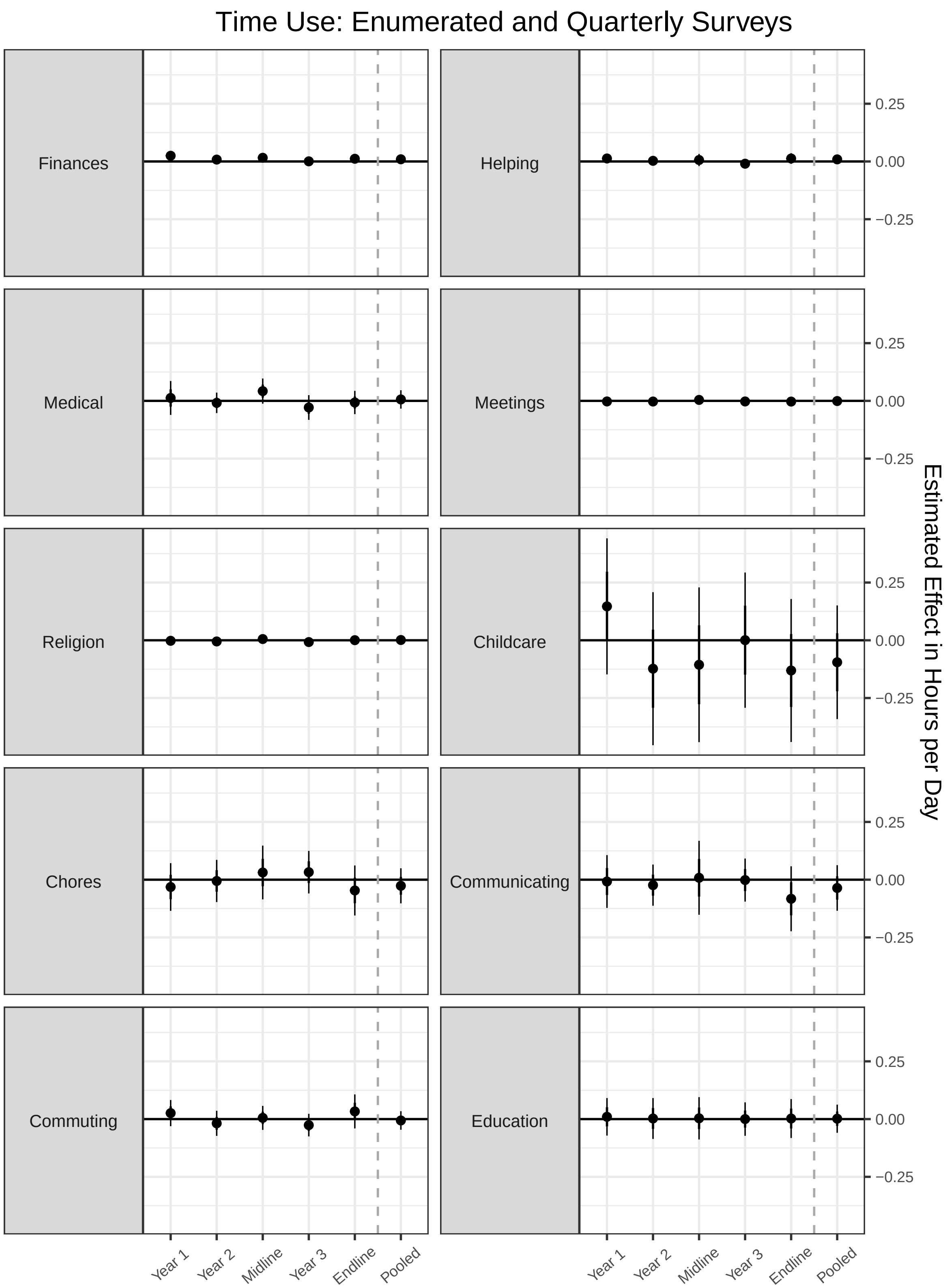
This figure plots the results for time use over time, using data from the mobile app.

Figure A12: Results for Time Use by Time Period: Mobile App (2)



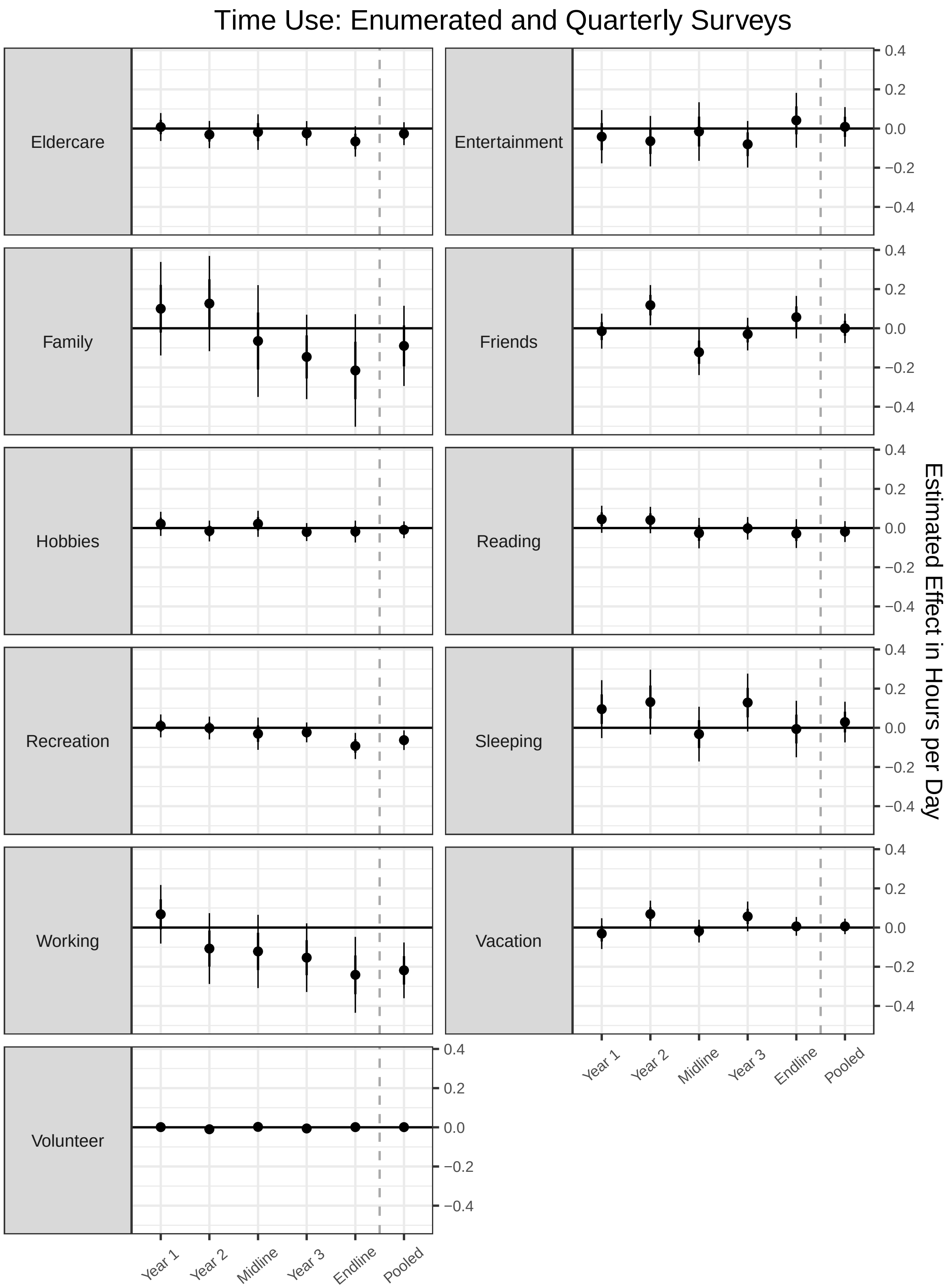
This figure plots the results for time use over time, using data from the mobile app.

Figure A13: Results for Time Use by Time Period: Enumerated and Quarterly Surveys (1)



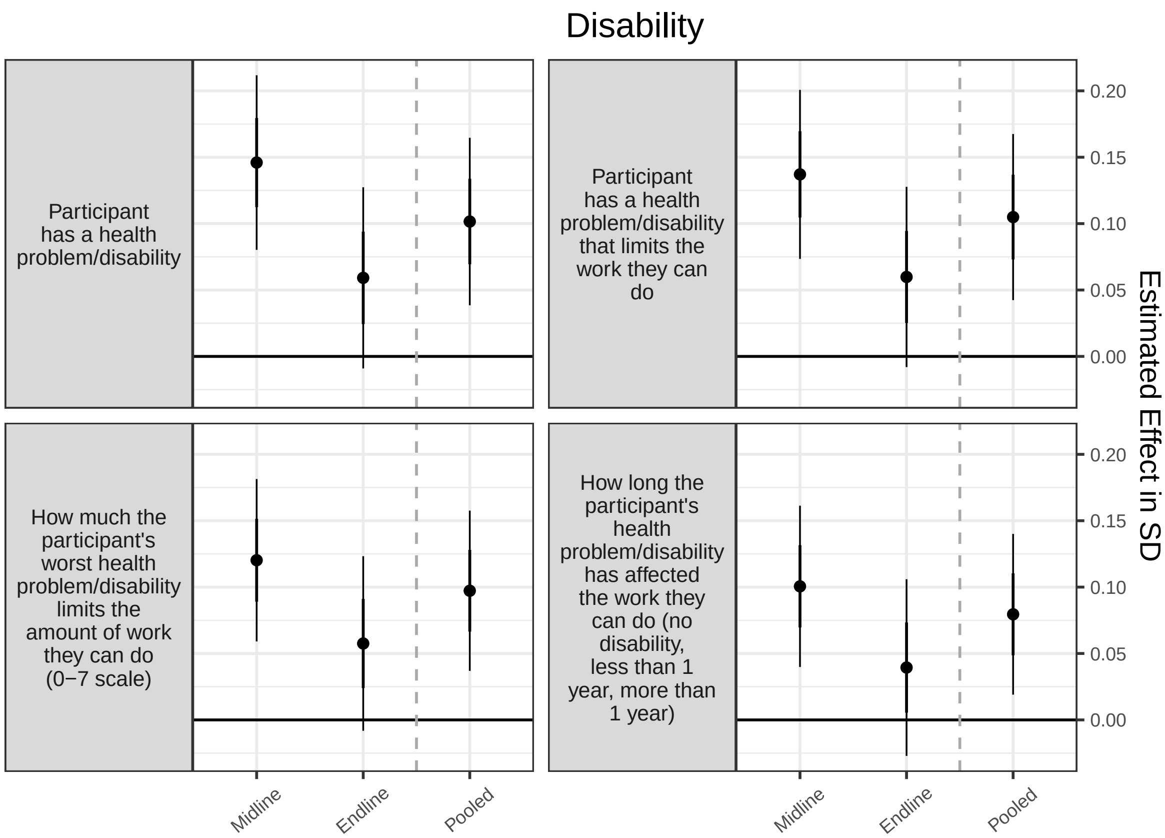
This figure plots the results for time use over time, using data from enumerated and quarterly surveys.

Figure A14: Results for Time Use by Time Period: Enumerated and Quarterly Surveys (2)



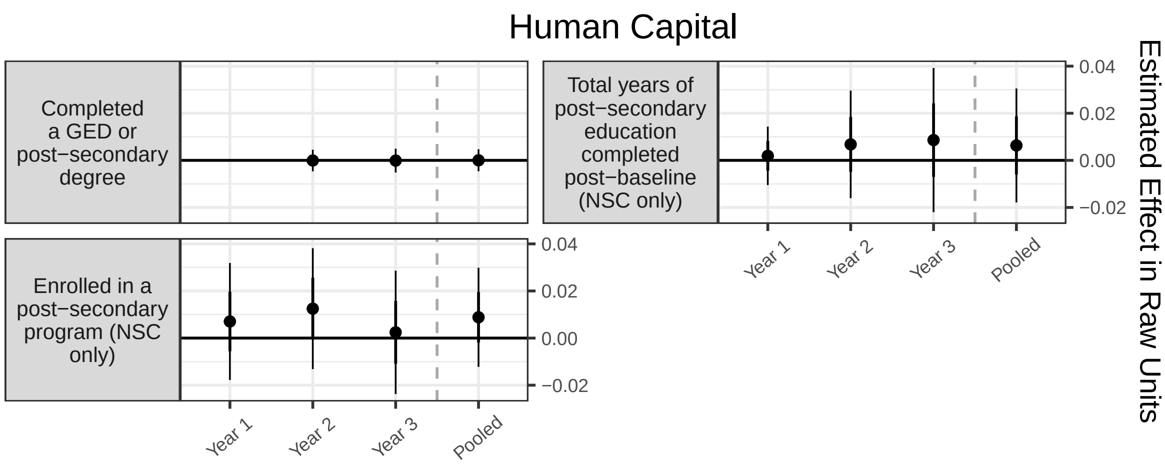
This figure plots the results for time use over time, using data from enumerated and quarterly surveys.

Figure A15: Results for Disability by Time Period



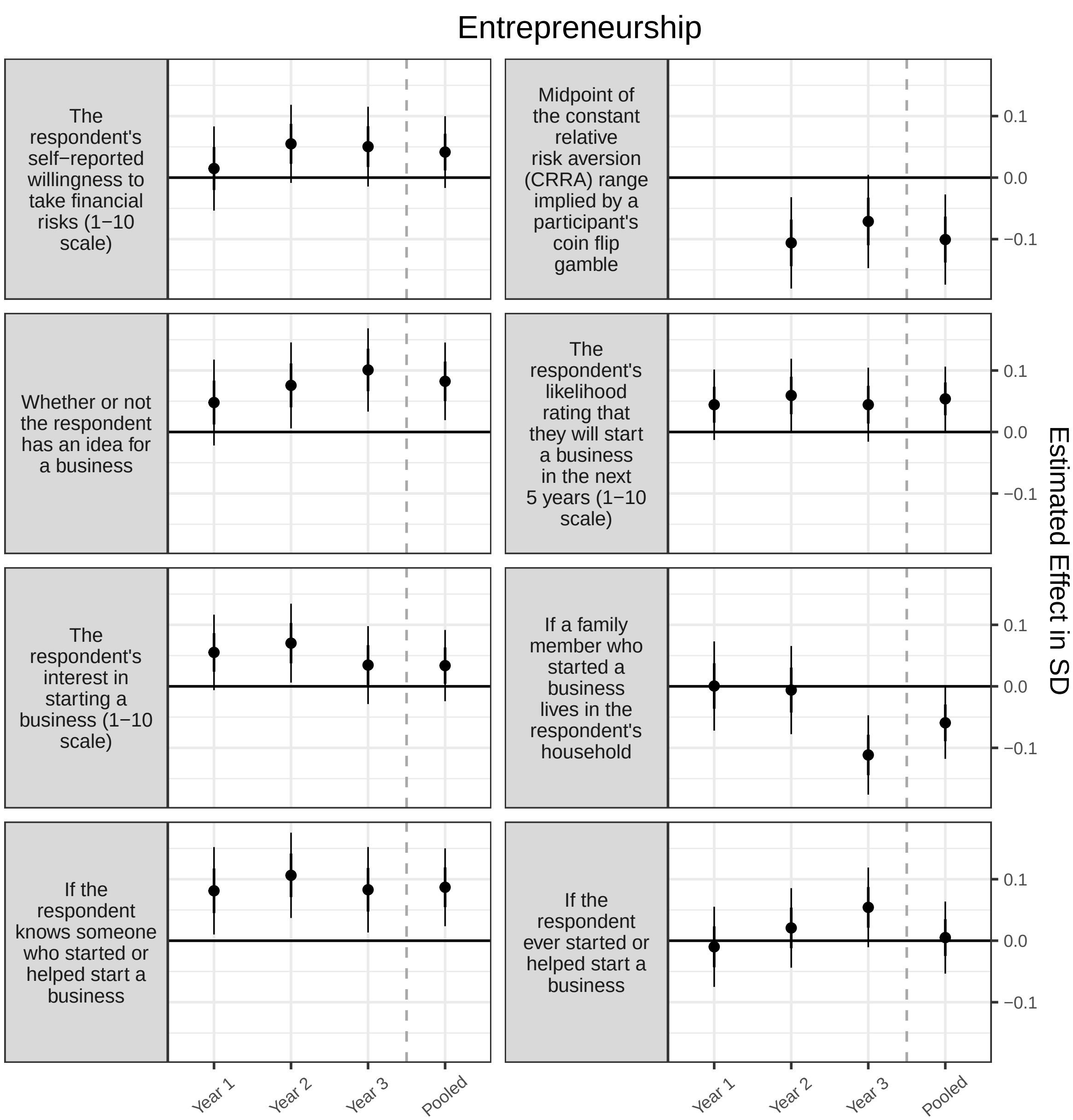
This figure plots the results of the estimates of the transfers on disability over time.

Figure A16: Results for Human Capital by Time Period



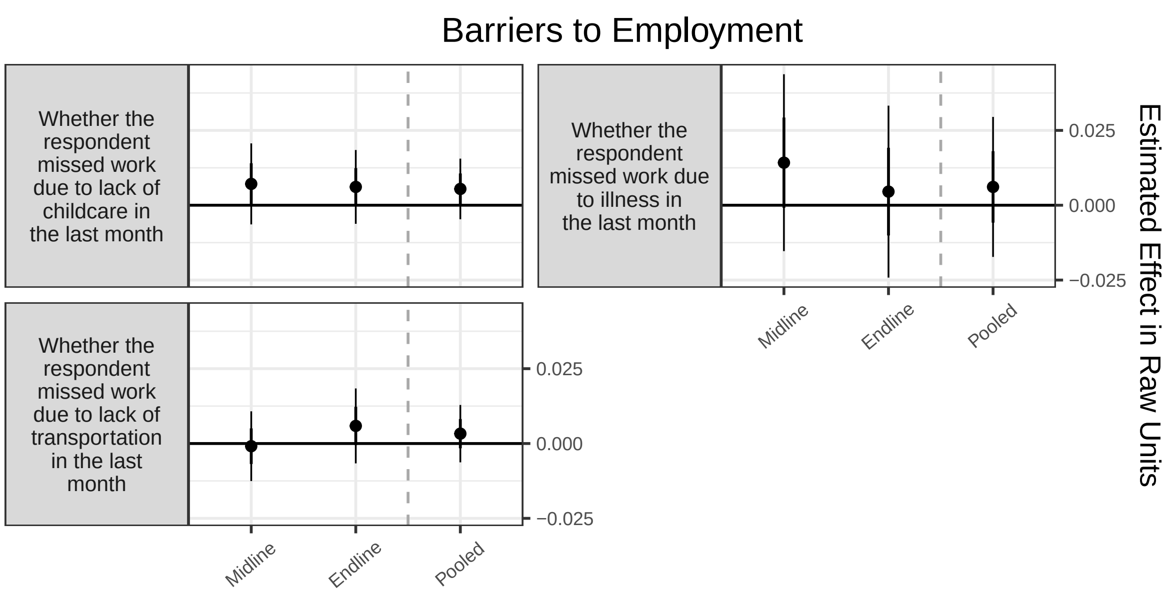
This figure plots the results for human capital over time, showing the point estimates for completion of a GED or post-secondary program trending upwards by the end of the study. There is no value for this variable for Year 1 because participants were only asked about whether they had completed a high school degree or GED in the midline and endline SRC survey. For all outcome variables, data from the National Student Clearinghouse (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.

Figure A17: Results for Entrepreneurship by Time Period



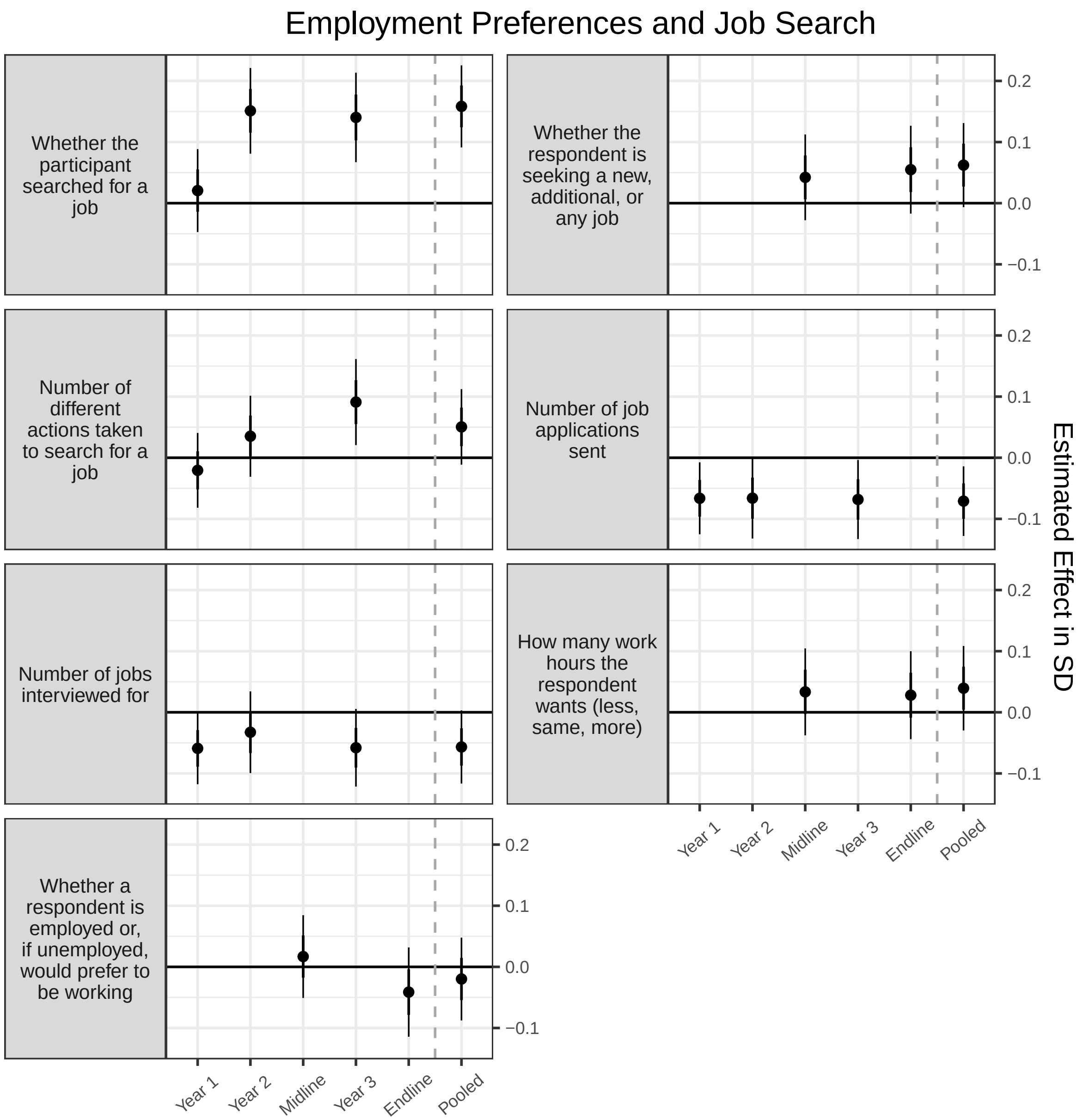
This figure plots the results of the estimates of the transfers on entrepreneurship over time.

Figure A18: Results for Barriers to Employment by Time Period



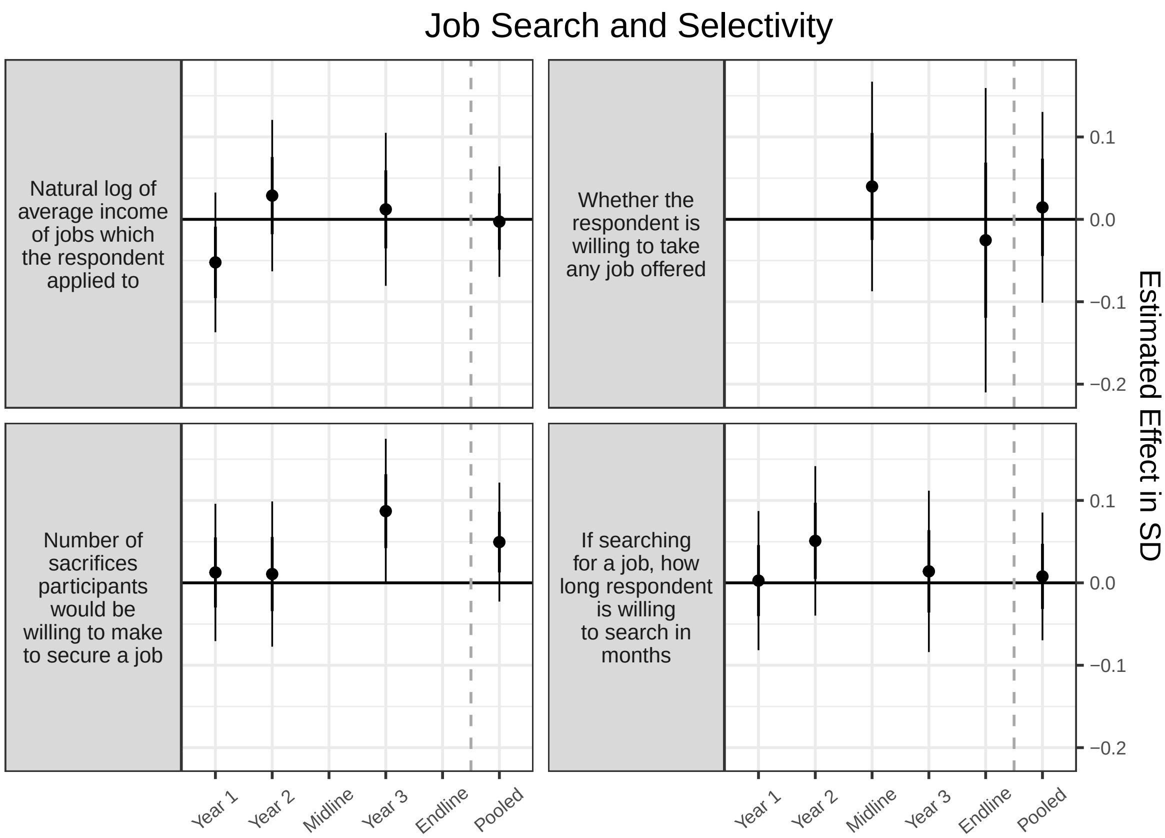
This figure plots the results of the estimates of the transfers on barriers to employment over time.

Figure A19: Results for Employment Preferences and Job Search by Time Period



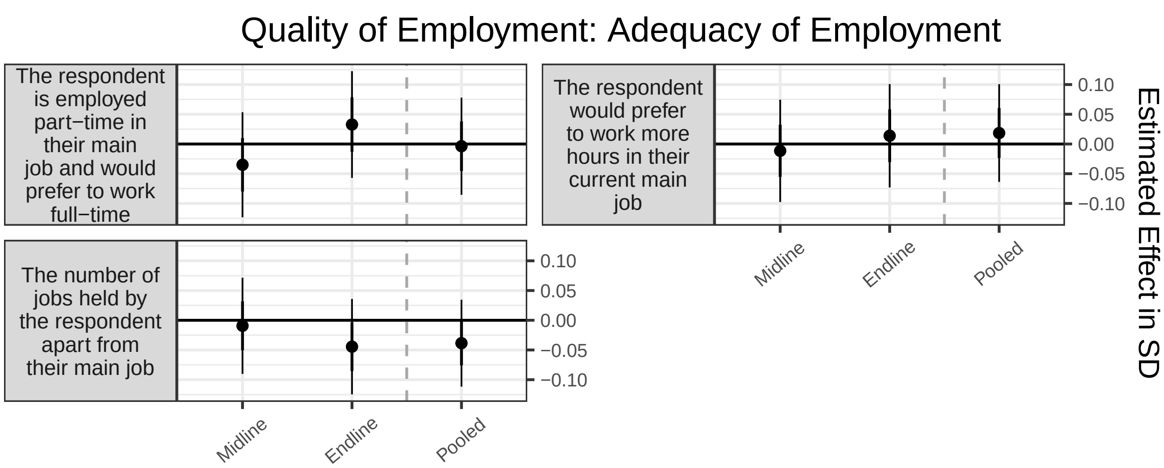
This figure plots the results of the estimates of the transfers on employment preferences and job search over time.

Figure A20: Results for Selectivity of Job Search by Time Period



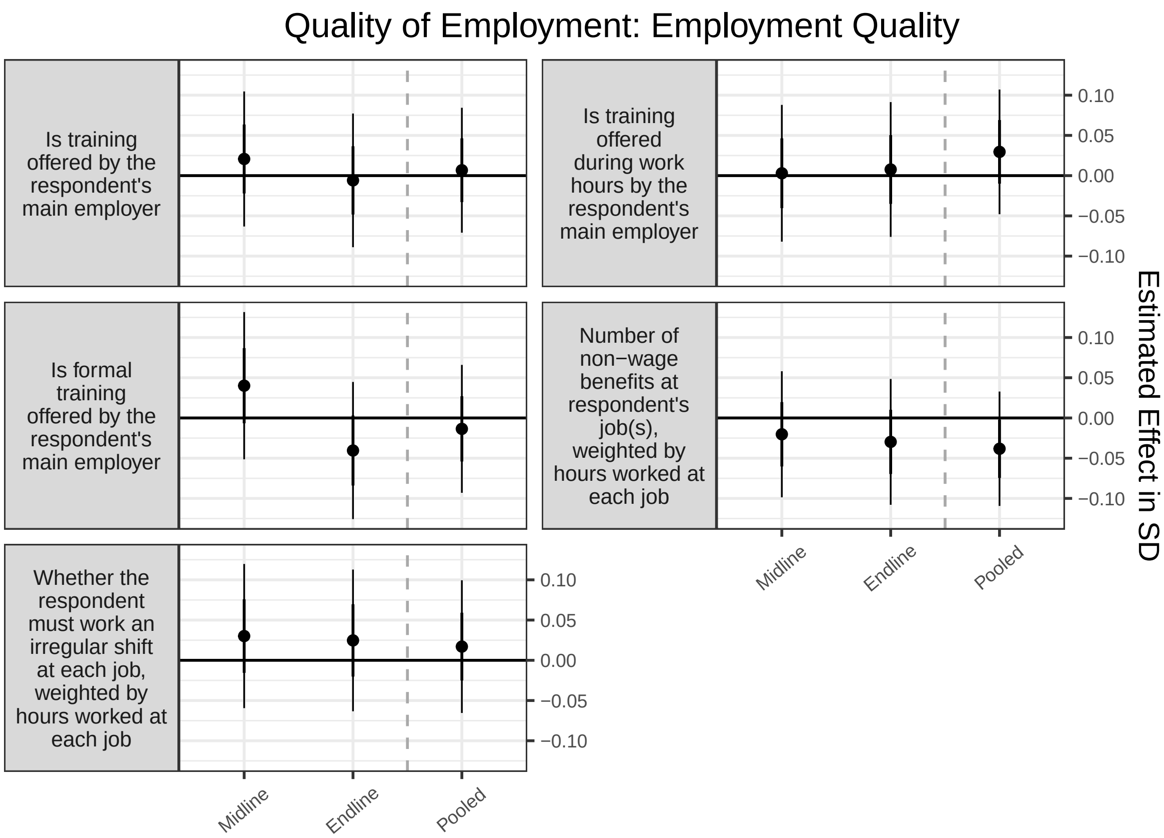
This figure plots the results of the estimates of the transfers on selectivity of job search over time.

Figure A21: Results for Adequacy of Employment by Time Period



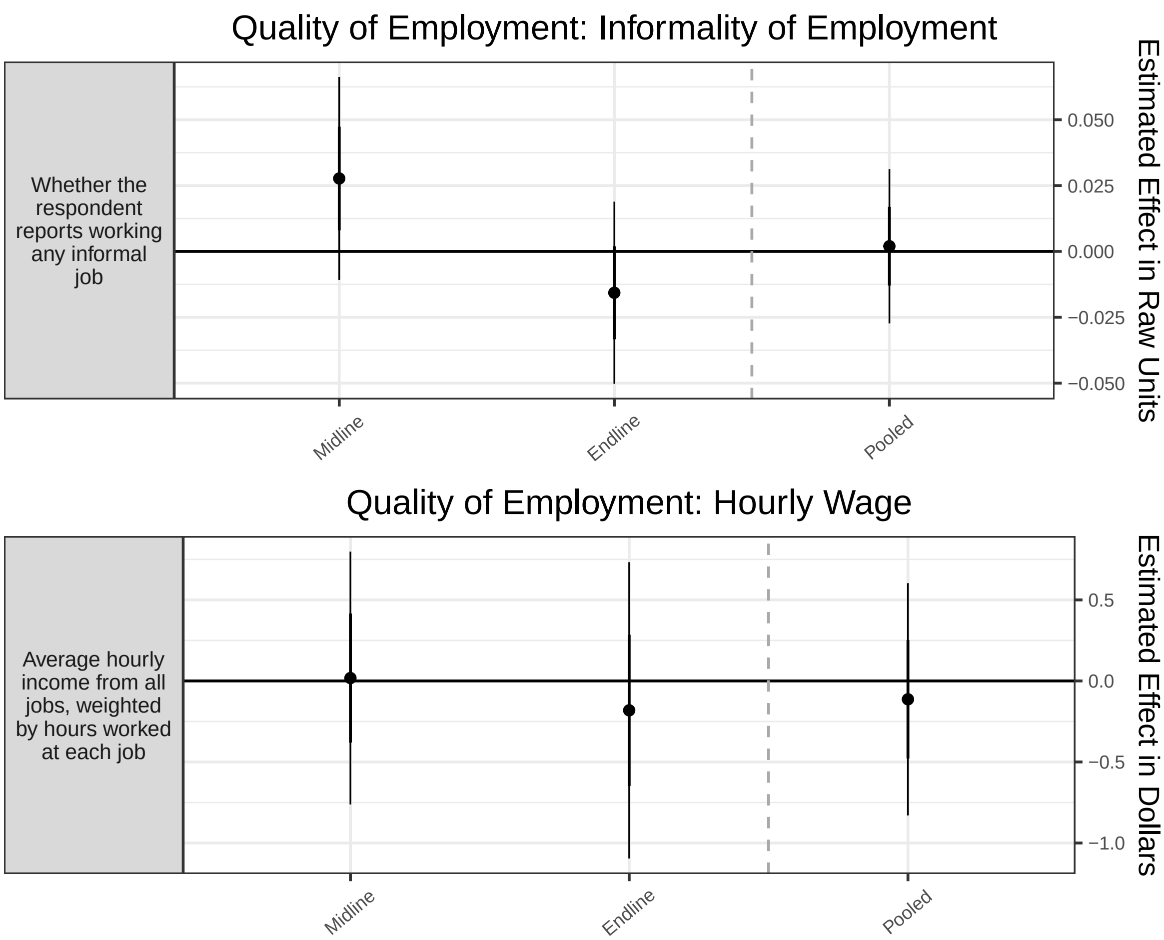
This figure plots the results of the estimates of the transfers on adequacy of employment over time.

Figure A22: Results for Employment Quality by Time Period



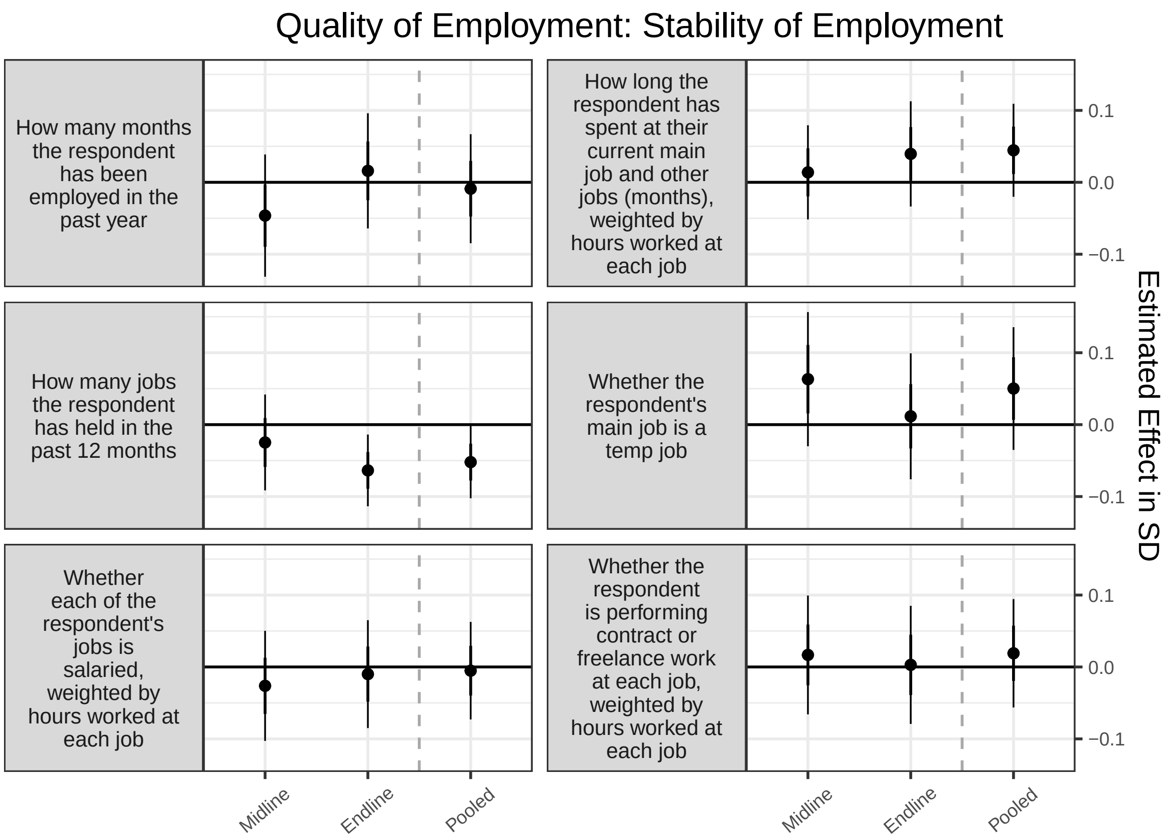
This figure plots the results of the estimates of the transfers on employment quality over time.

Figure A23: Results for Informality and Hourly Wage by Time Period



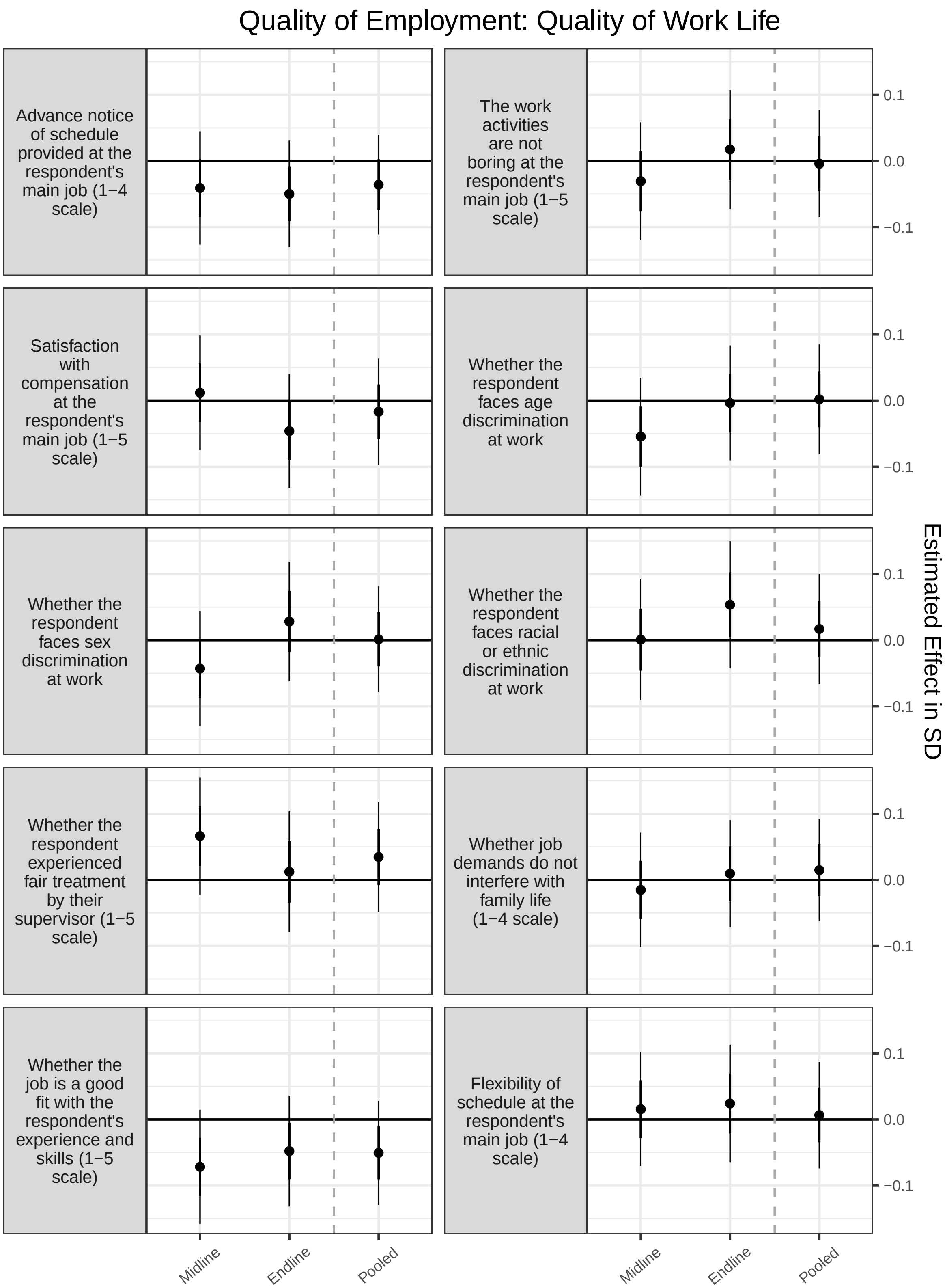
This figure plots the results of the estimates of the transfers on informality and hourly wage over time.

Figure A24: Results for Stability of Employment by Time Period



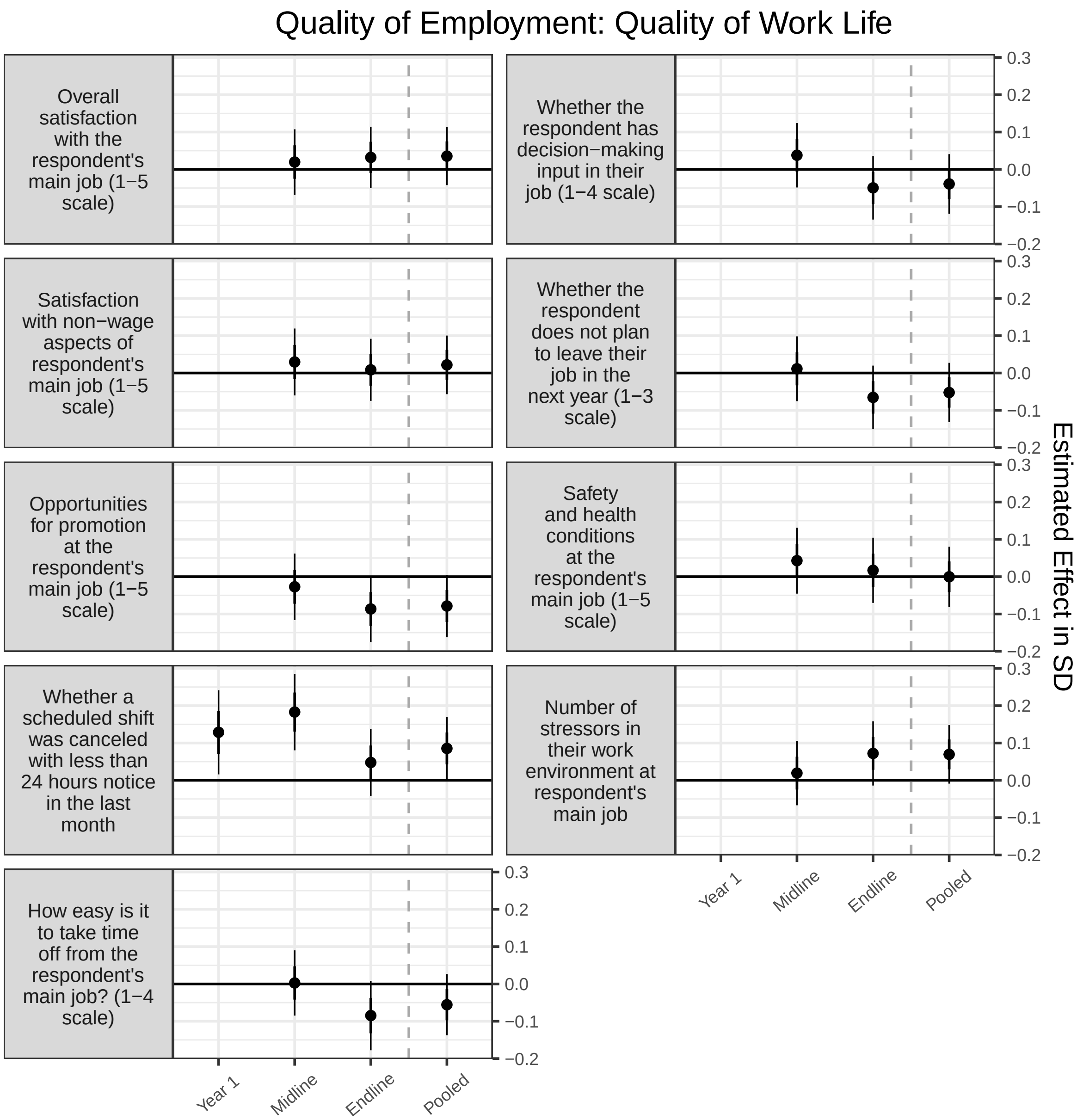
This figure plots the results of the estimates of the transfers on employment stability over time.

Figure A25: Results for Quality of Work Life by Time Period (1)



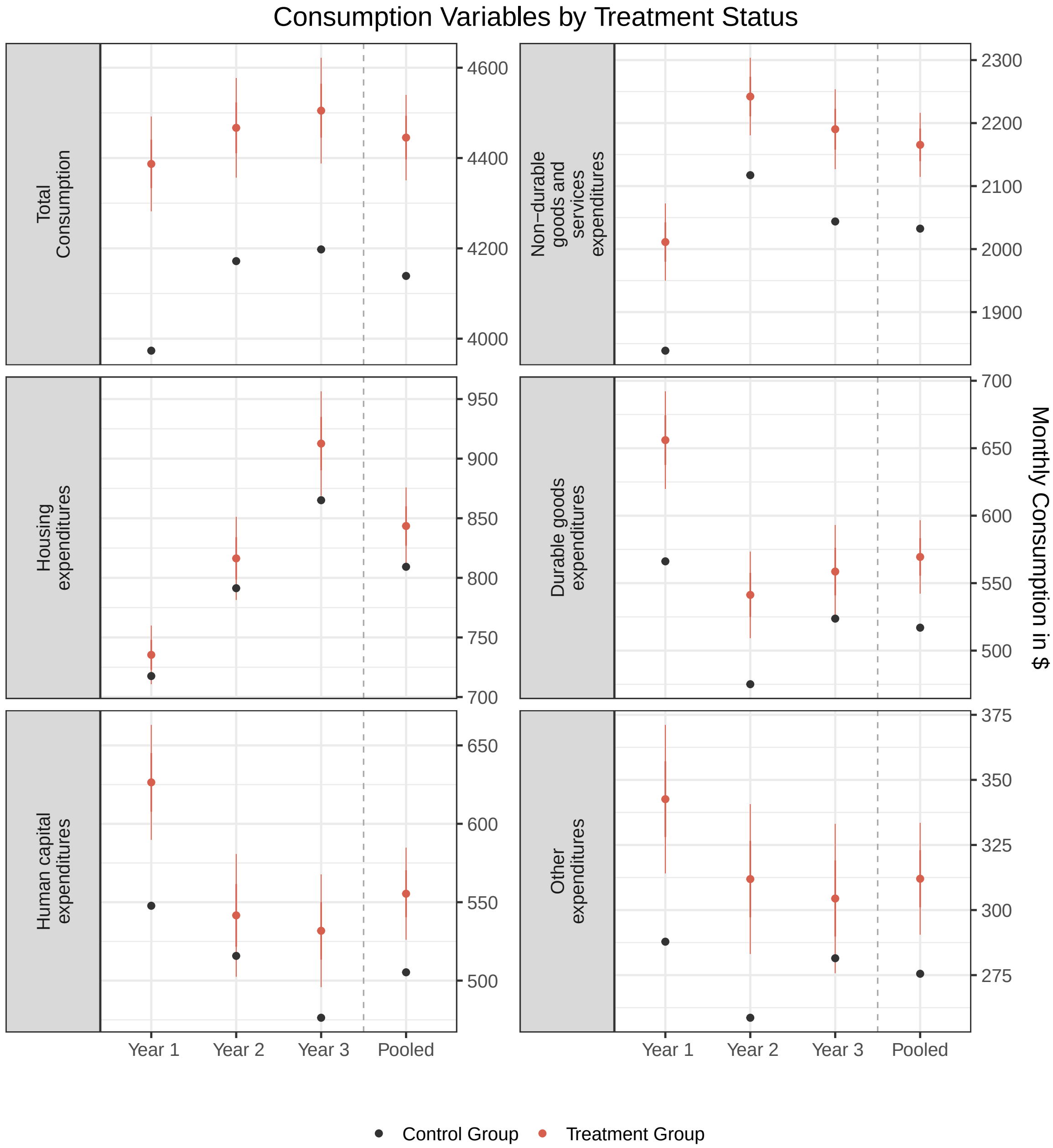
This figure plots the results of the estimates of the transfers on quality of work life over time.

Figure A26: Results for Quality of Work Life by Time Period (2)



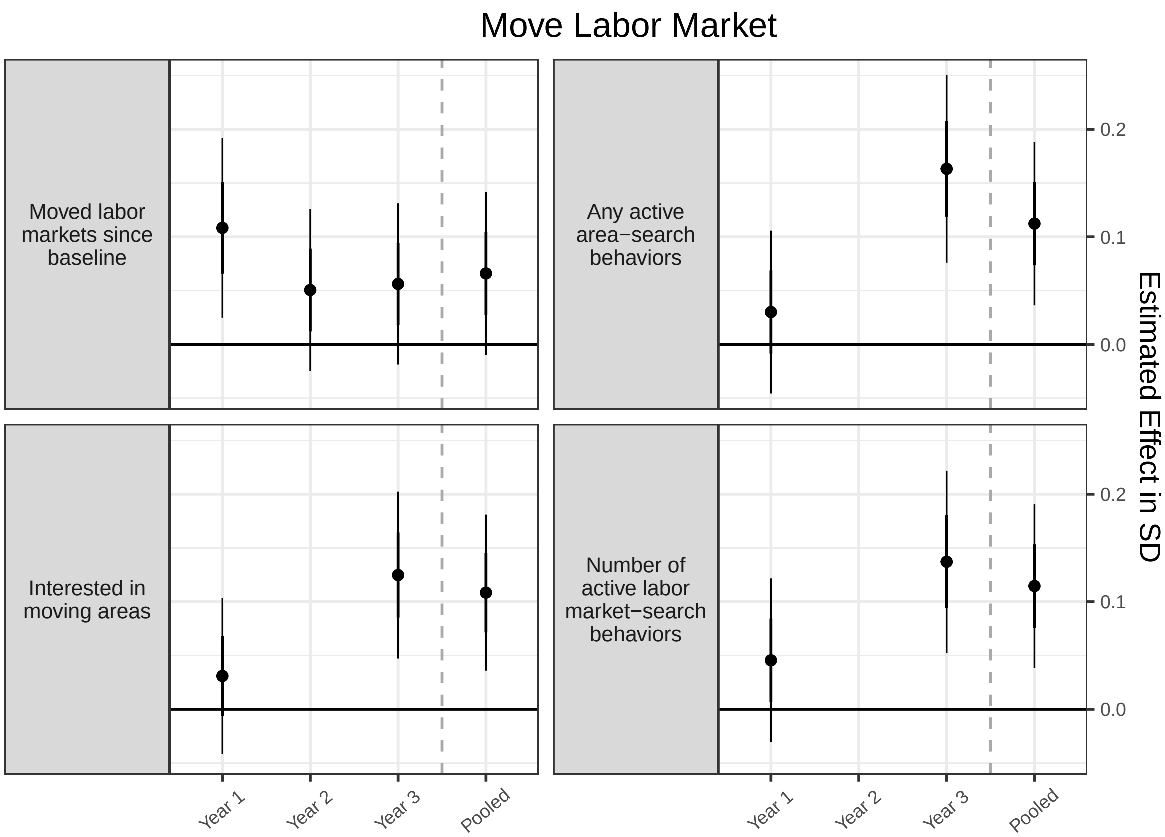
This figure plots the results of the estimates of the transfers on quality of work life over time.

Figure A27: Results for Consumption by Time Period



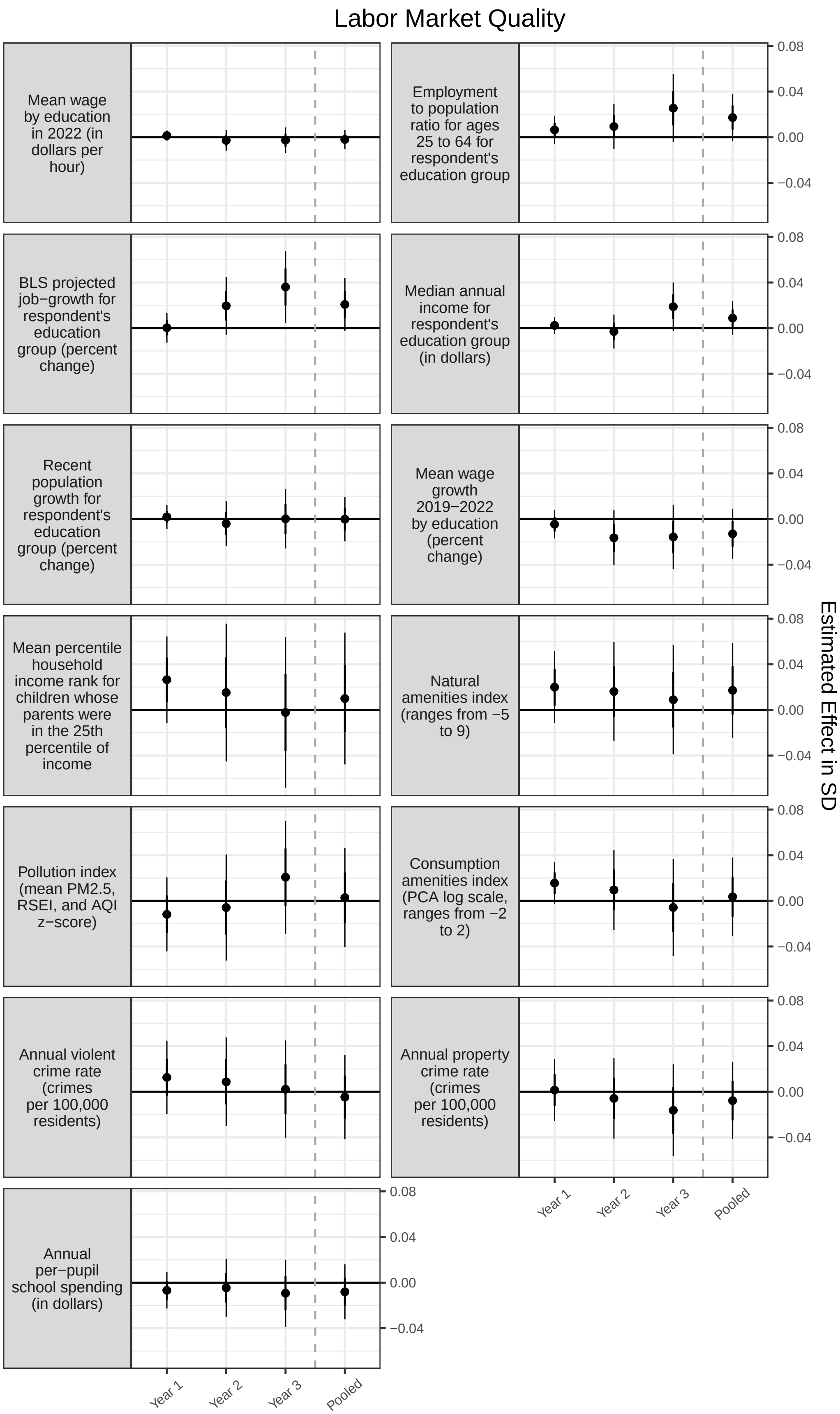
This figure plots the results of the estimates of the transfers on consumption over time.

Figure A28: Results for Moving Labor Markets by Time Period



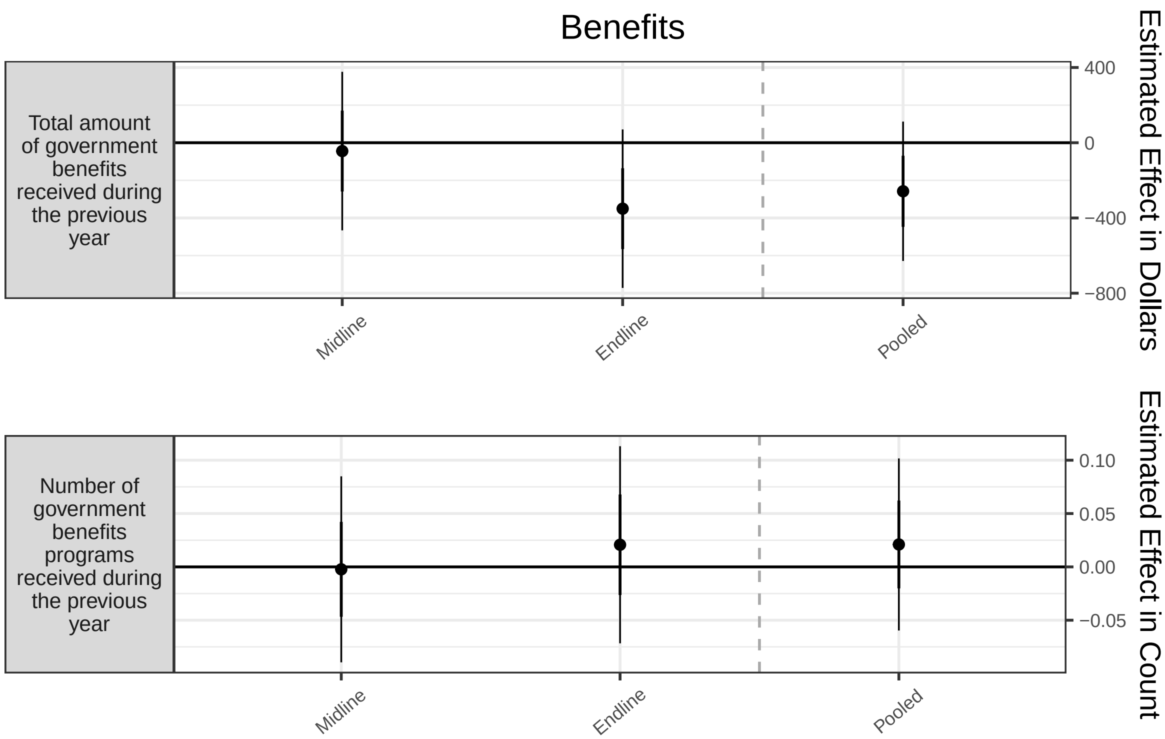
This figure plots the results of the estimates of the transfers on moving labor markets over time.

Figure A29: Results for Quality of Labor Market by Time Period



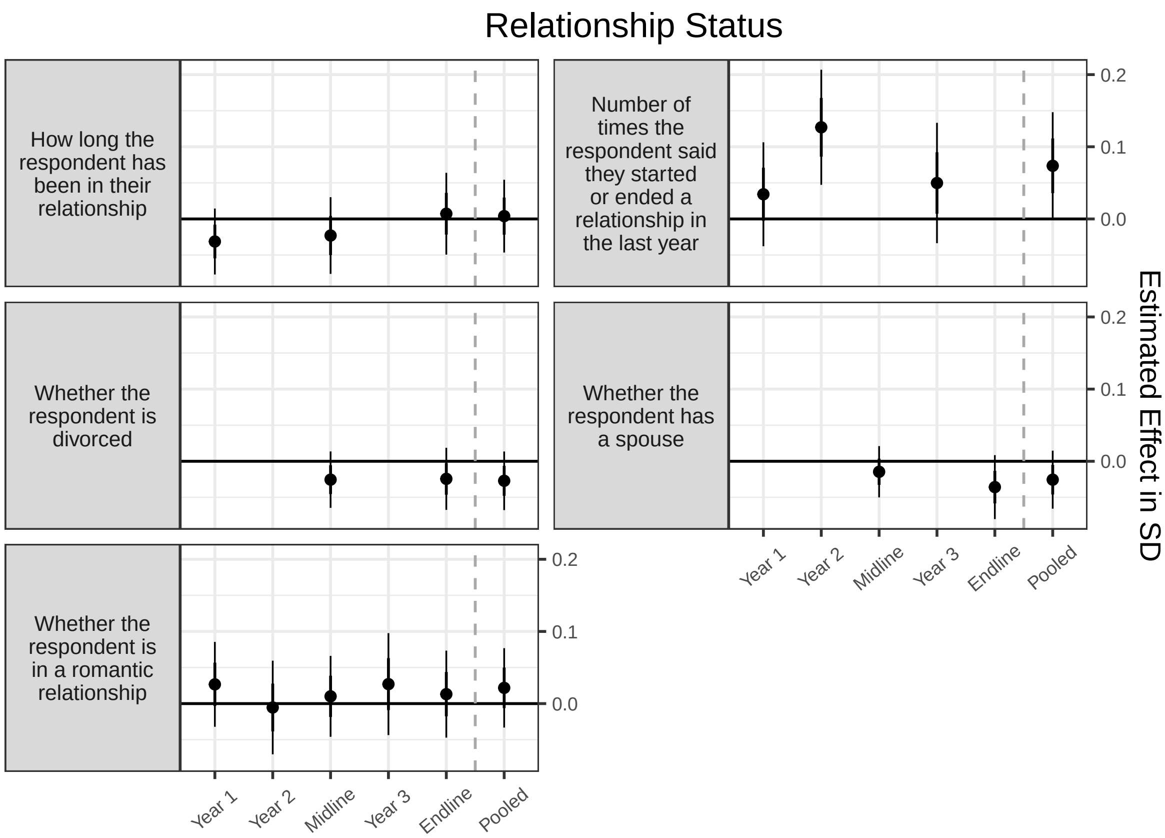
This figure plots the results of the estimates of the transfers on quality of labor markets over time.

Figure A30: Results for Benefits by Time Period



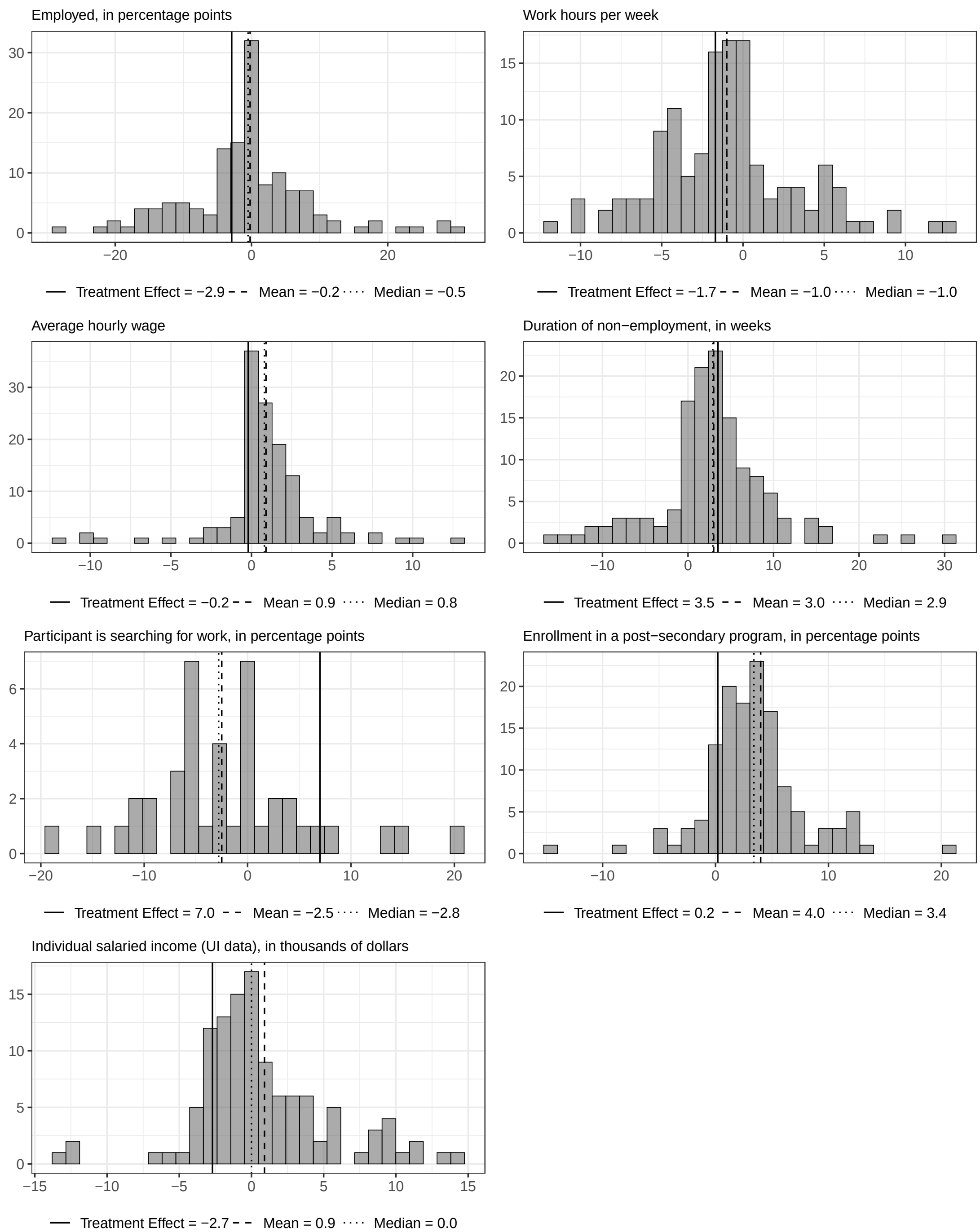
This figure plots the results of the estimates of the transfers on benefits over time.

Figure A31: Results for Relationship Status by Time Period



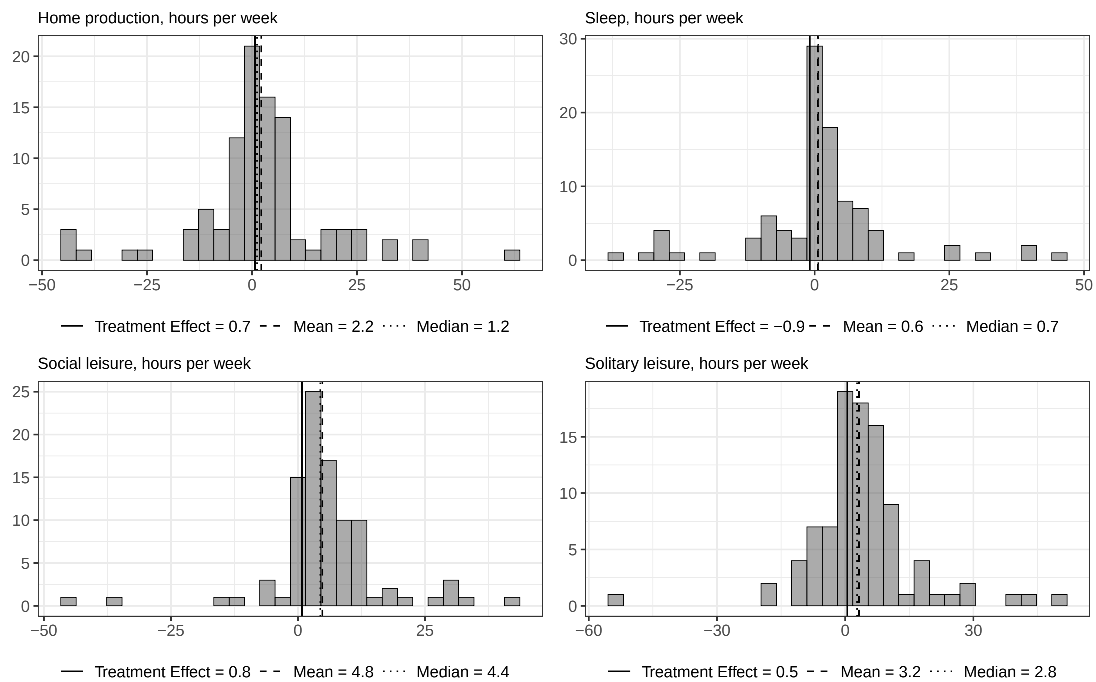
This figure plots the results of the estimates of the transfers on relationship status over time.

Figure A32: 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 A33: 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.