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

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December 31, 2024

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 $2,000/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. We observe no significant effects on investments in human capital, though younger participants may pursue more formal education. Overall, our results suggest a moderate labor supply effect that does not appear offset by other productive activities.

*We thank the non-profit organizations that implemented the program we study. We thank Leo Dai, Ethan Sansom, Jake Cosgrove, Kevin Didi, Taryn Eadie, Malek Hassouneh, Amy Huang, Joshua Lin, Anthony McCanny, Oliver Scott Pankratz, Idalina Sachango, Sophia Scaglioni, Stephen Stapleton, Derek Thiele, Angela Wang-Lin, Isaac Ahuvia, Francisco Brady, Jill Adona, Oscar Alonso, Jack Bunge, Rashad Dixon, Marc-Andrea Fiorina and Ricardo Robles for excellent research assistance. Alex Nawar, Sam Manning, Elizabeth Proehl, Tess Cotter, Karina Dotson, and Aristia Kinis provided invaluable support through their work at OpenResearch. We thank Carmelo Barbaro, Janelle Blackwood, Katie Buitrago, Melinda Croes, Crystal Godina, Kelly Hallberg, Kirsten Jacobson, Timi Koyejo, Misuzu Schexnider, and many others at the Inclusive Economy Lab at the University of Chicago for their pivotal role in supporting the project. This paper gratefully acknowledges funding from the NSF (#2149344) and private donors. This study received ethics approval from Advarra and the University of Toronto's Institutional Review Boards. The study was pre-registered on the American Economic Association RCT Registry (AEARCTR-0006750).

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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, advocates and policymakers have increasingly considered unconditional cash transfer programs that would not generate such distortions. However, even such unconditional programs will result in labor supply reductions if beneficiaries place a high value on leisure or derive a high disutility from the kind of work that is available to them. Typically such reductions in work due to income effects are not thought of as distortionary and instead reflect beneficiaries' high marginal utility of time off of work. But, even labor supply reductions due to such income effects could increase the fiscal costs of public programs, and they could also harm participants' long-term labor market prospects, especially to the extent that beneficiaries are not sufficiently forward-looking. 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.

Given that the U.S. government spends hundreds of billions of dollars each year on programs such as the Child Tax Credit (CTC), the Earned Income Tax Credit (EITC), Temporary Aid to Needy Families (TANF), and many other programs, it is important to understand the effects that cash transfers have on all these dimensions. Interest in these outcomes has driven extensive research on 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 job search, quality of employment, entrepreneurial activities, barriers to employment and disability, human capital formation, and labor market mobility. 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.

We investigate the causal effects of income on employment and these 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. This is the largest unconditional cash transfer program evaluated by a randomized controlled trial (RCT) in the U.S. to date in terms of the amount disbursed. We merge rich survey data with administrative records and mobile phone data. 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?

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 $2,000 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

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

in both the treatment and control group over the study period. The program caused a 3.9 percentage point reduction in the extensive margin of labor supply and a 1-2 hours/week reduction in labor hours for participants. The estimates of the effects of cash on income and labor hours represent 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 comparably to participants. Given the magnitude of these household effects, we cannot reject a unitary household model in which the household pools their income and makes decisions about labor supply and consumption jointly. For every one dollar received, total household income excluding the transfers fell by around 29 cents, and total individual income fell by around 18 cents. Estimates are similar using administrative data alone and survey data alone. 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 and 24-hour time diaries delivered through a mobile phone app on a randomly-selected weekday and 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.

We also saw significant impacts on duration of unemployment. Over the three years of the transfers, the duration of the average spell of non-employment in the control group was 7.7 months; the treatment had the effect of increasing this by 1.1 months. Those in the treatment group were more likely to have recently applied for work but applied to fewer positions on average.

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, reject-

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

ing changes in the index of more than 0.028 standard deviations and changes in wages of more than 73 cents. We find that those in the treatment group have more interest in entrepreneurial activities and are willing to take more financial risks. The coefficient on whether a participant started a business is positive, but not statistically significant. Using data from the National Student Clearinghouse on post-secondary education, we see some suggestive evidence that younger individuals are pursuing more education as a result of the transfers, which could potentially help to explain the labor supply effects within this subgroup. However, there are no significant effects in the broader sample. 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 see no significant reductions in barriers to employment. Finally, we see no significant changes in marriage or divorce, and participants do not appear to use the transfers to move to better labor markets.

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, particularly 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 small in magnitude ($1,606 USD in 2019), and casino disbursements to Native American families in the U.S. (Akee et al. 2010).[3]

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, including a 97% response rate to the midline survey and a 96% response rate to the endline survey.[4]

The comprehensive data collection enables us to estimate a structural model of labor and consumption responses with minimal assumptions relative to the literature. For example, as we observe consumption, savings and debt (Bartik et al., 2024), we can consider asset accumulation explicitly in our model. Compared to the literature, participants in our study spend nearly all the money they

[3]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.

[4]This does not account for mortality, so attrition due to other causes is even smaller.

receive each month on increased consumption or reduced labor. The relative lack of savings is important in calibrating our model as estimates of the marginal propensity to earn (MPE) greatly depend on the denominator, *i.e.*, how much of the transfers participants treat as theirs to spend that month as opposed to saving to spend in future time periods.

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 Nov. 2020 to Aug. 2021. They focus primarily on food consumption and financial well-being and do 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 (Stillwell et al., 2024; Sauval 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 larger transfers over a long period of time. 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, 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 build a structural model to explain our findings 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 locations (from Dallas and Chicago, respectively), suburban locations, medium-sized urban areas, and rural locations. 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 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

[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.

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 reflected the distribution of men and women in the eligible population according to data from the American Community Survey (ACS). To achieve the desired sample, we blocked participants on their 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. 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

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

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 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.

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.[7] 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 43% 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

[7] 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.

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.[8] Given the random assignment of clusters to treatment or control, the standard errors in our analyses are also clustered at this level.[9] 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. The 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.[10]

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

[8]In total, this approach yielded 18 clusters which had 2 people in them and 2 which had 3 people.

[9]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.

[10]This timing means that the majority of the transfers occurred after COVID-19 vaccines became widely available. As 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.

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.[11] 87.5% of participants consented for us to link their administrative records. Matching was done by state partners on SSN in Illinois and name, date of birth and SSN in Texas. The match rate was very high for those participants who provided a full SSN, but only 72% of participants in each of Illinois and Texas shared that information. In total, among those who consented to share administrative records but not conditioning on provision of SSN we obtained a 71% match rate in Illinois and a 73% match rate in Texas. In Illinois, we were able to analyze survey data alongside the administrative records. This allowed us to fill in missing earnings and employment records in the administrative data with the corresponding survey outcomes for those who were not successfully matched.[12,13]

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.[14] 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 the credit reporting agency Experian 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

[11] 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.

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

[13] 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. In Texas, we use the quarter that most participants took the survey in for post-baseline measures (Q2 in 2022 for midline and Q2 in 2023 for endline) and the latest baseline time period (Q3 in 2020).

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

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 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.[15]

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%.[16]

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.

[15]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.

[16]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%.

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-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 Attrition

We proactively curbed attrition and non-response by sending email and text reminders, as well as sending postcards that appeared to be handwritten and calling non-responders by phone. 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.

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% on average across the three years of the study.[17]

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,

[17] 4.4% in year 1, 7.5% in year 2, and 6.5% in year 3.

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.[18]

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%) 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

[18] We are unable to bring the full set of over a thousand baseline 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.

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.

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 a reduction in annual individual salaried or wage income of about $1,700 according to our preferred estimate, which pools administrative records and survey data (Table 3).[19] We asked participants survey questions about several types of income that go beyond income from the types of employment that would be reported in UI data. Specifically, we asked participants to state their total annual household income and total annual individual income, and we also asked them about several categories of income, including salaried income, wage income, self-employment income, supplemental income from gig work, passive income, and income from a variety of other sources including government transfers. Table 3 presents results, along with an aggregated total calculated income measure.

When participants are asked to report a single number capturing their total individual income, the estimated treatment effect is $2,500; when we ask about different categories of income in survey questions and aggregate them, the effect based on the aggregate calculated measure is $1,500. If we expect administrative data to capture the types of income that the survey data on salaried or wage income captured, then this aggregate calculated measure may be an under-estimate and the true effect on individual total income may be closer to $2,000.[20] The largest component of the reduction in calculated income appears to be salaried or wage income.

These estimates can be used to calculate participants' MPE. One subtlety is 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 consump-

[19]Specifically, we use administrative records for those who consent to sharing these data and are able to be matched as well as enumerated survey data for those who did not consent to share administrative records or, in Illinois, consented but were not matched. As described in the data section, leveraging survey data from this last group was possible in Illinois but not Texas.

[20]Considering that the gap between individual salaried income from aggregating administrative and survey data, $1,675, and the effect estimated from survey data, $1,150, is $525, and one might expect the calculated total income measure from survey data, $1,503, to be understated by at least this much.

tion in the future; and 2) the decision of how much to consume today in leisure out of the total amount reserved for today. 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 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.

The $2,000 figure lines up well with participants' reported reduction in work hours. Using data on hours participants report working at their various jobs, we estimate a 1.3 hour decrease in work hours per week (Table 4).[21] In our enumerated and quarterly time use surveys, we similarly observe a 1.4 hours/week reduction in work hours (Figure A3), and in the mobile phone app-based time diaries a 1.3 hours/week reduction in work hours plus other income-generating activities combined (Figure 5). A $2,000 reduction in annual income represents an approximate 5-6% reduction relative to the control mean for total individual income, while a 1.3-1.4 hours/week reduction in work represents an approximately 4-5% reduction relative to these variables' control mean. We expect that effects on work hours are slightly understated, similar to effects on income (the survey-based estimate of $1,500 represented a 4-5% reduction, in line with the work hours estimate). Based in large part on the administrative data, we estimate a 3.9 percentage point reduction in the extensive margin of labor supply (Table 4), which means that the great majority of the effect on total hours worked is a result of the effect on the extensive margin.[22] This makes intuitive sense: most low-income jobs do not come with a great amount of flexibility in work hours, so effects on labor hours are primarily driven by

[21] As with all our preferred measures, 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.

[22] Specifically, the estimate aggregating administrative and survey data is 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 5% of the control group mean, the entire labor supply effect would be driven by the extensive margin. If hours worked per week declined by 1.8, representing 6% of the control group mean, the extensive margin would represent 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 1.6-1.7 hours/week (Tables A24-A25).

people quitting their jobs or not getting new ones as quickly rather than through participants reducing work hours.

Participants also reported their total household income. The transfers decreased total household income by about $4,100/year relative to the control group in survey data (Table 3); however, as this measure was asked in a similar way to the question about their total individual income, we expect it may also somewhat overstate the decline. If we adjust this measure using the ratio of the administrative data-adjusted individual calculated income and the individual aggregate self-reported income, we get about a $3,300 reduction in total household income.[23] This represents a 6.9% decline relative to the control group mean. We estimate that there is a decrease in labor supply for all adults in the household of approximately 2.2 hours/week (Table 4), with the component that is not due to participants driven almost entirely by participants' partners. This estimate represents a 4.6% decrease compared to the control mean. 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 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, consistent with a unitary household model, and we cannot reject that they are the same.

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 1.3 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. The extra time participants have from reduced work is used largely

[23]Specifically, -$4,125*$2,028/$2,504=-$3,341.

for leisure,[24] non-commuting transportation, and other activities (Figure 5).[25] Survey data show a decrease in work hours of 1.4 hours/week, which is very similar to the results from the mobile phone app.

Figure A4 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. Figure A5 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 time "with my boss".

Both effects on income (Figure A13 and A15), effects on labor supply (Figure A14 and A15-A16), and effects on time use (Figure A17-A20), suggest a time trend: the treatment effect grows over the course of the study. 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 survey data that treated participants may start to "catch up" towards the end of the study (Figure A16): at least, we cannot reject null effects for some quarters close to the end, while we can in the second year of the program.[26] 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).

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.[27] We estimate η_e for participants as

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

[25]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 "finances"; people in the treatment group spent approximately 0.3 hours/week more on this activity (Figure A3).

[26]However, this is less apparent in the administrative data (Figure A15).

[27]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

-0.15 and η_i for participants as -0.13.

5.2 Duration of Unemployment

Duration of non-employment and unemployment both go up, as one might expect if, with the transfers, people feel less pressure to immediately take up a new job upon leaving one (Table 5). 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. These two types of measures do not have to coincide. The duration of the average spell of non-employment causally increased by 1.1 months relative to the control group mean of 7.7 months, with treated participants' longest spell of non-employment increasing by 0.8 months relative to the control group mean of 8.7 months. The cross-sectional measures had somewhat smaller estimated effects on lower control group means: the effect on the point-in-time measure of the duration of non-employment was 0.7 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.[28]

5.3 Job Search and Selectivity

Receiving unconditional cash transfers made recipients more likely to search for a job and apply for a job (Table 6). There is also some suggestive evidence that they took more actions to search for a job.[29] However, they applied to on average about 1 fewer job in the last 3 months (compared to a control mean of 5.5 applications in that time period) and perhaps interviewed for fewer as well.[30] These results suggest that while treated participants are more interested in finding work (perhaps in part due

unearned income of $950/month, and the elasticities are calculated accordingly.

[28] 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.

[29] This is marginally significant before multiple hypothesis corrections. "Actions" here include things like looking at job postings, directly contacting employers for a job, contacting job centers, contacting friends or relatives to find work, contacting a professional network to find work, posting a resume online, and taking other actions to find work. These are broken out in exploratory analysis in Table A40.

[30] This item is marginally significant before the multiple hypothesis correction, but not after it.

to more of them not being employed), they are more selective about the jobs they apply for. The mixed results mean that the "Active Search" component, and the overall index for this family of outcomes, are both insignificant. Nonetheless, the results for individual items seem to tell a compelling story.

We do not see much in the way of differences in the types of jobs participants applied for (Table 7). 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 is a requirement, but this result does not survive the false discovery rate correction (Table A42).

5.4 Quality of Employment

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 were fairly small (3.9 percentage points), we believe these quality estimates are still largely directly interpretable.

First, we consider adequacy of employment. Many low-income individuals would like to work more hours but are constrained by not being offered many hours of work by their employers. This component measures the adequacy of their employment, including whether they 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. Second, we consider employment quality as measured by 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. Finally, we consider quality of work life, which 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.

Despite the very detailed questions, the results do not support any changes in quality of employment, and for most items we can reject even small changes (see Table 8 for the component index measures and Table 9 for the raw item measures).[31] Overall, we can reject changes of more than 0.028 standard deviations in the family-level index. 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, as was the exploratory outcome of how many months they expect to remain in their main job.[32] 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 cancelled with less than 24 hours notice in the last month and report a larger number of stressors in their work environment, treated participants reported finding it slightly harder to take time off from work. Apart from results not being broadly significant, point estimates were generally small across the board.[33]

5.5 Entrepreneurship

In contrast to the quality of employment measures, we see some shifts in entrepreneurship, at least in terms of entrepreneurial orientation and intention (Table 10). 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. Both components, as well as the overall index, are very significant.

While treated participants exhibited more entrepreneurial orientation and intentions, this did not translate into significantly more entrepreneurial activity. The point estimate is positive, but small, and it is possible that very few people have the inclination to become entrepreneurs in general. We prespecified that we would consider entrepreneurial orientation and intentions as potential precursors to

[31] For example, people who had multiple jobs were asked exploratory questions about why they had multiple jobs.

[32] This latter question was asked only of those participants whose main job is temporary, and not much weight should be placed on it.

[33] Table A43 provides further exploratory analyses within this family of outcomes, obtaining a more detailed breakdown of which specific benefits are offered by participants' employers.

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 2.8 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 (Figure A30).

5.6 Disability and Barriers to Employment

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 4.0 percentage 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 4.0 percentage points on a base of 28 percentage points in the control group) (Table 11). 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 (Figure A31), 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 time of the survey.

We also asked participants 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 impacts on self-reported barriers to employment (Table 12).

5.7 Human Capital Formation

If treated participants are investing more in education, we can expect them to have better long-term employment outcomes, all else equal. As Hoynes and Rothstein (2019) have highlighted, education is a particularly important determinant of the long-run cost-effectiveness of cash transfers. To investigate these outcomes, we leverage National Student Clearinghouse (NSC) data. 87.5% of participants provided consent for their administrative records to be used; for those who did not, we supplement the NSC data with survey data. The NSC includes outcomes on completion of post-secondary programs, total years of post-secondary education completed, and enrollment in post-secondary programs. The survey data gathers these same variables, but additionally gathers information on attainment of high school degrees / GEDs and informal education.

By and large, we do not observe significantly improved education outcomes in our sample, though there are some indicators of minor improvements. 92 percent of the control group had completed a high school, GED or post-secondary program by endline, and the treatment group was 0.8 percentage points more likely to have completed such a program (Table 13). This result was not significant when pooling across time periods but appeared to grow over time such that by endline the result was only marginally insignificant (Figure A33). Exploratory analysis suggests that this response is more driven by participants getting a GED than by their getting a post-secondary degree, since we observe insignificant, negative coefficients when restricting attention to completion of post-secondary degrees (Table A44). Total years of post-secondary education completed post-baseline and enrollment in a post-secondary program showed positive but insignificant effects.

We pre-specified one heterogeneity analysis for this family of outcomes: a subgroup analysis based on the age of the participant at baseline. Since education is an investment that only provides returns over time, 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. Indeed, when we conduct this heterogeneity analysis, we observe that those participants who were under 30 at the time of the baseline survey appear more likely to be enrolled in a post-secondary program, and this is even marginally significant prior to the false discovery rate correction. The formal education component is also significant at $p < 0.05$ prior to these corrections.

While these results are fairly noisy, they would be consistent with individuals having different uses for the transfers, with only some individuals using them to pursue education.

5.8 Household Stability

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 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 (Table 14). A relatively small share of people in our sample were married at baseline, and other types of relationships are more common in our sample, 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. 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.

In exploratory analysis, we consider self-reported reasons why relationships ended (Table A45), 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.

5.9 Labor Market Mobility

We observe 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 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 a different commuting zone (Table 15). 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, although this is not statistically significant. The treatment group reported more active labor market search behaviors, however, and participants indicated significantly greater interest in moving areas, such that the overall index for moving labor markets remained 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 (Table 16). There are no significant differences in the labor markets that treated and control participants live in by the end of the study, with the exception that treated participants are perhaps more likely to live in areas with lower crime, though this does not survive the false discovery rate adjustments. Further, all differences are relatively small. However, just because participants are not necessarily moving to different labor markets 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.

5.10 Take-Up of Benefits

The welfare effects of cash transfer programs depend in part on whether recipients substitute away from other social programs. While the cash transfer program we are studying was carefully designed to minimize the risk that the cash transfer recipients would lose other benefits they might receive, if the transfer has effects on work or education outcomes it may nonetheless indirectly affect eligibility for programs such as the EITC, SNAP, Medicaid, or SSI. Further, a large literature suggests that the take-up of benefits often depends on having sufficient time and energy to apply for benefits, and the cash transfers might provide participants with more bandwidth. Conversely, if participants are doing well, they may feel less need to apply for benefits.

Overall, we do not observe statistically significant effects on benefits take-up in survey data (Table 17). Benefits decrease by about $300 per year, but this is a very noisy estimate.[34] 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, we would not anticipate them to survive the multiple hypothesis corrections, 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 A8). 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.

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

[34] 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.

more than those who did (Tables A9 and A13). In fact, those with a bachelor's degree had insignificant increases in individual salaried/wage income, while reducing self-employment income and 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 A14), and we also observe larger effects on formal education among those in this younger age group (Table A17). 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 (Tables A20-A21). 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 ambiguous. Males in the treatment group appear to have slightly larger reductions in income relative to the control group (Table A10), while females and others appear to have slightly larger treatment effects on labor supply (Table A15). These differences are not significant.

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 (Tables A18-A19).

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 18, A11, and A16). 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, and related to this, total household incomes at baseline were about \$2,700 lower ($p<0.001$) in Texas, so the transfers represented a relatively larger share of income (though only by about 4 percentage points). Third, 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. Fourth, 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 7), employed participants' wage rate (Table 8), or the weight participants placed on potential income in job search (Table A42), and in fact baseline wages were insignificantly higher in our sample in Texas, so this explanation may be less likely.

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 A12). 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 18 shows a full breakdown across the different data sources. In both sources of data, individual income and labor supply is lower 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 (Figures A15-A16). However, the magnitude of the treatment effect is somewhat 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 slightly different samples contribute to the gap (as a small share of participants in Texas cannot be matched to administrative data), though those who were unmatched in Illinois did not appear to be very different from those who were matched in survey data (Table 18).[35] Finally, the survey data may somewhat understate the declines in employment, especially 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 consistent with the estimates from the main analyses. 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. This is also true for all components except a few single-item components. 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 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 using survey data. The magnitudes of the point estimates remain comparable.

[35] 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.

6.4 Modeling Labor Supply Decisions

The literature on cash transfers often focuses on estimating MPEs. To estimate the MPE, one needs to know how much individuals decide to set aside for future time periods versus how much to spend in increased leisure or consumption this period. Out of the amount individuals decide to spend this period, one can then observe how much they spend on leisure.

Most papers that estimate MPEs do not have data on consumption or net assets and so have to make assumptions about how much people set aside for future time periods, *i.e.*, via assumptions about the discount rate. Assumptions about the intertemporal choices people make are particularly important in studies of lottery winners, since lottery winners often receive a large lump sum payment and it is not necessarily obvious how much people will allocate to spending each period. If people experience diminishing marginal utility to cash, they may decide to save most of it, so a large lump sum transfer gets reduced to a small annual spend. For example, in Golosov et al. (2023), the median winner receives a lump sum payment of $44,000, not too far off from the $34,200 that participants in our study receive over the duration of the program.[36] The reasonable assumptions in their paper lead to a share to be spent in the first year of 4.2%, or $1,855.[37]

In our setting, 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). Participants appear to spend approximately the full amount of the transfers each month, on average.[38] Regardless of the precise extent of savings and debt, this marks an interesting divergence from the literature on lottery winners. Since our participants are younger than those in lottery studies, we would expect them to live for more periods of time and therefore to save more, all else equal. However, if we were to follow the same model as in Golosov et al. (2023) and set the same values for the discount rate and real interest rate, we would predict participants allocate less than $100 per month to spending, whereas we observe much more than that in both our consumption and labor data.

This lower rate of savings in our sample compared to the literature is not unreasonable and there

[36] ($1,000-$50)*36, accounting for the control group payments.

[37] Based off a k-year old household with remaining lifetime of $T-k$ years and discount rate d allocating share λ of a lump-sum transfer to the first $\bar{t}$ years: $\lambda(r,d) = \Sigma_{t=1}^{\bar{t}} \left(\frac{1+r}{1+d}\right)^t \frac{d}{1+d} \left(1 - \left(\frac{1}{1+d}\right)^{T-k+1}\right)^{-1}$.

[38] There is a portion of the transfers that is not captured in our labor, consumption, and assets data, and it is possible that some of this unobserved amount flows to assets. However, the literature is clear on the tendency for consumption to be particularly underreported, so it is more likely that the bulk of this is underreported consumption (Meyer, Mok and Sullivan, 2015).

are many potential explanations for it. First, our sample is lower-income on average, which implies that more of the transfers could go to meeting immediate basic needs. The program we study also had a monthly disbursement schedule, and perhaps this might encourage participants to think of the transfers as money to spend that month, as a kind of mental accounting. At the same time, it is possible that in taking on new financial activities, participants also faced new unexpected shocks. For example, a participant that purchases a car may find that they have to do unexpected maintenance on it, or fix a broken window. Alternatively, perhaps participants in our study have different time preferences than those in the lottery studies. If participants fear that others will ask them for a portion of the transfers, they may have an extra incentive to spend it quickly.

In order to more closely compare our paper to the literature, we use a standard model to estimate the MPE. We consider the following objective function:

$$
\begin{aligned}
&\max_{c_t, l_t, A_{t+1}} \sum_{t=1}^{T} \delta^{t-1}[u(c_t - \gamma_c) - Bv(l_t)] \\
&s.t. \\
&A_{t+1} = (1+r)[A_t + w_t l_t - c_t + I_t + T_t] \\
&c_t \geq 0 \\
&A_{T+1} = 0
\end{aligned}
$$

where c is consumption, γ is a subsistence level of consumption, B is a scaling factor, v is the marginal disutility of labor or the marginal utility of leisure, l is labor supply, δ is the discount rate, A are assets, r is the real interest rate, w are real wages, I is non-labor income including passive income, government transfers, and gifts from family and friends, and T are the unconditional cash transfers. Consumption is assumed to be positive, and assets in the final period are assumed to be spent down, as is typical in the literature.

We assume individuals have a CRRA utility function for consumption:

$$
u(c_t) = \frac{c_t^{1-\sigma}}{1-\sigma}
$$

and the marginal disutility of labor is governed by:

$$v(l_t) = \frac{\eta}{1-\eta} l_t^{\frac{1-\eta}{\eta}}$$

where η is the Frisch elasticity of labor supply.

We estimate this model using moments from data on employment, consumption, and net assets.[39] We focus on estimating the unitary household model, as it appears to have the most support in our estimates. We then use the fitted parameter values to estimate the MPE.[40]

Table 19 compares the MPE as estimated by the model with the MPE estimates calculated from our reduced form estimates under different assumptions about changes in net assets over the study period. The aggregate estimate for net worth excluding real estate assets and real estate debts in Bartik et al. (2024) is about -\$2000; we also provide estimates that assume no net change in assets and a relatively high value of accumulation of \$5000 over the study period, representing approximately the upper end of the confidence interval and a value that might be a reasonable upper bound if there is under-reporting of net asset accumulation. Modest changes in net assets do not affect the MPE estimates much. For example, for a decline in total household income of \$3,341 per year, the pooled estimates range from -0.28 - -0.34.

The MPE estimates from the fitted model are roughly in line with the calculated MPE estimates but slightly lower, perhaps in part due to the fact that income decreases between midline and endline, and the model weights all time periods equally while our pooled results place more weight on results towards the end of the study. On the whole, these estimates are larger than in some of the lottery studies (Imbens, Rubin and Sacerdote, 2001; Cesarini et al., 2017) but smaller than in Golosov et al. (2023). However, the model overly smooths across time periods, indicating that one would need some kind of friction in order to get the time trends we observe. There could be several sources of frictions. For example, our results would be consistent with participants finding it harder than expected to find jobs over time.[41] Alternatively, individuals may take different amounts of time to decide what to

[39] Specifically, we use the quarterly employment estimates from UI records, the quarterly consumption data from surveys, and the data on net assets from the enumerated midline and endline. We believe that assets and debt are fairly reliably estimated in our data (debt is from Experian for those who consent to share administrative records), while consumption is somewhat underreported, similar to how studies leveraging the Consumer Expenditure Survey show it underreports consumption. In order to account for this, we generate a scaling factor using the employment and net asset estimates. We leverage the fact that by midline and endline, the total transfers up to that point minus the observed decreases in income minus the net assets accumulated should equal consumption up to that point).

[40] We assume r=0.02 yearly. Figure A8 shows the trajectory of labor earnings, consumption and net assets over time per the best-fitting parameter values: σ=1, η=0.3, δ=0.96.

[41] We observe some suggestive evidence that participants who leave a job are overly optimistic about their chances of finding an acceptable job quickly. When participants leave a job and are asked how likely it is they think they will find a new acceptable job within 6 months, approximately 59% of people consider this “Very likely”, however, only 34% actually

do with the transfers. If, over time, more treated participants decide to pursue education or start an informal business, this could also contribute to the patterns we observe. Since there are many potential explanations that fit our data, we do not take a stance on which potential explanation is correct but note that estimates based on different snapshots of time could come to quite different conclusions about the MPE and this heterogeneity can be obscured by the standard model.

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.[42] 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.[43] Researchers were not compensated, and the survey was unincentivized.[44]

We supplemented the sample by eliciting forecasts from users of the Social Science Prediction Platform (SSPP), including its Superforecaster Panel.[45] The Superforecaster Panel is a panel of researchers interested in forecasting who take nearly every survey posted on the platform. Panellists 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 20 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

do take up a new job in this timeframe. This optimism is not significantly different between the treatment and control group, but it is possible that if the treatment group feels more able to act on this optimism due to having more of a safety net, it could lead to the kind of path we observe. It is also possible that the labor supply effects in the earliest time periods could have been affected by the tight labor market or other benefits available during the COVID-19 pandemic.

[42] 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.

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

[44] 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.

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

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 and on individual salaried income as measured in the administrative data, as judged by their mean and median responses. These forecasts somewhat understated the observed effects, but are within their confidence intervals. However, there was great heterogeneity in beliefs. Figure A9 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 slightly underestimated the effects on the average duration of unemployment, though the median and mean lie within the confidence interval of the observed treatment effect. They predicted increases in the hourly wage, whereas the estimated effects on hourly wage were -$0.23 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 7.1 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 0.0. Again, the confidence interval on the point estimate excludes the mean and median of any subgroup's forecasts.[46]

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

[46] 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.

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 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 the high value that participants place on leisure at the margin or, equivalently, the high disutility they have for the kind of work that is available to them.

While the duration, magnitude of the transfers, and comprehensive nature of our data collection is unprecedented for a study of this size, 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, 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.

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 they placed a high value, at the margin, on additional leisure. 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	0.25	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	0.38	0.24	0.24	0.37	0.24	0.24
Rural County	0.26	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	0.16	0.16	0.16	0.15	0.16	0.16
Large Urban County	0.24	0.53	0.53	0.51	0.53	0.53
Panel B. Demographic Characteristics						
Any Children	0.59	0.59	0.62	0.57	0.59	0.57
HH Size	3.36	3.25	3.34	3.14	3.20	2.98
Age <30	0.52	0.54	0.53	0.54	0.54	0.54
White (non-hispanic)	0.59	0.46	0.41	0.48	0.46	0.47
Black (non-hispanic)	0.17	0.25	0.29	0.25	0.26	0.30
Hispanic	0.17	0.22	0.25	0.22	0.22	0.22
Female	0.57	0.59	0.61	0.68	0.69	0.67
HH Income	36,199	30,521	31,204	32,327	29,245	29,942
College Degree or more	0.17	0.16	0.16	0.28	0.25	0.20
Renter	0.56	0.68	0.66	0.82	0.84	0.79
N	919395	904792	35086	14573	14573	3000

Notes: 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
Male	0.328	0.319	0.627
Female	0.669	0.678	0.628
Non-binary/other	0.003	0.003	0.999
Non-Hispanic Black	0.295	0.305	0.554
Non-Hispanic Asian	0.036	0.038	0.790
Non-Hispanic White	0.473	0.463	0.597
Non-Hispanic Native American	0.020	0.025	0.428
Hispanic	0.220	0.214	0.694
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)	29.991	29.917	0.922
Total Individual Income (1000s)	21.355	21.217	0.861
Work Hours/Week	21.207	21.780	0.487
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 Romantic Partner	0.441	0.431	0.586
Married	0.221	0.222	0.951
Divorced	0.077	0.081	0.706

Notes: 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
Total household income (self-reported)	48.2 (33.9)	-4.1***†††	-0.34 - 0.42	-0.30	2898
		(1.0)			
		[0.00]			
Total individual income	36.6 (27.0)	-1.5*	-0.12 - -0.15	-0.15	2881
		(0.9)			
		[0.13]			
Total individual income (self-reported)	33.5 (25.1)	-2.5**†	-0.21 - -0.26	-0.26	2855
		(1.0)			
		[0.09]			
Individual salaried/wage income (pooled UI and survey data)	24.3 (25.0)	-1.7**	-0.14 - -0.17	-0.27	2614
		(0.9)			
		[0.13]			
Individual salaried/wage income	26.0 (26.2)	-1.1	-0.10 - -0.12	-0.18	2920
		(0.8)			
		[0.30]			
Self-employment income	5.9 (13.7)	-0.1	-0.01 - -0.01	-0.08	2902
		(0.5)			
		[0.42]			
Income from gig work	0.4 (1.3)	-0.1	0 - -0.01	-1.65	2925
		(0.0)			
		[0.19]			
Passive income	0.0 (0.2)	0.0	0.00 - 0.00	0.70	2923
		(0.0)			
		[0.19]			
Other income	4.7 (6.1)	-0.1	-0.01 - -0.01	-0.04	2935
		(0.2)			
		[0.38]			
Government transfers	3.6 (4.9)	-0.2	-0.01 - -0.02	-0.10	2962
		(0.1)			
		[0.43]			

Notes: 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 other families. 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-item components. The MPE range associated with each estimate is calculated assuming net asset accumulation of -$2000 to $5000 over the course of the study. The preferred MPE estimate for the total household income adjusts for the fact it may be misreported by adjusting it according to the ratio of the total calculated individual income and the aggregate self-reported individual income measure, as described in the text. Estimates are provided in terms of raw units ($). All measures are survey-based except for the pooled UI and survey data estimate. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table 4: Impact of Guaranteed Income on Employment

	Control Mean	Treatment Effect	Elasticity	N
Labor Supply Elasticity Index		-0.06**†† (0.03) [0.04]	.	
Labor Supply Elasticity Component		-0.06**†† (0.03) [0.04]	.	
Whether the respondent is employed (pooled UI and survey data)	0.72 (0.39)	-0.04***†† (0.01) [0.01]	-0.15	2638
Whether the respondent is employed	0.74 (0.39)	-0.02* (0.01) [0.67]	-0.07	2962
Hours worked per week	30.28 (19.83)	-1.28**†† (0.64) [0.03]	-0.13	2940
Number of other household members which are employed	0.47 (0.61)	-0.02 (0.02) [1.00]	-0.11	2943
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.16*** (0.78) [0.31]	-0.16	2945
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.21** (0.92) [0.37]	-0.13	2945
Total number of hours participant's parents in household work per week	3.22 (12.07)	-0.13 (0.35) [1.00]	-0.09	2941
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.30 (0.29) [0.99]	1.68	2945

Notes: This table shows the impacts of an unconditional cash transfer on the labor supply of participants. The top-level index, "Labor Supply", in bold, declines by about 0.06 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). Items that are italicized were exploratory items for the sake of the FDR corrections (post-pre-analysis plan, i.e., the lowest FDR tier). 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. All measures are survey-based except for the pooled UI and survey data estimate. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table 5: Impact of Guaranteed Income on Duration of Unemployment

	Control Mean	Treatment Effect	N
Duration of Unemployment Index		**-0.10*****††** **(0.04)** **[0.026]**	
Single-item Component: Average length of continuous spells of non-employment in months, over the study duration	7.68 (11.16)	1.09**†† (0.46) [0.018]	2930
Length of longest continuous spell of non-employment in months, over the study duration	8.73 (11.73)	0.85***†† (0.32) [0.031]	2930
Duration of unemployment in months at time of survey	2.87 (8.05)	0.60**†† (0.29) [0.036]	2940
Duration of non-employment in months at time of survey	6.07 (12.21)	0.72**†† (0.36) [0.036]	2938
Number of months of non-employment in the last year	3.38 (4.41)	0.26**† (0.13) [0.071]	2934

Notes: 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", in bold, declines by about 0.10 standard deviations. 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 6: 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)** **[0.377]**	
Active Search Component		0.03 (0.02) [0.735]	
Whether 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) [0.235]	2939
Number of different actions taken to search for a job	1.69 (1.72)	0.09* (0.05) [0.235]	2942
Whether the participant applied for a job	0.49 (0.39)	0.04***†† (0.01) [0.015]	2942
Number of job applications sent	5.45 (11.83)	-0.89**† (0.35) [0.079]	2942
Whether the participant interviewed for a job	0.36 (0.36)	0.01 (0.01) [0.777]	2942
Number of jobs interviewed for	0.73 (1.72)	-0.09* (0.05) [0.235]	2942
Preferences for Employment Component		0.01 (0.02) [0.735]	
How many work hours the respondent wants (less, same, more)	2.18 (0.52)	0.02 (0.02) [0.282]	2927
Whether a respondent is employed or, if unemployed, would prefer to be working	0.90 (0.26)	-0.01 (0.01) [0.463]	2942

Notes: 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 7: Impact of Guaranteed Income on Selectivity of Job Search

	Control Mean	Treatment Effect	N
Selectivity of Job Search Index		**-0.00** **(0.02)** **[0.752]**	
Perceived likelihood of finding an acceptable job in 6 months (1 - 4 scale)	3.38 (0.81)	-0.15*** (0.05) [0.146]	889
Participant's reservation wage, reported in minimum hourly remuneration	18.30 (8.73)	-0.17 (0.49) [1.000]	1068
Selectivity Component		-0.00 (0.02) [1.000]	
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)	0.08 (0.34) [1.000]	2476

Notes: 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). 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 8: Impact of Guaranteed Income on Quality of Employment: Summary of Top-Level Components

	Control Mean	Treatment Effect	N
Quality of Employment Index		**-0.01**	
		(0.01)	
		[0.449]	
Adequacy of Employment Component		0.01	
		(0.03)	
		[1.000]	
Employment Quality Component		-0.01	
		(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)	-0.18	2408
		(0.37)	
		[1.000]	
Stability of Employment Component		-0.02	
		(0.02)	
		[1.000]	
Quality of Work Life Component		-0.02	
		(0.02)	
		[1.000]	

Notes: 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 9: 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)	0.01 (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)	-0.12 (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 measure	4.53 (2.97)	-0.17 (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)	-0.03 (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)	1.70 (1.15) [1.000]	2403
How many jobs the respondent has held in the past 12 months	1.76 (1.60)	-0.12** (0.05) [1.000]	2390
How many jobs the respondent has held in the past two years	2.33 (3.67)	-0.17* (0.09) [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)	-0.01 (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)	0.00 (0.01) [1.000]	2402
How many months the respondent expects to remain in their main job	8.97 (6.56)	-1.30* (0.70) [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)	-0.03 (0.05) [1.000]	2361
The work activities are not boring at the respondent's main job (1-5 scale)	3.11 (1.05)	-0.01 (0.04) [1.000]	2249
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]	2249
Whether the respondent faces sex discrimination at work	0.08 (0.25)	0.00 (0.01) [1.000]	2248
Whether the respondent faces racial or ethnic discrimination at work	0.08 (0.25)	0.01 (0.01) [1.000]	2248
Whether the respondent experienced fair treatment by their supervisor (1-5 scale)	4.05 (0.91)	0.04 (0.04) [1.000]	2252
Whether job demands do not interfere with family life (1-4 scale)	2.91 (0.87)	0.02 (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]	2346
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)	-0.03 (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)	0.02 (0.03) [1.000]	2253
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]	2243
How easy is it to take time off from the respondent's main job? (1-4 scale)	3.18 (0.81)	-0.06* (0.03) [1.000]	2405

Notes: 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 10: Impact of Guaranteed Income on Entrepreneurship

	Control Mean	Treatment Effect	N
Entrepreneurship Index		**0.05*** ††** **(0.02)** **[0.012]**	
Entrepreneurial Orientation Component		0.07*** ††† (0.02) [0.009]	
The respondent's self-reported willingness to take financial risks (1-10 scale)	4.52 (2.09)	0.08† (0.06) [0.094]	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) [0.026]	2910
Entrepreneurial Intention Component		0.06** †† (0.02) [0.013]	
Whether or not the respondent has an idea for a business	0.58 (0.42)	0.03** †† (0.01) [0.026]	2909
The respondent's likelihood rating that they will start a business in the next 5 years (1-10 scale)	4.95 (3.05)	0.15* †† (0.08) [0.045]	2909
The respondent's interest in starting a business (1-10 scale)	6.21 (2.96)	0.12† (0.09) [0.094]	2910
Entrepreneurial Activity Component		0.01 (0.02) [0.162]	
If a family member who started a business lives in the respondent's household	0.06 (0.21)	-0.01** †† (0.01) [0.044]	2907
If the respondent knows someone who started or helped start a business	0.60 (0.41)	0.03*** †† (0.01) [0.026]	2907
If the respondent ever started or helped start a business	0.30 (0.40)	0.00 (0.01) [0.281]	2908

Notes: 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 11: Impact of Guaranteed Income on Disability

	Control Mean	Treatment Effect	N
Disability Index		**-0.09*****††	
		(0.03)	
		[0.011]	
Disability Component		-0.09***†††	
		(0.03)	
		[0.002]	
Whether the participant has a disability	0.31 (0.42)	0.04***†††	2874
		(0.01)	
		[0.003]	
Whether the respondent has a health problem or disability that limits the work they can do	0.28 (0.41)	0.04***†††	2872
		(0.01)	
		[0.003]	
How much the respondent's worst disability or health problem limits the of work they can do (1-7 scale)	1.11 (1.71)	0.15***†††	2872
		(0.05)	
		[0.003]	
How long the respondent's health problem or disability has affected the work they can do (more than 1 year continuously or intermitently, less than 1 year)	0.73 (1.06)	0.09***†††	2873
		(0.03)	
		[0.003]	

Notes: This table shows the impacts of an unconditional cash transfer on disability. The top-level index decreases significantly by about 0.09 standard deviations, representing an increase in disability. The q-values on the component and the top-level family index measures are different even as the point estimate is the same as they adjust for different sets of estimates in the FDR corrections (see Appendix 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 12: Impact of Guaranteed Income on Barriers to Employment

	Control Mean	Treatment Effect	N
Barriers to Employment Index		**-0.03**	
		(0.02)	
		[0.377]	
Barriers to Employment Component		-0.03	
		(0.02)	
		[0.425]	
Whether the respondent missed work due to lack of childcare in the last month	0.02 (0.13)	0.00	2941
		(0.01)	
		[1.000]	
Whether the respondent missed work due to illness in the last month	0.20 (0.34)	0.00	2940
		(0.01)	
		[1.000]	
Whether the respondent missed work due to lack of trasportation in the last month	0.03 (0.13)	0.00	2940
		(0.00)	
		[1.000]	

Notes: 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 13: Impact of Guaranteed Income on Human Capital

	Control Mean	Treatment Effect	N
Human Capital Index	0.22 (0.32)	**0.02** **(0.01)** **[0.219]**	
Formal Education Component		0.02 (0.02) [0.404]	
Completed a high school degree, GED or post-secondary program	0.92 (0.26)	0.01 (0.01) [0.607]	2986
Total years of post-secondary education completed post-baseline	0.13 (0.33)	0.01 (0.01) [0.611]	2623
Enrolled in post-secondary program	0.15 (0.29)	0.01 (0.01) [0.607]	2998
Average hours of school per week (full-time, part-time, withdrawn, etc.) in post-secondary program	3.80 (8.70)	0.29 (0.31) [0.936]	2615
Participation in informal education	0.10 (0.21)	0.01 (0.01) [0.936]	2987
Extent of participation in informal education (full-time, part-time, not enrolled)	0.07 (0.18)	-0.00 (0.01) [0.947]	2987
Whether the participant plans to receive job training	0.03 (0.14)	0.01** (0.01) [0.405]	2940

Notes: 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. 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 14: Impact of Guaranteed Income on Relationship Status

	Control Mean	All Surveys	N
Relationship Status Index		**-0.013**	
		(0.017)	
		[0.947]	
Relationship Stability Component		-0.037	
		(0.025)	
		[1.000]	
How long the respondent has been in their relationship	2.496 (2.483)	0.013	2900
		(0.064)	
		[0.971]	
Number of times the respondent said they started or ended a relationship in the last year	0.428 (0.723)	0.057**	2903
		(0.027)	
		[0.864]	
Relationship Status Component		0.011	
		(0.016)	
		[1.000]	
Whether the respondent is divorced	0.096 (0.289)	-0.008	2943
		(0.006)	
		[0.864]	
Whether the respondent has a spouse	0.284 (0.441)	-0.012	2945
		(0.009)	
		[0.864]	
Whether the respondent is in a romantic relationship	0.584 (0.435)	0.014	2989
		(0.012)	
		[0.864]	

Notes: 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 signficant 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 15: Impact of Guaranteed Income on Moving Labor Markets

	Control Mean	Treatment Effect	N
Move Labor Market Index		**0.09***†††**	
		(0.03)	
		[0.003]	
Single-item Component: Moved Labor Markets Since Baseline	0.12 (0.30)	0.02	2993
		(0.01)	
		[0.121]	
Search New Labor Market Component		0.11***†††	
		(0.03)	
		[0.003]	
Any active area-search behaviors	0.10 (0.22)	0.03***†††	2851
		(0.01)	
		[0.005]	
Interested in moving areas	0.23 (0.36)	0.04***†††	2851
		(0.01)	
		[0.005]	
Number of active labor market-search behaviors	0.27 (0.67)	0.08***†††	2851
		(0.03)	
		[0.005]	

Notes: 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 16: Impact of Guaranteed Income on Quality of Labor Market

	Control Mean	Treatment Effect	N
Labor Market Quality Index		**-0.01** **(0.01)** **[0.116]**	
Labor Quality Component		0.00 (0.01) [0.507]	
Mean wage by education in 2022 (in dollars per hour)	29.69 (12.38)	-0.02 (0.05) [0.907]	2988
Employment to population ratio for ages 25 to 64 for respondent's education group	0.76 (0.07)	0.00 (0.00) [0.733]	2995
BLS projected job-growth for respondent's education group (percent change)	11.78 (8.94)	-0.01 (0.13) [0.907]	2961
Median annual income for respondent's education group (in dollars)	41630.43 (13472.24)	136.88 (108.00) [0.733]	2995
Recent population growth for respondent's education group (percent change)	5.21 (11.36)	-0.11 (0.14) [0.822]	2995
Mean wage growth 2019-2022 by education (percent change)	12.21 (4.17)	-0.06 (0.05) [0.733]	2988
Labor Market Amenities Component		-0.02 (0.01) [0.507]	
Mean percentile household income rank for children whose parents were in the 25th percentile of income	0.40 (0.01)	0.00 (0.00) [0.907]	2993
Natural amenities index (ranges from -5 to 9)	-0.38 (1.51)	0.03 (0.03) [0.822]	2992
Pollution index (mean PM2.5, RSEI, and AQI z-score)	0.99 (0.33)	0.00 (0.01) [0.822]	2993
Consumption amenities index (PCA log scale, ranges from -2 to 2)	0.25 (0.27)	0.01 (0.01) [0.733]	2993
Annual violent crime rate (crimes per 100,000 residents)	393.31 (110.91)	-8.18* (4.60) [0.733]	2967
Annual property crime rate (crimes per 100,000 residents)	2329.32 (408.11)	-44.15** (17.54) [0.236]	2967
Annual per-pupil school spending (in dollars)	13695.98 (3433.77)	-10.87 (134.00) [0.907]	2993

Notes: This table shows the impacts of an unconditional cash transfer on quality of labor market. The top-level index decreases insignificantly by 0.01 standard deviations. Two components (Labor Quality and Labor Market Amenities) are both null. All the primary items under them are also null, with the exception that the labor markets participants move to perhaps have less crime, 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 17: Impact of Guaranteed Income on Benefits

	Control Mean	Treatment Effect	N
Benefits Index		**-0.02** **(0.02)** **[0.242]**	
Take-Up Benefits Component		-0.02 (0.02) [0.639]	
Total amount of government benefits received during the previous year	5623.44 (6918.62)	-299.13 (189.00) [0.517]	2911
Number of government benefits programs received during the previous year	1.85 (1.56)	0.00 (0.04) [1.000]	2912
Number of government benefits programs received during the previous year (excluding educational assistance)	1.75 (1.50)	-0.01 (0.06) [1.000]	2911

Notes: This table shows the impacts of an unconditional cash transfer on benefits. The top-level index decreases insignificantly by about 0.02 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 18: Comparison of Administrative and Survey Data

		Illinois			Texas			Aggregate		
		Midline	Endline	Pooled	Midline	Endline	Pooled	Midline	Endline	Pooled
Income	(a) Survey, did not consent§	-3.83	2.03	-1.79	-0.69	-4.70	-7.21	-1.09	-2.40	-1.67
(Annual		(4.04)	(5.40)	(4.40)	(4.42)	(6.65)	(5.33)	(2.72)	(3.99)	(3.00)
salary/wage		176	164	184	153	153	167	329	317	351
income in	(b) Survey, consented§	0.66	-0.59	-0.29	-1.29	-0.97	-1.37	-0.27	-1.18	-0.94
thousands		(1.23)	(1.42)	(1.20)	(1.16)	(1.36)	(1.14)	(0.84)	(0.96)	(0.82)
of dollars)		1235	1226	1270	1260	1240	1299	2495	2466	2569
	(c) UI, matched	0.49	-1.91	-0.73	-2.06	-3.87**	-3.41**	-0.41	-2.63**	-1.64*
		(1.08)	(1.34)	(1.16)	(1.47)	(1.76)	(1.62)	(0.87)	(1.07)	(0.94)
		932	932	932	975	975	975	1907	1907	1907
	(d) UI, matched + survey,	0.40	-1.36	-0.72	N/A	N/A	N/A	N/A	N/A	N/A
	consented and unmatched	(1.01)	(1.21)	(1.05)	(.)	(.)	(.)	(.)	(.)	(.)
		1262	1258	1288	-	-	-	-	-	-
	Pooling, (a) + (d) (IL) or (a) +	0.15	-1.20	-0.78	-1.93	-3.93**	-3.73**	-0.54	-2.09**	-1.67**
	(c) (TX)	(0.98)	(1.18)	(1.02)	(1.39)	(1.70)	(1.55)	(0.80)	(0.97)	(0.85)
		1438	1422	1472	1128	1128	1142	2566	2550	2614
Employment	(a) Survey, did not consent§	0.08	0.08	0.03	-0.03	-0.04	-0.03	0.01	0.04	0.01
(in		(0.07)	(0.06)	(0.05)	(0.07)	(0.07)	(0.07)	(0.04)	(0.04)	(0.04)
percentage		191	187	196	168	166	172	359	353	368
points)	(b) Survey, consented§	0.01	-0.01	-0.01	-0.02	-0.05**	-0.03*	-0.01	-0.03*	-0.02*†
		(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	-0.04	-0.03	-0.05	-0.08**	-0.07**†	-0.04**	-0.06***	-0.04**†
		(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
		932	932	932	975	975	975	1907	1907	1907
	(d) UI, matched + survey,	-0.02	-0.04**	-0.04**†	N/A	N/A	N/A	N/A	N/A	N/A
	consented and unmatched	(0.02)	(0.02)	(0.02)	(.)	(.)	(.)	(.)	(.)	(.)
		1287	1282	1295	-	-	-	-	-	-
	Pooling, (a) + (d) (IL) or (a) +	-0.02	-0.03	-0.03*†	-0.05	-0.07***	-0.06**†	-0.03	-0.04***	-0.04***†
	(c) (TX)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.01)
		1478	1469	1491	1143	1141	1147	2621	2610	2638

Notes: 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 few individuals who did not consent to share administrative data; (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 (SSN in Illinois and SSN, name and age in Texas); (d) shows the effects as estimated in the Unemployment Insurance data for those who consented to share these data and could be matched pooled with the SRC data for those who consented but could not be matched (per the main text, this could not be run in Texas); (e) 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 can be run on the full sample directly. All estimates are considered subgroup analyses for the FDR corrections except for those results that are both pooled across data sources and aggregated across states. 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 19: Estimates of the Marginal Propensity to Earn (MPE) by Net Change in Assets

		MPE		
		Midline	Endline	Pooled
Observed	Net change in assets			
	-2000	-0.07	-0.35	-0.28
	0	-0.07	-0.37	-0.29
	5000	-0.08	-0.43	-0.34
Model		-0.24	-0.24	-0.24

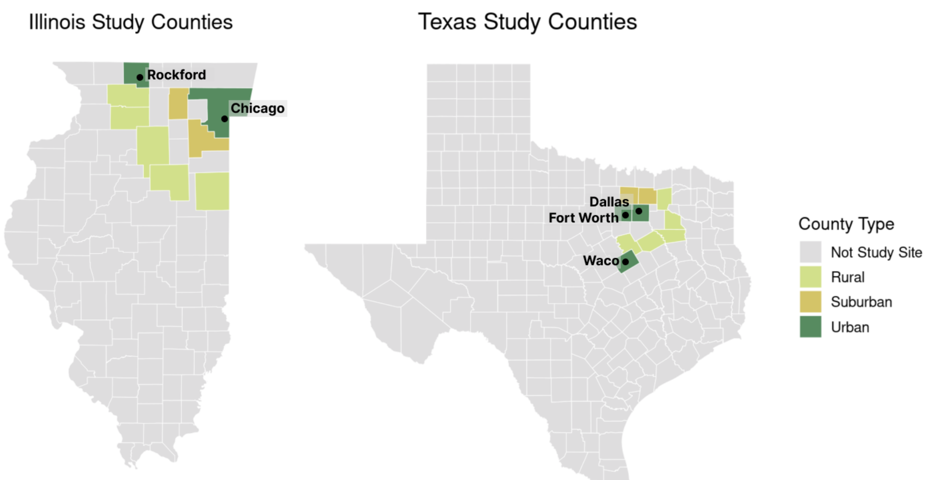
Notes: This table shows how our estimates of reductions in total household income translate into different MPEs depending on the net change in assets. The values of the net change in assets displayed in this table were selected to represent reasonable values for our data, with a change in assets of $5000 representing an upper bound. The net asset accumulation is assumed for simplicity to be constant over the time period as small changes will not affect the estimates much. The pooled estimate places 30% weight on the midline results and 70% weight on the endline results, for comparison to the main estimates reported in the paper. Total household income is adjusted as described in the text by comparing the estimates of the aggregate total individual income reported by participants with the calculated total individual income reported by participants when asked about various specific types of income, after adjusting this to take into consideration the larger estimate for salaried or wage income from the administrative data. The model estimates are provided for comparison. As the model does not put more emphasis on later time periods, the pooled estimate is lower. The model-based estimates are also substantially smoothed.

Table 20: 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	-0.7	0.3	94	-2.8
Work hours per week	-0.9	-0.6	42	-1.2	-1.3	16	-1.4	-1.2	94	-1.6
Average hourly wage	1.0	1.2	42	0.5	0.9	16	0.7	0.8	94	-0.2
Duration of unemployment, in weeks	3.7	3.9	41	3.1	3.5	16	2.4	2.6	94	4.7
Participant is searching for work, in percentage points	-2.8	-2.5	42	-4.8	-2.9	16	-	-	-	7.2
Enrollment in a post-secondary program	2.9	3.2	41	2.5	2.6	16	3.5	4.4	94	0.0
Individual salaried income, in thousands of dollars	-0.7	-0.3	21	-	-	-	0	1.1	95	-1.6
Home production hours per week	-	-	-	-	-	-	0.8	1.8	93	0.8
Sleep, hours per week	-	-	-	-	-	-	0.7	0.6	93	-0.7
Social leisure, hours per week	-	-	-	-	-	-	4.7	5.0	92	0.9
Solitary leisure, hours per week	-	-	-	-	-	-	2.9	3.5	92	0.5

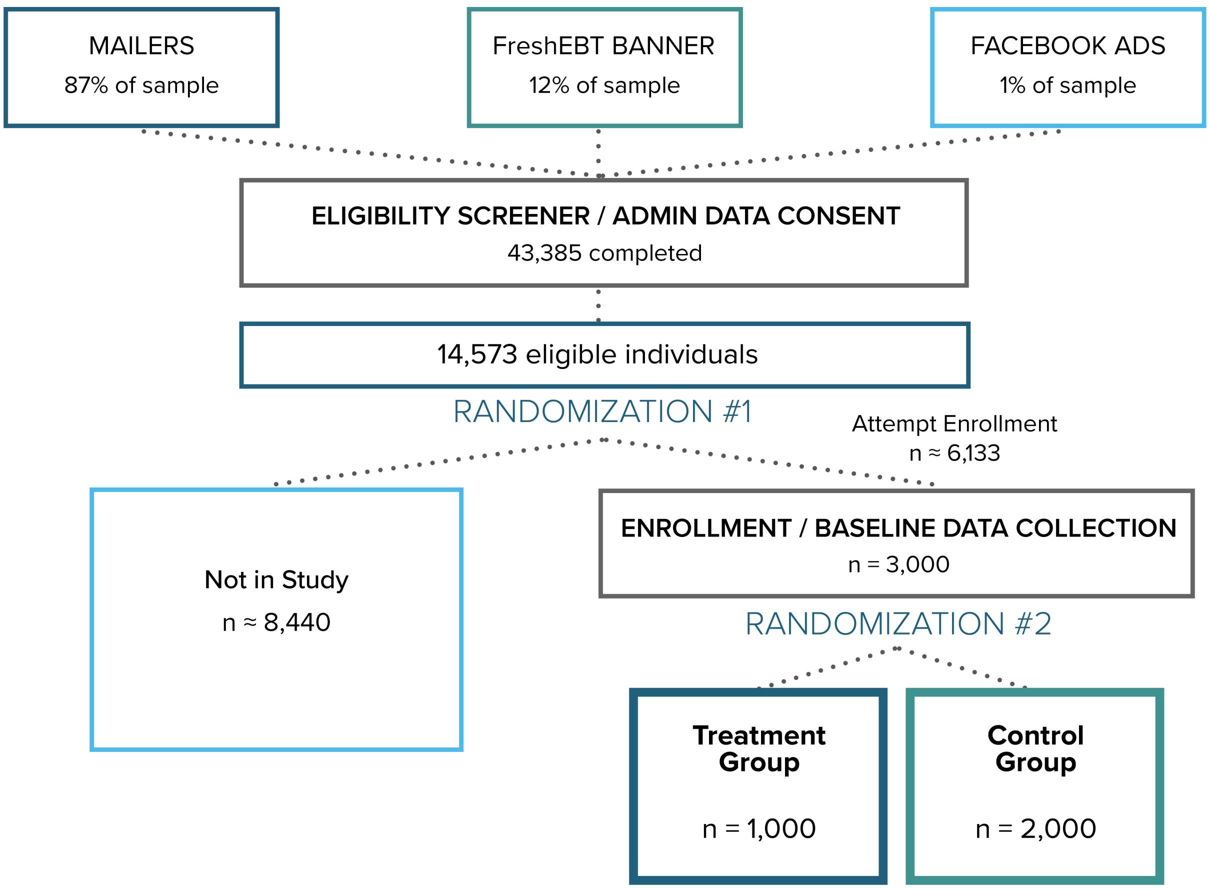
Notes: This table shows forecasts of NBER affiliates and users of the Social Science Prediction Platform (SSPP). As described in the text, forecasts were elicited from NBER affiliates in several related Programs, and these forecasts were supplemented by forecasts from the SSPP, including from members 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 comes from the combined NSC and survey data. All other results come from the survey data.

Figure 1: Location of Study



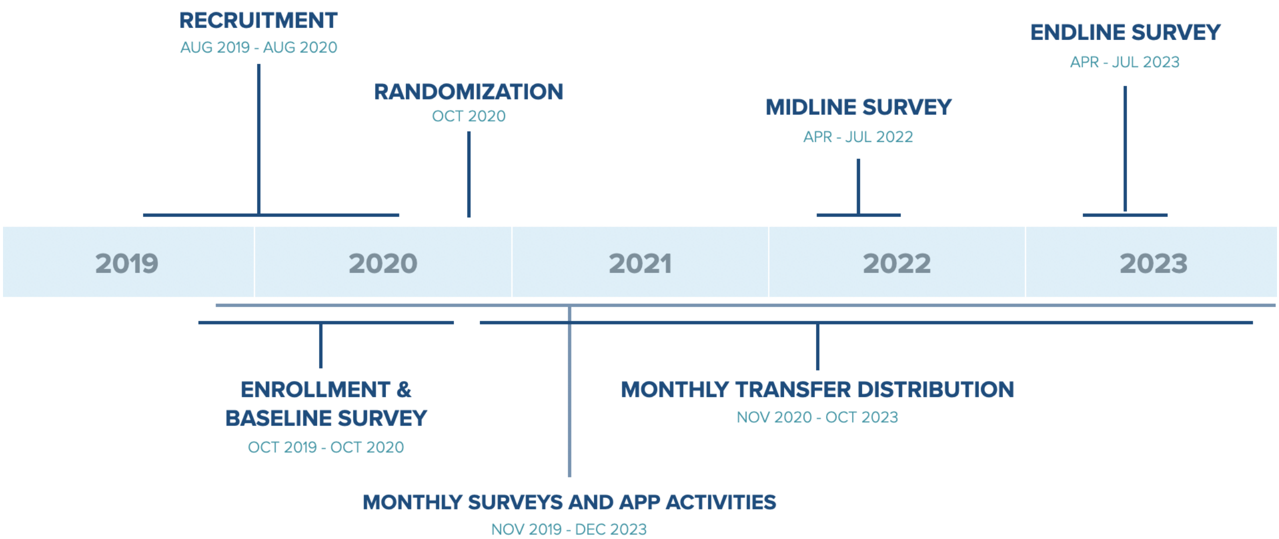
Notes: This figure plots the location of the sites in the study. Reproduced from Bartik et al. (2024).

Figure 2: Flowchart of Recruitment Process



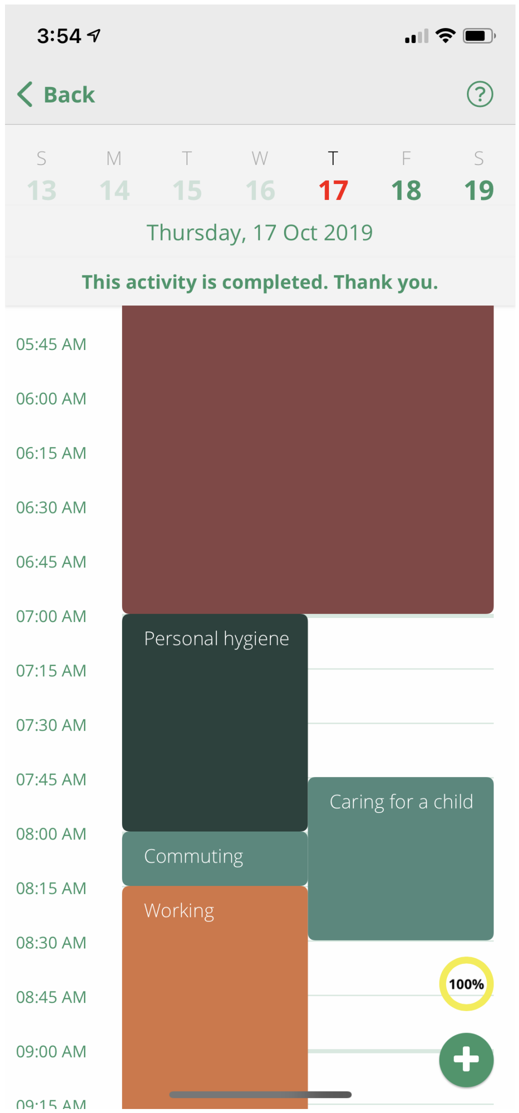
Notes: This figure shows a representation of the recruitment process.

Figure 3: Timeline of Study



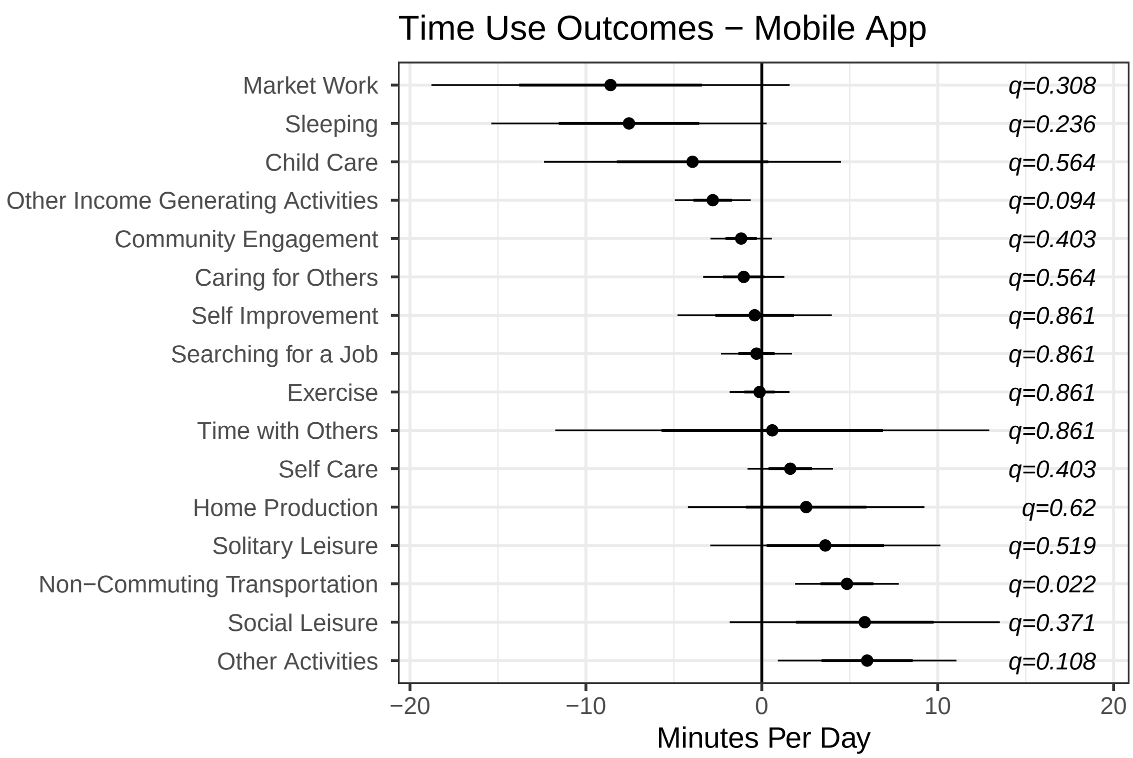
Notes: This figure shows a timeline of the program and study.

Figure 4: Time Use Mobile App



Notes: 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



Notes: This figure shows the main results from the time diaries.

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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 A11, and Figure A12 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.

Table A7 summarizes the FDR tiers of our estimates.

E 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 small changes were made prior to receiving midline survey data. At this stage, the following 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;

- 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 (i.e., if we are missing a survey round for an individual, we replace that measure for that individual at that time period with the treatment-arm-specific mean following Kling, Liebman and Katz (2007));
  - 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 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;

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;
- 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;

– For duration of unemployment, 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.

F Relationship with Other Papers

It should be noted that the analyses in this paper come in part from three different pre-analysis plans that focus, alternatively, on employment; income and financial health;, and housing and geographic mobility. While we did not know at the time of registering the pre-analysis plans which outcome variables would be included in which papers, we pre-specified that we would conduct our multiple hypothesis corrections according to how the tests were originally registered. For example, if one family of outcomes from the "income, expenditures and financial health" pre-analysis plan was included in the paper based primarily off results from the "employment" pre-analysis plan, that family of outcomes would be subject to 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), and Miller et al. (2024) for information on household finance, political, and health outcomes.

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 A6 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 A7 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[4] that they engaged in certain activities. Figure A3 shows the estimates on these outcomes.

[4] 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.

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

Notes: 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
Male	0.313	0.321	0.683	0.565	0.800	0.071
Female	0.684	0.676	0.685	0.435	0.200	0.071
Non-binary/other	0.003	0.003	0.990	0.000	0.000	0.661
Non-Hispanic Black	0.307	0.295	0.516	0.239	0.267	0.835
Non-Hispanic Asian	0.038	0.037	0.810	0.022	0.000	0.321
Non-Hispanic White	0.462	0.472	0.620	0.478	0.533	0.715
Non-Hispanic Native American	0.025	0.019	0.308	0.000	0.067	0.310
Hispanic	0.212	0.221	0.589	0.304	0.200	0.407
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 (1000*s*)	29.949	29.959	0.990	28.571	32.146	0.612
Total Individual Income (1000*s*)	21.235	21.319	0.916	20.459	23.716	0.490
Work Hours/Week	21.812	21.025	0.344	20.413	33.173	0.039
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 Romantic Partner	0.432	0.440	0.694	0.370	0.533	0.275
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

Notes: 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
Male	0.315	0.319	0.841	0.419	0.600	0.086
Female	0.682	0.678	0.843	0.581	0.400	0.086
Non-binary/other	0.003	0.003	0.984	0.000	0.000	0.013
Non-Hispanic Black	0.304	0.295	0.623	0.337	0.300	0.706
Non-Hispanic Asian	0.038	0.037	0.950	0.035	0.000	0.082
Non-Hispanic White	0.464	0.471	0.753	0.430	0.533	0.334
Non-Hispanic Native American	0.026	0.019	0.214	0.000	0.067	0.148
Hispanic	0.214	0.223	0.553	0.233	0.133	0.203
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 (1000*s*)	30.013	30.034	0.979	28.378	29.111	0.871
Total Individual Income (1000*s*)	21.264	21.203	0.939	20.281	26.052	0.182
Work Hours/Week	21.805	20.735	0.199	21.372	35.800	0.004
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 Romantic Partner	0.436	0.445	0.670	0.302	0.333	0.757
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

Notes: 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
Male	0.306	0.324	0.351	0.496	0.433	0.531
Female	0.691	0.673	0.341	0.496	0.567	0.485
Non-binary/other	0.003	0.003	0.848	0.007	0.000	0.319
Non-Hispanic Black	0.307	0.295	0.504	0.277	0.333	0.555
Non-Hispanic Asian	0.037	0.037	0.978	0.044	0.000	0.014
Non-Hispanic White	0.466	0.470	0.823	0.423	0.500	0.449
Non-Hispanic Native American	0.025	0.018	0.202	0.022	0.100	0.168
Hispanic	0.209	0.222	0.424	0.285	0.167	0.135
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)	29.881	29.943	0.937	30.806	34.815	0.289
Total Individual Income (1000s)	21.273	21.237	0.965	20.751	26.775	0.109
Work Hours/Week	21.811	20.833	0.245	21.591	33.767	0.008
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 Romantic Partner	0.435	0.445	0.595	0.380	0.400	0.837
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

Notes: 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
Male	0.317	0.325	0.678	0.387	0.450	0.615
Female	0.680	0.672	0.681	0.613	0.550	0.615
Non-binary/other	0.003	0.003	0.978	0.000	0.000	.
Non-Hispanic Black	0.307	0.297	0.583	0.267	0.200	0.522
Non-Hispanic Asian	0.037	0.035	0.765	0.053	0.100	0.521
Non-Hispanic White	0.463	0.471	0.689	0.467	0.550	0.511
Non-Hispanic Native American	0.026	0.016	0.093	0.000	0.200	0.029
Hispanic	0.213	0.221	0.621	0.240	0.200	0.698
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 (1000*s*)	29.963	30.170	0.786	29.538	21.969	0.059
Total Individual Income (1000*s*)	21.227	21.481	0.753	21.097	14.862	0.132
Work Hours/Week	21.832	21.287	0.515	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 Romantic Partner	0.433	0.444	0.548	0.387	0.300	0.463
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

Notes: 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
Male	0.313	0.326	0.478	0.447	0.350	0.415
Female	0.685	0.671	0.465	0.544	0.650	0.370
Non-binary/other	0.003	0.003	0.838	0.010	0.000	0.320
Non-Hispanic Black	0.308	0.297	0.515	0.243	0.250	0.945
Non-Hispanic Asian	0.038	0.037	0.892	0.029	0.000	0.083
Non-Hispanic White	0.466	0.470	0.836	0.417	0.550	0.280
Non-Hispanic Native American	0.025	0.019	0.261	0.019	0.100	0.244
Hispanic	0.208	0.222	0.393	0.320	0.150	0.068
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 (1000*s*)	29.951	30.171	0.774	29.865	25.930	0.337
Total Individual Income (1000*s*)	21.248	21.450	0.802	20.769	18.539	0.654
Work Hours/Week	21.750	21.286	0.580	22.400	18.050	0.468
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 Romantic Partner	0.432	0.443	0.579	0.408	0.500	0.453
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

Notes: 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 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 (self-reported)	48.2 (33.9)	-4.1***†††	-2.8*	-4.8***††
		(1.0)	(1.7)	(1.2)
		[0.001]	[0.448]	[0.011]
Total individual income	36.6 (27.0)	-1.5*	0.2	-2.6**
		(0.9)	(1.6)	(1.0)
		[0.185]	[1.000]	[0.164]
Total individual income (self-reported)	33.5 (25.1)	-2.5**	-3.4**	-2.3*
		(1.0)	(1.4)	(1.2)
		[0.105]	[0.164]	[0.375]
Individual salaried/wage income	26.0 (26.2)	-1.1	-0.2	-1.7
		(0.8)	(1.3)	(1.0)
		[0.258]	[1.000]	[0.454]
Self-employment income	5.9 (13.7)	-0.1	0.6	-0.8
		(0.5)	(0.8)	(0.7)
		[0.423]	[0.897]	[0.641]
Income from gig work	0.4 (1.3)	-0.1	-0.0	-0.1
		(0.0)	(0.1)	(0.1)
		[0.263]	[1.000]	[0.582]
Passive income	0.0 (0.2)	0.0	0.0	0.0
		(0.0)	(0.0)	(0.0)
		[0.258]	[0.910]	[0.568]
Other income	4.7 (6.1)	-0.1	-0.1	-0.1
		(0.2)	(0.3)	(0.2)
		[0.377]	[1.000]	[1.000]
Government transfers	3.6 (4.9)	-0.2	-0.0	-0.2
		(0.1)	(0.3)	(0.2)
		[0.356]	[1.000]	[0.731]

Notes: 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 for greater comparability across tables with subgroup analyses, so the individual salaried/wage income is promoted to a primary item for the sake of FDR corrections. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A9: 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 (self-reported)	48.2 (33.9)	-4.1***†††	-4.3***††	-1.2
		(1.0)	(1.1)	(3.4)
		[0.001]	[0.011]	[1.000]
Total individual income	36.6 (27.0)	-1.5*	-1.5	-1.4
		(0.9)	(1.0)	(2.4)
		[0.185]	[0.524]	[0.934]
Total individual income (self-reported)	33.5 (25.1)	-2.5**	-2.8***	-2.1
		(1.0)	(1.0)	(2.6)
		[0.105]	[0.145]	[0.829]
Individual salaried/wage income	26.0 (26.2)	-1.1	-1.5*	1.2
		(0.8)	(0.8)	(2.4)
		[0.258]	[0.400]	[1.000]
Self-employment income	5.9 (13.7)	-0.1	0.4	-3.4***†
		(0.5)	(0.6)	(1.2)
		[0.423]	[0.897]	[0.098]
Income from gig work	0.4 (1.3)	-0.1	0.0	-0.4***††
		(0.0)	(0.1)	(0.1)
		[0.263]	[1.000]	[0.016]
Passive income	0.0 (0.2)	0.0	0.0	0.0
		(0.0)	(0.0)	(0.0)
		[0.258]	[0.897]	[1.000]
Other income	4.7 (6.1)	-0.1	-0.2	0.1
		(0.2)	(0.2)	(0.5)
		[0.377]	[0.807]	[1.000]
Government transfers	3.6 (4.9)	-0.2	-0.3	0.1
		(0.1)	(0.2)	(0.3)
		[0.356]	[0.525]	[1.000]

Notes: This table compares results for income for participants by whether or not they had a bachelor's degree at baseline. Survey data are used for greater comparability across tables with subgroup analyses, so the individual salaried/wage income is promoted to a primary item for the sake of FDR corrections. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A10: Impact of Guaranteed Income on Annual Earned and Other Unearned Income: Comparison of Impacts by Sex at Baseline

	Control Mean	Entire Sample	Female/Other	Male
Total household income (self-reported)	48.2 (33.9)	-4.1***†††	-3.6***†	-4.9**
		(1.0)	(1.2)	(1.9)
		[0.001]	[0.066]	[0.164]
Total individual income	36.6 (27.0)	-1.5*	-1.2	-1.8
		(0.9)	(1.0)	(1.8)
		[0.185]	[0.629]	[0.752]
Total individual income (self-reported)	33.5 (25.1)	-2.5**	-1.9*	-3.9**
		(1.0)	(1.1)	(1.9)
		[0.105]	[0.425]	[0.326]
Individual salaried/wage income	26.0 (26.2)	-1.1	-1.2	-1.2
		(0.8)	(0.9)	(1.6)
		[0.258]	[0.561]	[0.861]
Self-employment income	5.9 (13.7)	-0.1	0.3	-0.7
		(0.5)	(0.6)	(1.1)
		[0.423]	[1.000]	[0.897]
Income from gig work	0.4 (1.3)	-0.1	-0.1	-0.0
		(0.0)	(0.0)	(0.1)
		[0.263]	[0.554]	[1.000]
Passive income	0.0 (0.2)	0.0	0.0**	-0.0
		(0.0)	(0.0)	(0.0)
		[0.258]	[0.166]	[1.000]
Other income	4.7 (6.1)	-0.1	0.0	-0.3
		(0.2)	(0.2)	(0.3)
		[0.377]	[1.000]	[0.626]
Government transfers	3.6 (4.9)	-0.2	-0.1	-0.2
		(0.1)	(0.2)	(0.2)
		[0.356]	[0.934]	[0.728]

Notes: This table compares results for income for participants by sex at baseline. Survey data are used for greater comparability across tables with subgroup analyses, so the individual salaried/wage income is promoted to a primary item for the sake of FDR corrections. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A11: 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 (self-reported)	48.2 (33.9)	-4.1***††† (1.0) [0.001]	-3.0** (1.4) [0.125]	-5.7***††† (1.4) [0.002]
Total individual income	36.6 (27.0)	-1.5* (0.9) [0.185]	-0.4 (1.3) [0.899]	-1.7 (1.1) [0.383]
Total individual income (self-reported)	33.5 (25.1)	-2.5**† (1.0) [0.063]	-1.6 (1.4) [0.519]	-3.4** (1.4) [0.100]
Individual salaried/wage income	26.0 (26.2)	-1.1 (0.8) [0.258]	-0.4 (1.1) [0.877]	-1.5 (1.1) [0.427]
Self-employment income	5.9 (13.7)	-0.1 (0.5) [0.423]	0.1 (0.8) [0.991]	-0.4 (0.7) [0.733]
Income from gig work	0.4 (1.3)	-0.1 (0.0) [0.263]	-0.2***† (0.1) [0.080]	0.0 (0.1) [0.717]
Passive income	0.0 (0.2)	0.0 (0.0) [0.258]	-0.0 (0.0) [0.645]	0.0***†† (0.0) [0.044]
Other income	4.7 (6.1)	-0.1 (0.2) [0.377]	0.1 (0.3) [0.980]	-0.2 (0.3) [0.696]
Government transfers	3.6 (4.9)	-0.2 (0.1) [0.297]	-0.1 (0.2) [0.733]	-0.2 (0.2) [0.629]

Notes: This table compares results for income for participants by whether they lived in Illinois or Texas at baseline. Survey data are used for greater comparability across tables with subgroup analyses, so the individual salaried/wage income is promoted to a primary item for the sake of FDR corrections. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A12: 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	Has Children
Total household income (self-reported)	48.2 (33.9)	-4.1***†††	-5.9***††	-2.6**
		(1.0)	(1.6)	(1.3)
		[0.001]	[0.016]	[0.333]
Total individual income	36.6 (27.0)	-1.5*	-2.4*	-1.2
		(0.9)	(1.3)	(1.1)
		[0.185]	[0.406]	[0.670]
Total individual income (self-reported)	33.5 (25.1)	-2.5**	-3.1**	-2.0
		(1.0)	(1.4)	(1.3)
		[0.105]	[0.279]	[0.495]
Individual salaried/wage income	26.0 (26.2)	-1.1	-2.0	-0.5
		(0.8)	(1.3)	(1.0)
		[0.258]	[0.464]	[1.000]
Self-employment income	5.9 (13.7)	-0.1	-0.8	0.4
		(0.5)	(0.8)	(0.7)
		[0.423]	[0.777]	[0.939]
Income from gig work	0.4 (1.3)	-0.1	-0.0	-0.1
		(0.0)	(0.1)	(0.1)
		[0.263]	[1.000]	[0.464]
Passive income	0.0 (0.2)	0.0	0.0*	-0.0
		(0.0)	(0.0)	(0.0)
		[0.258]	[0.368]	[1.000]
Other income	4.7 (6.1)	-0.1	-0.0	0.0
		(0.2)	(0.3)	(0.3)
		[0.377]	[1.000]	[1.000]
Government transfers	3.6 (4.9)	-0.2	-0.1	-0.1
		(0.1)	(0.2)	(0.2)
		[0.356]	[0.833]	[1.000]

Notes: This table compares results for income for participants by whether or not they had children at baseline. Survey data are used for greater comparability across tables with subgroup analyses, so the individual salaried/wage income is promoted to a primary item for the sake of FDR corrections. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A13: 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
Labor Supply Elasticity Index		**-0.06**†**	**-0.08****	**0.06**
		(0.03)	**(0.03)**	**(0.06)**
		[0.076]	**[0.493]**	**[1.000]**
Labor Supply Elasticity Component		-0.06**††	-0.08**	0.06
		(0.03)	(0.03)	(0.06)
		[0.043]	[0.493]	[1.000]
Whether the respondent is employed	0.74 (0.39)	-0.02*†	-0.04**	0.02
		(0.01)	(0.02)	(0.02)
		[0.072]	[0.432]	[1.000]
Hours worked per week	30.28 (19.83)	-1.28**†	-1.35*	0.78
		(0.64)	(0.79)	(1.54)
		[0.072]	[0.814]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.02	-0.02	-0.02
		(0.02)	(0.03)	(0.05)
		[1.000]	[1.000]	[1.000]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.16***	-2.36**	-0.53
		(0.78)	(0.95)	(2.24)
		[0.316]	[0.388]	[1.000]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.21**	-2.49**	-0.69
		(0.92)	(1.12)	(2.45)
		[0.429]	[0.526]	[1.000]
Total number of hours participant's parents in household work per week	3.22 (12.07)	-0.13	0.02	0.69
		(0.35)	(0.41)	(0.99)
		[1.000]	[1.000]	[1.000]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.30	0.07	0.11
		(0.29)	(0.34)	(0.26)
		[1.000]	[1.000]	[1.000]

Notes: 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 for greater comparability across tables with subgroup analyses, so whether an individual is employed is promoted to a primary item for the sake of FDR corrections. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A14: Impact of Guaranteed Income on Labor Supply: Comparison of Impacts by Age at Baseline

	Control Mean	Entire Sample	Under 30	30+
Labor Supply Elasticity Index		**-0.06**†**	**-0.10****	**-0.01**
		(0.03)	**(0.04)**	**(0.04)**
		[0.076]	**[0.379]**	**[1.000]**
Labor Supply Elasticity Component		-0.06**††	-0.10**	-0.01
		(0.03)	(0.04)	(0.04)
		[0.043]	[0.379]	[1.000]
Whether the respondent is employed	0.74 (0.39)	-0.02*†	-0.04**	0.00
		(0.01)	(0.02)	(0.02)
		[0.072]	[0.450]	[1.000]
Hours worked per week	30.28 (19.83)	-1.28**†	-1.84**	-0.59
		(0.64)	(0.88)	(0.95)
		[0.072]	[0.613]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.02	-0.04	0.01
		(0.02)	(0.03)	(0.03)
		[1.000]	[1.000]	[1.000]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.16***	-2.91***	-1.65
		(0.78)	(1.08)	(1.19)
		[0.316]	[0.322]	[0.970]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.21**	-3.50***	-0.49
		(0.92)	(1.30)	(1.35)
		[0.429]	[0.322]	[1.000]
Total number of hours participant's parents in household work per week	3.22 (12.07)	-0.13	-0.27	-0.18
		(0.35)	(0.56)	(0.33)
		[1.000]	[1.000]	[1.000]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.30	0.19	0.38
		(0.29)	(0.12)	(0.61)
		[1.000]	[0.940]	[1.000]

Notes: This table compares results for labor supply for participants by age at baseline. Survey data are used for greater comparability across tables with subgroup analyses, so whether an individual is employed is promoted to a primary item for the sake of FDR corrections. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A15: Impact of Guaranteed Income on Labor Supply: Comparison of Impacts by Sex at Baseline

	Control Mean	Entire Sample	Female/Other	Male
Labor Supply Elasticity Index		**-0.06**†**	**-0.06***	**-0.05**
		(0.03)	**(0.03)**	**(0.05)**
		[0.076]	**[0.763]**	**[1.000]**
Labor Supply Elasticity Component		-0.06**††	-0.06*	-0.05
		(0.03)	(0.03)	(0.05)
		[0.043]	[0.763]	[1.000]
Whether the respondent is employed	0.74 (0.39)	-0.02*†	-0.02	-0.01
		(0.01)	(0.02)	(0.02)
		[0.072]	[0.961]	[1.000]
Hours worked per week	30.28 (19.83)	-1.28**†	-1.37*	-1.16
		(0.64)	(0.78)	(1.20)
		[0.072]	[0.763]	[1.000]
Number of other household members which are employed	0.47 (0.61)	-0.02	0.00	-0.06
		(0.02)	(0.03)	(0.04)
		[1.000]	[1.000]	[0.940]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.16***	-2.38**	-2.10
		(0.78)	(0.98)	(1.39)
		[0.316]	[0.432]	[0.940]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.21**	-1.85	-4.26***
		(0.92)	(1.16)	(1.62)
		[0.429]	[0.896]	[0.322]
Total number of hours participant's parents in household work per week	3.22 (12.07)	-0.13	-0.03	-0.17
		(0.35)	(0.39)	(0.67)
		[1.000]	[1.000]	[1.000]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.30	0.64	-0.37*
		(0.29)	(0.42)	(0.21)
		[1.000]	[0.940]	[0.763]

Notes: This table compares results for labor supply for participants by sex at baseline. Survey data are used for greater comparability across tables with subgroup analyses, so whether an individual is employed is promoted to a primary item for the sake of FDR corrections. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A16: Impact of Guaranteed Income on Labor Supply: Comparison of Impacts by State

	Control Mean	Entire Sample	Illinois	Texas
Labor Supply Elasticity Index		**-0.06**††**	**-0.03**	**-0.08***
		(0.03)	**(0.04)**	**(0.04)**
		[0.041]	**[1.000]**	**[0.900]**
Labor Supply Elasticity Component		-0.06**††	-0.03	-0.08*
		(0.03)	(0.04)	(0.04)
		[0.043]	[1.000]	[0.900]
Whether the respondent is employed	0.74 (0.39)	-0.02*†	-0.01	-0.03*
		(0.01)	(0.02)	(0.02)
		[0.072]	[1.000]	[0.900]
Hours worked per week	30.28 (19.83)	-1.28**†	-1.06	-1.55*
		(0.64)	(0.90)	(0.93)
		[0.072]	[1.000]	[0.900]
Number of other household members which are employed	0.47 (0.61)	-0.02	-0.01	-0.03
		(0.02)	(0.04)	(0.03)
		[1.000]	[1.000]	[1.000]
Total number of hours participant and spouse/partner works per week	40.69 (24.84)	-2.16***	-2.16**	-2.12*
		(0.78)	(1.08)	(1.18)
		[0.703]	[0.900]	[0.900]
Total number of hours all household members (including the participant) work per week	48.22 (29.64)	-2.21**	-1.89	-3.36**
		(0.92)	(1.34)	(1.34)
		[0.703]	[0.952]	[0.900]
Total number of hours participant's parents in household work per week	3.22 (12.07)	-0.13	-0.22	-0.07
		(0.35)	(0.49)	(0.49)
		[1.000]	[1.000]	[1.000]
Total number of hours participant's adult children in household work per week	1.23 (6.75)	0.30	0.60	0.00
		(0.29)	(0.41)	(0.42)
		[1.000]	[0.952]	[1.000]

Notes: This table compares results for labor supply for participants by whether they lived in Illinois or Texas at baseline. Survey data are used for greater comparability across tables with subgroup analyses, so whether an individual is employed is promoted to a primary item for the sake of FDR corrections. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; † refers to comparable q-value thresholds.

Table A17: 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)**
		[0.219]	**[1.000]**	**[1.000]**
Formal Education Component		0.02	0.06**	-0.04
		(0.02)	(0.03)	(0.03)
		[0.404]	[1.000]	[1.000]
Completed a high school degree, GED or post-secondary program	0.92 (0.26)	0.01	0.01	0.01
		(0.01)	(0.01)	(0.01)
		[0.607]	[1.000]	[1.000]
Total years of post-secondary education completed post-baseline	0.13 (0.33)	0.01	0.03	-0.02**
		(0.01)	(0.02)	(0.01)
		[0.611]	[1.000]	[1.000]
Enrolled in post-secondary program	0.15 (0.29)	0.01	0.02*	-0.01
		(0.01)	(0.01)	(0.01)
		[0.607]	[1.000]	[1.000]
Average hours of school per week (full-time, part-time, withdrawn, etc.) in post-secondary program	3.80 (8.70)	0.29	1.08**	-0.60*
		(0.31)	(0.49)	(0.33)
		[0.936]	[1.000]	[1.000]
Participation in informal education	0.10 (0.21)	0.01	0.01	0.01
		(0.01)	(0.01)	(0.01)
		[0.936]	[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)
		[0.947]	[1.000]	[1.000]
Whether the participant plans to receive job training	0.03 (0.14)	0.01**	0.01*	0.02*
		(0.01)	(0.01)	(0.01)
		[0.405]	[1.000]	[1.000]

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

Table A18: 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*****†††	**0.04****†	**0.05**
		(0.02)	**(0.02)**	**(0.03)**
		[0.010]	**[0.091]**	**[0.227]**
Entrepreneurial Orientation Component		0.07***†††	0.07**†	0.09
		(0.02)	(0.03)	(0.06)
		[0.008]	[0.076]	[0.201]
The respondent's self-reported willingness to take financial risks (1-10 scale)	4.52 (2.09)	0.08†	0.05	0.22
		(0.06)	(0.08)	(0.14)
		[0.092]	[0.436]	[0.201]
Midpoint of the constant relative risk aversion (CRRA) range implied by a participant's coin flip gamble	1.82 (1.55)	-0.16***††	-0.19***†	-0.10
		(0.06)	(0.07)	(0.14)
		[0.025]	[0.060]	[0.435]
Entrepreneurial Intention Component		0.06**††	0.06**†	-0.02
		(0.02)	(0.03)	(0.05)
		[0.016]	[0.094]	[0.516]
Whether or not the respondent has an idea for a business	0.58 (0.42)	0.03**††	0.03*	0.03
		(0.01)	(0.02)	(0.04)
		[0.027]	[0.130]	[0.406]
The respondent's likelihood rating that they will start a business in the next 5 years (1-10 scale)	4.95 (3.05)	0.15*††	0.18	-0.16
		(0.08)	(0.11)	(0.20)
		[0.040]	[0.191]	[0.406]
The respondent's interest in starting a business (1-10 scale)	6.21 (2.96)	0.12†	0.18	-0.20
		(0.09)	(0.12)	(0.21)
		[0.092]	[0.209]	[0.359]
Entrepreneurial Activity Component		0.01	-0.01	0.07
		(0.02)	(0.03)	(0.05)
		[0.189]	[0.441]	[0.229]
If a family member who started a business lives in the respondent's household	0.06 (0.21)	-0.01**††	-0.02***†	0.01
		(0.01)	(0.01)	(0.02)
		[0.037]	[0.060]	[0.433]
If the respondent knows someone who started or helped start a business	0.60 (0.41)	0.03***††	0.02	0.05
		(0.01)	(0.02)	(0.03)
		[0.025]	[0.310]	[0.227]
If the respondent ever started or helped start a business	0.30 (0.40)	0.00	0.00	0.01
		(0.01)	(0.02)	(0.03)
		[0.291]	[0.544]	[0.516]

Notes: 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 A19: Impact of Guaranteed Income on Entrepreneurship: Comparison of Impacts by Age at Baseline

	Control Mean	Entire Sample	Under 30	30+
Entrepreneurship Index		**0.05***†††**	**0.05**†**	**0.06***†**
		(0.02)	**(0.02)**	**(0.02)**
		[0.010]	**[0.087]**	**[0.064]**
Entrepreneurial Orientation Component		0.07***†††	0.07**†	0.06*
		(0.02)	(0.03)	(0.04)
		[0.008]	[0.087]	[0.168]
The respondent's self-reported willingness to take financial risks (1-10 scale)	4.52 (2.09)	0.08†	0.07	0.10
		(0.06)	(0.08)	(0.09)
		[0.092]	[0.377]	[0.326]
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**†	-0.12
		(0.06)	(0.08)	(0.09)
		[0.025]	[0.087]	[0.227]
Entrepreneurial Intention Component		0.06**††	0.05	0.09**†
		(0.02)	(0.03)	(0.04)
		[0.016]	[0.201]	[0.076]
Whether or not the respondent has an idea for a business	0.58 (0.42)	0.03**††	0.05**†	0.04*
		(0.01)	(0.02)	(0.02)
		[0.027]	[0.087]	[0.127]
The respondent's likelihood rating that they will start a business in the next 5 years (1-10 scale)	4.95 (3.05)	0.15*††	0.12	0.30**†
		(0.08)	(0.13)	(0.13)
		[0.040]	[0.374]	[0.087]
The respondent's interest in starting a business (1-10 scale)	6.21 (2.96)	0.12†	0.07	0.20
		(0.09)	(0.13)	(0.15)
		[0.092]	[0.441]	[0.227]
Entrepreneurial Activity Component		0.01	0.01	0.02
		(0.02)	(0.03)	(0.03)
		[0.189]	[0.479]	[0.406]
If a family member who started a business lives in the respondent's household	0.06 (0.21)	-0.01**††	-0.00	-0.03***††
		(0.01)	(0.01)	(0.01)
		[0.037]	[0.485]	[0.042]
If the respondent knows someone who started or helped start a business	0.60 (0.41)	0.03***††	0.03	0.05**†
		(0.01)	(0.02)	(0.02)
		[0.025]	[0.218]	[0.065]
If the respondent ever started or helped start a business	0.30 (0.40)	0.00	-0.00	0.03
		(0.01)	(0.02)	(0.02)
		[0.291]	[0.566]	[0.201]

Notes: 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 A20: 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**	**-0.02**
		(0.01)	**(0.02)**	**(0.03)**
		[0.449]	**[1.000]**	**[1.000]**
Adequacy of Employment Component		0.01	0.01	0.00
		(0.03)	(0.03)	(0.05)
		[1.000]	[1.000]	[1.000]
Employment Quality Component		-0.01	-0.01	0.02
		(0.02)	(0.03)	(0.05)
		[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)	-0.18	-0.37	-0.75
		(0.37)	(0.43)	(0.99)
		[1.000]	[1.000]	[1.000]
Stability of Employment Component		-0.02	-0.01	-0.04
		(0.02)	(0.03)	(0.04)
		[1.000]	[1.000]	[1.000]
Quality of Work Life Component		-0.02	-0.03	0.02
		(0.02)	(0.02)	(0.03)
		[1.000]	[1.000]	[1.000]

Notes: 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 A21: 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]	0.00 (0.02) [1.000]	0.03 (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]	0.01 (0.02) [1.000]	-0.03 (0.03) [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]	-0.05 (0.03) [1.000]	0.01 (0.06) [1.000]
Employment Quality				
Whether training is offered by the respondent's main employer	0.53 (0.45)	0.01 (0.02) [1.000]	0.00 (0.02) [1.000]	0.02 (0.04) [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 (0.04) [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]	0.01 (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)	-0.12 (0.11) [1.000]	-0.14 (0.13) [1.000]	-0.14 (0.25) [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]	0.00 (0.02) [1.000]	0.01 (0.03) [1.000]
Number of non-wage benefits at respondent's job(s), alternate measure	4.53 (2.97)	-0.17 (0.11) [1.000]	-0.22 (0.14) [1.000]	-0.15 (0.26) [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]	-0.00 (0.02) [1.000]
Stability of Employment				
How many months the respondent has been employed in the past year	10.69 (2.66)	-0.03 (0.10) [1.000]	-0.01 (0.13) [1.000]	-0.17 (0.19) [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)	1.70 (1.15) [1.000]	3.13** (1.41) [1.000]	-1.13 (2.10) [1.000]
How many jobs the respondent has held in the past 12 months	1.76 (1.60)	-0.12** (0.05) [1.000]	-0.15** (0.06) [1.000]	-0.17 (0.17) [1.000]
How many jobs the respondent has held in the past two years	2.33 (3.67)	-0.17* (0.09) [1.000]	-0.20** (0.09) [1.000]	-0.19 (0.23) [1.000]
Whether the respondent's main job is a temp job	0.10 (0.26)	0.01 (0.01) [1.000]	0.00 (0.01) [1.000]	0.02 (0.03) [1.000]
Whether each of the respondent's jobs is salaried, weighted by hours worked at each job	0.23 (0.39)	-0.01 (0.01) [1.000]	-0.02 (0.01) [1.000]	0.01 (0.04) [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)	0.00 (0.01) [1.000]	0.01 (0.02) [1.000]	-0.01 (0.03) [1.000]
How many months the respondent expects to remain in their main job	8.97 (6.56)	-1.30* (0.70)	-1.10 (0.87)	-1.38 (1.66)

		[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)	-0.03 (0.05) [1.000]	-0.01 (0.06) [1.000]	-0.11 (0.11) [1.000]
The work activities are not boring at the respondent's main job (1-5 scale)	3.11 (1.05)	-0.01 (0.04) [1.000]	-0.08 (0.06) [1.000]	0.18* (0.09) [1.000]
Satisfaction with compensation at the respondent's main job (1-5 scale)	3.51 (1.06)	-0.02 (0.04) [1.000]	-0.05 (0.05) [1.000]	0.03 (0.10) [1.000]
Whether the respondent faces age discrimination at work	0.06 (0.21)	0.00 (0.01) [1.000]	-0.00 (0.01) [1.000]	0.02 (0.02) [1.000]
Whether the respondent faces sex discrimination at work	0.08 (0.25)	0.00 (0.01) [1.000]	0.00 (0.01) [1.000]	-0.01 (0.02) [1.000]
Whether the respondent faces racial or ethnic discrimination at work	0.08 (0.25)	0.01 (0.01) [1.000]	-0.00 (0.01) [1.000]	0.00 (0.02) [1.000]
Whether the respondent experienced fair treatment by their supervisor (1-5 scale)	4.05 (0.91)	0.04 (0.04) [1.000]	0.03 (0.05) [1.000]	0.12 (0.08) [1.000]
Whether job demands do not interfere with family life (1-4 scale)	2.91 (0.87)	0.02 (0.03) [1.000]	-0.02 (0.04) [1.000]	0.01 (0.07) [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 (0.04) [1.000]	-0.03 (0.05) [1.000]	-0.08 (0.08) [1.000]
Flexibility of schedule at the respondent's main job (1-4 scale)	1.91 (0.91)	0.01 (0.04) [1.000]	-0.02 (0.05) [1.000]	0.11 (0.09) [1.000]
Overall satisfaction with the respondent's main job (1-5 scale)	3.96 (0.96)	0.03 (0.04) [1.000]	-0.01 (0.05) [1.000]	0.03 (0.09) [1.000]
Whether the respondent has decision-making input in their job (1-4 scale)	2.67 (0.98)	-0.03 (0.04) [1.000]	-0.06 (0.05) [1.000]	0.12 (0.08) [1.000]
Satisfaction with non-wage aspects of respondent's main job (1-5 scale)	3.69 (1.12)	0.02 (0.04) [1.000]	-0.01 (0.06) [1.000]	0.14 (0.10) [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 (0.03) [1.000]	-0.05 (0.04) [1.000]	0.07 (0.07) [1.000]
Opportunities for promotion at the respondent's main job (1-5 scale)	3.41 (1.27)	-0.10* (0.05) [1.000]	-0.09 (0.07) [1.000]	0.08 (0.11) [1.000]
Safety and health conditions at the respondent's main job (1-5 scale)	4.22 (0.79)	0.02 (0.03) [1.000]	0.01 (0.04) [1.000]	0.03 (0.07) [1.000]
Whether a scheduled shift was canceled with less than 24 hours notice in the last month	0.09 (0.26)	0.02* (0.01) [1.000]	0.02 (0.01) [1.000]	0.03 (0.02) [1.000]
Number of stressors in their work environment at respondent's main job	1.25 (1.24)	0.09* (0.05) [1.000]	0.07 (0.06) [1.000]	0.09 (0.11) [1.000]
How easy is it to take time off from the respondent's main job? (1-4 scale)	3.18 (0.81)	-0.06* (0.03) [1.000]	-0.07* (0.04) [1.000]	-0.09 (0.07) [1.000]

Notes: 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 A22: 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	-4.1***	-4.6***	-4.2***	-4.1***	-4.1***	-5.4***	-3.8***
(self-reported)	(1.0)	(1.3)	(1.0)	(1.0)	(1.0)	(0.9)	(1.0)
Total individual income	-1.5*	-1.8*	-2.4***	-1.4*	-1.5*	-3.1***	-1.1
	(0.9)	(1.1)	(0.9)	(0.9)	(0.9)	(0.8)	(0.9)
Total individual income	-2.5**	-2.5**	-3.1**	-3.3***	-2.5**	-4.5***	-1.6*
(self-reported)	(1.0)	(1.0)	(1.3)	(0.9)	(1.0)	(0.9)	(1.0)
Individual salaried/wage	-1.1	-1.7*	-1.3	-1.1	-1.1	-2.2***	-1.0
income	(0.8)	(1.0)	(0.9)	(0.8)	(0.8)	(0.7)	(0.8)
Self-employment income	-0.1	0.1	-0.0	-0.1	-0.1	-1.2***	0.0
	(0.5)	(0.6)	(0.0)	(0.5)	(0.5)	(0.4)	(0.5)
Income from 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	N/A	-0.2	-0.2	-0.3**	-0.1
	(0.1)	(0.2)	(.)	(0.1)	(0.1)	(0.1)	(0.1)

Notes: 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 A23: 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
Labor Supply Elasticity Index	**-0.06****	**-0.08****	**-0.06****	**-0.05****	**-0.06****	**-0.07*****	**-0.05***
	(0.03)	**(0.04)**	**(0.03)**	**(0.03)**	**(0.03)**	**(0.03)**	**(0.03)**
Labor Supply Elasticity	-0.06**	-0.08**	-0.06**	-0.05**	-0.06**	-0.07***	-0.05*
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
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)
Hours worked per week	-1.28**	-1.77**	N/A	-1.28**	-1.28**	-1.87***	-1.07*
	(0.64)	(0.80)	(.)	(0.64)	(0.64)	(0.62)	(0.64)
Number of other household members which are	-0.02	-0.02	N/A	0.03	-0.02	-0.04**	-0.02
employed	(0.02)	(0.02)	(.)	(0.02)	(0.02)	(0.02)	(0.02)
Total number of hours participant and	-2.16***	-2.42**	N/A	-2.49***	-2.16***	-2.60***	-1.95**
spouse/partner works per week	(0.78)	(1.00)	(.)	(0.85)	(0.78)	(0.77)	(0.78)
Total number of hours all household members	-2.21**	-2.94**	-1.67	-2.21**	-2.21**	-2.81***	-2.01**
(including the participant) work per week	(0.92)	(1.17)	(1.13)	(0.92)	(0.92)	(0.90)	(0.92)
Total number of hours participant's parents in	-0.13	-0.21	N/A	-0.34	-0.13	-0.68**	-0.12
household work per week	(0.35)	(0.46)	(.)	(0.39)	(0.35)	(0.30)	(0.35)
Total number of hours participant's adult children	0.30	0.30	N/A	0.07	0.30	-0.23	0.32
in household work per week	(0.29)	(0.29)	(.)	(0.27)	(0.29)	(0.22)	(0.29)

Notes: 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 A24: 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	0.31*	0.44**	0.33***	0.31*	0.41**	0.22	0.32*
	(0.17)	(0.20)	(0.11)	(0.17)	(0.20)	(0.16)	(0.17)
Helping Hrs/Mo	0.38	0.42	0.37***	0.38	0.37	0.13	0.40
	(0.26)	(0.28)	(0.14)	(0.26)	(0.31)	(0.24)	(0.26)
Medical Hrs/Mo	0.35	0.82	0.07	0.35	0.59	-0.01	0.35
	(0.63)	(0.76)	(0.06)	(0.63)	(0.70)	(0.60)	(0.63)
Meetings Hrs/Mo	-0.01	0.03	-0.00	-0.01	0.01	-0.07	-0.01
	(0.07)	(0.08)	(0.01)	(0.07)	(0.09)	(0.07)	(0.08)
Religion Hrs/Mo	0.05	0.17	-0.01	0.05	0.13	-0.02	0.05
	(0.12)	(0.16)	(0.02)	(0.12)	(0.14)	(0.12)	(0.12)
Childcare Hrs/Wk	-0.51	-0.68	0.06	-0.51	-0.69	-0.95	-0.46
	(0.88)	(1.41)	(0.24)	(0.88)	(1.01)	(0.86)	(0.88)
Chores Hrs/Wk	-0.21	0.01	0.18	-0.21	-0.09	-0.36	-0.17
	(0.27)	(0.33)	(0.25)	(0.27)	(0.32)	(0.27)	(0.27)
Communicating Hrs/Wk	-0.16	-0.15	0.01	-0.16	-0.26	-0.41	-0.11
	(0.35)	(0.40)	(0.20)	(0.35)	(0.43)	(0.34)	(0.35)
Commuting Hrs/Wk	-0.05	-0.00	-0.10	-0.05	0.06	-0.16	-0.04
	(0.14)	(0.17)	(0.12)	(0.14)	(0.18)	(0.13)	(0.14)
Education Hrs/Wk	-0.03	0.13	-0.01	-0.03	-0.01	-0.19	-0.01
	(0.22)	(0.25)	(0.10)	(0.22)	(0.26)	(0.21)	(0.22)
Eldercare Hrs/Wk	-0.13	-0.07	-0.01	-0.13	-0.24	-0.32	-0.13
	(0.21)	(0.25)	(0.01)	(0.21)	(0.25)	(0.19)	(0.21)
Entertainment Hrs/Wk	-0.01	0.13	-0.05	-0.01	0.06	-0.22	0.04
	(0.36)	(0.45)	(0.36)	(0.36)	(0.42)	(0.35)	(0.36)
Family Hrs/Wk	-0.66	-0.79	0.03	-0.66	-1.25	-1.01	-0.62
	(0.73)	(0.93)	(0.70)	(0.73)	(0.87)	(0.72)	(0.73)
Friends Hrs/Wk	0.09	0.28	-0.02	0.09	0.09	-0.05	0.11
	(0.26)	(0.31)	(0.22)	(0.26)	(0.33)	(0.25)	(0.27)
Hobbies Hrs/Wk	-0.05	-0.00	-0.06	-0.05	-0.08	-0.19	-0.04
	(0.15)	(0.17)	(0.09)	(0.15)	(0.18)	(0.14)	(0.15)
Reading Hrs/Wk	-0.12	-0.02	-0.05	-0.12	-0.29	-0.23	-0.09
	(0.19)	(0.22)	(0.14)	(0.19)	(0.23)	(0.19)	(0.19)

Recreation Hrs/Wk	-0.47***	-0.39**	-0.37**	-0.47***	-0.57***	-0.61***	-0.45**
	(0.18)	(0.20)	(0.15)	(0.18)	(0.22)	(0.17)	(0.18)
Sleeping Hrs/Wk	0.19	0.49	0.15	0.19	-0.09	0.06	0.33
	(0.37)	(0.47)	(0.44)	(0.37)	(0.43)	(0.37)	(0.37)
Working Hrs/Wk	-1.40***	-1.67**	N/A	-1.40***	-1.59***	-1.57***	-1.32***
	(0.50)	(0.66)	(.)	(0.50)	(0.58)	(0.50)	(0.50)
Vacation Days/Yr	0.12	0.15	0.03	0.12	-0.12	-0.01	0.14
	(0.31)	(0.37)	(0.22)	(0.31)	(0.32)	(0.30)	(0.31)
Volunteer Hrs/Yr	0.35	1.70	0.01	0.35	1.00	-1.03	0.38
	(1.48)	(1.73)	(0.21)	(1.48)	(1.76)	(1.37)	(1.48)

Notes: 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 A25: 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	-1.03	-2.09	N/A	-1.12	N/A	-3.88***	-0.97
	(1.18)	(1.30)	(.)	(1.18)	(.)	(0.90)	(1.20)
Childcare Min/Day	-3.94	-4.13	N/A	-3.99	N/A	-15.11***	-3.55
	(4.31)	(6.08)	(.)	(4.32)	(.)	(3.63)	(4.39)
Community Engagement Min/Day	-1.17	-0.44	0.00	-1.04	N/A	-3.44***	-1.03
	(0.89)	(1.01)	(0.02)	(0.90)	(.)	(0.72)	(0.90)
Exercise Min/Day	-0.12	-0.07	0.29	-0.17	N/A	-2.76***	0.19
	(0.87)	(1.04)	(0.24)	(0.88)	(.)	(0.64)	(0.88)
Home Production Min/Day	2.52	4.51	4.12	2.69	N/A	-4.77	5.70*
	(3.43)	(4.10)	(3.15)	(3.48)	(.)	(3.08)	(3.42)
Market Work Min/Day	-8.60*	-12.20*	-9.59*	-8.78*	N/A	-17.77***	-5.68
	(5.20)	(6.57)	(5.68)	(5.23)	(.)	(4.88)	(5.27)
Non-Commuting Transportation Min/Day	4.84***	5.34***	N/A	4.80***	N/A	0.71	5.67***
	(1.50)	(1.66)	(.)	(1.51)	(.)	(1.18)	(1.52)
Other Min/Day	5.99**	5.21*	1.44***	5.99**	N/A	-2.20	6.53**
	(2.59)	(2.97)	(0.50)	(2.59)	(.)	(1.92)	(2.63)
Other Income Min/Day	-2.79**	-2.50**	N/A	-2.74**	N/A	-5.74***	-2.68**
	(1.10)	(1.15)	(.)	(1.10)	(.)	(0.86)	(1.11)
Search for a job Min/Day	-0.30	0.18	N/A	-0.38	N/A	-3.64***	-0.19
	(1.03)	(1.12)	(.)	(1.03)	(.)	(0.71)	(1.05)
Self-care Min/Day	1.62	1.21	2.42**	1.62	N/A	-1.37	2.56**
	(1.24)	(1.41)	(1.22)	(1.24)	(.)	(1.05)	(1.26)
Self-Improvement Min/Day	-0.41	-0.53	0.47	-0.57	N/A	-6.16***	0.30
	(2.24)	(2.78)	(0.75)	(2.30)	(.)	(1.89)	(2.27)
Sleep Min/Day	-7.55*	-4.52	-4.80	-8.92**	N/A	-13.20***	-0.34
	(3.99)	(5.05)	(3.56)	(4.28)	(.)	(3.87)	(3.73)
Social Leisure Min/Day	5.85	5.27	5.07	5.82	N/A	-1.58	9.17**
	(3.92)	(4.65)	(4.02)	(3.96)	(.)	(3.67)	(3.93)
Solo Leisure Min/Day	3.61	4.87	1.04	3.07	N/A	-3.23	5.17
	(3.34)	(5.16)	(2.78)	(3.35)	(.)	(3.03)	(3.39)
Time with Others Min/Day	0.59	-1.07	3.13	0.59	N/A	-13.35**	4.33

	(6.29)	(8.34)	(5.50)	(6.29)	(.)	(5.63)	(6.35)

Notes: 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 A26: 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	**-0.10*****	**-0.10****	**-0.10*****	**N/A**	**-0.10*****	**-0.11*****	**-0.07***
	(0.04)	**(0.04)**	**(0.04)**	**(.)**	**(0.04)**	**(0.04)**	**(0.04)**

Notes: 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 A27: 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**	**0.02**	**0.01**	**0.04**	**0.00**	**0.04****
Index	**(0.02)**	**(0.02)**	**(0.02)**	**(0.01)**	**(0.02)**	**(0.02)**	**(0.02)**
Active Search	0.03	0.03	0.03	0.01	N/A	0.00	0.04*
	(0.02)	(0.03)	(0.02)	(0.02)	(.)	(0.02)	(0.02)
Preferences for Employment	0.01	0.01	0.01	0.01	0.01	-0.00	0.04
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)

Notes: 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 A28: 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	**-0.00**	**-0.02**	**-0.00**	**0.01**	**0.02**	**-0.08*****	**0.08*****
	(0.02)	**(0.03)**	**(0.02)**	**(0.02)**	**(0.06)**	**(0.02)**	**(0.02)**
Selectivity	-0.00	-0.02	-0.00	0.01	N/A	-0.08***	0.08***
	(0.02)	(0.03)	(0.02)	(0.02)	(.)	(0.02)	(0.02)

Notes: 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 A29: 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**	**-0.01**	**-0.01**	**-0.01**	**-0.04****	**0.01**
	(0.01)	**(0.02)**	**(0.01)**	**(0.01)**	**(0.01)**	**(0.01)**	**(0.01)**
Adequacy of Employment	0.01	-0.01	0.01	0.01	0.01	-0.03	0.01
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
Employment Quality	-0.01	-0.03	-0.01	-0.01	-0.01	-0.02	0.01
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Whether the respondent reports working any	-0.00	0.01	N/A	-0.00	-0.00	0.01	-0.00
informal job	(0.01)	(0.02)	(.)	(0.01)	(0.01)	(0.01)	(0.01)
Average hourly income from all jobs,	-0.18	-0.18	-0.24	-0.18	-0.18	-0.36	0.13
weighted by hours worked at each job	(0.37)	(0.44)	(0.37)	(0.37)	(0.37)	(0.37)	(0.37)
Stability of Employment	-0.02	-0.01	-0.02	-0.01	-0.02	-0.06***	0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Quality of Work Life	-0.02	-0.02	-0.02	-0.02	-0.02	-0.04**	-0.00
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)

Notes: 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 A30: 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*****	**0.05*****	**0.04*****	**N/A**	**0.04*****	**0.05*****
	(0.02)	**(0.02)**	**(0.02)**	**(0.01)**	**(.)**	**(0.02)**	**(0.01)**
Entrepreneurial Orientation	0.07***	0.10***	0.07***	0.06***	N/A	0.07***	0.08***
	(0.02)	(0.03)	(0.02)	(0.02)	(.)	(0.02)	(0.02)
Entrepreneurial Intention	0.06**	0.08**	0.06**	0.06**	N/A	0.05**	0.06***
	(0.02)	(0.03)	(0.02)	(0.02)	(.)	(0.02)	(0.02)
Entrepreneurial Activity	0.01	0.04	0.01	0.01	N/A	-0.00	0.02
	(0.02)	(0.03)	(0.02)	(0.02)	(.)	(0.02)	(0.02)

Notes: 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 A31: Robustness checks for Impact of Guaranteed Income on Disability

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

Notes: 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 A32: 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**	**-0.03**	**-0.02**	**-0.03**	**-0.03**	**0.03**
	(0.02)	**(0.03)**	**(0.02)**	**(0.02)**	**(0.02)**	**(0.02)**	**(0.02)**
Barriers to Employment	-0.03	-0.03	-0.03	-0.02	-0.03	-0.03	0.03
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)

Notes: 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 A33: 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	0.02	0.03	-0.00	-0.00	0.03
	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)

Notes: 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 A34: 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	**-0.013**	**-0.002**	**-0.013**	**-0.027**	**0.006**	**-0.019**	**0.012**
	(0.017)	**(0.027)**	**(0.017)**	**(0.020)**	**(0.019)**	**(0.017)**	**(0.016)**
Relationship Stability	-0.037	-0.028	-0.037	-0.065**	N/A	-0.048*	0.008
	(0.025)	(0.033)	(0.025)	(0.026)		(0.025)	(0.023)
Relationship Status	0.011	0.024	0.011	0.011	0.007	0.010	0.016
	(0.016)	(0.027)	(0.016)	(0.022)	(0.016)	(0.016)	(0.016)

Notes: 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 A35: 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	**0.087*** **	**0.096*** **	**0.087*** **	**0.072*** **	**0.041**	**0.040**	**0.096*** **
	(0.026)	**(0.029)**	**(0.026)**	**(0.022)**	**(0.038)**	**(0.025)**	**(0.026)**
Moved labor markets since baseline	0.018	0.023*	N/A	N/A	0.013	0.017	0.019*
	(0.011)	(0.012)	(.)	(.)	(0.012)	(0.011)	(0.011)
Search new labor market	0.113***	0.116***	0.111***	0.072***	N/A	0.025	0.128***
	(0.034)	(0.038)	(0.034)	(0.022)		(0.030)	(0.035)

Notes: 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 A36: 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	**-0.01**	**0.00**	**-0.01**	**-0.01**	**N/A**	**-0.01***	**-0.00**
	(0.01)	**(0.01)**	**(0.01)**	**(0.01)**		**(0.01)**	**(0.01)**
Labor Quality	0.00	0.02	0.00	0.00	N/A	-0.00	0.00
	(0.01)	(0.02)	(0.01)	(0.02)		(0.01)	(0.01)
Labor Market Amenities	-0.02	-0.02*	-0.02	-0.02	N/A	-0.03**	-0.01
	(0.01)	(0.01)	(0.01)	(0.02)		(0.01)	(0.01)

Notes: 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 A37: 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	**-0.02**	**-0.03**	**-0.02**	**-0.02**	**-0.02**	**-0.05****	**-0.02**
	(0.02)	**(0.04)**	**(0.02)**	**(0.03)**	**(0.02)**	**(0.02)**	**(0.02)**
Take-Up Benefits	-0.02	-0.03	-0.02	-0.02	-0.02	-0.05**	-0.02
	(0.02)	(0.04)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)

Notes: 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 A38: 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	0.38	2941
		(0.01)		
		[0.80]		
Not working due to attending school	0.04 (0.15)	0.01	0.18	2941
		(0.01)		
		[0.88]		
Not working due to caring for elderly	0.02 (0.13)	0.01	0.76	2941
		(0.01)		
		[0.81]		
Not working due to have given up looking for work	0.04 (0.17)	-0.01	-1.64	2941
		(0.01)		
		[0.80]		
Not working due to illness	0.07 (0.23)	0.01	0.36	2941
		(0.01)		
		[0.80]		
Not working due to lack in necessary skills	0.08 (0.24)	0.00	0.14	2941
		(0.01)		
		[1.00]		
Not working due to other reasons	0.06 (0.19)	0.01	0.13	2941
		(0.01)		
		[0.88]		
Not working due to personal or family responsibilities	0.13 (0.29)	0.01	0.13	2941
		(0.01)		
		[1.00]		
Not working due to preferring to stay at home	0.09 (0.26)	0.00	0.10	2941
		(0.01)		
		[1.00]		
Not working due to lack in transportation to/from work	0.06 (0.20)	0.01	0.29	2941
		(0.01)		
		[0.88]		
Not working due to suitable work being unavailable	0.13 (0.29)	0.01	0.16	2941
		(0.01)		
		[0.94]		

Notes: 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 A39: 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) [1.00]	-0.11	2939
Whether the respondent has a third job	0.06 (0.20)	0.00 (0.01) [1.00]	0.01	2939
Whether the respondent has a fourth job	0.02 (0.10)	-0.01 (0.00) [0.70]	-0.84	2939
Hours per week worked at 1st job	27.27 (17.98)	-1.31** (0.57) [0.38]	-0.14	2939
Hours per week worked at 2nd job	2.41 (5.69)	-0.08 (0.22) [1.00]	-0.10	2937
Hours per week worked at 3rd job	0.49 (2.37)	-0.01 (0.09) [1.00]	-0.04	2938
Hours per week worked at 4th job	0.10 (0.94)	-0.03 (0.03) [1.00]	-0.53	2939
Hours per week worked at 1st job (conditional on having 1st job)	36.39 (12.95)	-0.97* (0.52) [0.46]	-0.06	2404
Hours per week worked at 2nd job (conditional on having 2nd job)	12.88 (11.48)	-0.09 (0.80) [1.00]	-0.02	795
Hours per week worked at 3rd job (conditional on having 3rd job)	8.94 (8.23)	-0.28 (1.06) [1.00]	-0.08	259
Hours per week worked at 4th job (conditional on having 4th job)	7.78 (7.19)	-1.58 (1.81) [1.00]	-0.29	58
Maximum number of hours worked in a typical week	33.12 (21.68)	-1.64** (0.68) [0.47]	-0.15	2940
Minimum number of hours worked in a typical week	22.18 (16.70)	-0.69 (0.52) [0.80]	-0.10	2940

Notes: 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 A40: Impact of Guaranteed Income on Employment Preferences and Job Search: Actions Taken to Search for Work

	Control Mean	Treatment Effect	N
Whether participant looked at any job postings in the last 3 months	0.54 (0.39)	0.06***††† (0.01) [0.001]	2942
Whether participant directly contacted any employers for a job in the last 3 months	0.36 (0.38)	0.03** (0.01) [0.243]	2942
Whether participant contacted any job centers in the last 3 months	0.28 (0.35)	0.01 (0.01) [0.521]	2942
Whether participant contacted friends or relatives to find work in the last 3 months	0.36 (0.37)	0.04***† (0.01) [0.065]	2942
Whether participant contacted professional network to find work in the last 3 months	0.22 (0.32)	0.00 (0.01) [0.845]	2942
Whether participant posted a resume online in the last 3 months	0.38 (0.38)	0.02* (0.01) [0.267]	2942
Whether participant took other actions to find work in the last 3 months	0.03 (0.13)	0.01* (0.01) [0.250]	2942

Notes: 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 A41: 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 measure)	0.37 (0.40)	0.02 (0.01) [0.386]	2939
Number of job applications sent (alternate measure)	5.76 (12.92)	-0.25 (0.43) [0.475]	2980
Number of job applications sent, conditional on having applied for a job	11.47 (17.85)	-2.16***††† (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) [0.055]	2491
Whether the participant applied for a job that they were unqualified for	0.37 (0.42)	-0.01 (0.02) [0.817]	2064
Proportion of jobs the participant applied to that the participant was unqualified for	0.19 (0.29)	-0.01 (0.01) [0.551]	2064

Notes: 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 presented 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 A42: 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)	0.03 (0.02) [1.000]	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) [0.286]	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)	-0.02 (0.02) [1.000]	965
Participant is not willing to work under any conditions	0.00 (0.04)	0.00 (0.00) [1.000]	1106
Work requirement: other	0.21 (0.38)	-0.01 (0.02) [1.000]	966

Notes: 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 A43: 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)	-0.00 (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)	-0.01 (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)	-0.02 (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

Notes: 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 A44: 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 (0.30)	-0.00 (0.00) [1.000]	2932
Studied business in post secondary education	0.04 (0.20)	0.00 (0.00) [0.947]	2932
Studied education in post secondary education	0.02 (0.14)	-0.00 (0.00) [0.947]	2932
Studied health in post secondary education	0.05 (0.22)	-0.01* (0.00) [0.829]	2932
Studied social sciences in post secondary education	0.08 (0.26)	-0.00 (0.00) [0.947]	2932
Studied STEM in post secondary education	0.06 (0.22)	0.01 (0.00) [0.829]	2932
Studied a vocational major in post secondary education	0.03 (0.17)	0.00 (0.00) [0.947]	2932
Whether the participant has an Associate's degree	0.15 (0.36)	-0.01 (0.00) [1.000]	2593
Whether the participant has a Bachelor's degree	0.23 (0.42)	-0.00 (0.01) [1.000]	2593
Whether the participant has a Master's or Doctoral degree	0.08 (0.26)	-0.00 (0.01) [1.000]	2593
Whether the participant has a Master's degree	0.07 (0.25)	-0.01 (0.00) [1.000]	2593
Whether the participant has a Doctoral degree	0.02 (0.12)	0.00 (0.00) [1.000]	2593

Notes: 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 A45: Impact of Guaranteed Income on Relationship Status: Reasons for Relationships Ending

	Control Mean	All Surveys	N
Whether relationship ended because of abuse	0.031 (0.128)	0.001 (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)	0.004 (0.005) [1.000]	2903
Whether relationship ended because of family	0.019 (0.096)	-0.000 (0.003) [1.000]	2903
Whether relationship ended because of financial issues	0.033 (0.127)	0.006 (0.005) [1.000]	2903
Whether relationship ended because of illness	0.006 (0.057)	0.001 (0.002) [1.000]	2903
Whether relationship ended because of relationship issues	0.123 (0.241)	0.022** (0.010) [0.386]	2903
Whether relationship ended because of religion	0.005 (0.049)	0.004* (0.002) [1.000]	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)	0.024*** (0.008) [0.227]	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)	0.002 (0.006) [1.000]	2903

Notes: 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.

Figure A1: Illinois Bill SB 1735

Illinois General Assembly

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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

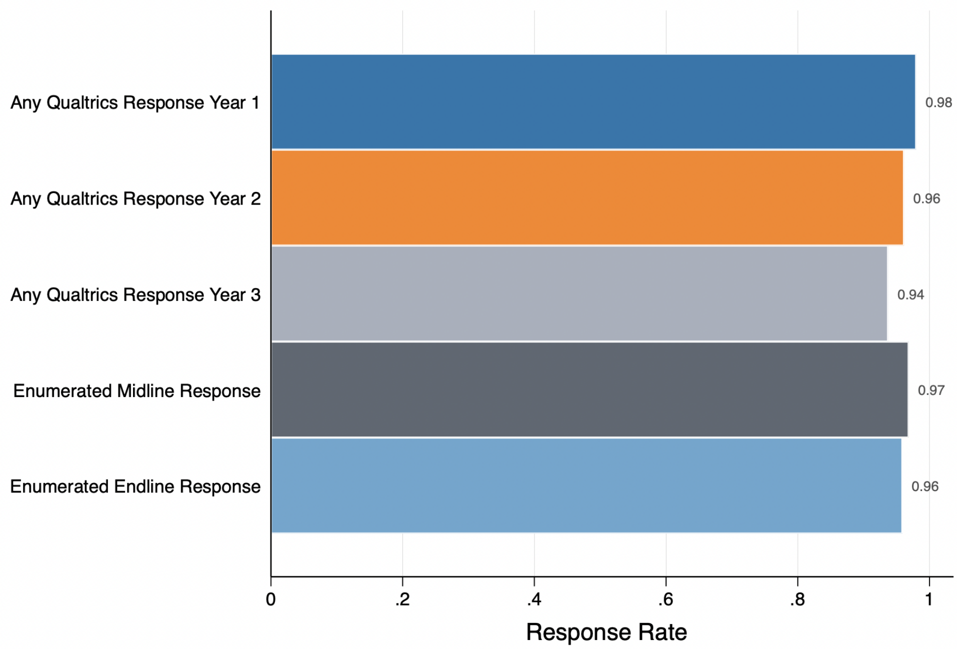
Synopsis As Introduced
Amends the Illinois Public Aid Code. Provides that for purposes of determining eligibility and the amount of assistance under the Code, the Department of Human Services and local governmental units shall exclude from consideration, for a period of no more than 60 months, any financial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

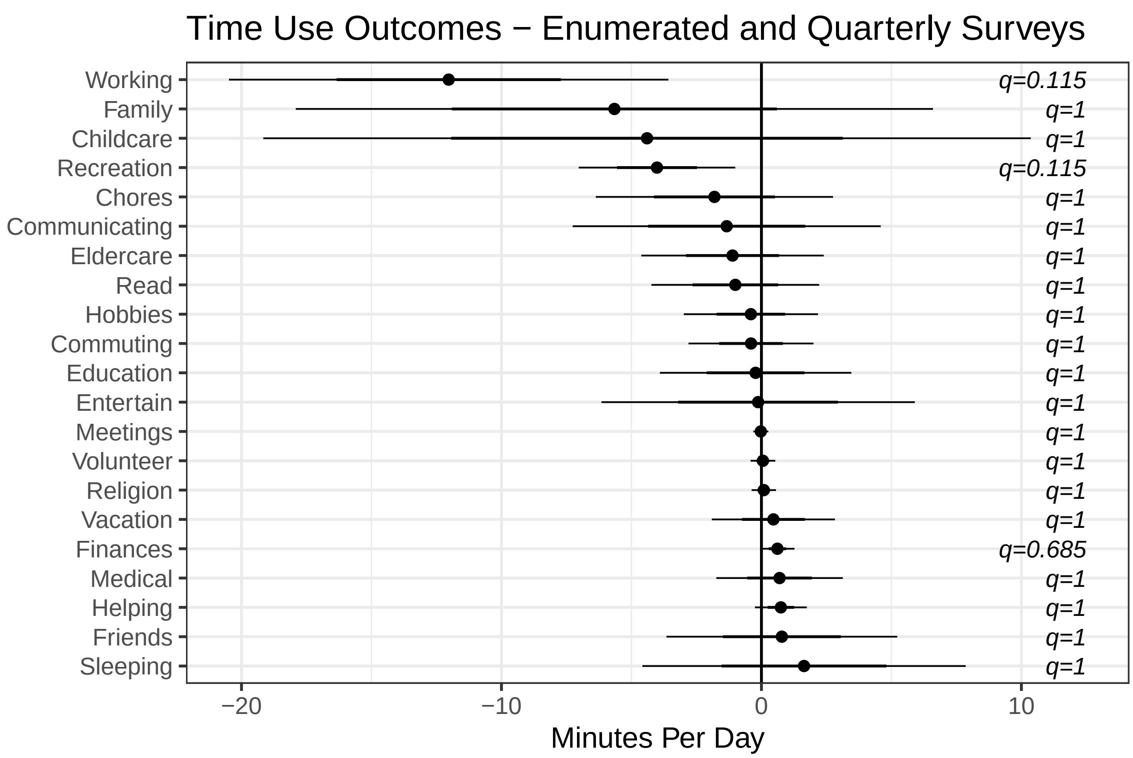
Notes: This figure provides a synopsis of the bill that was passed to protect benefits in Illinois.

Figure A2: Response Rates Over Time



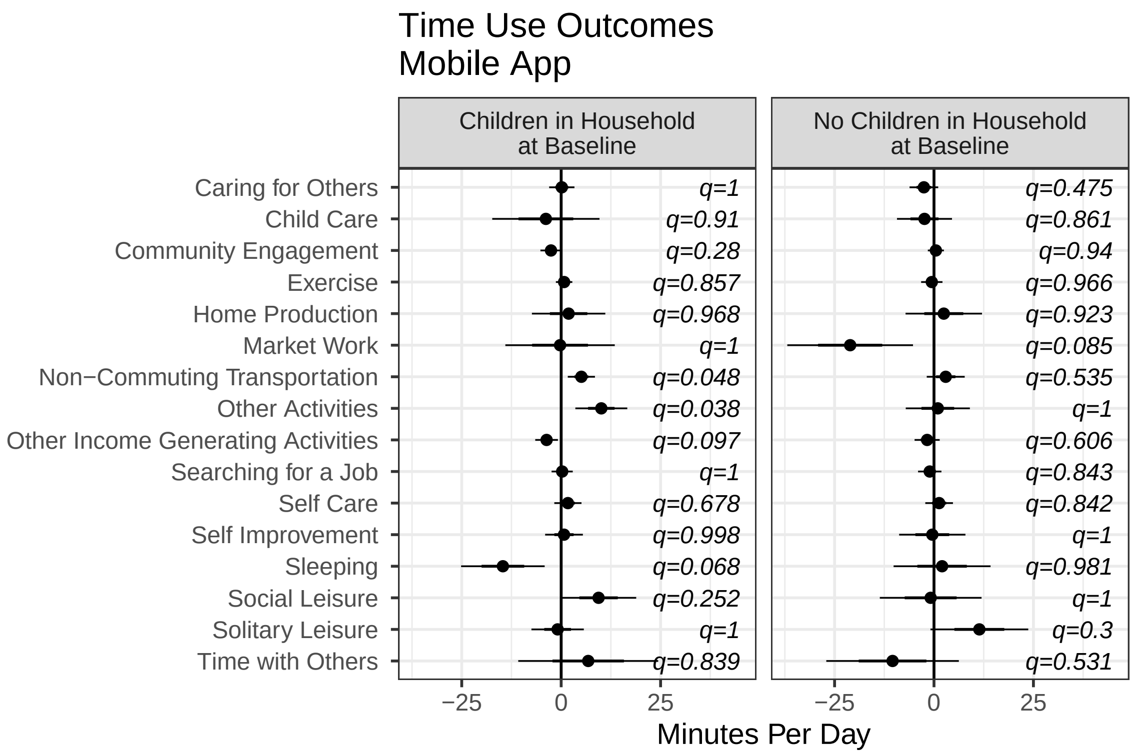
Notes: This figure shows response rates for the Qualtrics surveys and enumerated surveys over time.

Figure A3: Time Use Results: Enumerated and Quarterly Surveys



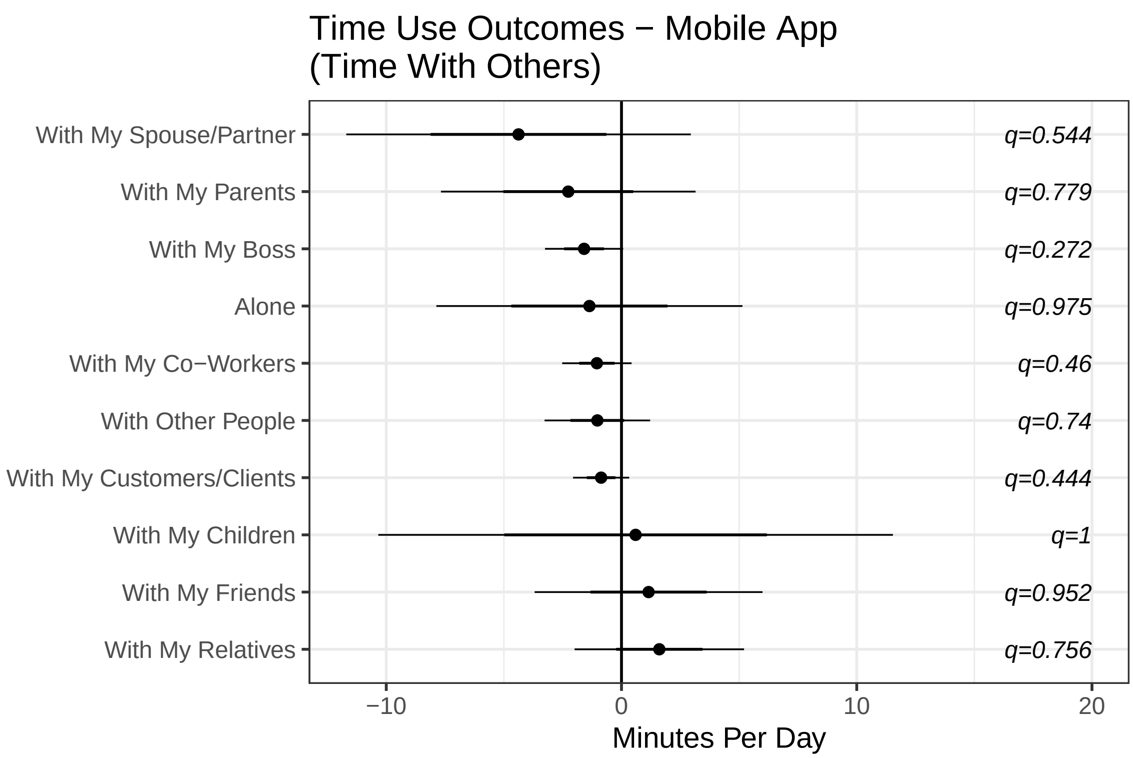
Notes: This figure shows the results from the enumerated and quarterly time use surveys.

Figure A4: Time Use Results: Mobile App - By Children in Household at Baseline



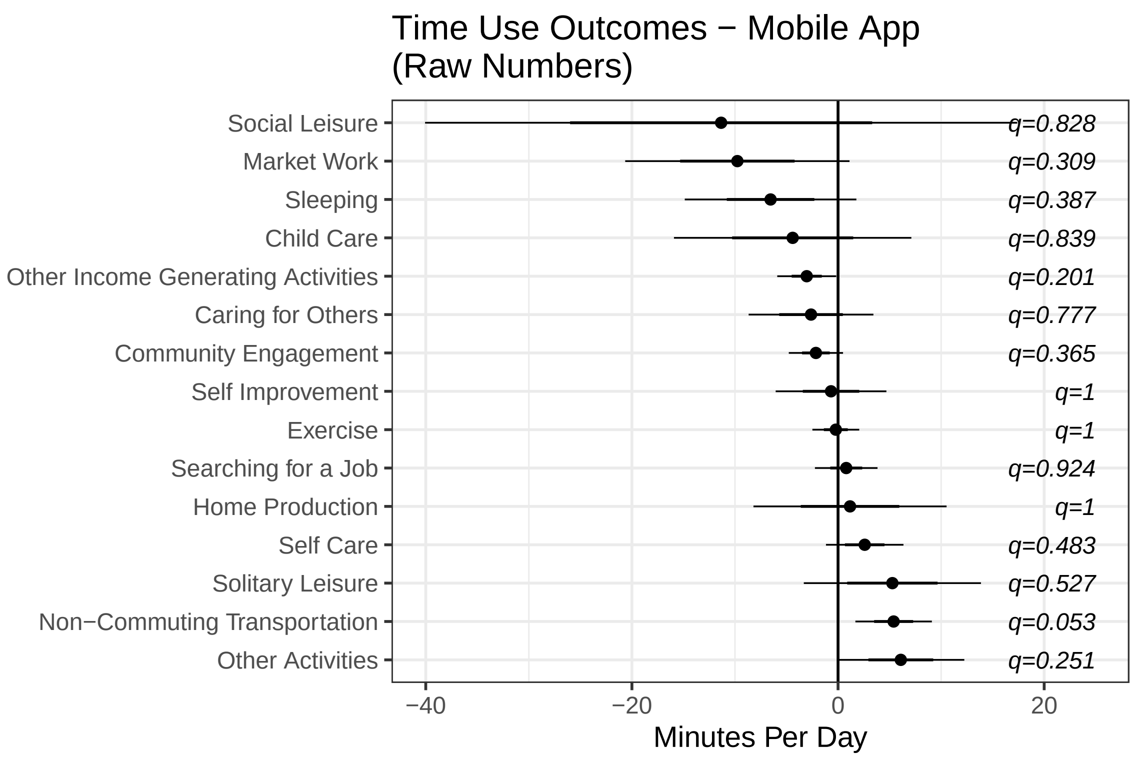
Notes: This figure shows the results from the mobile phone app, by whether participants had children in the household at baseline.

Figure A5: Time Use Results: Mobile App (Time Spent With Others)



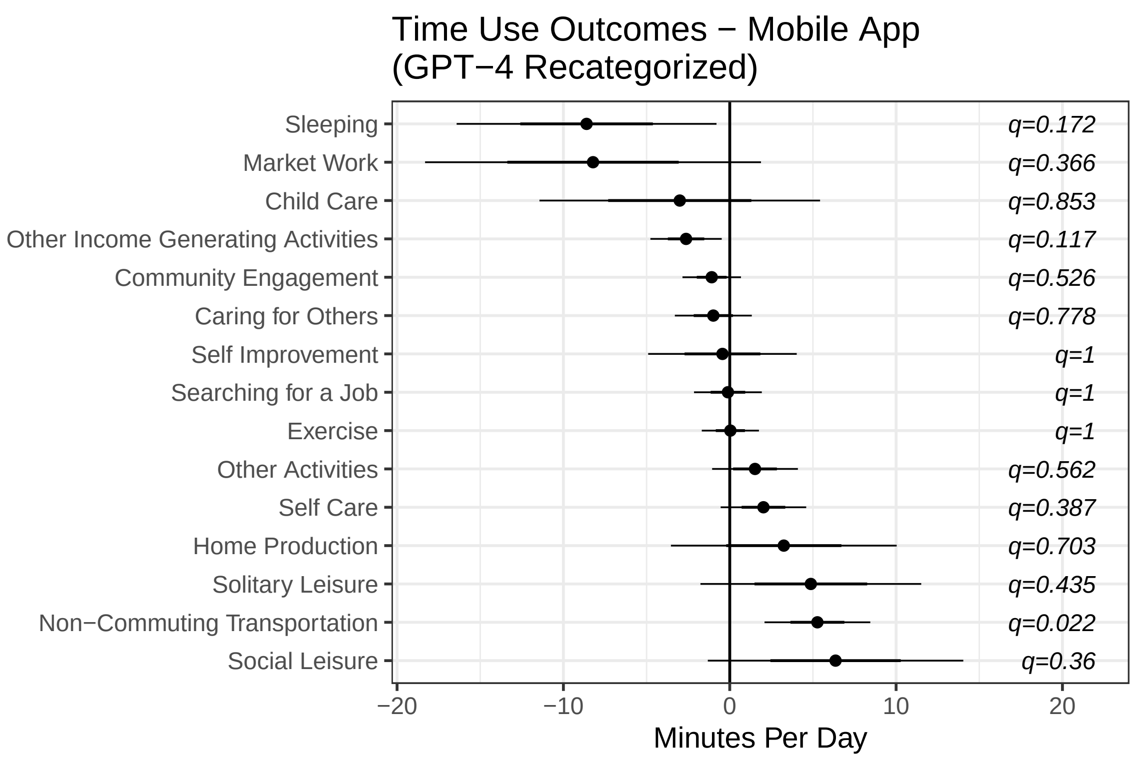
Notes: This figure shows the results from the mobile phone app for time spent with others.

Figure A6: Time Use Results: Mobile App (Raw Times)



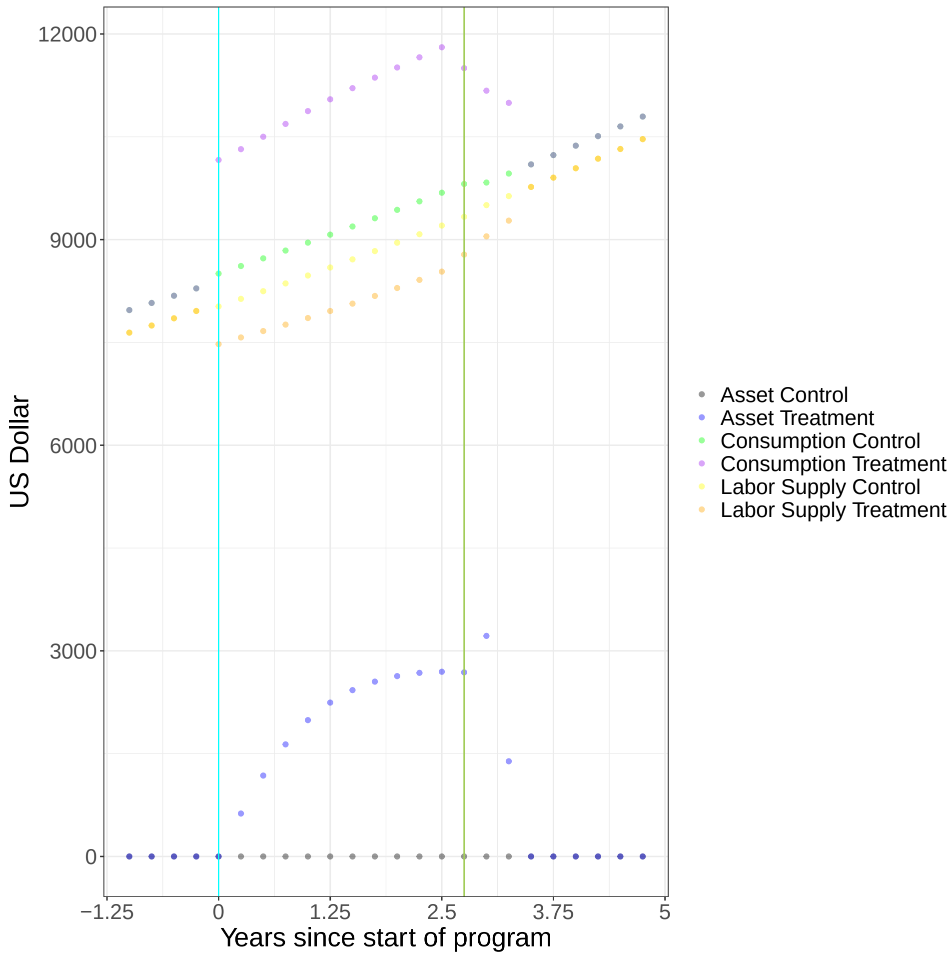
Notes: This figure shows the results from the mobile phone app, without adjusting for simultaneous activities.

Figure A7: Time Use Results: Mobile App (ChatGPT-4 Recoded)



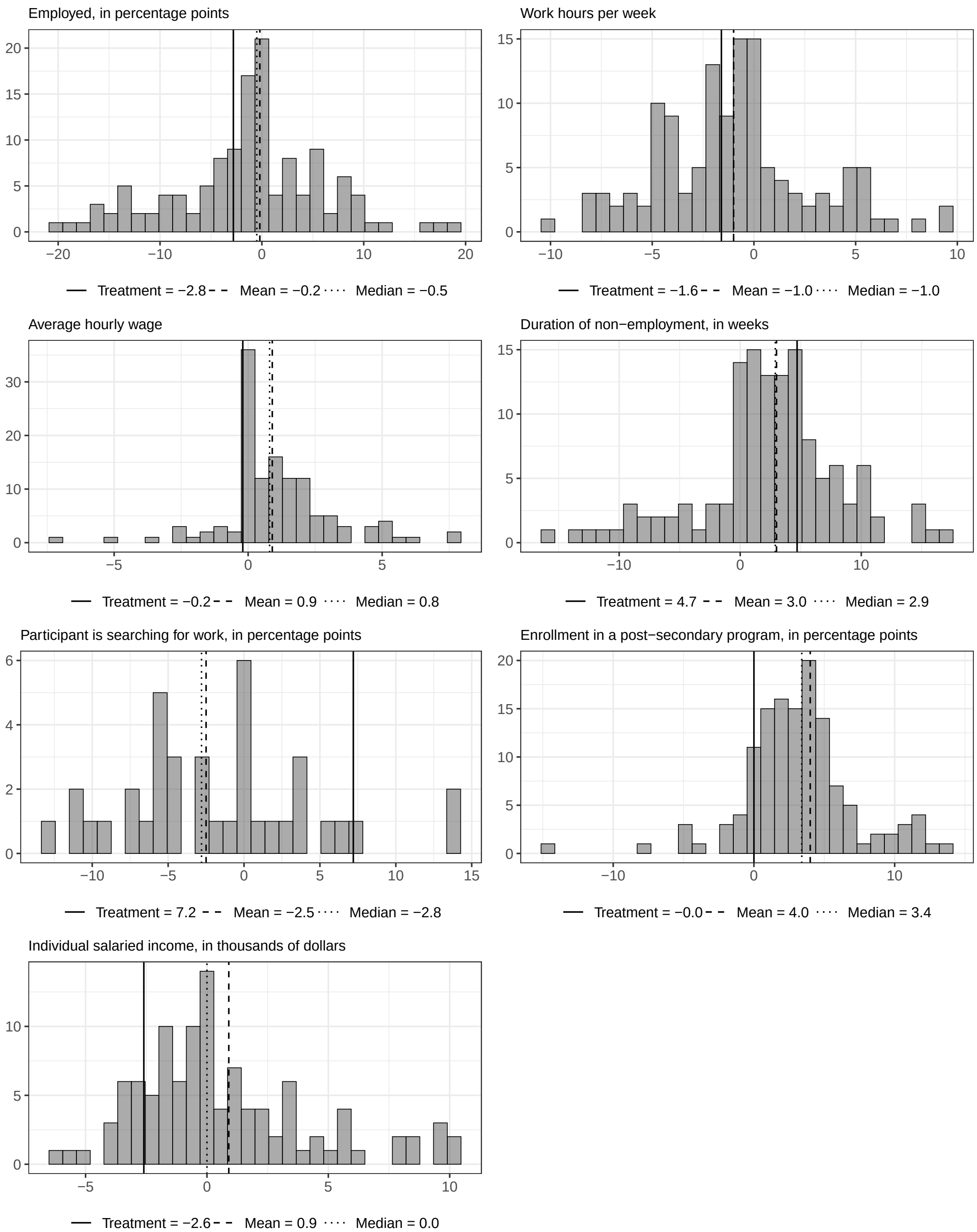
Notes: This figure shows the results from the mobile phone app, using GPT to recode open-ended responses.

Figure A8: Labor, Consumption, and Asset Paths of Fitted Model



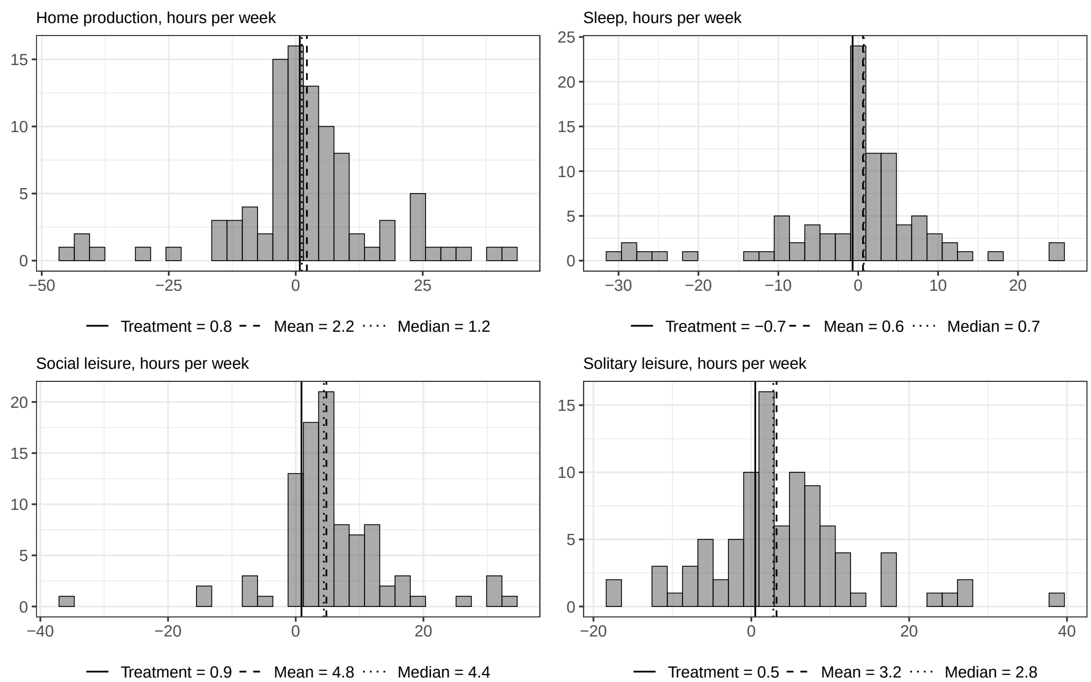
Notes: This figure shows the fitted labor supply, consumption, and net asset paths over time. A few things are worth noting. First, labor and consumption values are presented per quarter, while asset accumulation is cumulative. The light blue and green vertical lines represent the start and end of the transfers, respectively. Assets continue to go up for one period after the end of the transfers because the agent decided to save a portion of their transfer in the preceding period.

Figure A9: Forecasts of Employment Outcomes



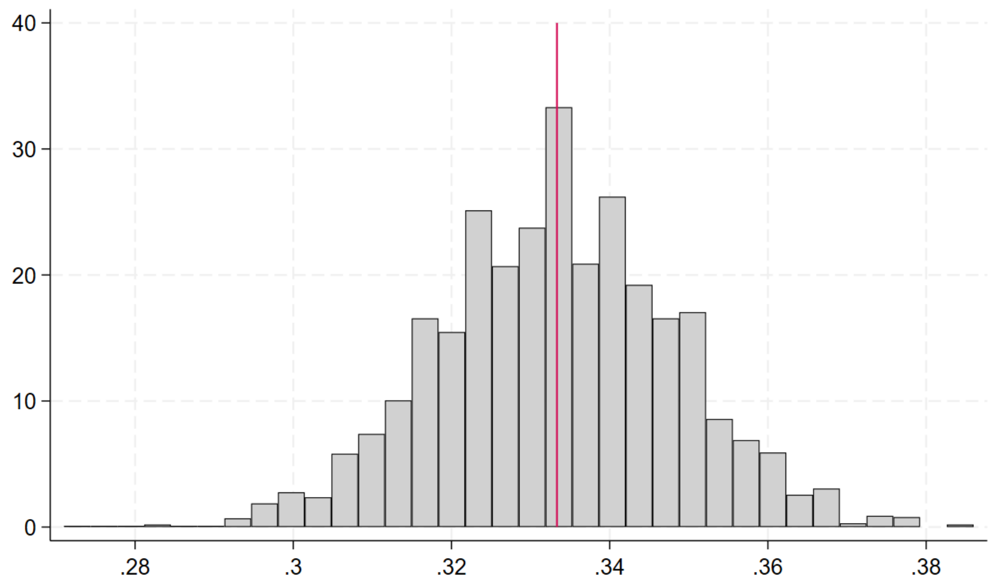
Notes: These figures show the full distribution of forecasts provided by NBER affiliates and users of the Social Science Prediction Platform. A few rare outliers more than two SD from the mean are omitted from these figures for the sake of legibility.

Figure A10: Forecasts of Time Use Outcomes



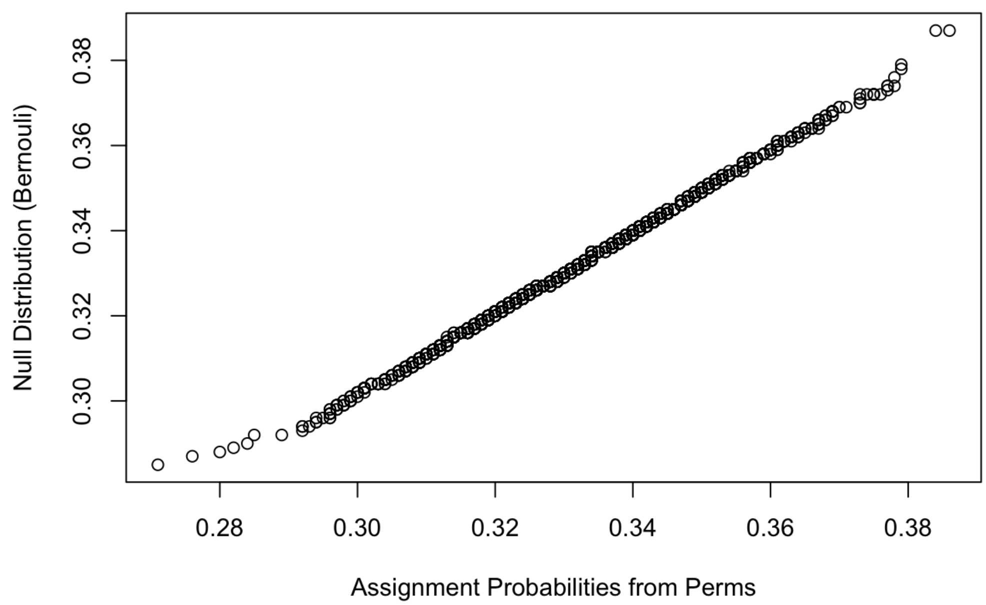
Notes: These figures show the full distribution of forecasts provided by NBER affiliates and users of the Social Science Prediction Platform. A few rare outliers more than two SD from the mean are omitted from these figures for the sake of legibility.

Figure A11: Histogram of Treatment Assignment Probabilities



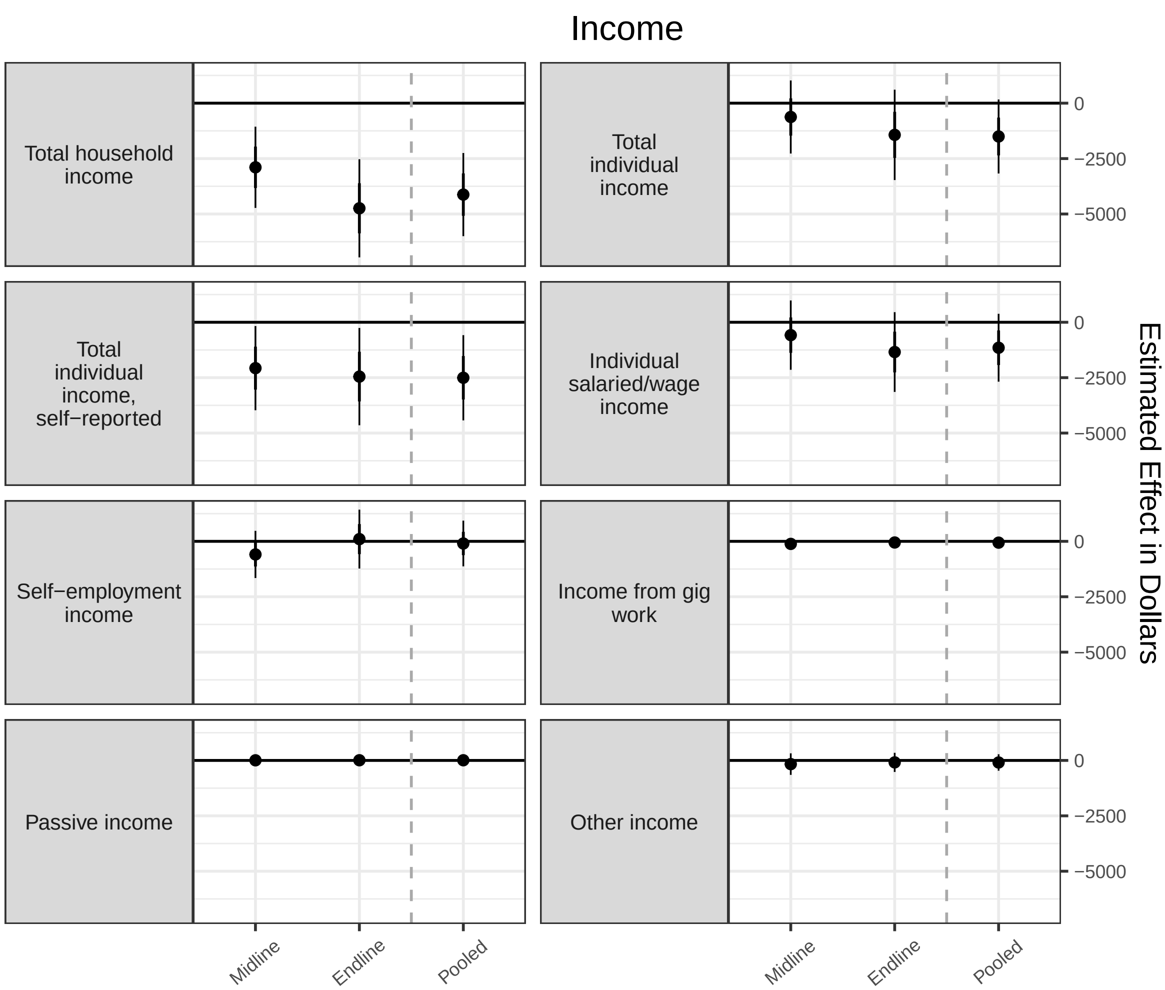
Note: This graph displays a frequency distribution of participants' average treatment assignments, based on 1,000 simulated runs of the assignment process. The vertical line on the graph is positioned at 0.33333, representing the 1 in 3 probability of assignment.

Figure A12: QQ-plot of Treatment Probability against Bernoulli Distribution



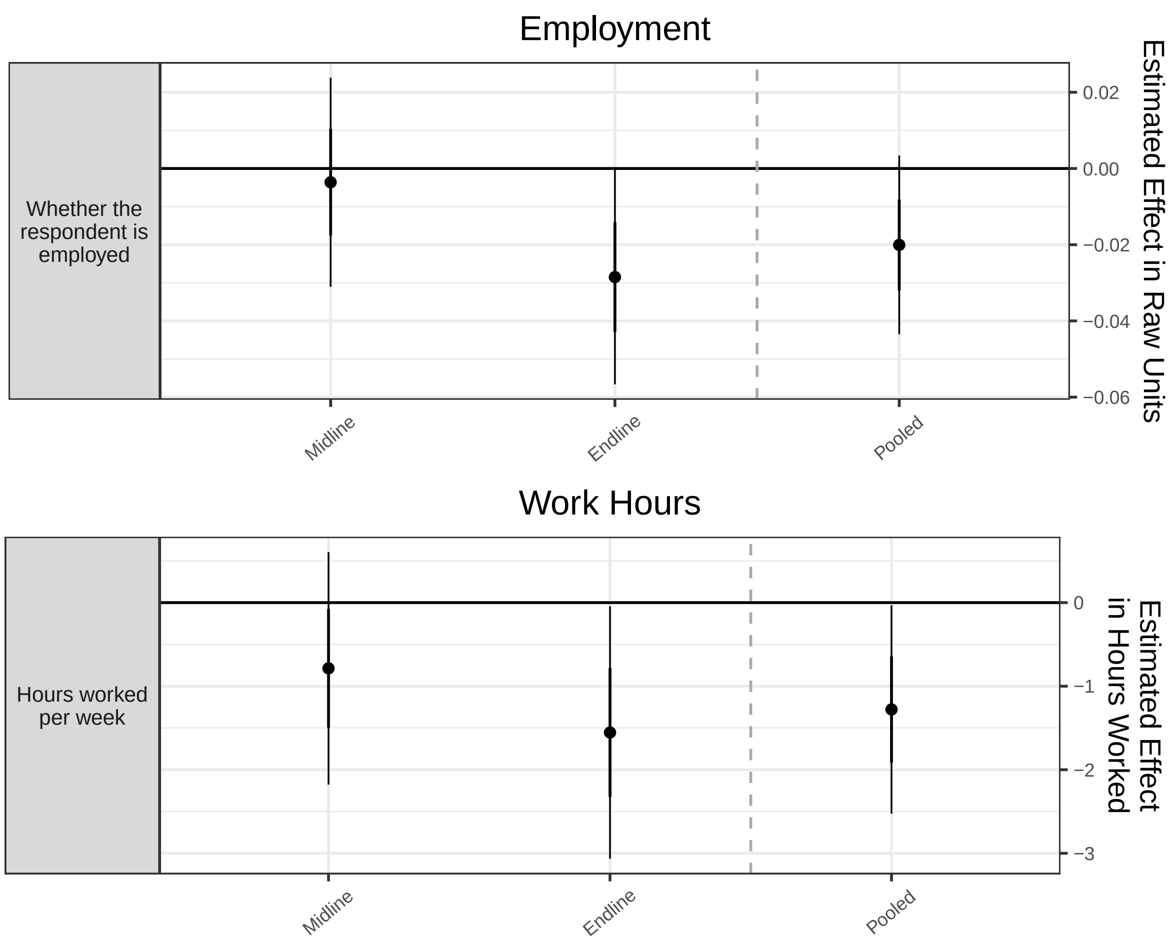
Note: This graph compares the actual distribution of treatment assignments with the theoretical distribution expected from a random assignment process where each participant has a one in three chance of being assigned to the treatment group. The x-axis shows the quantiles of the observed treatment assignments, 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 A13: Results for Income by Time Period



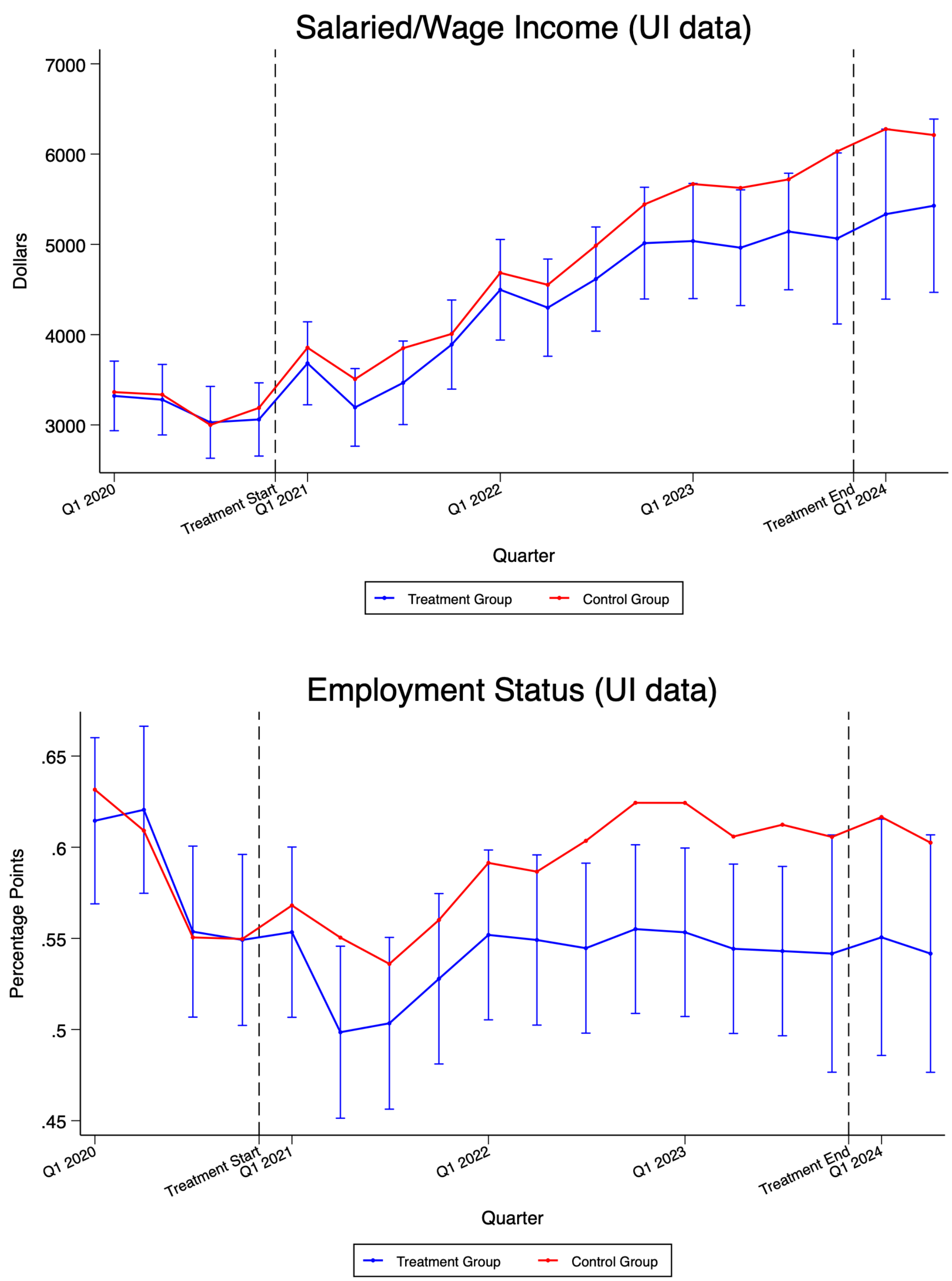
Notes: This figure plots the results for treatment effects on income over time from the survey data, showing a clear time trend in the major categories of income.

Figure A14: Results for Employment by Time Period



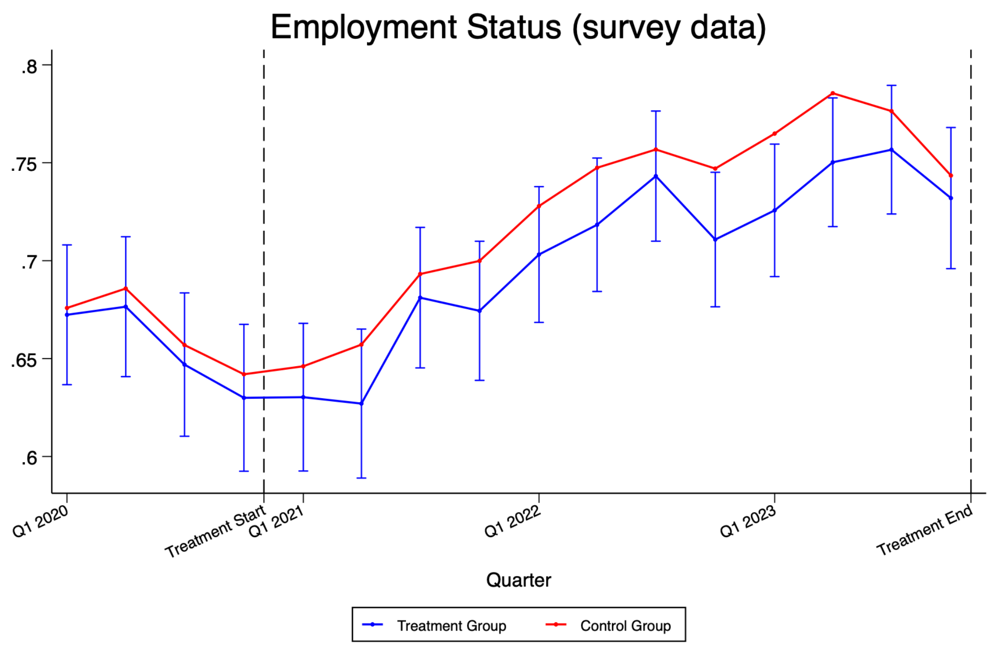
Notes: This figure plots the results for employment over time from the survey data, showing treatment effects on employment trending more negative towards the end of the study.

Figure A15: Results for Income and Employment Over Time: Quarterly UI Data



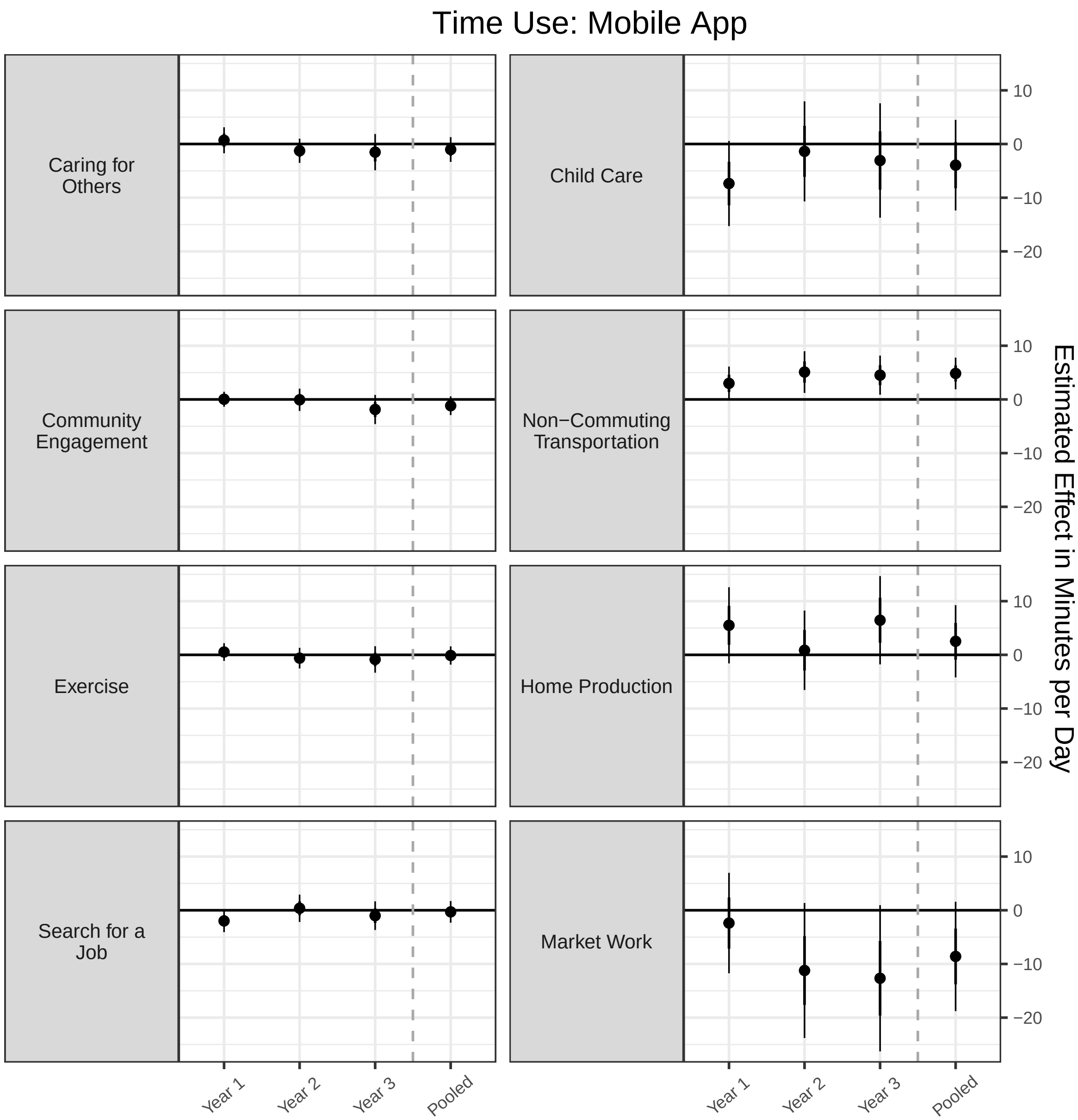
Notes: This figure plots the results for income and employment over time, using data from UI data in each state for those who could be matched. The data points in this figure represent estimated effects on individual salaried income or employment for the preceding quarter and are formed via regressions within each quarter (*i.e.*, the value for the treatment group is the estimated treatment effect added to the constant term, with confidence intervals). No controls are included in these regressions. The last three quarters use data for Texas only as administrative data for the end of the study has yet to be made available in Illinois.

Figure A16: Results for Employment Over Time: Quarterly Survey Data



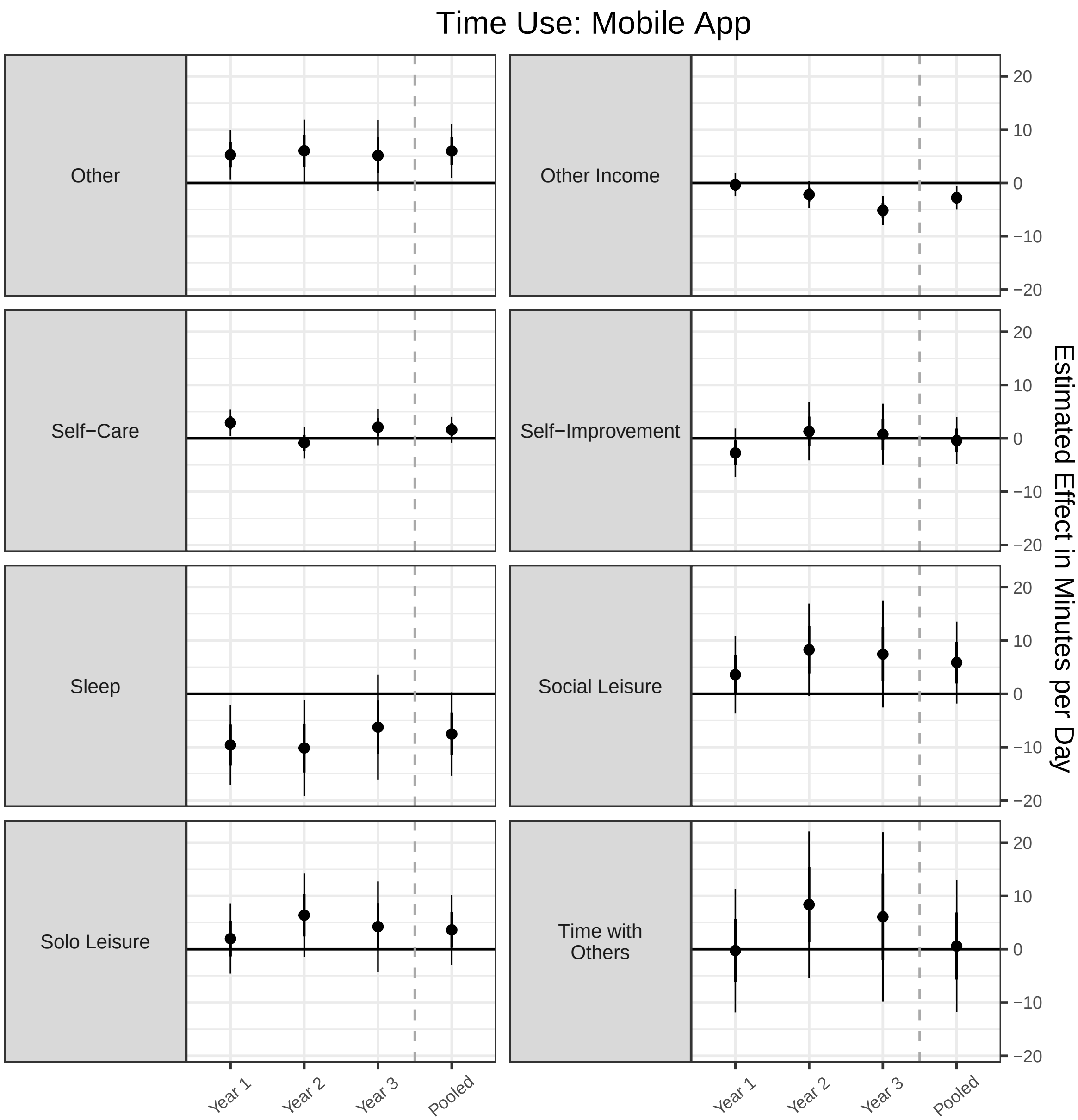
Notes: This figure plots the results for employment over time, using survey data. Employment history is constructed from a several different survey questions. First, the baseline/midline/endline are used as tentpoles, as participants are asked which months they were employed over the past year. Second, we fill in the gaps using data from the more frequent web-based surveys. The data points in this figure represent whether participants were ever employed during 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, with confidence intervals). No controls are included in these regressions.

Figure A17: Results for Time Use by Time Period: Mobile App (1)



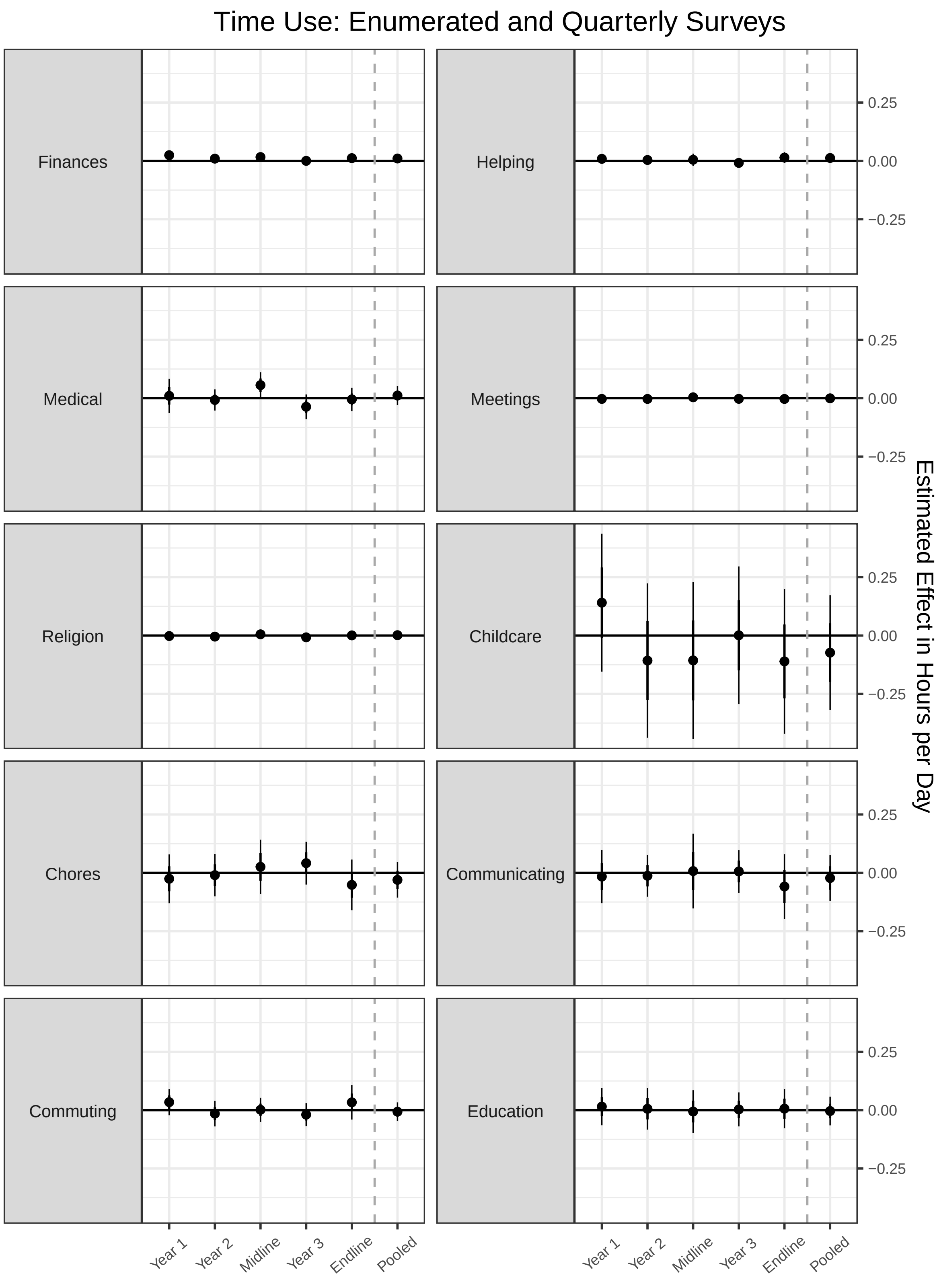
Notes: This figure plots the results for time use over time, using data from the mobile app.

Figure A18: Results for Time Use by Time Period: Mobile App (2)



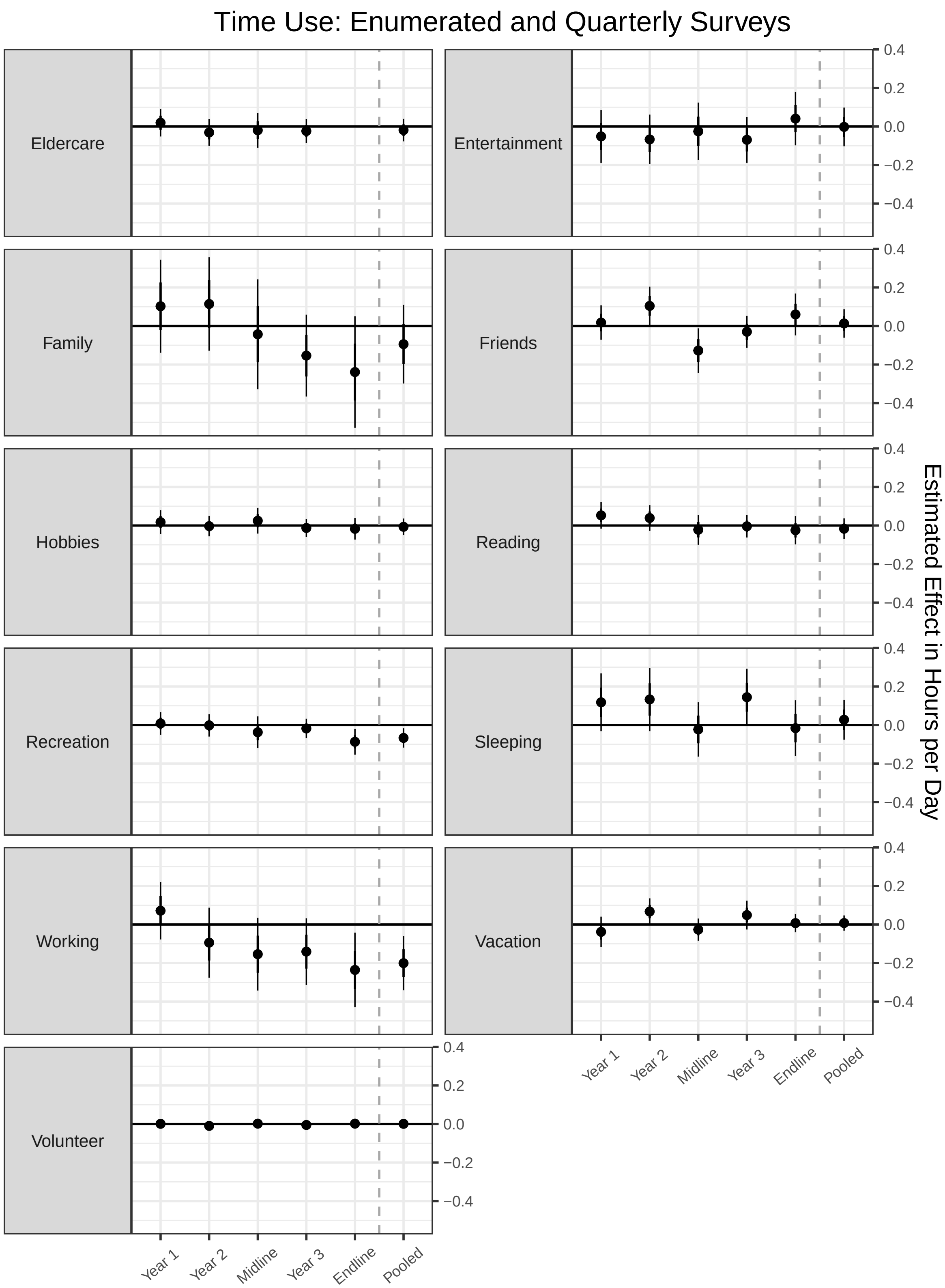
Notes: This figure plots the results for time use over time, using data from the mobile app.

Figure A19: Results for Time Use by Time Period: Enumerated and Quarterly Surveys (1)



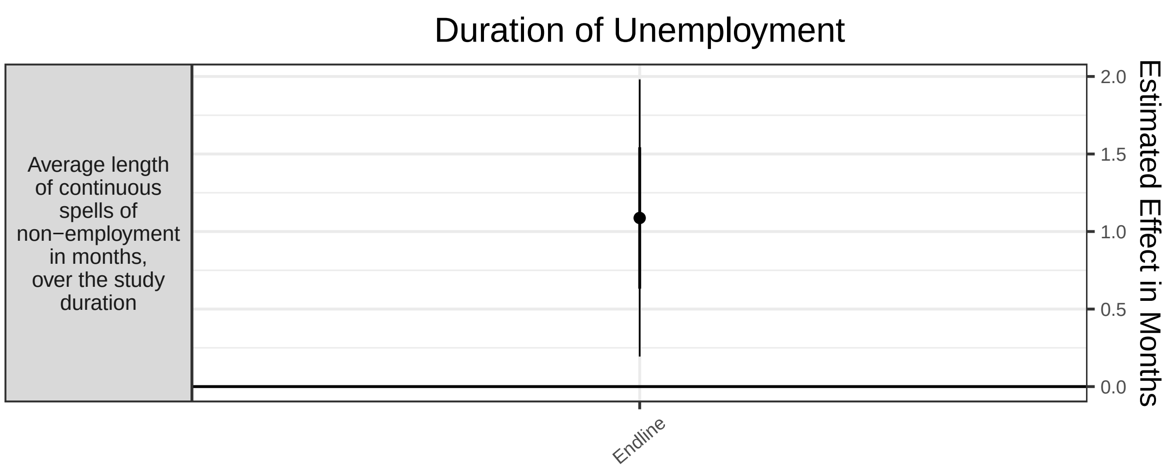
Notes: This figure plots the results for time use over time, using data from enumerated and quarterly surveys.

Figure A20: Results for Time Use by Time Period: Enumerated and Quarterly Surveys (2)



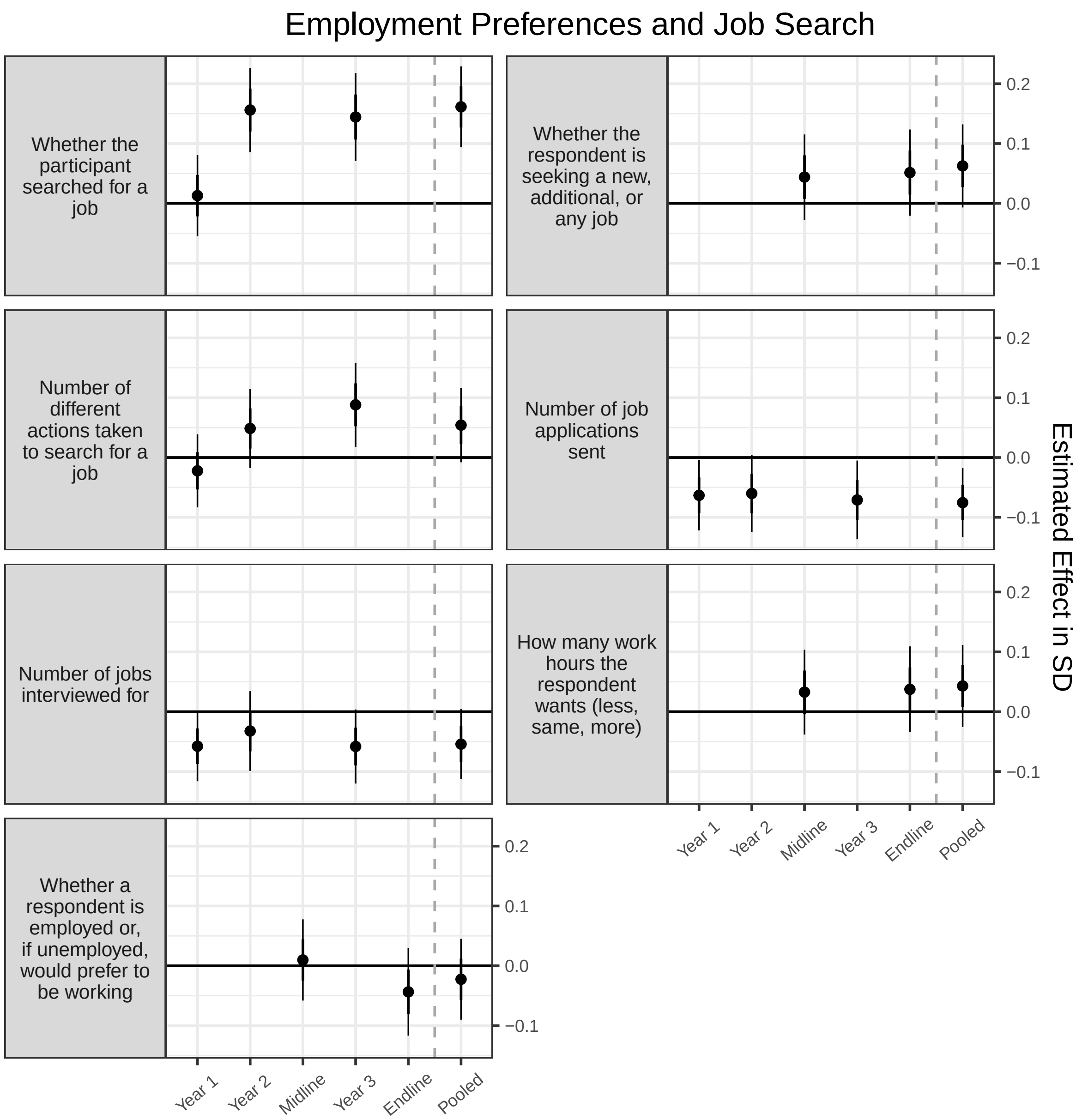
Notes: This figure plots the results for time use over time, using data from enumerated and quarterly surveys.

Figure A21: Results for Duration of Unemployment by Time Period



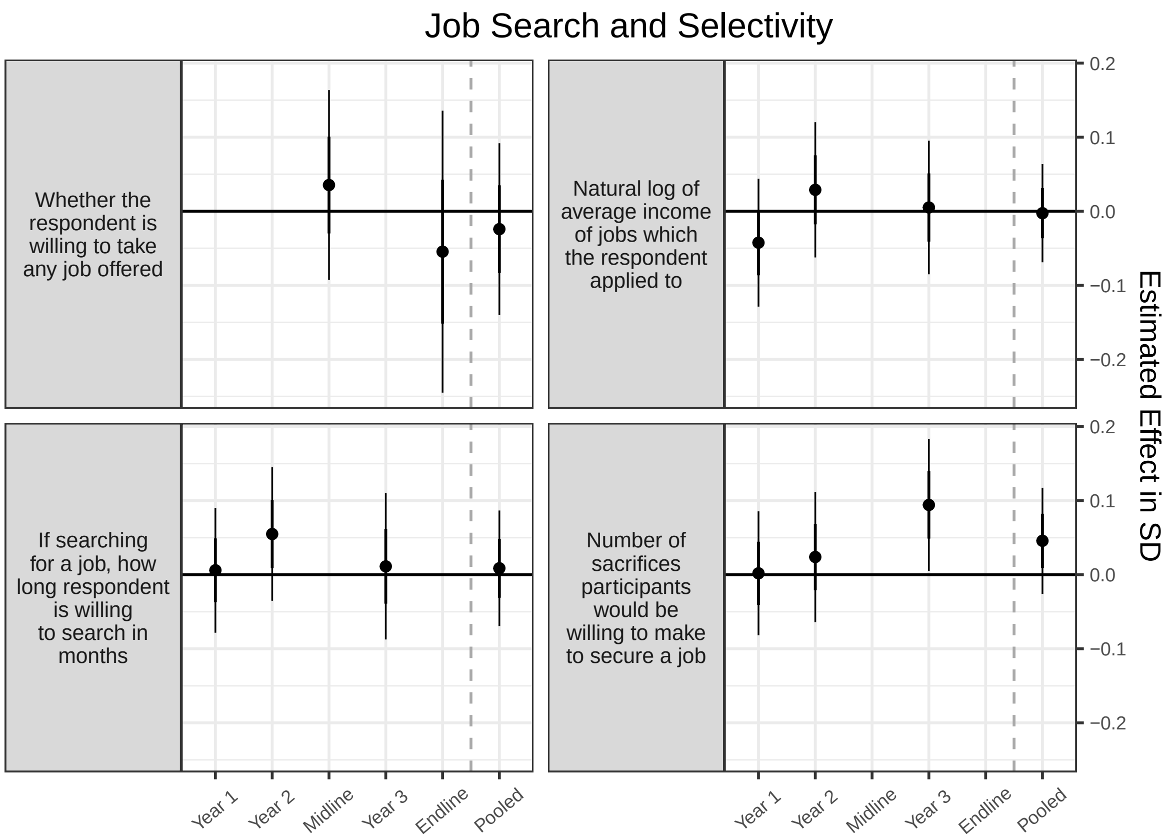
Notes: This figure plots the results of the estimates of the transfers on duration of unemployment over time.

Figure A22: Results for Employment Preferences and Job Search by Time Period



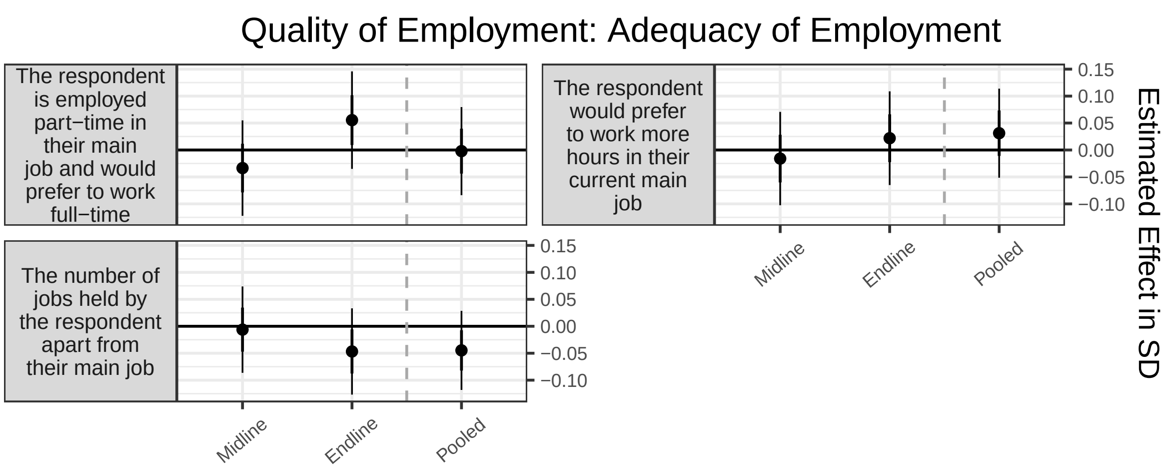
Notes: This figure plots the results of the estimates of the transfers on employment preferences and job search over time.

Figure A23: Results for Selectivity of Job Search by Time Period



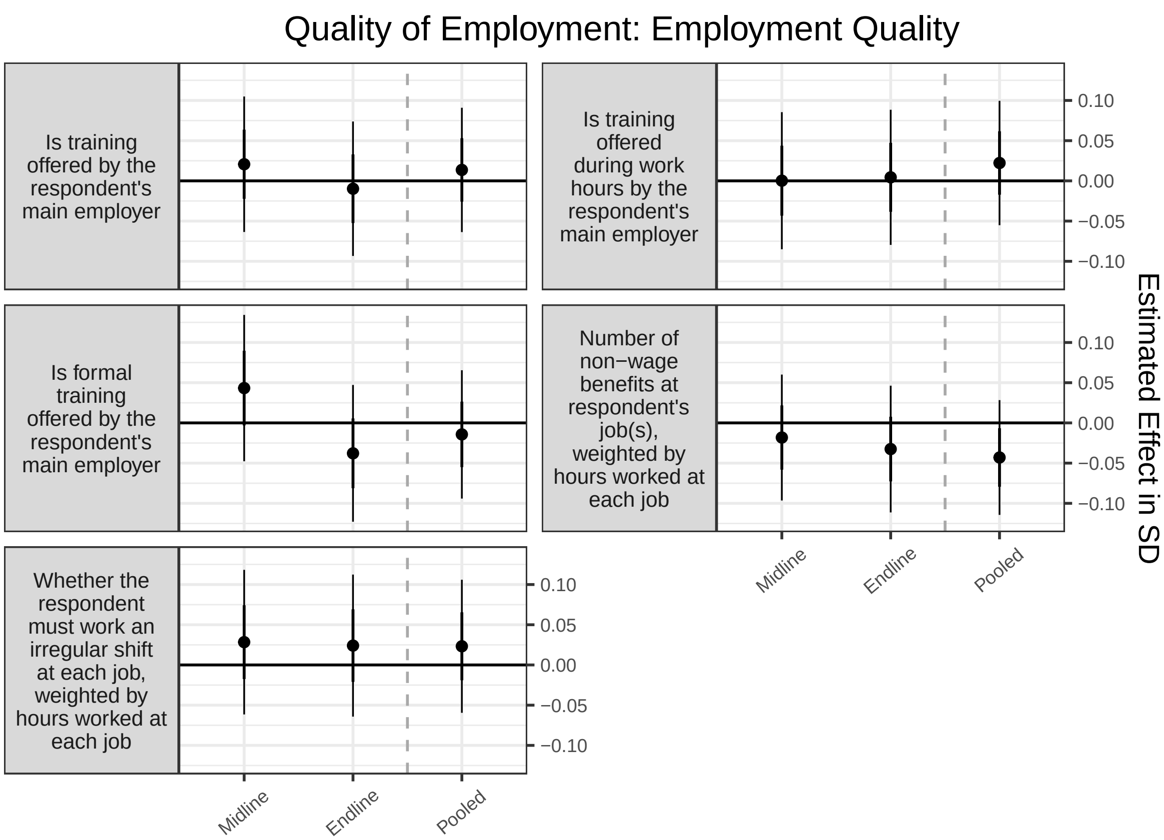
Notes: This figure plots the results of the estimates of the transfers on selectivity of job search over time.

Figure A24: Results for Adequacy of Employment by Time Period



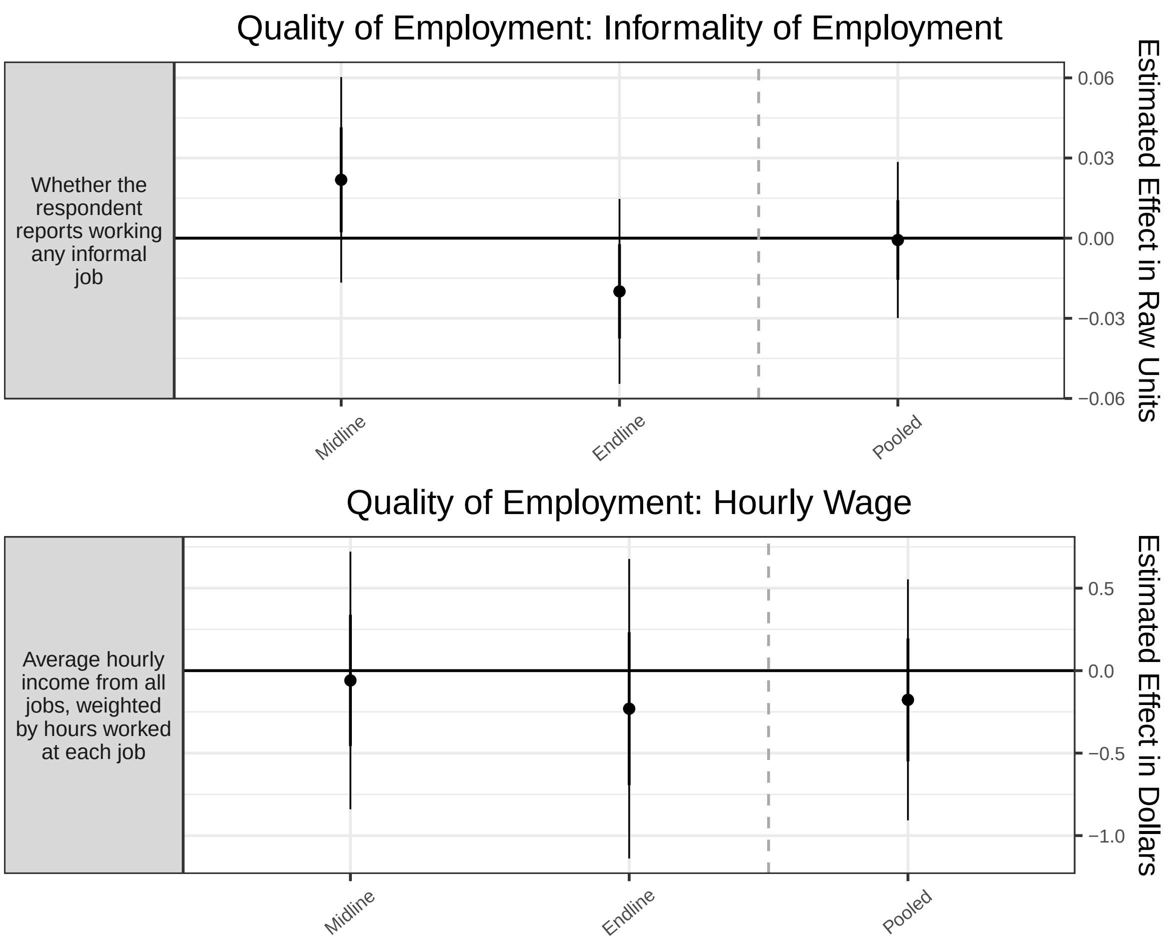
Notes: This figure plots the results of the estimates of the transfers on adequacy of employment over time.

Figure A25: Results for Employment Quality by Time Period



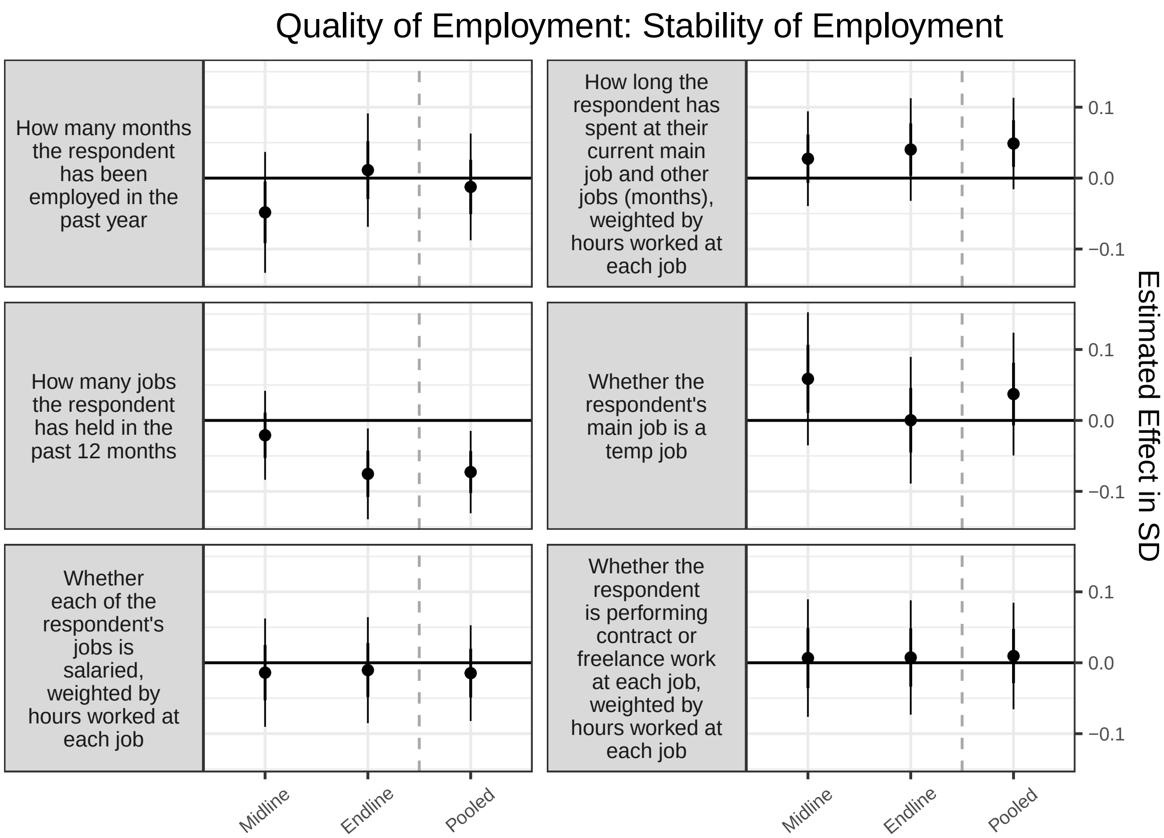
Notes: This figure plots the results of the estimates of the transfers on employment quality over time.

Figure A26: Results for Informality and Hourly Wage by Time Period



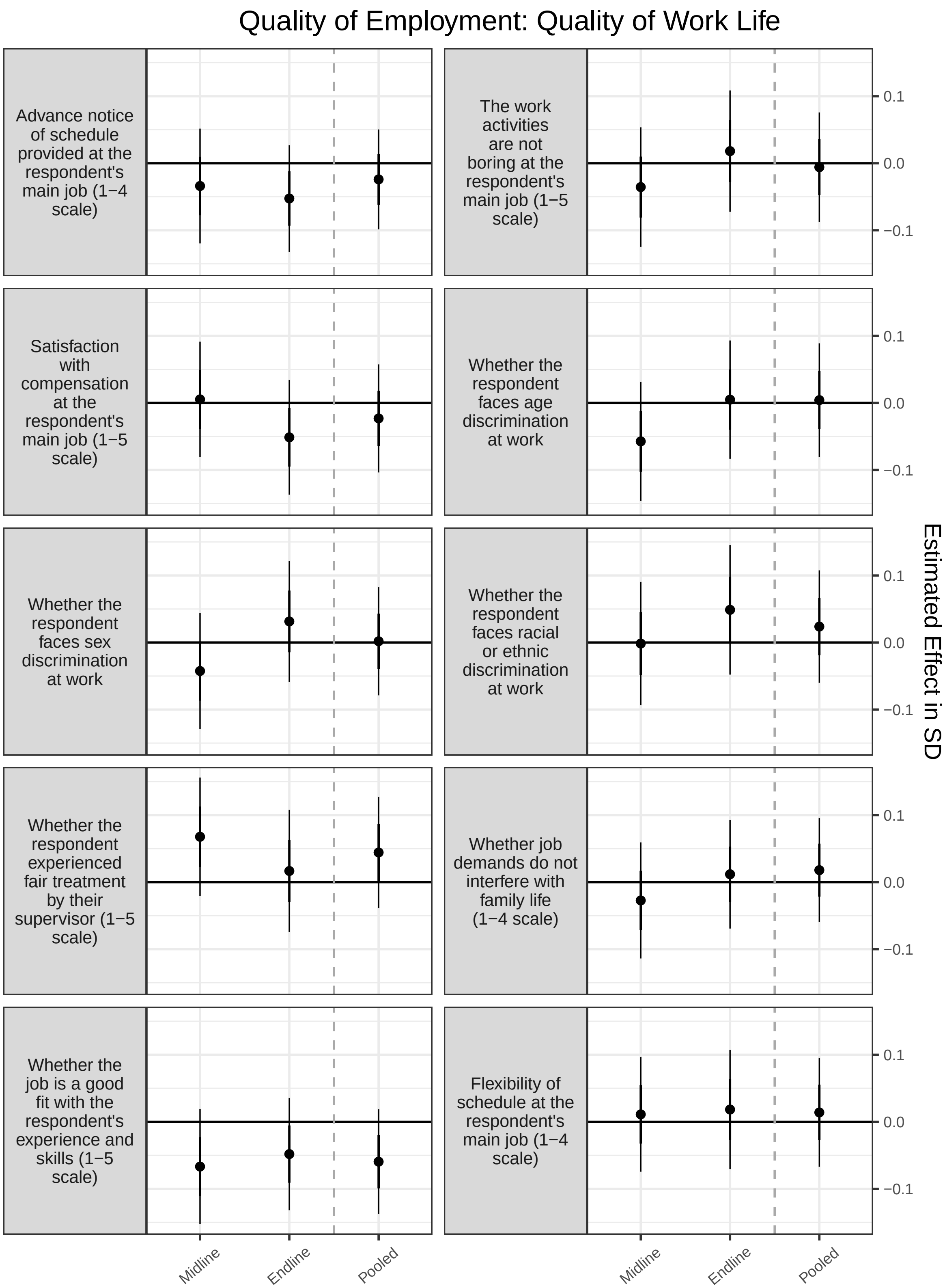
Notes: This figure plots the results of the estimates of the transfers on informality and hourly wage over time.

Figure A27: Results for Stability of Employment by Time Period



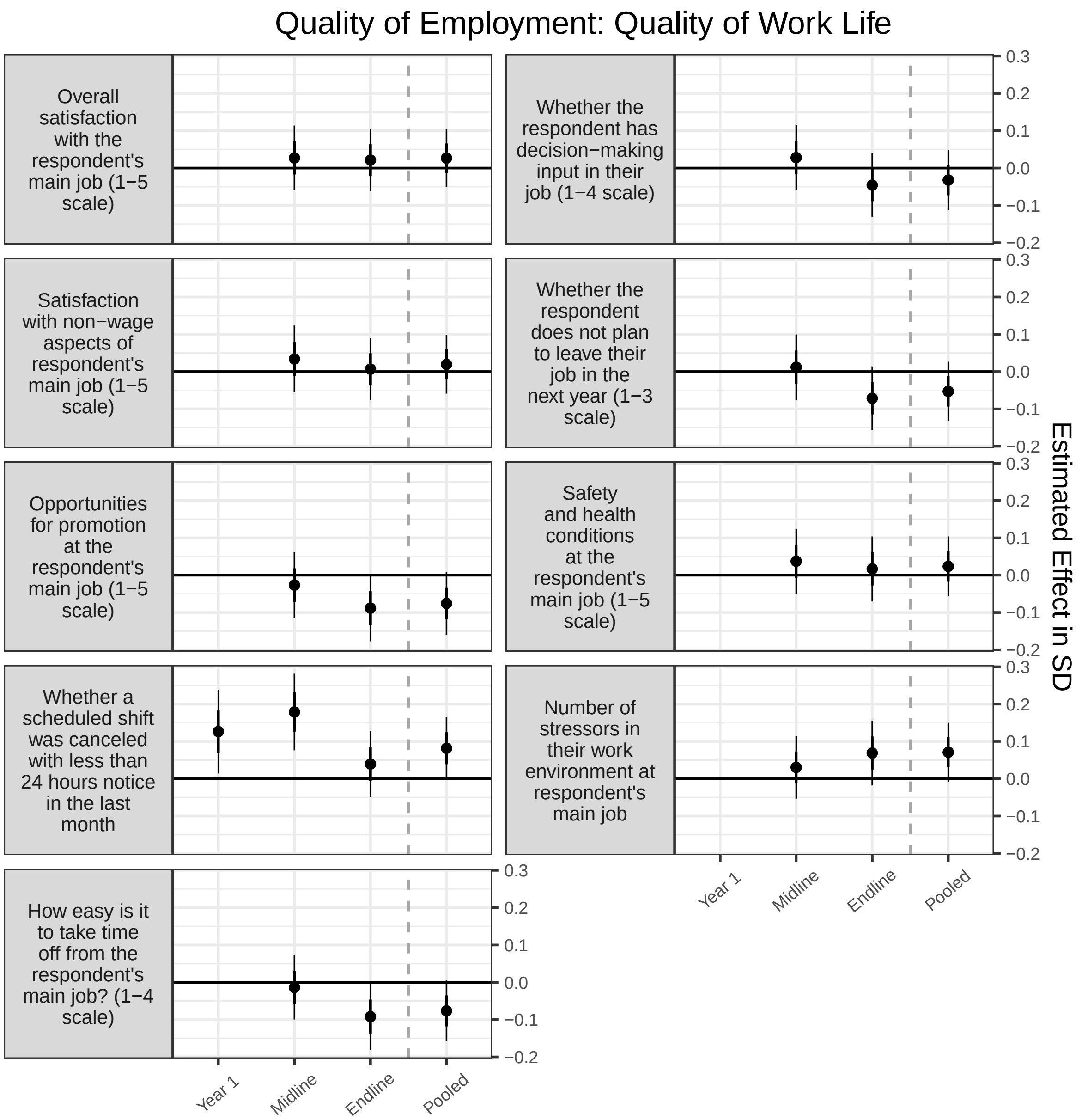
Notes: This figure plots the results of the estimates of the transfers on employment stability over time.

Figure A28: Results for Quality of Work Life by Time Period (1)



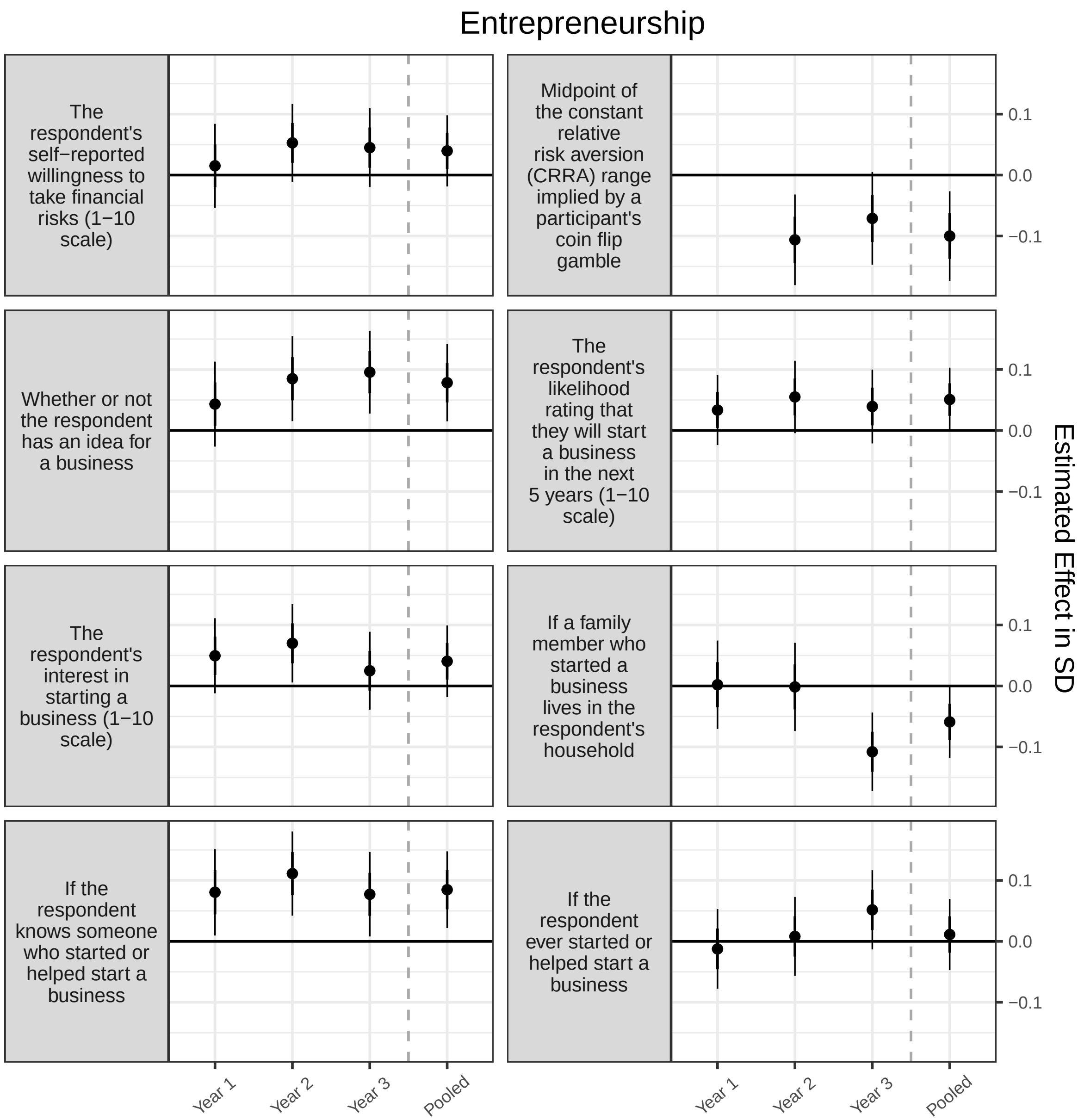
Notes: This figure plots the results of the estimates of the transfers on quality of work life over time.

Figure A29: Results for Quality of Work Life by Time Period (2)



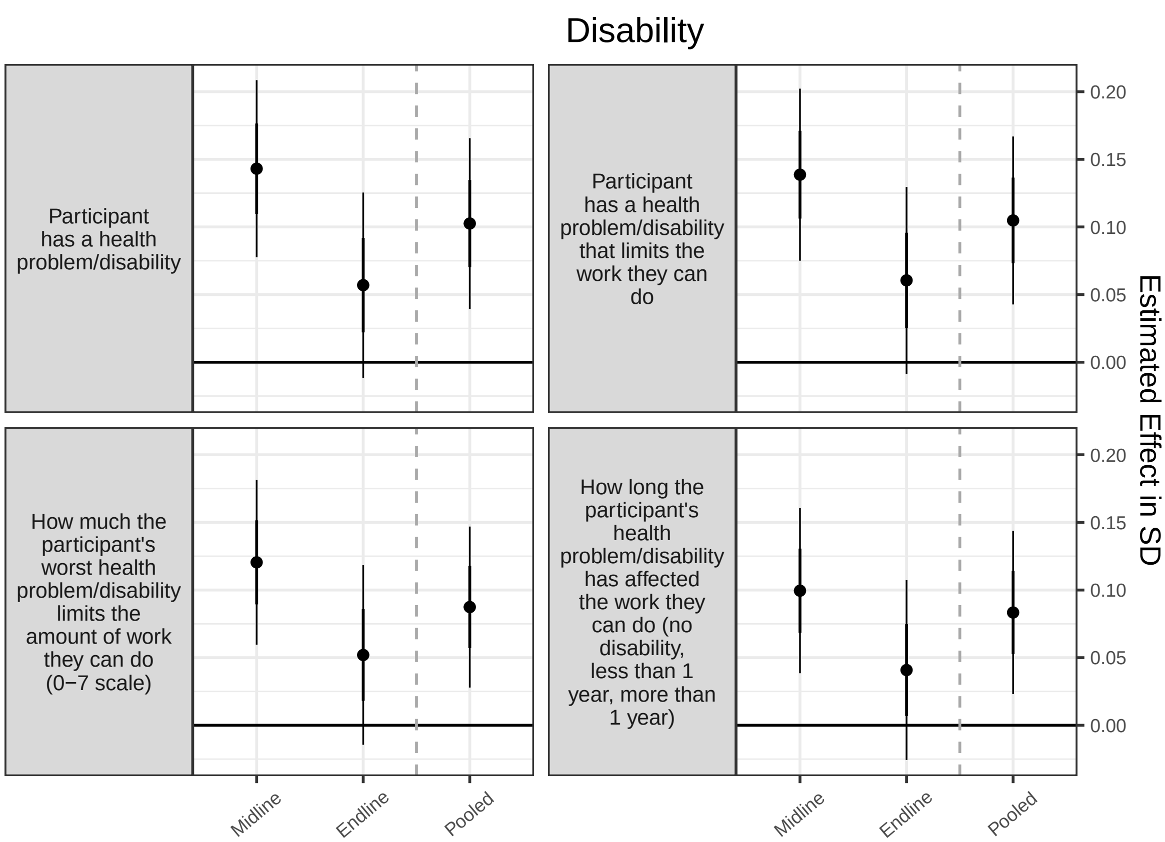
Notes: This figure plots the results of the estimates of the transfers on quality of work life over time.

Figure A30: Results for Entrepreneurship by Time Period



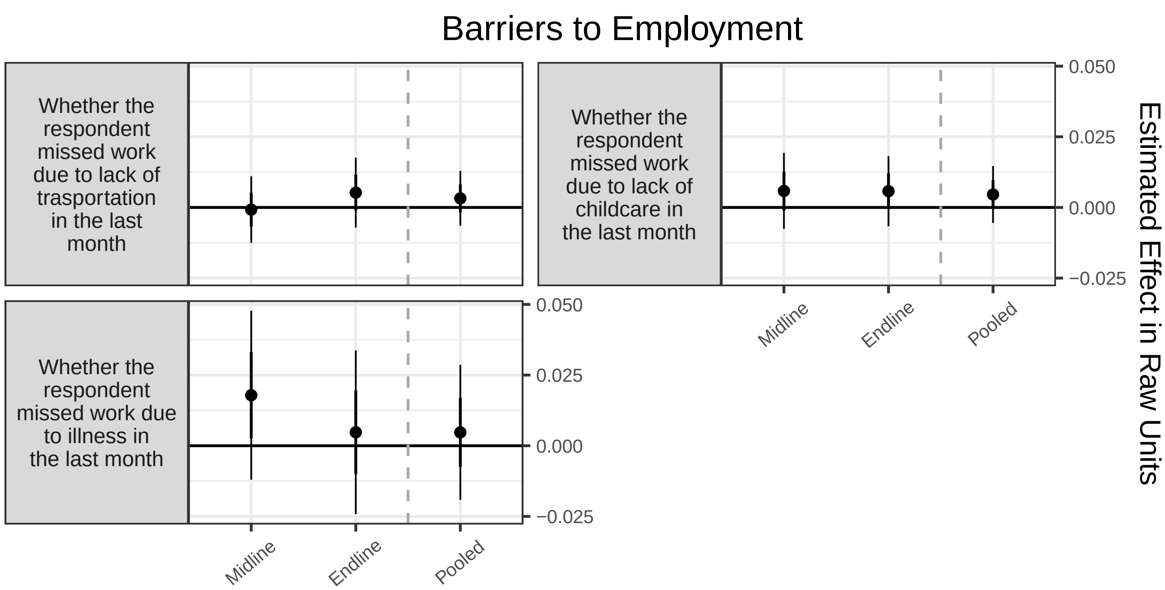
Notes: This figure plots the results of the estimates of the transfers on entrepreneurship over time.

Figure A31: Results for Disability by Time Period



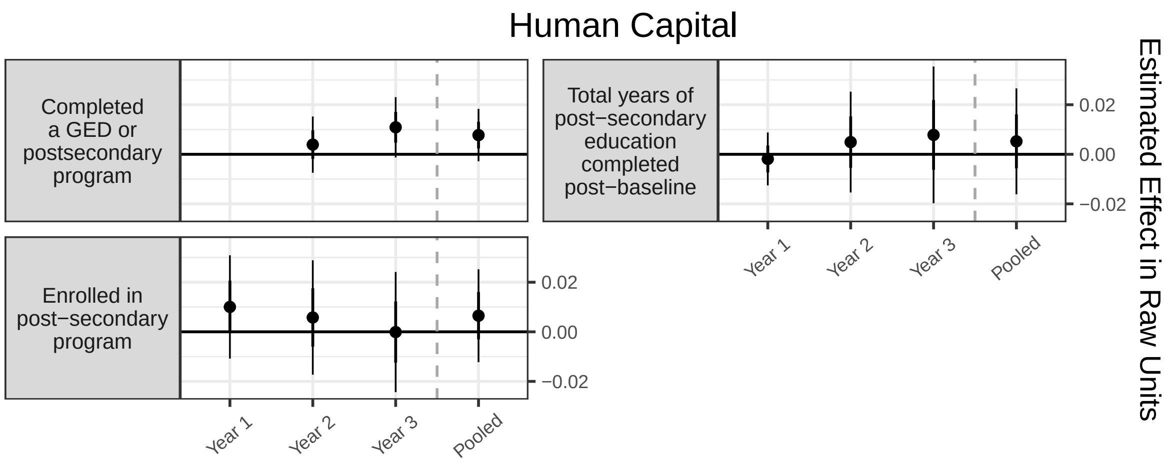
Notes: This figure plots the results of the estimates of the transfers on disability over time.

Figure A32: Results for Barriers to Employment by Time Period



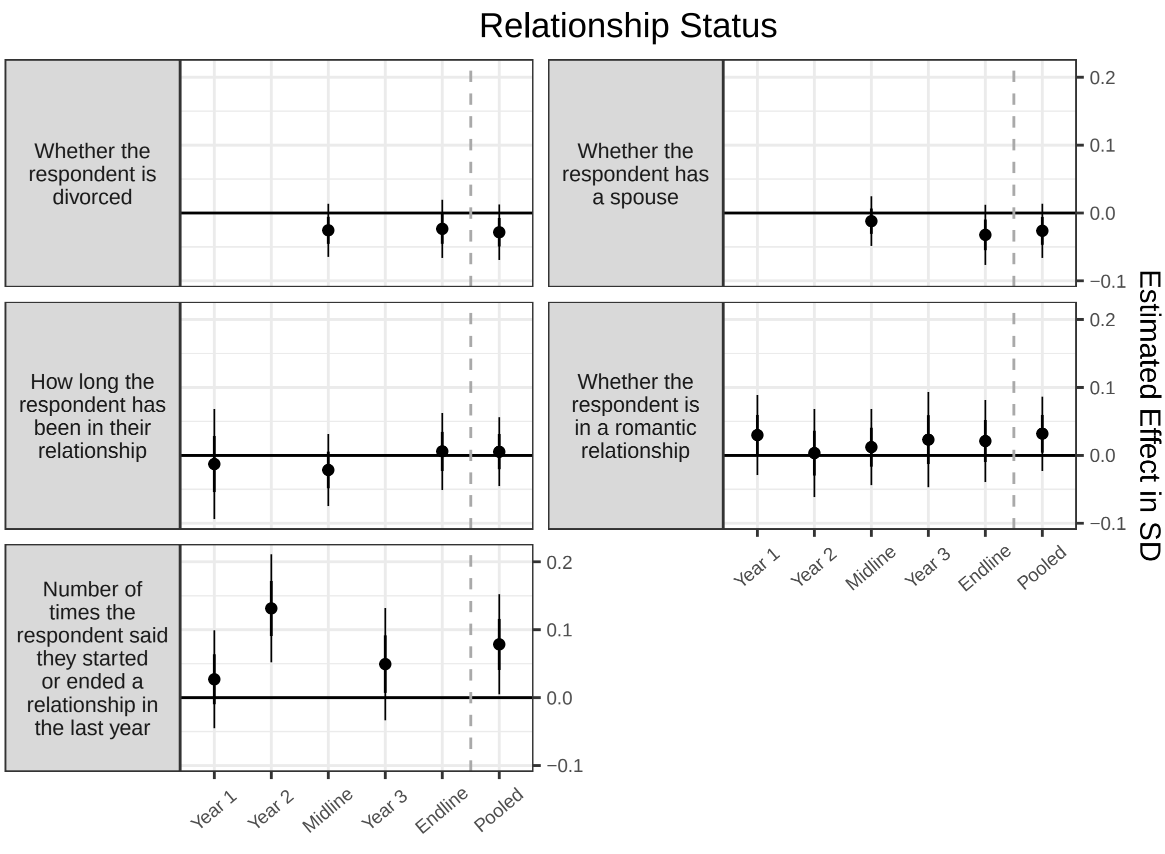
Notes: This figure plots the results of the estimates of the transfers on barriers to employment over time.

Figure A33: Results for Human Capital by Time Period



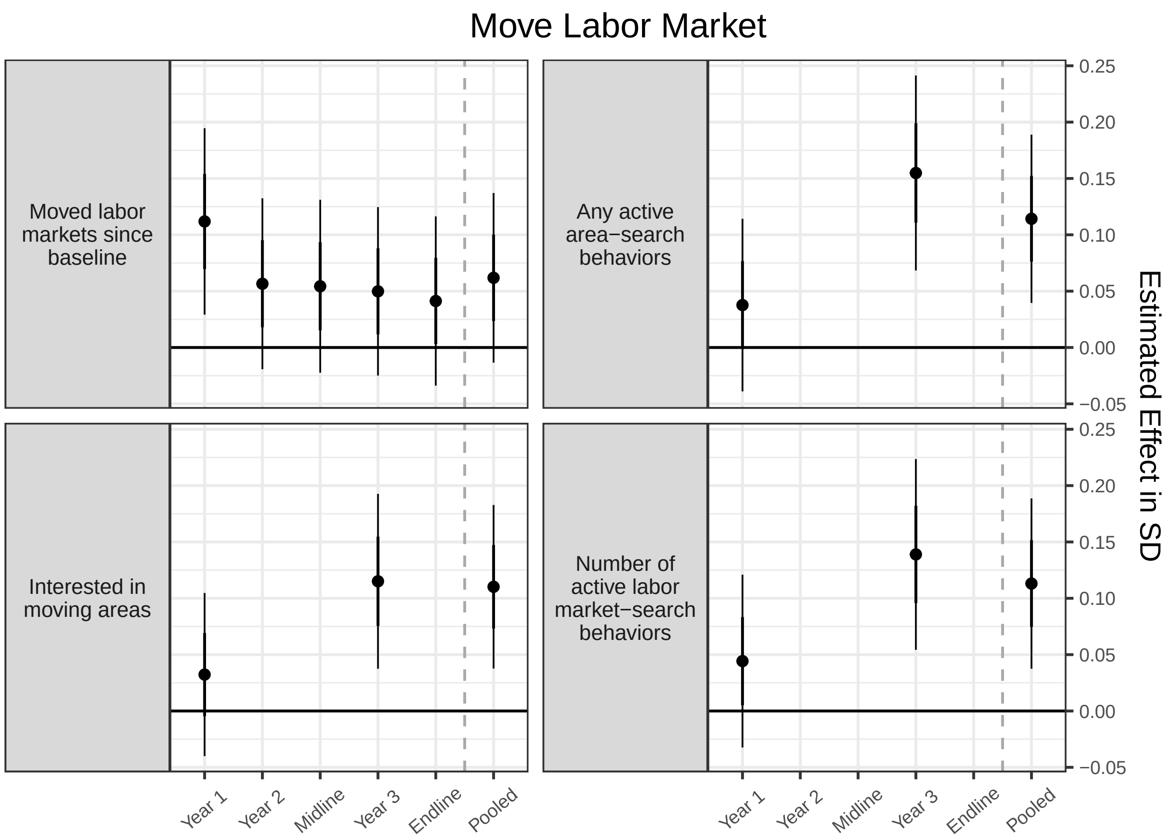
Notes: This figure plots the results for human capital over time, showing the point estimates for completion of a GED or post-secondary program trending upwards by the end of the study. There is no value for this variable for Year 1 because participants were only asked about whether they had completed 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.

Figure A34: Results for Relationship Status by Time Period



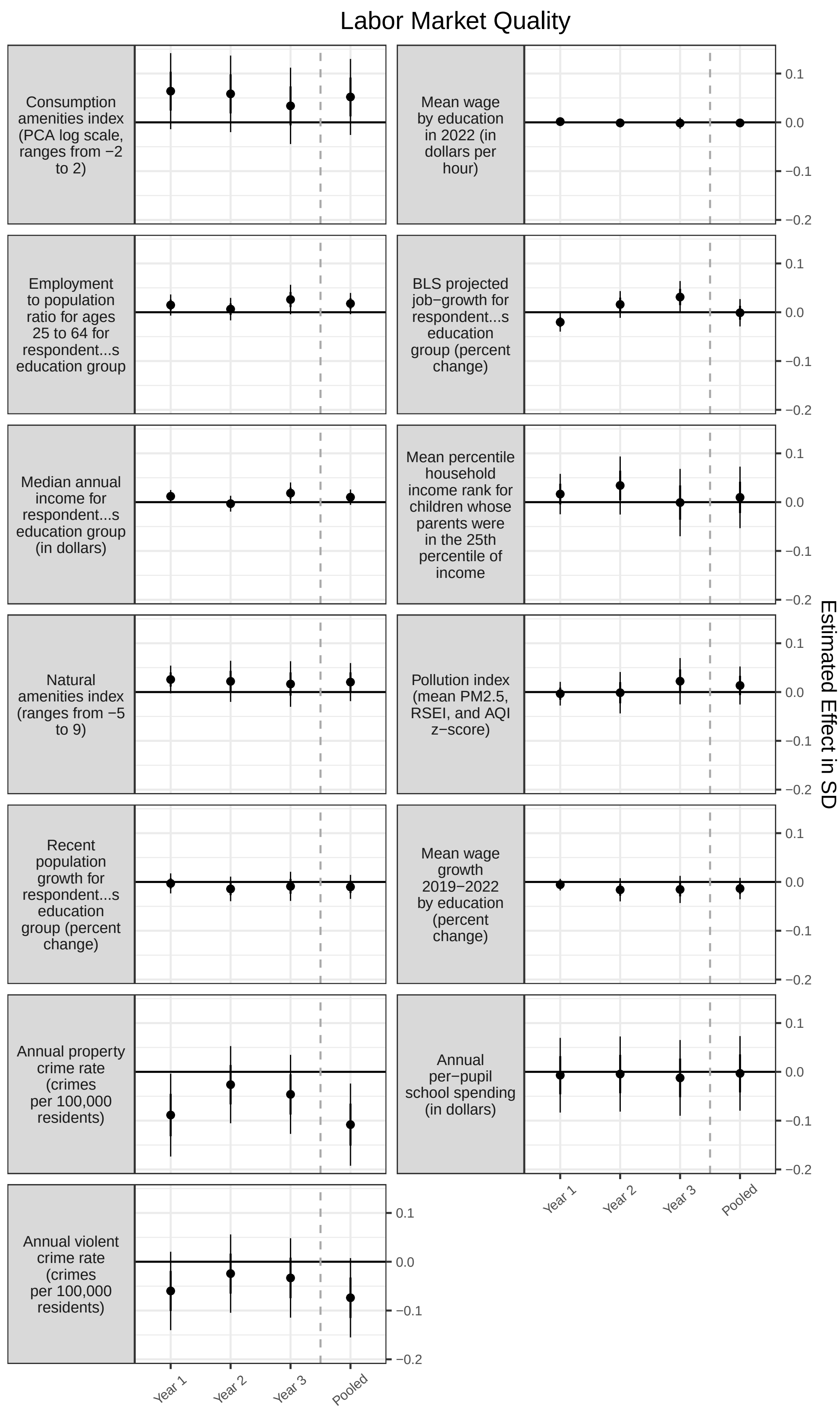
Notes: This figure plots the results of the estimates of the transfers on relationship status over time.

Figure A35: Results for Moving Labor Markets by Time Period



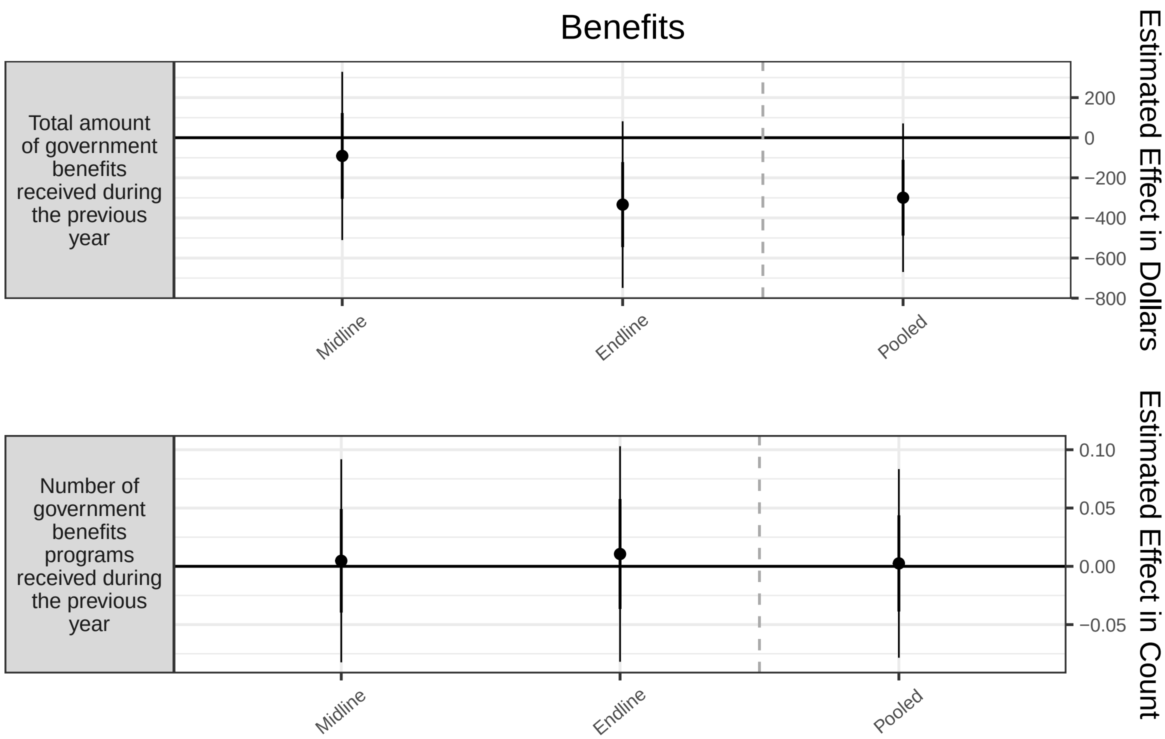
Notes: This figure plots the results of the estimates of the transfers on moving labor markets over time.

Figure A36: Results for Quality of Labor Market by Time Period



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

Figure A37: Results for Benefits by Time Period



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