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THE CAUSAL EFFECTS OF INCOME ON POLITICAL ATTITUDES AND BEHAVIOR: A RANDOMIZED FIELD EXPERIMENT

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ABSTRACT

We study the causal effects of income on political attitudes and behavior with a field experiment. In the experiment, a non-profit gifted 1,000 low-income Americans $1,000 per month for three years tax-free, and 2,000 control participants $50 monthly. Contrary to resource models of participation, we find no effects on political participation or engagement, and rule out effects equivalent to the observational association between turnout and income. Political preferences largely do not change, with the estimates again distinguishable from the observational relationship that economic conservatism increases with income. Dispositions such as trust in government, polarization, and support for democracy also do not change. We do find effects consistent with mood misattribution: affect towards one's own racial group, other racial groups, and some politicians slightly improves. There is also some evidence that treated participants saw work as more important for individuals, society, or even as a requirement for accessing government programs; qualitative evidence illuminates potential mechanisms. Our findings contrast with findings from other economic shocks such as government-sponsored or taxable transfers—thereby helping clarify the mechanisms likely responsible for their effects—and underscore the durability of political predispositions.

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A randomized controlled trials registry entry is available at
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1 Introduction

Income correlates with many political attitudes and behaviors, including voter turnout, policy preferences, intergroup attitudes, trust in government, and support for democracy. A range of foundational theories posit explanations for why these correlations may be causal. For example, resource theories of political participation (e.g., Brady, Verba and Schlozman, 1995) argue that income has a causal effect on political participation because individuals with more resources (time, money, etc.) are more easily able to follow political affairs and participate in politics. Likewise, a large literature argues that economic insecurity increases support for populist parties by, e.g., reducing trust, increasing authoritarian aggression, or increasing resentment (for review, see Margalit, 2019*a*); or, conversely, that income growth increases support for incumbents (e.g., Achen and Bartels, 2016; Healy and Malhotra, 2013).

Yet other theory and evidence suggests that these relationships may not be due to the causal effects of income. First, the political effects of many income shocks may not be due to income itself, but instead to other changes which accompany those shocks. For example, the experience of receiving a government transfer, losing one's job due to import competition, winning a state-sponsored lottery, or receiving a large raise from one's employer may change political attitudes and behavior for other reasons, such as the experience of interacting with government to claim a benefit, changing attributions of responsibility for one's economic circumstances, changing marginal tax rates, or learning about the labor market (for review, see Margalit, 2019*b*). Second, neither income itself nor most income shocks are randomly assigned, frustrating causal inferences. There are also reasons to doubt that observational associations between income and political attitudes and behaviors are causal. Dispositions such as party identification, racial attitudes, and interest in politics are malleable in one's "impressionable years," but then tend to remain stable in the decades afterwards (Ghitza, Gelman and Auerbach, 2023; Green and Palmquist, 1994; Prior, 2019; Sears and Funk, 1999). Indeed, shocks that affect political preferences and behavior often decay rapidly, as individuals return to a pre-existing baseline (Bechtel and Hainmueller, 2011; Coppock and Green, 2016). Publication bias in this literature is also of concern (Margalit, 2019*b*).

This paper provides new evidence on the causal relationship between income and political attitudes and behaviors from a large-scale, pre-registered, randomized field experiment on a guaranteed income—the OpenResearch Unconditional income Study (ORUS). This study occurred in the United States between November 2020 and October 2023. To conduct this study, we assisted two non-profit partners in the recruitment of 3,000 low-income adults and randomly assigned 1,000 to receive $1,000 per month for 3 years, while the control group of 2,000 adults received $50 per month over the same period. Over $40 million was given away in total as part of the study (in addition to nearly $4 million in research participation incentives). The sample was comprised of low-income adults in the US. At baseline, the average household income in the sample was slightly under $30,000, meaning the $12,000 annual transfer represented a sizable change in income. The study was entirely privately funded, and we show that participants largely did not credit the government for their receipt of the funds. As a gift from a private charity, the transfer was not taxable, meaning it did not change participants' marginal tax rates and should have minimal direct effects on participants' views towards government. We were also careful to ensure the transfer did not affect receipt of government benefits when possible. The large absolute and relative size of the transfer, the prolonged period of its administration, the meaningful sample size, the randomized design, and the private funding source therefore present a unique opportunity to study the causal effects of income, complementing previous research.

To measure the effects of the transfer, we administered longitudinal surveys measuring participants' political attitudes before and during the transfer period (among other variables analyzed in companion papers, such as income, consumption, and health). We also measured voter registration and turnout from administrative records. Alongside this quantitative data collection, we conducted five rounds of semi-structured in-depth interviews before and throughout the transfer period.

Contrary to resource models of participation, we find that this large cash transfer had no effects on voter registration, turnout, or political knowledge, and rule out effects equivalent to the association between income and participation. Political preferences largely do not change, with the estimates again distinguishable from the observational relationship that economic conservatism increases with income. Dispositions such as trust in government, authoritarianism, and support for democracy also do not change. Using machine learning, we do not find any evidence of heterogeneous treatment effects (Inoue et al., 2024). These results contrast with findings from the large literatures on the observational relationships between income and political attitudes and behaviors, as well as the causal effects of government-sponsored programs or shocks which change taxable income. Our null findings on turnout and political preferences thus bolster interpretations of previous results on the effects of other economic shocks that stress the importance of changing economic incentives and recipients' attributions of responsibility for the shocks in generating effects on political attitudes or behavior. These findings also contrast with psychological theories that would expect direct effects of income on political preferences, underscoring the durability of political predispositions.

Qualitative interviews were consistent with these null findings. When describing their decisions regarding whether to participate in politics or their policy attitudes, participants in the treatment and control groups provided similar comments, and to the extent they referenced the transfer they largely interpreted it through the lens of longstanding attitudes.

We observed effects in three areas. First, consistent with mood misattribution (Schwarz and Clore, 1983), affect towards one's own racial group, other racial groups, and some politicians slightly improves. However, these effects are not large. Second, participants said they would give away more money to others in a hypothetical scenario (and did give away more money to friends and family), although there were more limited differences in other forms of pro-social intent. Finally, treated participants saw work as more important for individuals, society, and even as a requirement for accessing government programs. Qualitative evidence offers several potential mechanisms for this finding. First, participants receiving the transfer sought to distance themselves from others who might have used the transfer "the wrong way" (e.g., to work less). Second, some recipients used the transfer in ways that affirmed their beliefs about their own work ethic and ability to get ahead through work. In addition, some participants who felt the additional income particularly benefited them expressed greater motivation to earn future income as a result.

Our findings regarding the generally limited effects of a large, exogenous change in economic circumstances on individuals' political attitudes and behaviors have several implications. First, these findings underscore the durability of political predispositions. Second, our findings complement research from prior studies on the effects of income shocks for which government was directly or plausibly indirectly responsible or which change marginal tax rates. In particular, our contrasting findings reinforce interpretations of prior studies that voters' reaction to income shocks depend greatly on the particular features of those shocks, such as whether they attribute those shocks to government (e.g., Alik-Lagrange et al., 2021; Hamel, 2024) and how the shocks change their economic incentives (e.g., Meltzer and Richard, 1981). Last, our findings regarding effects on attitudes towards others and towards work suggest opportunities for future research that we explore in the discussion section.

2 Theoretical Perspectives: Income's Effect on Political Attitudes and Behavior

Existing research offers a number of hypotheses to account for the relationship between income and various political attitudes and behaviors. However, much of the causally identified empirical research on this topic studies the effects of income shocks which accompany other changes that could have (and existing research argues do have) their own effects. For example, many studies examine the effects of shocks for which government was either directly responsible (such as visible government transfers) or plausibly indirectly responsible (such as losing one's job during an economic downturn). In line with research on traceability, however, the impacts of income on political attitudes and behaviors might be more muted in cases when government is not plausibly responsible for the change. Likewise, shocks to income which accompany changes in employment status or taxable income might affect attitudes through other mechanisms as well, such by changing marginal tax rates. In line with some other research, we theorize that it is these concomitant changes which may be responsible for effects observed in previous literature.

Political participation. A long line of research has documented a strong relationship between measures of economic resources and political participation such as voting. The resource model of political participation (Verba et al., 1995) argues that resources such as money and free time are causally related to political participation. For instance, if someone has more free time, they may be more likely to be able to turn out to vote or to follow political affairs; likewise, those with more income may find it easier to afford to take time off work to vote. In a companion paper (Vivalt et al., 2024), we found that the cash transfer we study modestly reduced working hours and increased time spent on leisure activities, so such an effect is plausible in our setting.

Some studies of the effects of government-sponsored transfer programs find that they increase turnout: Alaska's Permanent Fund Divided program appears to increase turnout (James, Rivera and Smith, 2022), as did a randomized government cash transfer in Finland (Hirvonen, Schafer and Tukiainen, 2024) and randomized access to Medicaid in Oregon (Baicker and Finkelstein, 2018). Other studies, though, find no effect on turnout from cash transfers or other wealth shocks (Blattman, Emeriau and Fiala, 2018; Blattman, Fiala and Martinez, 2020; Brännlund et al., 2024). Some studies even find potential *negative* effects of employment or income on participation: Grøtting (2024) find that becoming *unemployed* actually *increases* turnout; and Charles and Stephens (2013) found that employment lowers turnout in downballot elections, perhaps because employed people have less time to follow politics (see also Brunner, Ross and Washington, 2011). Other research has found effects of income and especially government cash transfers on other forms of participation, such as contacting politicians (e.g., Schober, 2019; Williams, 2023).

Contrary to expectations from these studies, a different perspective suggests that interest in politics is formed early in life and endures thereafter (Prior, 2019). Consistent with this perspective, some research finds fairly minimal effects of, for example, changes in the cost of voting on voter turnout in competitive elections (e.g., Fraga and Hersh, 2010).

Support for incumbents. A large body of research finds that government-sponsored cash transfer programs increase support for implementing politicians and political parties (e.g., Araújo, 2021; Baez et al., 2012; De La O, 2013; Manacorda, Miguel and Vigorito, 2011; Pop-Eleches, Pop-Eleches et al., 2012; Rendleman and Yoder, 2024; Zucco, 2013), although these results are not universal (e.g., Blattman, Emeriau and Fiala, 2018; Blattman, Fiala and Martinez, 2020; Imai, King and Velasco Rivera, 2020; Ponce and Curvale, 2020; Samuels, 2002).

There are at least two mechanisms for any such effects, which many existing studies of government-sponsored cash transfer programs cannot easily disentangle. The first is that voters seek to reward politicians for efforts which improve their welfare. Consistent with this perspective, some studies find that voters attempt to distinguish between shocks that can and cannot be attributed to politicians' efforts, even though they do so imperfectly (Guiteras and Mobarak, 2015; Hamel, 2024). Politicians thus seek to claim credit for positive economic performance (Vavreck, 2009). A second mechanism is psychological—what psychologists refer to as mood misattribution (Schwarz and Clore, 1983). Under this mechanism, voters in a better mood are more likely to support re-electing incumbents—even if the reason for their improved mood is due to events irrelevant to government performance, such as football team wins (Busby, Druckman and Fredendall, 2017; Graham et al., 2023; Liberini, Redoano and Proto, 2017; Healy, Malhotra and Mo, 2010). Our study offers a unique test of this perspective, as we study a randomized large positive shock which participants should not (and we show do not) attribute to the government.

Attitudes towards other people (ingroup and outgroup members). It is also possible that mood misattribution may spill over into attitudes towards others: being in a better mood might affect attitudes towards other social groups and people, not only incumbents. Such an effect could contribute to Jha, Shayo and Weiss's (2024) finding that being allocated stocks and Di Tella, Galiant and Schargrodsky's (2007) finding that being allocated land titles increases social trust. Relatedly, some research argues that poverty reduces social trust (Betkó et al., 2022). Psychological research suggests the opposite is also possible, however, with some research arguing that those with higher incomes are more likely to believe the world is just and be more prejudiced towards other groups, especially other groups who are less well-off (Carvacho et al., 2013).

Policy preferences. Income is generally correlated with conservative political preferences on economic issues. However, theoretical expectations for the causal effect of a non-governmental cash transfer are ambiguous. Here again, many previous studies find that positive income shocks increase economic conservatism and negative income shocks increase economic liberalism, but there are at least two mechanisms for such effects: changing economic incentives (self-interest) and the direct effect of income itself.

On the former, there is a variety of evidence that has been interpreted as consistent with the self-interest channel (for review, see Margalit, 2019*b*): for instance, squatters who get land rights have more pro-market attitudes (Di Tella, Galiant and Schargrodsky, 2007), positive economic shocks have been found to decrease support for redistributive policies (Brunner, Ross and Washington, 2011), and lottery winners become slightly more conservative, particularly on issues related to wealth and inheritance taxation (Brännlund et al., 2024; Doherty, Gerber and Green, 2006; Peterson, 2016; Powdthavee and Oswald, 2014), among other findings (see also Alt, Barfort and Lassen, 2017; Cerkez et al., 2024; Margalit, 2013; Margalit and Shayo, 2021).[1]

[1]However, see Turney et al. (2017) and Mutz (2018). Some studies also find that negative economic shocks can increase extremism on both the left and right (e.g., Autor et al., 2020); as our sample begins left-leaning, this would predict leftwards moves in political preferences in our sample.

However, there may also be direct effects of income shocks separate from effects on economic incentives. Several literatures predict such effects. First, theories of post-materialism (e.g., Inglehart, 1981; Enke, Polborn and Wu, 2022) predict that as people's material needs are increasingly met, their interest in redistributive policies declines, but their views on social and environmental issues grow more liberal. Second, some theories posit that the experience of income changes can shift attitudes towards redistributive policies. For example, Alesina and Giuliano (2011) speculate that people may "realize the importance of government intervention more after experiencing a negative shock." Those who experience a large cash transfer may be less likely to see the need for government intervention (or, conversely, one might expect that those who experience the benefits of a positive shock for themselves may be more likely to believe it would benefit others). Third, relative deprivation theory may similarly predict that individuals who become higher status may support less redistribution (Condon and Wichowsky, 2020). Fourth, psychologists argue that people who experience positive economic shocks tend to attribute them to their own effort, leading them to similarly attribute economic misfortune to dispositional rather than situational features of others, increasing conservatism (e.g., Carvacho et al., 2013; Piff et al., 2020) (see also Andersen et al., 2023; Di Tella, Galiant and Schargrodsky, 2007). Because our study randomly introduces a non-taxable positive economic shock, we are able to shed light on the extent to which this mechanism operates. Finally, as with effects on participation, evidence regarding the durable nature of moral values and political preferences over the lifespan suggest that even large shocks may not affect political preferences (e.g., Sears and Funk, 1999).

Although it has been the focus of less research, a cash transfer could also affect attitudes on social issues, as higher income tends to correlate with more liberal preferences on social issues. First, as noted above, theories of post-materialism predict such effects (e.g., Inglehart, 1981). Another possible mechanism may be perceptions of social status: a positive (negative) economic shock may make people feel higher (lower) status, leading them to be less (more) hostile towards policies that enhance other groups' status (e.g., Colantone and Stanig, 2018; Hopkins, Margalit and Solodoch, 2024). Finally, economic (in)security has been found to decrease (increase) authoritarian values or generalized aggression (Ballard-Rosa et al., 2021), with implications for attitudes towards some social policies.

Pro-social intent. Increased income could increase or decrease pro-social behaviors. As Piff et al. (2012) review, a more resource-scarce environment could lead individuals to focus more on increasing their own resources rather than helping others; on the other hand, it is possible that "greater resources, freedom, and independence from others among the upper class give rise to self-focused social-cognitive tendencies."

Empirical research on this topic is mixed. Some psychologists argue that there are negative causal effects of income and wealth on prosociality, with higher income individuals being less prosocial and more unethical (Piff et al., 2012). Most forms of charitable giving are normal not luxury goods (that is, the share of spending on charitable giving does not increase with income), suggesting that tastes for prosociality do not meaningfully *increase* with income (Evans, Evans and Mayo, 2017). However, evidence on the causal effects of income on prosociality outside lab settings are fairly limited. For instance, Haushofer et al. (2023) find that a cash transfer did not increase pro-social behavior among children.

Attitudes towards work. Finally, a variety of theories for how cash transfers may affect political preferences operate through the mechanism of how income may change attitudes towards work. For instance, studying a land lottery in Ethiopia, Andersen et al. (2023) find that winners are *less* likely to attribute success to luck and more likely to attribute it to hard work (see also Di Tella, Galiant and Schargrodsky, 2007). Recipients of a cash transfer might similarly be more likely to make internal attributions about their success, and thus to believe that others need to work harder in order to attain their recent improvement in living standards (Gottschalk, 2005). On the other hand, Brännlund et al. (2024) find no evidence that winning the lottery changes beliefs about work.

3 The OpenResearch Unconditional Income Study

The OpenResearch Unconditional Income Study (ORUS) analyzes the impact of a guaranteed income program that was implemented by two non-profit organizations. Analysis of this program was pre-registered.[2] However, over the several-years-long course of the study and in seeking feedback prior to and after the release of our results, we made a small number of amendments to our original pre-registered plan. For transparency, these are described in Appendix Section D.

This section describes eligibility, recruitment, randomization, and implementation of the program.[3]

3.1 Eligibility and Recruitment

The unconditional cash transfer program was implemented by two non-profit organizations in two states: Illinois and Texas. These states contain a variety of location types, including counties with large urban, suburban, medium-sized urban and rural areas. We identified 1-5 counties of each type in each state that were demographically representative of these geographic types from which to recruit participants. Appendix Figure OA1 shows a map of study counties with their geographic designation.

Appendix Figure OA2 provides a high-level timeline of the study's recruitment and implementation. Participants were eligible for the program if they lived in eligible counties, were age 21 to 40 (inclusive) at the time of recruitment, and had total (self-reported) household income in the prior calendar year not exceeding 300% of the Federal Poverty Level (FPL). We excluded participants receiving Supplemental Security Income (SSI) or Social Security Disability Income (SSDI),

[2]The current and previous versions of the pre-analysis plan are also available via the AEA RCT registry as AEARCTR-0006750. A blinded version of the PAP for this paper is available for download at `https://www.socialscienceregistry.org/versions/229624/docs/version/document`.

[3]This section and the following two sections of the paper are shared in large part with companion papers: Vivalt et al. (2024), Bartik et al. (2024), and Miller et al. (2024) describe the effects of ORUS on employment, consumption, and health respectively. The following exhibits, which describe the program's timeline, intervention, data collection methods, randomization procedure, and participant characteristics, are likewise produced in at least one of these companion manuscripts: Tables OA1 and OA2, and Figures OA1, OA2, OA4, OA11, and OA12.

living in public housing or using a housing choice voucher, or living in a household with another member who receives SSI. The exclusions for the means-tested government programs were due to the fact that the guaranteed income program may have made participants ineligible and it could be difficult for participants to re-enroll in these programs at the conclusion of the guaranteed income intervention.

We assisted the partner organizations in recruiting participants to the program using a variety of methods. We attempted to recruit a sample that matched population shares in each county geographic type (large urban, medium-sized urban, suburban, and rural) and to over-sample participants in lower-income households. Most participants (about 87%) were recruited via direct mailers that contained a unique code for each applicant. We selected addresses in eligible Census tracts from a marketing firm. Using these data, we sent mailers both to individuals who appeared age and income eligible for the program based on TargetSmart's provided variables and to addresses that were chosen randomly without regard to information from the TargetSmart data. For each mailer sent to an individual, we appended "or current resident" to the name printed on the address. The mailer itself informed individuals that they may be eligible to participate in a new program and could receive "$50 or more per month" for three years. We specifically did not alert applicants to the fact that they might receive $1,000 per month to avoid coercing participation or emotionally harming the control participants (e.g., by disappointing or angering them) and to improve the likelihood that control group members would continue participating in surveys despite not being selected for treatment. This should also avoid any potential negative effects of being denied a benefit on attitudes (e.g., Kosec and Mo, 2024), as control participants were not aware that they were eligible to receive much higher payments.

Following Broockman, Kalla and Sekhon (2017), the mailers directed recipients to a website where they could register their interest in the program and complete a short eligibility screening survey. Individuals were not informed of the age or income eligibility criteria prior to their completion of the online screener, reducing incentives for strategic misreporting. Incentives to complete the eligibility questionnaire varied randomly from $0 to $20. We also sent follow-up letters for non-responding households and randomized the number of follow-ups from 0 to 4. In total, of the approximately 1.1 million mailers sent, 38,823 individuals responded to the mailers and completed the eligibility survey, of whom 12,745 were program eligible (33%).

The remaining 13% of the sample were recruited via two alternative methods. First, the partner organizations purchased ads on the Facebook and Instagram platforms that were shown to all age-eligible individuals located in program counties. Participants recruited through this method make up about 1 percent of study participants. Second, the partners placed ads on the FreshEBT platform that were shown to users in eligible zip codes. FreshEBT is a free mobile application developed by Propel (`www.joinpropel.com`) that allows Supplemental Nutrition Assistance Program (SNAP) recipients to check their balance and manage their benefits. Participants recruited through this method comprise roughly 12% of study participants.

In total, across all recruitment methods, 43,385 applicants completed the online eligibility screening survey. Of these applicants, 14,573 were determined to be eligible for the program. Table OA1 compares the characteristics of those who applied by filling out the initial screener to the eligible population using the American Community Survey (ACS) as a benchmark. Overall, respondents do appear to be broadly representative of the population we targeted.

3.2 Randomization and Enrollment

After determining applicants' eligibility, we conducted two randomizations within the pool of eligible applicants. First, we randomly sampled applicants to be invited to participate in the program and study. We sampled participants in such a way to ensure the sample exhibited certain characteristics: i) the share of women in the program resembled the share in the eligible population in the study counties; ii) the sample was at least 20% non-Hispanic white, 20% non-Hispanic Black, and 20% Hispanic; iii) the household income of at least 30% of the sample was under the FPL, at least 30% was 101-200% of the FPL, and no more than 25% of the sample had income above 200%

of the FPL. We implemented these quotas by blocking participants on characteristics and drawing different numbers of participants from within these blocks. That is, in the first randomization, from the broader pool of applicants to the program participants, the probability of being selected depended on participant characteristics.

Once this sample was selected, participants were enrolled in the program by the University of Michigan Survey Research Organization (SRO), a survey research firm with extensive experience fielding national studies. During enrollment, program participants who consented to take part in the research completed a baseline intake survey and provided bank account information so program funds could be directly deposited. For participants with no bank account, a no fee/no minimums online bank account was opened for them. As part of the enrollment process, participants also were invited to consent to have their data linked to administrative records. This enrollment was conducted in person from October 2019 until March of 2020, at which point enrollment was switched to phone due to the COVID-19 pandemic. Enrollment concluded in October 2020. To keep participants engaged and to collect additional baseline information, we sent enrolled individuals monthly surveys over this pre-treatment period. In addition, all enrolled participants received $50 per month via direct deposit into their bank account for the duration of the enrollment period.

The focal randomization occurred once all 3,000 participants were enrolled. This randomization assigned participants to either continue receiving $50 per month ("control group") or to receive $1,000 per month ("treatment group") for 3 years. Unlike the first randomization, this assignment did not depend on participant characteristics and all participants had a 1/3 probability of being assigned to treatment. The comparison across these two treatment arms, within the 3,000 program participants, is the focus of our analysis.

We used a blocked random assignment procedure. We also identified, over the course of the enrollment period, a small number of study participants who knew each other; we placed these individuals together in clusters so they would be assigned to either treatment or control together. We then formed blocks of clusters as follows. We formed strata based on race/ethnicity, income group (0-100% FPL, 101-200% FPL, 201-300% FPL), and state; any clusters with more than one individual within them were placed in their own strata. Within these strata, we grouped participants into blocks of three based on how similar they were across several dozen pre-treatment covariates, using Mahalanobis distance to measure similarity. Within each block of three, we selected one of three observations to be in the treatment group and placed the remaining two in the program control group.

Finally, we implemented a constrained re-randomization procedure to ensure our final comparison groups were balanced across key covariates (Zhao and Ding, 2024). Appendix Section E provides more details.

Table 1 demonstrates that across treatment and control arms, baseline characteristics are very similar along a number of dimensions including demographic and economic characteristics. Furthermore, it illustrates that the sample was on average very low income, making the transfer a meaningful increase in financial resources. Table OA1 provides further comparisons, including a comparison to population statistics, which shows that the study yielded a representative sample.

3.3 Intervention period

Randomization occurred in October of 2020. The higher ($1,000) transfer payments to the treatment group began in November of 2020 and continued until October of 2023. Over the same period, the control group continued to receive the $50 per month transfer. Note that the majority of the treatment period occurred after the introduction and widespread adoption of the COVID-19 vaccine, although the first year did include a few months before the vaccine was available. We therefore expect our results to be largely representative of the post-COVID-19 era. Receipt of the transfers was not conditional on participation in any of the research activities. Since the transfers were provided as an unconditional gift from a non-profit organization, they were not subject to income tax. Furthermore, the non-profit organizations worked with state benefit offices to ensure the transfer did not affect eligibility for public benefits whenever possible. This effort was facilitated

Table 1: Baseline characteristics by treatment arm

	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
Any Children in Household	0.568	0.571	0.897
# Children	1.435	1.398	0.558
Economic			
Employed	0.578	0.586	0.675
Personal Income ($1000s)	21.355	21.217	0.861
Household Income ($1000s)	29.991	29.917	0.922
Under FPL	0.323	0.336	0.475
HS Degree/GED or higher	0.953	0.939	0.100

Notes: This table displays means of baseline characteristics of the treatment and control group, and a p-value associated with a test of equality of those means.

by the passage of state-level legislation in the state of Illinois (SB 1735) that specifically excluded cash transfers made as part of research studies such as the ORUS payments from the calculation of eligibility for several state programs. The non-profit organizations worked with benefits administrators to alter instruction manuals for public employees to ensure this law was implemented. Appendix Table OA2 contains detailed information on how government benefits were affected by the transfer.

At the end of the transfer period, the non-profit partners offered services to both treatment and control group participants to help them transition off of the program. All participants were given updated resource lists for services and support in the counties from which participants were selected at baseline, as well as national hotlines and services for those that moved during the program. Additionally, program staff were available by phone, text message, and email to assist participants. If appropriate, they connected the participants to services provided by the organization itself or referred them to relevant non-profit or government entities in the area.

4 Data

We collected data on participants' political attitudes and behavior through a variety of sources.

4.1 Survey data

First, we administered short, online-only surveys each month through Qualtrics. As an incentive to respond, $10 was deposited in the participant's bank account upon completion of the survey. By maintaining monthly contact with participants, we were able to keep participants engaged in the program and track and update their contact information. Frequent surveys also gave researchers multiple opportunities to get questions answered in each year. For example, if a respondent missed a question about self-reported health on the April survey, they may have another chance to provide that information on the June survey of the same year. For the purpose of analysis, we treat responses to the same questions provided within the same year as capturing similar information and collapse our outcomes to the respondent by survey year level for analysis of these monthly surveys, taking the average within respondent/year if multiple responses to the same questions were provided.

Second, we conducted two in depth, enumerated surveys—a "midline" and an "endline" survey. These surveys were administered by the University of Michigan Survey Research Organization (SRO) and respondents received a $50 incentive payment for completing them. For these surveys, trained enumerators scheduled phone interviews with respondents. At baseline, we collected names and contact information for people outside the participant's household who would be able to get in touch with the participant if we were unable to reach them at any point during the study. If repeated outreach attempts via email, telephone, text message, and postcards were unsuccessful, interviewers reached out to alternative contacts. As a last resort, interviewers visited the last known address of participants if the address was located within the geographic area covered by interviewers. The midline survey was conducted from April 3 until August 2, 2022, and the endline survey was conducted from March 30 until August 15, 2023. To keep the phone interviews reasonably short, we had respondents complete additional midline and endline questions in three follow-up online surveys. We incentivized participation in these follow-up surveys by providing $15 per completed survey, which was escalated to $30 per survey for remaining non-respondents at the end of the final endline survey period.

Response rates for all types of surveys were high. Figure OA3 shows response rates for the control group and treatment participants. Almost all participants responded to at least one of the monthly mobile surveys in each of the three study years, with 98% completing at least one Qualtrics survey in year 1, 96% in year 2, and 94% in year 3. About 97% of participants responded to the enumerated midline survey and 96% responded to the enumerated endline survey. The online surveys that followed the midline and endline enumerated surveys had somewhat lower, but still high, average response rates of 91.3% and at 92.0%, respectively.

Differential response rates across treatment arms were present but modest for all survey types. The midline and endline exhibited a less than 1.8pp and 3.2pp difference in response rates respectively. For mobile surveys, the differential probability of responding to any survey in a given survey year ranged from less than 1pp (in year 1) to about 4.3pp (in year 3). On average, the fraction of the treatment and control participants completing at least one monthly survey in years 1-2 was not significantly different, with a somewhat greater differential attrition observed in year 3 (see Appendix Figure OA4). However, as shown in Appendix Tables OA3-OA7, we observe that the treatment and control groups remain highly balanced on a large number of pre-treatment characteristics even when limiting our sample to survey respondents only. This suggests that the modest differential response rates we observe do not undo the balance generated by the randomized treatment group assignment.

Participants also enrolled in a mobile application which collected nutrition diaries and time diaries. We do not rely on this data in this paper; further details are available in Vivalt et al. (2024), Bartik et al. (2024), and Miller et al. (2024).

4.2 Administrative data

To measure voter registration and turnout, we also attempted to match consenting participants to the L2 voter file.

Consent to be linked to administrative records was obtained prior to randomization, although some participants who initially did not consent ultimately did so over the course of the study. In total, 87.5% consented to be linked by the end of the study, and consent rates were reasonably balanced across treatment arms, with 86.9% consenting in the control group and 88.8% consenting in the treatment group (difference in consent rates $p = 0.12$). These consenting participants formed the pool of participants we could attempt to link to the L2 voter file. We also conduct a robustness check among just those who consented at baseline, prior to randomization; 78.7% consented at baseline (difference in baseline consent rates between treatment and control $p = 0.73$).

We attempted to match participants on both their initial address as well as any subsequent addresses they provided during the study. We record anyone who does not match to the L2 file as non-registered and as having not voted to avoid conditioning on a post-treatment variable (Nyhan, Skovron and Titiunik, 2017), although it is possible we may miss some voter registrations.

4.3 Qualitative Interviews

We also conducted qualitative interviews with a subsample of participants. Participants for the qualitative sample were selected purposively from the full sample of 3,000, drawing the majority of the sub-sample from the treatment group to provide in-depth insight into how recipients experience and understand the role of the cash transfer in their lives. Additionally, to investigate if and how income at enrollment and neighborhood and environmental factors mediate the impact of the transfer, we divided our full sample into four strata and individuals were randomly selected for the qualitative sub-sample from within these strata.[4] This resulted in a qualitative sample of 156 individuals.

We conducted five rounds of semi-structured interviews with these 156 participants between July 2020 and February 2024. The majority of baseline interviews took place before treatment group participants were notified that they would receive $1,000 monthly cash transfers. The remaining rounds of interviews took place at approximately six-month intervals. Among other topics in the qualitative interviews, participants were asked questions about political engagement, attitudes about work, opinions about government programs and policies, and sentiments of trust.

Across all five rounds, interviews averaged 2 hours in length. All interviews were audio-recorded and transcribed, and respondents were compensated $50 for their participation. Response rates were high across all rounds of interviews: 100% at baseline, 95% at round two, 96% at round three, and 94% at round four. Round five had a slightly lower, but still high, response rate of 87%.

Interviews were analyzed using thematic analysis Braun and Clarke (2006) with support from MaxQDA, a program designed for qualitative and mixed-methods data analysis. In analyzing the interview data, we summarized the political attitudes and behaviors experienced by both treated and control participants and wrote analytic memos identifying mechanisms through which changes in these attitudes and behaviors were produced.

[4] To create these strata we identified participants from the full sample whose household income at enrollment was below the federal poverty threshold and participants whose household income was above the federal poverty threshold but did not exceed 175% of the federal poverty threshold. Though still low income, households above the federal poverty threshold but not exceeding 175% are often ineligible for most forms of assistance. Next we utilized data from the Child Opportunity Index 2.0, a composite measure of 29 indicators at the Census tract level that characterize the educational, health and environmental, and social and economic resources available to neighborhood children. We divided participants from the full sample into strata based on their income level and neighborhood opportunity score, and individuals were randomly selected for the qualitative sub-sample from within these strata.

5 Estimation

We estimate the impact of the treatment on each outcome using the following regression:

$$Y_i = \beta_0 + \beta_1 Treat_i + \beta_2 X_i + \epsilon_i. \tag{1}$$

We estimate robust standard errors clustered at the level of treatment assignment, as recommended in Abadie et al. (2022).[5]

Selecting covariates. X_i are characteristics of individual i measured in the pre-treatment period, which we include to improve the precision of our estimate. Because we collected many baseline measures, in a first step we use the LASSO to select which covariates to include, selecting the penalty term λ using cross-validation following Bloniarz et al. (2016).

Pooling outcomes across time. The outcomes Y_i may be observed at multiple time periods. In some specifications, we use these repeated observations to estimate time period specific effects. For example, Qualtrics survey outcomes are examined at the first, second, and third year of the study while midline and endline survey outcomes are examined at those time periods. To increase statistical power and reduce multiple testing, our pre-registered primary specification pools these time periods together to arrive at a single "effect" of the transfer over the entire study period. When doing this, consistent with our pre-analysis plan, we place greater weight on observations towards the end of the study period and on observations derived from the midline and endline surveys, for which we observe higher response rates. For the midline and endline estimates, we place 70% of the weight on the endline and 30% of the weight on the midline. For the monthly surveys, we place 50% of the weight on surveys conducted in the final year, 30% of the weight on surveys conducted in the second year, and 20% of the weight on surveys conducted in the

[5] Recall most individuals are a cluster of one, but a small number of individuals are placed together if it was discovered in the pre-treatment period that they knew each other, see Section 3.2.

first year. For outcomes where we can aggregate across both types of surveys, we further weight the midline/endline outcomes with 70% of the weight and the monthly surveys with 30% of the weight, reflecting the higher response rates and likely higher quality of the data derived from the midline/endline surveys due to the fact that they were enumerated. If we have no measures of an item within a particular time period (e.g., year 2, at midline, etc.) for an individual but do have measures of that item at other time periods, we average over the non-missing time periods and redistribute weights accordingly. Note that because we place greater weight on the later years of the study, our results will be mostly based on data collected after the COVID-19 pandemic, during 2022 and 2023.

Grouping items. Income might affect a large number of political outcomes, and in some cases it may be interesting to know whether we can reject the null hypothesis that certain groups of outcomes collectively were affected by the transfer. Grouping outcomes can also reduce the number of hypotheses tested, helping address multiple testing concerns, and reduce measurement error (Anderson, 2008).

To facilitate this type of analysis, we group items at two levels. First, we group closely related items into groups that we refer to as "components."[6] For example, we group the items measuring reported trust in one's state government, the US Congress, the legal system, and politicians into a component called "Trust In Institutions." Some components are comprised of a single item. In some cases, multiple survey questions form a single pre-registered item; in these cases, we combine the survey questions by forming a simple additive index. As an example of the two previous points, we measure political knowledge with two political knowledge questions. However, we combine these into an additive index which represents a single item in the analysis. This item is also the only item in its respective component.

Second, we aggregate related components into broader families. For example, we aggregate

[6]The non-aggregated outcomes we refer to as "items" for clarity.

components "Trust in Institutions," "Support for Incumbents," "Affective Polarization," and others into a family "Political Trust and Polarization."

To construct these components and families, we start with the individual-level item regression estimates $\hat{\beta}_1$. We standardize these estimates by dividing by the control group standard deviation. To take into account the correlation between the estimates, we then aggregate the estimates using seemingly unrelated regression (SUR) into components and, subsequently, families, by averaging the standardized effects.

Adjusting for multiple testing. We account for the fact that we are conducting many statistical tests by using a false discovery rate (FDR) adjustment. We use Benjamini and Hochberg (1995)'s false discovery rate adjustment to compute q-values; following Benjamini and Hochberg (1995) we do this within families of outcomes. Furthermore, we follow Guess et al. (2023) by placing the family-, component-, and item-level estimates into tiers for the purpose of this adjustment, corresponding with our prioritization of the estimates. We place tests within these pre-specified tiers so that analysis that is considered exploratory or ad hoc can be conducted without affecting the statistical power of our primary outcomes of interest. We consider family-level estimates (estimated using both all data and only midline/endline data) in the top tier and pool all family-level estimates across this paper in a single tier. We place component-level estimates in the next tier and pool these tests with the family-level tests and other components within the family. Then we place all of our primary outcomes (items) in the next tier; these are computed by pooling the family-level estimates and all component-level and other outcome estimates within the family. The last tier is comprised of estimates we consider to be more exploratory in nature: estimates by each time period, subgroup analyses, and outcomes pre-specified as secondary. We do not conduct an FDR adjustment for robustness checks.

We also estimate the correlation between baseline personal income and each outcome to generate a point of comparison with our causal effects. We do this by regressing the outcome observed at baseline on pre-treatment personal income scaled to be the total annual size of the net transfer (i.e., \$12,000 received by the treatment group minus \$600 received by the control group, or \$11,400). This baseline "gradient" therefore tells us how much we might expect each outcome to change if the correlations between income and our outcomes were fully causal.[7]

6 Results

6.1 Perceived source of the transfer

We first validate that participants understood that the transfer came from private sources and not the government. To verify this, after the transfer ended, we asked participants a broad question about which entities or people "ma[de] it possible for them to participate" in the transfer program. Table 2 shows treated participants' perceptions of the source of the transfer. Very few selected government entities, and the vast majority were aware that the money came from private charitable and nonprofit sources. (The transfer was administered by local non-profits and funded by charitable donations, so both "local nonprofits" and "charity" are accurate answers.)

6.2 Political Participation

We next examine the treatment effects, beginning with political participation. Figure 1 shows the estimated effects for the political participation family with all outcomes rescaled to standard deviation one. Table OA8 shows numerical results on the raw (non-rescaled) outcomes. Appendix section C shows the effects for this and the other outcome areas at each point in time.

Contrary to resource theories of political participation, we do not find effects on any measures

[7]Note that this gradient captures the relationship between current (both permanent and transitory) income and our outcomes, whereas our transfer is a transitory income shock. To the extent permanent and transitory income predict our outcomes differently, the gradient may overstate our expectations for changes due to the temporary income transfer. Nevertheless, it provides a useful benchmark regarding differences in our outcomes across the distribution of current income in the specific sample we study.

Table 2: Treated participants' perceptions of cash transfer's source

Entity	% Selecting
Local nonprofits	49%
Charity	42%
Other entity	31%
Governor and state government	11%
President Biden and national government	7%
Local businesses	8%

Notes: The Table shows the percent of participants in the treatment group who selected each of the options shown in response to the question "Which of the following do you think helped make it possible for you to participate in the [Program Name] program? Please select all that apply." A "none of the above" option was present. $N = 801$ *participants responded to the question, which was asked in January - March, 2024, after the transfer ended.*

of political participation.

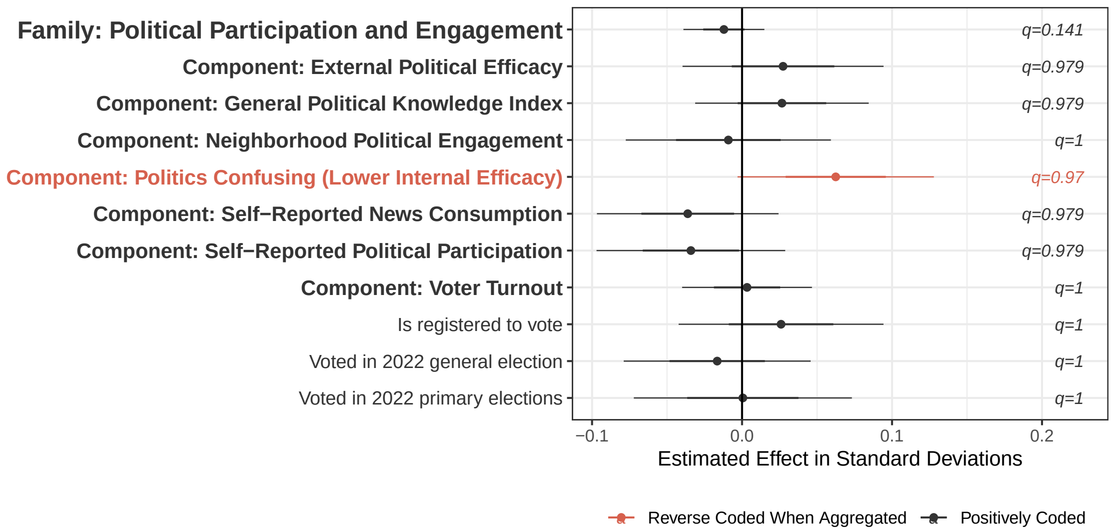
First, we do not find effects on internal or external political efficacy, political knowledge, or political news consumption.[8] As reported in Table OA8, we can rule out causal effects on political knowledge, internal political efficacy, and self-reported news consumption equivalent in size to the observational association between these variables and income.

Second, we find no effects on validated measures of voter turnout.[9] As reported in Table OA8, the observational relationship in our sample between general election turnout and income is such that we would expect a $11,400 increase in income to correspond with a 3.1 percentage point increase in voter turnout. However, we estimate a null effect on general election voter turnout which is statistically distinguishable from 3.1 percentage points ($\beta = -0.008, \text{SE} = 0.015$). Table

[8] External efficacy is measured with the ANES questions "People like me don't have any say about what government does." and "I don't think public officials care much what people like me think." Internal efficacy is measured with the ANES question "Sometimes politics and government seem so complicated that a person like me can't really understand what is going on." Political knowledge was assessed with open-ended questions asking people to recall the jobs held by John Roberts and the chancellor of Germany at the time of the survey. Political news consumption is measured with the question "How much attention do you pay to news about national politics on TV, radio, printed newspapers, or the Internet?" on a 5-point scale ranging from "A great deal" to "None at all."

[9] We use the 2022 general and primary elections to measure turnout. The 2020 elections were held just before the transfers began, and the 2024 elections were held after the transfer had ended. Turnout is coded as 0 for those not registered to vote.

Figure 1: Estimated Effects on Political Participation



Notes: The Figure shows the estimated treatment effects on standardized versions of the items. The top row shows the family-level estimate, which is an average of the component-level estimates below. The component-level estimates are listed in bold. The component-level estimates are averages of the item-level estimates listed below the component; or, in cases of a single-item component, the standardized estimate on that item. FDR adjusted q-values are shown at right. Table OA8 shows numerical estimates and a comparison with the observational association with income. Figure OA5 shows the estimates at each time point.

OA8 also shows that the estimated effects on turnout are similarly null when restricting the sample to those who consented to be linked to administrative data prior to randomization.

Finally, we find no differences in self-reported measures of political participation or neighborhood political engagement.[10]

[10]We measured self-reported political participation with the following question drawn from the General Social Survey: "Here are some different forms of political and social action that people can take. Please indicate, for each one, whether you have done any of these things in the past year, whether you have done it in the more distant past, whether you have not done it but might do it, or have not done it and would never, under any circumstances, do it." We counted up the number of the following where the respondent selected "In the past year": "Sign a petition," "Attend a demonstration," "Attend a political meeting/rally," "Contacted a politician, "Contacted the media," "Expressed political views online," "Contributed money to a political cause," and "Tried to show other people why they should vote for one of the parties or candidates." Neighborhood political engagement is measured with the following question from the US Census American Housing Survey: "Sometimes people in a neighborhood do things to take care of a local problem, or to make the neighborhood a better place to live. Please tell me if you or anyone in your household have been involved in the following activities since you lived in this neighborhood." We counted up how many of the following the respondent selected: "Spoken with a local politician like a city council member or county supervisor about a

These null findings suggest two interpretations. First, the contrast between our null findings and the mobilizing effects of government transfers found in prior studies reinforces the importance of citizens' attribution of responsibility towards the state for generating the mobilizing effects of those transfers. Second, our findings contradict the longstanding resource model of political participation and suggest that omitted third factors such as human capital might explain the positive relationship between the two.

Data from the qualitative interviews reinforce these interpretations. Participants in the treatment and control group provided similar explanations regarding their level of political participation. Across both groups, many participants who were politically engaged drew connections between their political participation and their social networks. Similarly, many participants in both groups who reported a decline in participation described that politics have become too stressful and expressed a desire to minimize stress from this input. Almost universally, participants stated that their political participation had not changed as a result of the transfer.

6.3 Intergroup Attitudes

Next, Figure 2 and Table OA9 show the estimated effects on intergroup attitudes. The first two outcomes, attitudes towards respondent's own racial group and other racial groups, are measured using a feeling thermometer.[11] Consistent with mood misattribution (Schwarz and Clore, 1983), we find small (approximately 2 on a 0-100 scale) but statistically significant increases in respondents' feeling thermometer scores towards both their own and other racial groups.[12] Although these

local problem?", "Gotten together with neighbors to do something about a neighborhood problem or to organize neighborhood improvement?", "Talked to a person or group causing a problem in the neighborhood?", and "Attended a meeting of a block or neighborhood group about a neighborhood problem or neighborhood improvement?"

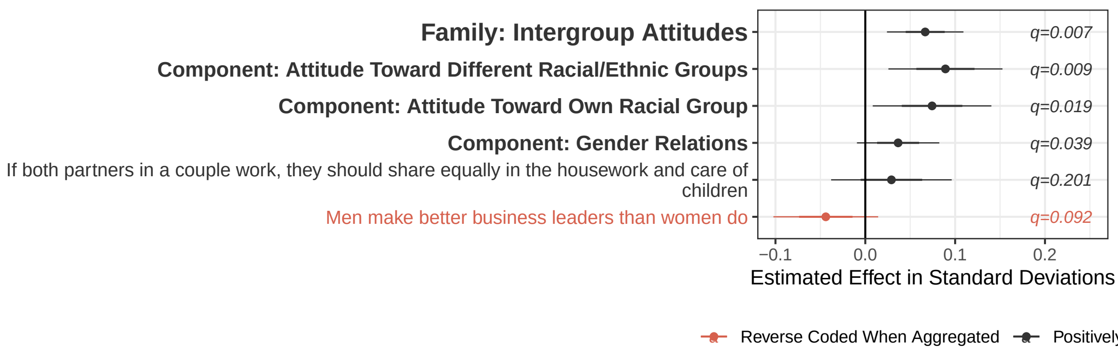
[11] We ask about "African-Americans," "White people," "Hispanics," and "Asians." For the outgroup measure, we average feeling thermometers for all groups a respondent is not a member of.

[12] We did not see any evidence that participants moved to more racially diverse neighborhoods, nor any evidence that change in racial composition was correlated with changes in attitudes towards those of other races. We also examined whether this could be because people acquired or left customer-facing jobs, which could have either improved or worsened their attitudes towards other people. However, we found that the transfer had no effect on whether individuals held customer-facing jobs.

effects are small, our findings are consistent with the perspective that income itself can liberalize attitudes towards outgroups.

We find no effects on items related to gender relations; we can again reject the observational relationship for one of these items.[13]

Figure 2: Estimated Effects on Intergroup Attitudes



Notes: The Figure shows the estimated treatment effects on standardized versions of the items. The top row shows the family-level estimate, which is an average of the component-level estimates below. The component-level estimates are listed in bold. The component-level estimates are averages of the item-level estimates listed below the component; or, in cases of a single-item component, the standardized estimate on that item. FDR adjusted q-values are shown at right. Table OA9 shows numerical estimates and a comparison with the observational association with income. Figure OA6 shows the estimates at each time point.

6.4 Political Trust and Polarization

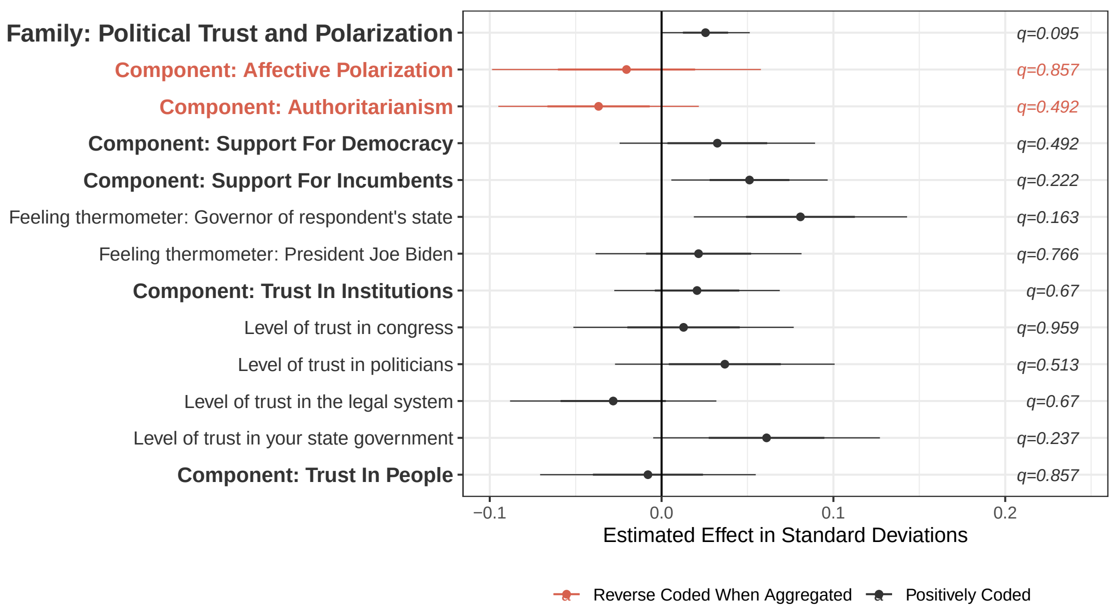
Figure 3 and Table OA10 show the estimated effects in the political trust and polarization area. The family-level estimate is positive and nears statistical significance, driven largely by an increase in favorability towards one's incumbent governor;[14] this was also measured using a feeling

[13]The effects on the Gender Relations variable is not statistically significant using conventional p-values but the q-value is less than 0.05, due to the very low p-values for the other tests in this family.

[14]This effect was larger for participants in Illinois than in Texas, although we did not pre-register this expectation. The effect for those in states with Democratic governors (largely those in Illinois; some participants moved before the study concluded) was 4.7 degrees (SE $= 1.2, p < 0.0002$), while the effect in states with Republican governors (largely those in Texas) was indistinguishable from zero ($\beta = -0.04, \text{SE} = 1.2, p = 0.97$). We did not find any differences between states in whether treated participants credited their state government or governor for helping them receive the transfer. Qualitative interviews did not surface any indications as to what could account for this difference.

thermometer and increases by about 2 points on average, similar to the measures of attitudes towards various racial groups reported in the previous section.[15] In contrast to the null effects we found on voter turnout, these suggestive effects on attitudes towards incumbents imply that some of the positive effects of income on attitudes towards incumbents may not require citizens to attribute these changes to politicians, consistent with mood misattribution.

Figure 3: Estimated Effects on Political Trust and Polarization



Notes: The Figure shows the estimated treatment effects on standardized versions of the items. The top row shows the family-level estimate, which is an average of the component-level estimates below. The component-level estimates are listed in bold. The component-level estimates are averages of the item-level estimates listed below the component; or, in cases of a single-item component, the standardized estimate on that item. FDR adjusted q-values are shown at right. Table OA10 shows numerical estimates and a comparison with the observational association with income. Figure OA9 shows the estimates at each time point.

We find no change in affective polarization (measured as a difference in feeling thermometer scores between one's own party and the opposite party), although the point estimates on ratings

[15] We also pre-registered that we would measure affect towards the mayor of participants' city, but we ended up not asking this question because locating the name of the mayor for all participants proved logistically infeasible.

of both one's own *and* the opposite party are small and positive,[16] again consistent with mood misattribution.

Despite the positive relationships we replicate between income and authoritarianism, support for democracy, trust in the legal system, and trust in others (see Table OA10)—and some prior research arguing that each of these relationships are causal—we find no effects on these outcomes.[17] In the case of authoritarianism, trust in the legal system, and trust in others, we can rule out a causal relationship equivalent to the observational relationship we observed in this data.[18]

6.5 Political Preferences and Attitudes

Next, Figure 4 and Table OA11 find no evidence of meaningful changes in left-right political preferences. Items in this family are oriented so that positive estimates correspond with the transfer making individuals more liberal. The family-level point estimate is very small but is nearly distinguishable from zero; it is possible that there may have been a liberalizing effect of the transfer, but any such effect is very small.

There is not clear evidence that the transfer led individuals to be any more liberal in any specific

[16]The point estimates are 1.2 and 1.1 degrees on a 0-100 scale, respectively, and neither is statistically significant.

[17]Support for democracy was measured using the World Values Survey question "I'm going to describe various types of political systems and ask you what you think about each as the way of governing this country. For each one, would you say it is a very good, fairly good, fairly bad or very bad way of governing this country?" with the following items: "Having a strong leader who does not have to bother with Congress and elections?" (reverse coded); "Having experts, not government, make decisions according to what they think is best for the country?" (reverse coded); "Having the army rule?" (reverse coded); "Having a democratic political system?" We measuring authoritarianism using the aggression subscale: "Do you agree or disagree with the statements below? "It is necessary to use force against people who are a threat to authority.", "Police should avoid using violence against suspects.", "Using force against people is wrong even if done so by those in authority." (reverse coded), "Strong punishments are necessary in order to send a message." Answer choices were on a 5-point scale ranging from Strongly agree to Strongly disagree. Trust in others was measured using the question "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?" with answer choices on a 1-10 scale. Trust in institutions was measured with the question stem " How much do you personally trust each of these institutions on a scale of 0-10 (with 10 being most trustworthy and 0 being least trustworthy)?"

[18]Based on findings in the Work Attitudes area and in other companion papers, in exploratory analysis, we investigated effects among single parents specifically. We found that single parents may have shown larger increases in trust in politicians ($\beta = 0.359, \mathrm{SE} = 0.119, p = 0.003$), perhaps because this group may have benefited most from the transfers. However, this result should be considered exploratory.

area, even as the point estimates in many areas were positive. First, party identification[19] did not change. Second, the Figure and Table list a variety of policy questions assessing views on economic issues and preferences towards redistribution.[20] Some items suggested treated participants may have grown slightly more economically liberal; for example, they may have been slightly more likely to disagree that incomes should be unequal as an incentive for effort, and to agree that government funding for programs for the poor and unemployed should be made more generous. However, this pattern was not universal across items. Moreover, inconsistent with liberalization, participants were more likely to support work requirements for Medicaid, a finding consistent with our findings on attitudes towards work we report later. In general, though, economic attitudes did not appear to change. Finally, there is some evidence of a potential small increase in social liberalism, although this is again not statistically significant and is at most small: we can rule out effects on the Social Liberalism component of larger than 0.07 SDs.

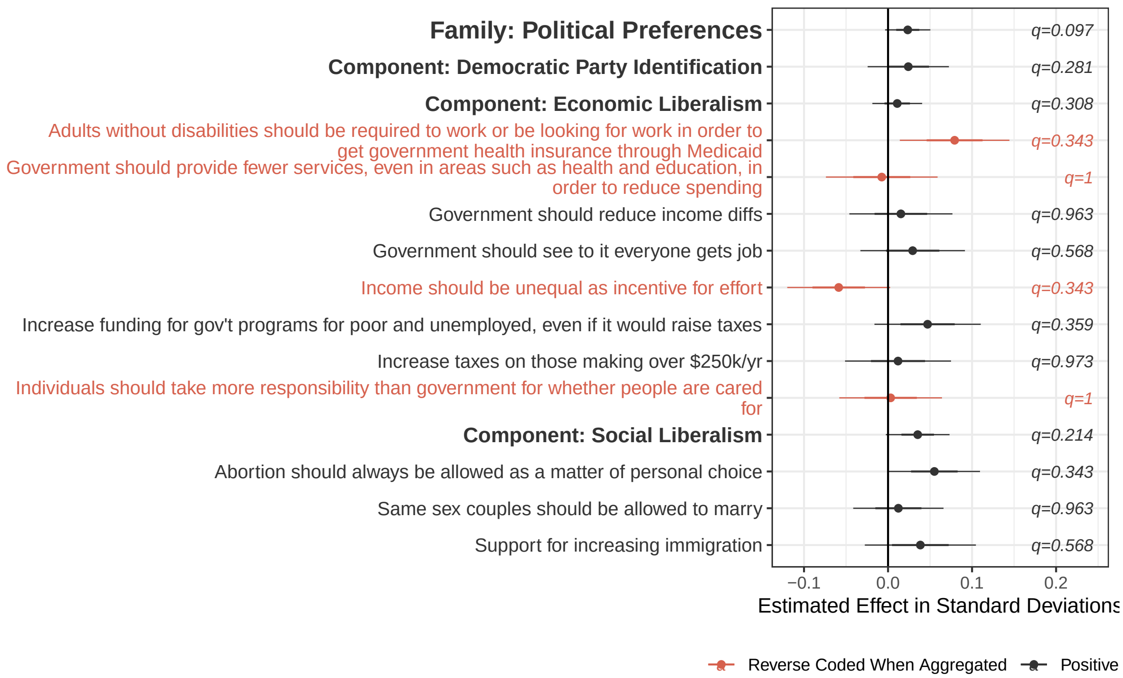
These largely null effects on political preferences contrast with findings on the impacts of economic shocks that change self-interest or that individuals can attribute to government or the economy, reinforcing interpretations of the effects of those shocks as driven by participant interpretations and incentives, not income per se (Margalit, 2019*b*). However, the suggestive evidence of increases in social liberalism may be consistent with theories of post-materialism. The findings also reinforce the durability of political attitudes: even receiving a very large transfer generally did not affect participants' views on whether government should provide such transfers.

Qualitative evidence is largely consistent with this. Participants in both the treatment and control group expressed similar views on whether government should provide such transfers, the majority of whom justify their views largely through the lens of pre-existing beliefs. For example, both treated and control participants brought up themes related to deservingness and concern that

[19]This was assessed using the standard ANES branching question, which was recoded to range from 1-7.

[20]These were asked in a binary agree/disagree format, except for the "Income should be unequal" and "Individuals should take more responsibility than government" questions; these were adapted from GiveDirectly's surveys and respondents were asked to respond on a 1-10 scale.

Figure 4: Estimated Effects on Political Preferences and Attitudes



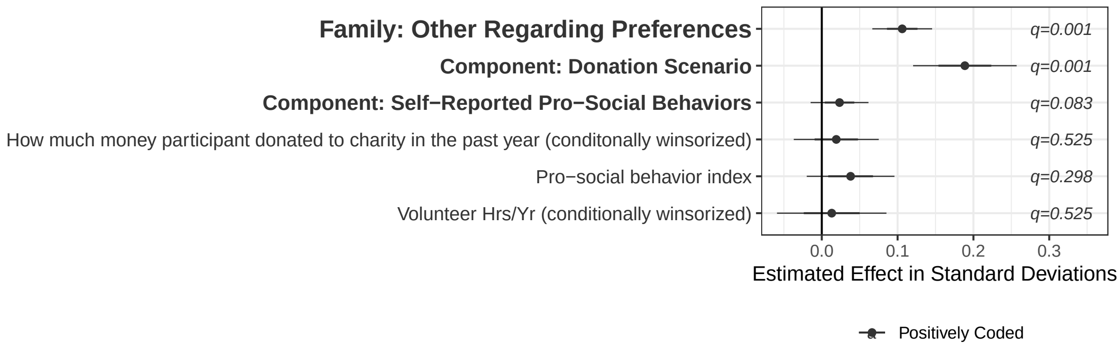
Notes: The Figure shows the estimated treatment effects on standardized versions of the items. The top row shows the family-level estimate, which is an average of the component-level estimates below. The component-level estimates are listed in bold. The component-level estimates are averages of the item-level estimates listed below the component; or, in cases of a single-item component, the standardized estimate on that item. FDR adjusted q-values are shown at right. Table OA11 shows numerical estimates and a comparison with the observational association with income. Figure OA8 shows the estimates at each time point.

others would take advantage of redistributive programs on the one hand, as well as support for lifting people out of poverty on the other hand.

6.6 Other-Regarding Preferences

Figure 5 and OA12 show that we see no meaningful changes in other-regarding preferences outside of an item asking how much of a $1,600 windfall individuals would donate to a good cause. This is consistent with our findings in Bartik et al. (2024), where we find a meaningful increase in transfers to charity, family members, and friends. We see no changes in self-reported pro-social behavior or the number of volunteer hours per year.[21] These findings are inconsistent with arguments that individuals become less generous as their income increases. Figure OA7 also suggests that the effects in this area may have decreased over time over the course of the transfer.

Figure 5: Estimated Effects on Other-Regarding Preferences



Notes: The Figure shows the estimated treatment effects on standardized versions of the items. The top row shows the family-level estimate, which is an average of the component-level estimates below. The component-level estimates are listed in bold. The component-level estimates are averages of the item-level estimates listed below the component; or, in cases of a single-item component, the standardized estimate on that item. FDR adjusted q-values are shown at right. Table OA12 shows numerical estimates and a comparison with the observational association with income. Figure OA7 shows the estimates at each time point.

6.7 Attitudes Towards Work

Last, Figure 6 and Table OA13 report some changes in attitudes towards work. First, views regarding what explains economic inequality (dispositional or contextual factors) did not change.[22]

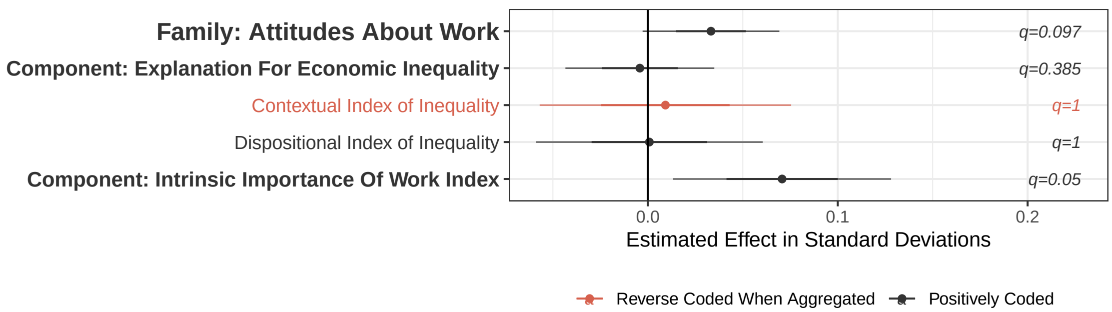
[21] Pro-social behavior was measured using the General Social Survey question "During the past 12 months, have you..." with the following options: "Allowed a stranger to go ahead of you in line?" "Carried a stranger's belongings, like groceries, a suitcase, or shopping bag?" "Given directions to a stranger?" "Let someone you didn't know well borrow an item of some value like dishes or tools?" "Helped someone outside of your household with housework or shopping?" and "Spent time talking with someone who was a bit down or depressed?" We summed up the the number of selections to form this index.

[22] This was assessed with an item adapted from Kraus, Piff and Keltner (2009): "Please indicate how important you think the contribution of each of the following factors is to growing (decreasing) economic disparity." The options coded as Contextual were "Personal background," "Educational opportunity," and "Luck." The options coded as "Dispositional" were "Ability and talent" and "Hard work."

This finding is contrary to prior research showing that people who receive an economic windfall are more likely to credit dispositional rather than contextual factors for economic inequality.

However, treated participants were more likely to say that work was important, consistent with our earlier finding that treated participants were more supportive of work requirements for Medicaid.[23]

Figure 6: Estimated Effects on Attitudes Towards Work



Notes: The Figure shows the estimated treatment effects on standardized versions of the items. The top row shows the family-level estimate, which is an average of the component-level estimates below. The component-level estimates are listed in bold. The component-level estimates are averages of the item-level estimates listed below the component; or, in cases of a single-item component, the standardized estimate on that item. FDR adjusted q-values are shown at right. Table OA13 shows numerical estimates and a comparison with the observational association with income. Figure OA10 shows the estimates at each time point.

Qualitative interviews suggested three potential explanations for the effect on attitudes towards the intrinsic importance of work. First, participants in the treatment group sought to distinguish themselves from others who they perceived as potentially using the transfer "the wrong way." For example, one treatment participant stated: "[the cash transfer] is an opportunity for you to better yourself with the money...but I imagine people that are in this program it's no telling what they do with the money that they get. Everybody's not gonna do what I did."[24]

[23]The importance of work was assessed with a survey question from the World Values Survey: "Do you agree or disagree with the following statements?" with the statements "To fully develop your talents, you need to have a job.", "People who don't work turn lazy" (reverse coded), "Work is a duty towards society.", and "People should not have to work if they don't want to" (reverse coded).

[24]Another said "I see the result and impact that [the cash transfer] had on me, but I'd like to think that some of it

Second, though treatment participants are not more likely to attribute economic inequality to dispositional factors, treatment participants in the qualitative sample whose importance of work scores increased from baseline were more likely to attribute their own gains to dispositional factors. They expressed that it was their work ethic that enabled them to use the cash transfer to advance in ways that individuals without similar work ethic would not have been able to. For example, participants reported using the transfer to bridge periods of unemployment, pay for job training or certificate programs that made it possible for them to advance at work, or using the transfer as a safety net to feel more comfortable making risky employment decisions. For these participants, being able to secure employment at the end of the transfer period—many of them in jobs they described as meaningful—affirmed their beliefs about their own work ethic and ability to get ahead through hard work, making them valorize work even more.

Finally, receiving the transfer also appeared to lead some participants to conclude that additional income produced larger benefits than they expected, and these participants might therefore have developed greater motivation to earn additional future income.[25] These participants expressed that the transfer provided them increased independence from others, more financial breathing room, and a sense of stability that they previously had not experienced. The perspective of having experienced such stability and independence due to additional income led them to place increased value on work as a means to achieving similar stability through future earned income.

6.8 Heterogeneous Treatment Effects

While we did not pre-register hypotheses about heterogeneous treatment effects, following Inoue et al. (2024) we conducted a rigorous post-hoc analysis to investigate whether our observed

was because of the kind of person that I was to begin with. ... But if somebody is not willing to better their life or feel like it should be better, like what's the point?"

[25]When investigating which subgroup was most responsible for the effects on work attitudes, we found that they appeared to be largest among single parents, although this finding should be considered exploratory. Single parents who received the transfer were also markedly more likely to support work requirements for Medicaid ($\beta = 0.098, \mathrm{SE} = 0.028, p < 0.001$), although this result was not pre-registered so should also be considered exploratory.

null average treatment effects masked meaningful effects within specific subgroups. To do so, we employed causal forests (Wager and Athey, 2018), implementing a 20-fold cross-validation procedure. Specifically, we partitioned the sample into 20 folds and iteratively trained the causal forest algorithm on the training sets, using baseline covariates to estimate conditional average treatment effects. We then validated these estimates on the held-out samples, aggregating across all held-out folds to assess the algorithm's ability to detect systematic patterns of treatment effect heterogeneity. We used this procedure for several outcomes: attitudes about work; an index of economic, social and political outcomes that collectively captured more liberal attitudes; and voter turnout in the 2022 general election. In none of these cases did we find evidence of robust heterogeneous treatment effects.

7 Discussion

Our findings provide new evidence about the causal effects of income on political attitudes and behaviors. We find that a large positive income shock delivered through a private guaranteed income program had limited effects on most political outcomes, with a few notable exceptions. These findings have several implications.

First, our results underscore the durability of political predispositions. Despite receiving a substantial income increase ($12,000 annually) over three years, participants showed minimal changes in a wide range of political views and behaviors, including political participation, party identification, policy preferences, trust in institutions, and support for democracy. We can often reject effects equivalent to the observational relationship between income and these outcomes. This stability of political attitudes and behaviors, even in the face of meaningful changes in economic circumstances, aligns with research suggesting that many political predispositions form early in life and tend to persist thereafter (Sears and Funk, 1999; Prior, 2019).

Second, our findings complement and help interpret prior research on the effects of government transfers and economic shocks. Previous studies have found that government-sponsored cash transfers often increase support for incumbents and voter turnout (e.g., James, Rivera and Smith, 2022; Rendleman and Yoder, 2024). Our contrasting findings—from a privately-funded transfer that participants clearly understood did not come from government—reinforce interpretations that emphasize the importance of whether voters attribute economic changes to government action (e.g., Margalit, 2019*b*; Alik-Lagrange et al., 2021; Hamel, 2024). The null effects we find on political participation and preferences suggest that prior findings of effects from government transfers likely operate through mechanisms beyond just the income change itself, such as through changes in participants' relationship with and attributions to government. Relatedly, economic shocks for which government might be responsible (such as layoffs during poor macroeconomic conditions), that change incentives, or that lead individuals to learn about the economy have been found to have effects across a variety of political outcomes we examined. The lack of such effects in our setting reinforces the interpretations of prior studies that stress the importance of these other features of economic shocks, and not the change in income itself, in generating the patterns seen in prior studies.

Third, our finding that the transfer increased positive affect towards participants' own racial group, other racial groups, and some politicians—while small in magnitude—provides further evidence for mood misattribution effects theorized by psychologists (Schwarz and Clore, 1983) and found in some prior studies of politics (e.g., Healy, Malhotra and Mo, 2010; Liberini, Redoano and Proto, 2017). This suggests that economic circumstances may influence political and social attitudes in part through their effects on general mood and affect, even when the economic changes are not attributed to particular political actors or institutions.

Fourth, our finding that receiving the transfer increased participants' views of the importance of work provides an interesting counterpoint to concerns that guaranteed income programs might reduce the perceived value of work. This finding is all the more surprising in light of our finding in a companion paper that treated participants worked slightly less (Vivalt et al., 2024). Our qualitative interviews suggest three potential mechanisms: recipients seeking to distance themselves from others who might use such transfers "the wrong way," recipients using the transfer in ways that affirmed their beliefs about their own work ethic and ability to get ahead through hard work, and some recipients developing greater motivation to earn future income after experiencing the benefits of additional resources. This finding invites future research into how experiencing guaranteed income programs shapes attitudes about work and deservingness.

The low-income composition of our sample makes some of our null findings all the more surprising, as prior research expects effects would be the largest in such a sample (e.g., Kweon, 2018). Nevertheless, several limitations of this research are worth noting. First, although the transfer represented a substantial income increase for our low-income sample, it was temporary rather than permanent. Different effects might emerge from permanent income changes. Second, while our finding that the transfer came from private rather than government sources helped isolate certain mechanisms, receipt of resources from a private charity could potentially have its own unique effects. Third, our sample was limited to adults aged 21-40 in two US states, so the effects could differ in other populations or contexts. We also note that we necessarily study the partial equilibrium effects of these transfers, and that general equilibrium effects of a large-scale private or public transfer program may be very different, and depend upon the financing, framing, and implementation of such a program (e.g., Egger et al., 2022).

These limitations notwithstanding, our findings make several contributions to understanding the relationship between economic circumstances and political behavior. Most notably, they suggest that many observed correlations between income and political outcomes may reflect factors beyond the causal effects of income itself. The relative stability of most political attitudes in the face of a substantial income shock suggests that economic circumstances may primarily affect political behavior through mechanisms other than direct effects of income—such as through changes in economic incentives, institutional interactions, or attributions of responsibility for economic conditions.

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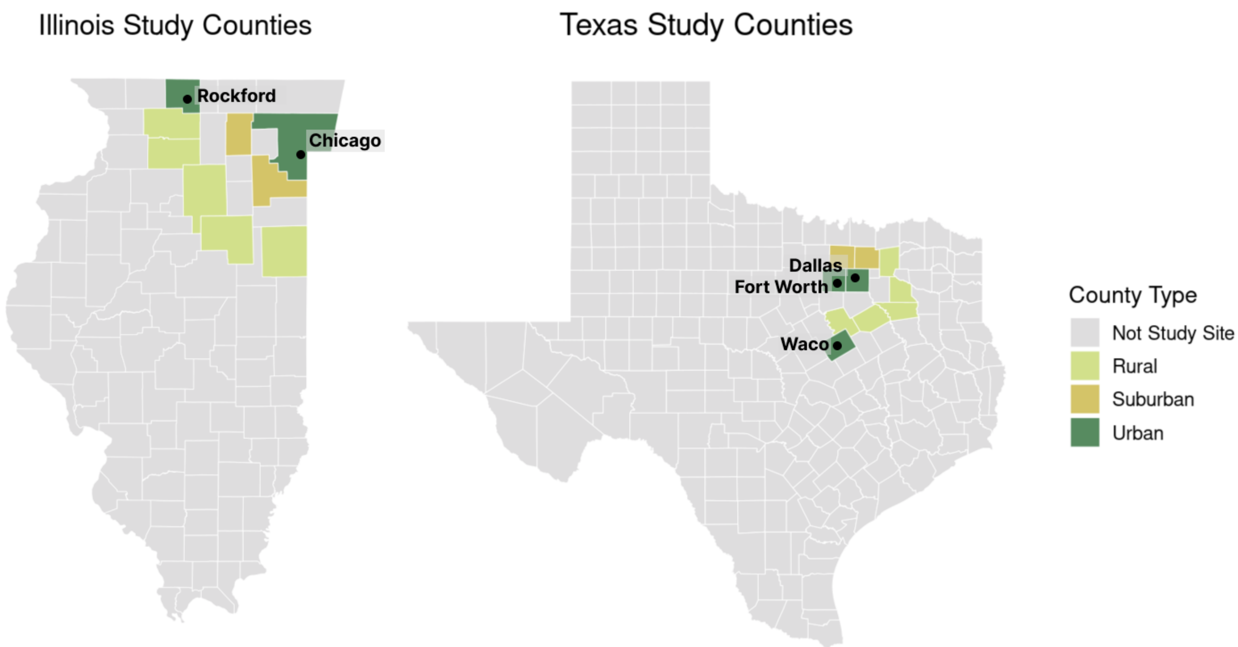
Online Appendix

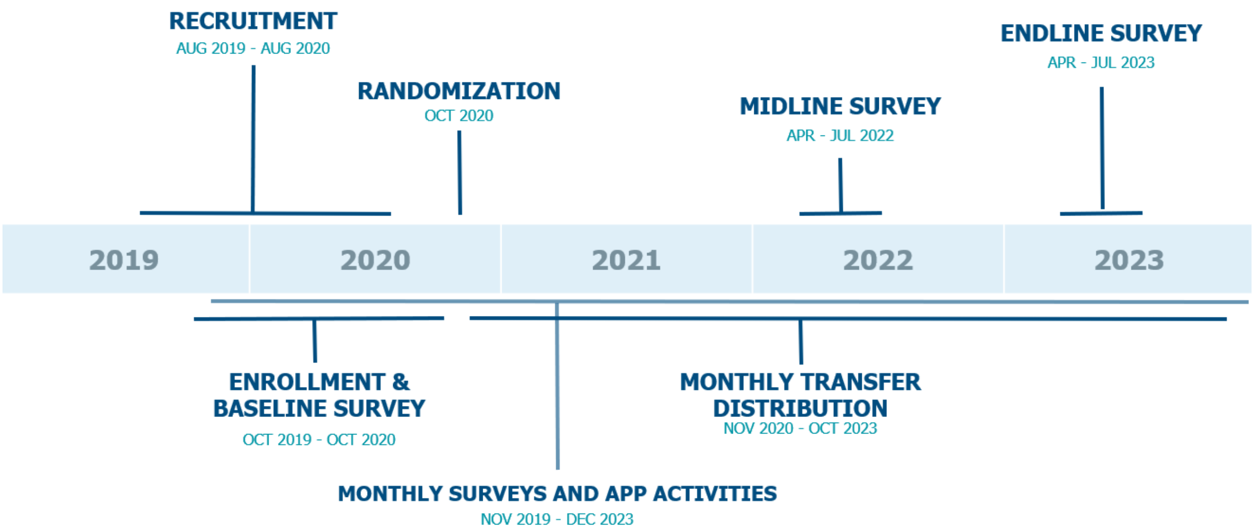
A Appendix Tables and Figures

Figure OA1: Map of Study Counties



Note: Figures display counties from which participants were recruited with designation. This figure is reproduced from Vivalt et al. (2024).

Figure OA2: Timeline of Recruitment, Enrollment, Treatment, and Research Activities



Note: Figure displays a timeline reporting the period of recruitment, enrollment of participants, cash disbursements, and research activities. See text for more details.

Table OA1: 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 the study sample to estimates of the characteristics of the study in the US as a whole. Eligible individuals are those ages 21-40 with household incomes of less than 300% of the federal poverty line. Columns (1) - (4) report estimates of the characteristics of eligible households using the American Community Survey (ACS) 2013-2017 pooled sample. Column (1) presents the unweighted means for eligible individuals, Column (2) reweights this sample to match the enrolled sample distribution of income groups as a share of the FPL (which was a stratification target when assigning individuals to the active survey group), Column (3) reweights the ACS sample to match both the income group distribution and the county-type distribution in the enrolled active survey group sample, and Column (4) presents estimates of characteristics of eligible individuals in study counties, reweighted to match the enrolled sample FPL group and county type distribution. Columns (5)-(7) report characteristics of the study sample. Columns (5) and (6) report characteristics of eligible respondents to the mailer and online advertisement reruitment methods. Column (5) is unweighted, while Column (6) is reweighted o match the enrolled sample FPL and county type distribution. Column (7) reports the unweighted mean of the ultimate enrolled active survey group (i.e. the 3000 individuals assigned to the active group who completed the baseline survey and participated in the program). In some cases variables may not add to one due to missing values.

Table OA2: Impact of ORUS program payments on public 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, eligibility was affected for other localities	Eligibility was affected by the cash transfer.
SSI	Not eligible to participate	Not eligible to participate

Notes: Table describes how ORUS program payments affected participants' eligibility for other public programs.

Table OA3: Comparison of Baseline Characteristics of those Responding to Enumerated Midline Survey 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	0.125
Male	0.317	0.325	0.678	0.387	0.450	0.616
Female	0.680	0.672	0.681	0.613	0.550	0.616
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	0.523
Non-Hispanic Asian	0.037	0.035	0.765	0.053	0.100	0.522
Non-Hispanic White	0.463	0.471	0.689	0.467	0.550	0.512
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	0.699
Household Size	3.002	2.947	0.423	2.867	2.850	0.969
Any Children	0.573	0.569	0.851	0.520	0.550	0.813
# Children	1.402	1.437	0.576	1.320	1.400	0.841
Economic						
Employed	0.587	0.580	0.738	0.560	0.450	0.387
Personal Income $(1000s)$	21.227	21.481	0.753	21.097	14.862	0.133
Household Income $(1000s)$	29.963	30.170	0.786	29.538	21.969	0.060
Under FPL	0.335	0.320	0.415	0.360	0.500	0.270
HS Degree/GED or higher	0.938	0.953	0.084	0.960	0.950	0.854

Notes: This table compares baseline characteristics for each treatment arm across respondents and non-respondents to the midline survey.

Table OA4: Comparison of Baseline Characteristics of those Responding to Enumerated Endline Survey 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	0.420
Male	0.313	0.326	0.478	0.447	0.350	0.416
Female	0.685	0.671	0.465	0.544	0.650	0.371
Non-binary/other	0.003	0.003	0.838	0.010	0.000	0.321
Non-Hispanic Black	0.308	0.297	0.515	0.243	0.250	0.946
Non-Hispanic Asian	0.038	0.037	0.892	0.029	0.000	0.084
Non-Hispanic White	0.466	0.470	0.836	0.417	0.550	0.281
Non-Hispanic Native American	0.025	0.019	0.261	0.019	0.100	0.245
Hispanic	0.208	0.222	0.393	0.320	0.150	0.069
Household Size	3.008	2.952	0.411	2.806	2.850	0.925
Any Children	0.574	0.571	0.883	0.505	0.550	0.713
# Children	1.411	1.433	0.738	1.146	1.900	0.095
Economic						
Employed	0.585	0.582	0.871	0.602	0.400	0.097
Personal Income $(1000s)$	21.248	21.450	0.802	20.769	18.539	0.655
Household Income $(1000s)$	29.951	30.171	0.774	29.865	25.930	0.338
Under FPL	0.338	0.321	0.352	0.301	0.350	0.675
HS Degree/GED or higher	0.939	0.954	0.082	0.941	0.950	0.872

Notes: This table compares baseline characteristics for each treatment arm across respondents and non-respondents to the endline survey.

Table OA5: Comparison of Baseline Characteristics of those Responding to At Least One 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	0.848
Male	0.313	0.321	0.683	0.565	0.800	0.073
Female	0.684	0.676	0.685	0.435	0.200	0.073
Non-binary/other	0.003	0.003	0.990	0.000	0.000	.
Non-Hispanic Black	0.307	0.295	0.516	0.239	0.267	0.836
Non-Hispanic Asian	0.038	0.037	0.810	0.022	0.000	0.324
Non-Hispanic White	0.462	0.472	0.620	0.478	0.533	0.716
Non-Hispanic Native American	0.025	0.019	0.308	0.000	0.067	0.313
Hispanic	0.212	0.221	0.589	0.304	0.200	0.410
Household Size	2.999	2.947	0.445	2.848	2.667	0.707
Any Children	0.573	0.570	0.851	0.457	0.467	0.947
# Children	1.402	1.448	0.470	1.239	0.600	0.102
Economic						
Employed	0.585	0.575	0.574	0.609	0.800	0.140
Personal Income $(1000s)$	21.235	21.319	0.916	20.459	23.716	0.492
Household Income $(1000s)$	29.949	29.959	0.990	28.571	32.146	0.614
Under FPL	0.336	0.322	0.433	0.326	0.400	0.616
HS Degree/GED or higher	0.938	0.952	0.101	0.977	1.000	0.298

Notes: This table compares baseline characteristics for each treatment arm across respondents and non-respondents to the Qualtrics surveys in year 1 of the study.

Table OA6: Comparison of Baseline Characteristics of those Responding to At Least One 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	0.087
Female	0.682	0.678	0.843	0.581	0.400	0.087
Non-binary/other	0.003	0.003	0.984	0.000	0.000	.
Non-Hispanic Black	0.304	0.295	0.623	0.337	0.300	0.707
Non-Hispanic Asian	0.038	0.037	0.950	0.035	0.000	0.083
Non-Hispanic White	0.464	0.471	0.753	0.430	0.533	0.335
Non-Hispanic Native American	0.026	0.019	0.214	0.000	0.067	0.149
Hispanic	0.214	0.223	0.553	0.233	0.133	0.204
Household Size	3.018	2.948	0.317	2.512	2.833	0.348
Any Children	0.575	0.569	0.741	0.465	0.567	0.341
# Children	1.405	1.445	0.532	1.244	1.167	0.808
Economic						
Employed	0.588	0.571	0.384	0.547	0.800	0.006
Personal Income $(1000s)$	21.264	21.203	0.939	20.281	26.052	0.183
Household Income $(1000s)$	30.013	30.034	0.979	28.378	29.111	0.872
Under FPL	0.335	0.322	0.468	0.337	0.367	0.774
HS Degree/GED or higher	0.938	0.953	0.102	0.953	0.967	0.732

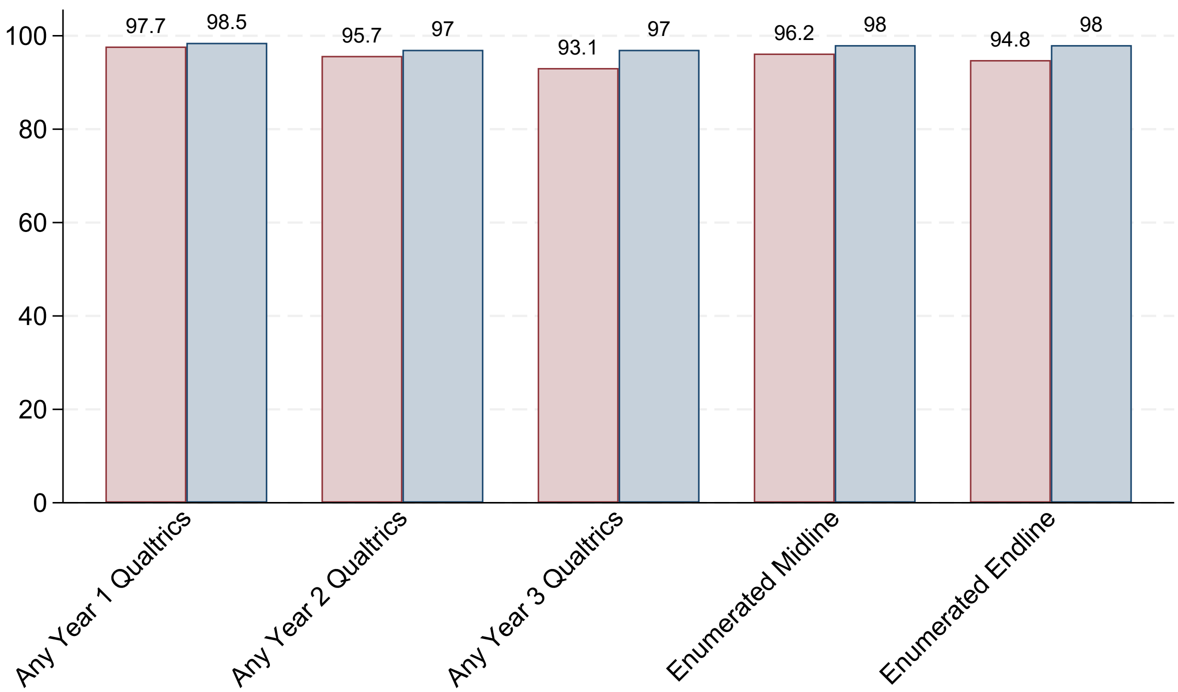
Notes: This table compares baseline characteristics for each treatment arm across respondents and non-respondents to the Qualtrics surveys in year 2 of the study.

Table OA7: Comparison of Baseline Characteristics of those Responding to At Least One 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	0.542
Male	0.306	0.324	0.351	0.496	0.433	0.532
Female	0.691	0.673	0.341	0.496	0.567	0.486
Non-binary/other	0.003	0.003	0.848	0.007	0.000	0.320
Non-Hispanic Black	0.307	0.295	0.504	0.277	0.333	0.556
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	0.450
Non-Hispanic Native American	0.025	0.018	0.202	0.022	0.100	0.169
Hispanic	0.209	0.222	0.424	0.285	0.167	0.136
Household Size	3.019	2.950	0.324	2.737	2.867	0.703
Any Children	0.578	0.572	0.748	0.482	0.533	0.611
# Children	1.420	1.448	0.659	1.117	1.233	0.737
Economic						
Employed	0.585	0.573	0.513	0.591	0.767	0.049
Personal Income $(1000s)$	21.273	21.237	0.965	20.751	26.775	0.109
Household Income $(1000s)$	29.881	29.943	0.937	30.806	34.815	0.290
Under FPL	0.342	0.324	0.318	0.255	0.233	0.798
HS Degree/GED or higher	0.937	0.955	0.039	0.956	0.900	0.336

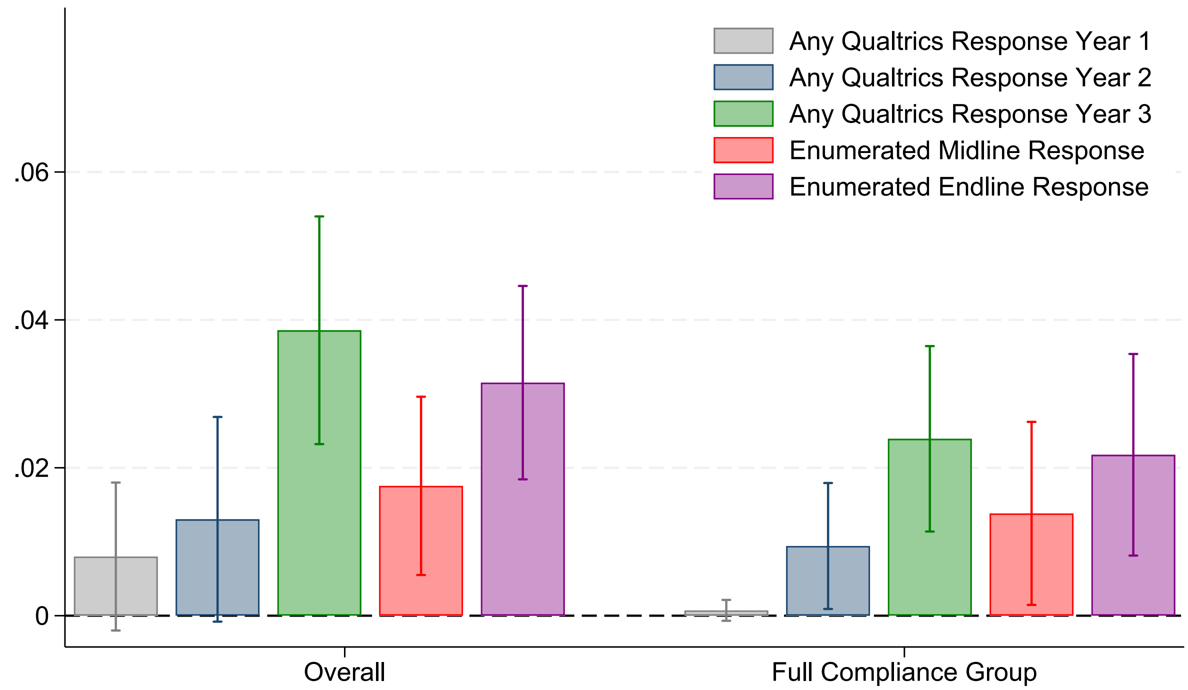
Notes: This table compares baseline characteristics for each treatment arm across respondents and non-respondents to the Qualtrics surveys in year 3 of the study.

Figure OA3: Response Rates by Control (red) and Treated (grey) Participants



Note: Figure plots response rates by treatment arm to different surveys and data collection activities. The top panel shows response rates to mobile surveys through Qualtrics and for the enumerated midline and endline surveys. The bottom panel shows participation in nutrition diaries and biomarker collection.

Figure OA4: Differential (Treatment minus Control) Response Based on Pre-Treatment Responsiveness



Note: Figure displays differential (treatment minus control) response rates in full sample ("Overall") and in the approximately 71% of the sample who completed all pre-treatment surveys ("Full Compliance Group"). Bars indicate 95% confidence interval of this difference.

B Numerical Estimates and Observational Gradients

Table OA8: Estimated Effects on Political Participation

	Gradient	Control Mean and SD	Estimated Effect	Can Reject Effect Greater Than:
Political Participation Family			**-0.012 (0.014) [0.141]**	0.015 SDs
External political efficacy	0.0126	2.46 (0.79)	0.022 (0.027) [0.979]	0.074 SDs
General political knowledge index	0.0287‡‡	0.19 (0.31)	0.008 (0.009) [0.979]	0.026 SDs
Politics confusing (lower internal efficacy)	-0.0425‡‡‡	2.93 (1.12)	0.070 (0.038)* [0.970]	0.144 SDs
Neighborhood political engagement	0.0028	0.20 (0.35)	-0.003 (0.012) [1.000]	0.021 SDs
Self-reported News Consumption	0.0435‡‡	2.69 (1.04)	-0.038 (0.032) [0.979]	0.025 SDs
Self-reported political participation index	0.0034‡	0.12 (0.18)	-0.006 (0.006) [0.979]	0.005 SDs
Voter Turnout			0.003 (0.022) [1.000]	0.047 SDs
Respondent is registered to vote	0.0098	0.78 (0.41)	0.011 (0.014) [1.000]	0.039 SDs
Respondent voted in the 2022 general election	0.0312‡‡‡	0.30 (0.46)	-0.008 (0.015) [1.000]	0.021 SDs
Respondent voted in the 2022 primary elections	0.0077	0.11 (0.31)	0.000 (0.012) [1.000]	0.023 SDs
Respondent is registered to vote (using BL consent only)	0.0098	0.80 (0.40)	0.016 (0.014) [1.000]	0.044 SDs
Respondent voted in the 2022 general election (using BL consent only)	0.0312‡‡‡	0.30 (0.46)	-0.008 (0.015) [1.000]	0.022 SDs
Respondent voted in the 2022 primary elections (using BL consent only)	0.0077	0.11 (0.31)	0.001 (0.012) [1.000]	0.024 SDs

Notes: This table reports estimated treatment effects of the transfer payments on outcomes listed in the rows. The family-level effect is reported in bold at the top of the table. Underlined outcomes represent components that aggregate individual level outcomes listed below them into a single index; when no outcomes are listed below, this component was measured with the single item listed only. In instances when there is more than one outcome related to the component topic, these are measured in standard deviations. The column "Gradient" shows the size of the effect a $11,400 increase in annual income would be predicted to have based solely on the pre-treatment correlation. The column "Can Reject Effect Greater Than" shows the effect size that can be ruled out with a two-sided test based on the 95% confidence interval of the estimate. See text for further details. ‡ denotes the significance level of the test with which we can reject the size of the baseline gradient. ∗ denotes traditional significance levels. † denotes significance levels based on q-values adjusted to control the false discovery rate. In all cases, three symbols denotes significance at the 1% level; two denote significance at the 5% level; one denotes significance at the 10% level of the test.

Table OA9: Estimated Effects on Intergroup Attitudes

	Gradient	Control Mean and SD	Estimated Effect	Can Reject Effect Greater Than:
Intergroup Attitudes Family			**0.067 (0.022)*** ††† [0.007]**	
Attitude towards other racial groups besides the respondent's own	0.6974‡	66.08 (24.08)	2.148 (0.780)***††† [0.009]	
Favorability rating of own ethnic / racial group	0.6267	71.13 (23.29)	1.731 (0.785)**†† [0.019]	
Gender Relations			0.037 (0.023)†† [0.039]	0.082 SDs
Men make better business leaders than women do	0.0301‡‡	2.80 (1.54)	-0.068 (0.046)† [0.092]	0.022 SDs
If both partners in a couple work, they should share equally in the housework and care of children	-0.0255	5.95 (1.17)	0.034 (0.040) [0.201]	0.113 SDs

Notes: This table reports estimated treatment effects of the transfer payments on outcomes listed in the rows. The family-level effect is reported in bold at the top of the table. Underlined outcomes represent components that aggregate individual level outcomes listed below them into a single index; when no outcomes are listed below, this component was measured with the single item listed only. In instances when there is more than one outcome related to the component topic, these are measured in standard deviations. The column "Gradient" shows the size of the effect a $11,400 increase in annual income would be predicted to have based solely on the pre-treatment correlation. The column "Can Reject Effect Greater Than" shows the effect size that can be ruled out with a two-sided test based on the 95% confidence interval of the estimate. See text for further details. ‡ denotes the significance level of the test with which we can reject the size of the baseline gradient. ∗ denotes traditional significance levels. † denotes significance levels based on q-values adjusted to control the false discovery rate. In all cases, three symbols denotes significance at the 1% level; two denote significance at the 5% level; one denotes significance at the 10% level of the test.

Table OA10: Estimated Effects on Political Trust and Polarization

	Gradient	Control Mean and SD	Estimated Effect	Can Reject Effect Greater Than:
Political Trust and Polarization Family			**0.026 (0.013)*†** **[0.095]**	0.051 SDs
Affective polarization	0.3960	38.22 (29.15)	-0.595 (1.163) [0.857]	-2.875 SDs
Authoritarian index	0.0342‡‡‡	2.69 (0.69)	-0.025 (0.021) [0.492]	-0.066 SDs
Support for democracy index	0.0320	2.81 (0.55)	0.018 (0.016) [0.492]	0.049 SDs
Support for incumbent governor	0.5397‡‡	35.97 (29.83)	2.411 (0.944)** [0.163]	
Trust In Institutions			0.021 (0.025) [0.670]	0.069 SDs
Level of trust in congress	-0.0144	1.94 (1.86)	0.024 (0.061) [0.959]	0.143 SDs
Level of trust in politicians	-0.0075	1.70 (1.74)	0.064 (0.057) [0.513]	0.175 SDs
Level of trust in the legal system	0.0773‡‡	2.42 (2.19)	-0.062 (0.067) [0.670]	0.070 SDs
Level of trust in your state government	0.0213	3.39 (2.39)	0.146 (0.081)* [0.237]	0.304 SDs
Level of trust in most people	0.1233‡‡	4.70 (2.15)	-0.017 (0.069) [0.857]	0.118 SDs

Notes: This table reports estimated treatment effects of the transfer payments on outcomes listed in the rows. The family-level effect is reported in bold at the top of the table. Underlined outcomes represent components that aggregate individual level outcomes listed below them into a single index; when no outcomes are listed below, this component was measured with the single item listed only. In instances when there is more than one outcome related to the component topic, these are measured in standard deviations. The column "Gradient" shows the size of the effect a $11,400 increase in annual income would be predicted to have based solely on the pre-treatment correlation. The column "Can Reject Effect Greater Than" shows the effect size that can be ruled out with a two-sided test based on the 95% confidence interval of the estimate. See text for further details. ‡ denotes the significance level of the test with which we can reject the size of the baseline gradient. ∗ denotes traditional significance levels. † denotes significance levels based on q-values adjusted to control the false discovery rate. In all cases, three symbols denotes significance at the 1% level; two denote significance at the 5% level; one denotes significance at the 10% level of the test.

Table OA11: Estimated Effects on Political Preferences and Attitudes

	Gradient	Control Mean and SD	Estimated Effect	Can Reject Effect Greater Than:
Political Preferences Family			**0.023 (0.014)*†** **[0.097]**	0.050 SDs
<u>Dem party identification index (1 = Republican, 5 = Democrat)</u>	-0.0056	3.45 (1.33)	0.032 (0.033) [0.281]	0.096 SDs
<u>Economic Liberalism And Preferences For Redistribution</u>			0.011 (0.015) [0.308]	0.041 SDs
Adults without disabilities should be required to work or be looking for work in order to get government health insurance through Medicaid	0.0421	0.61 (0.43)	0.034 (0.014)** [0.343]	
Government should provide fewer services, even in areas such as health and education, in order to reduce spending	0.0158‡	0.15 (0.32)	-0.002 (0.011) [1.000]	0.019 SDs
Government should reduce income diffs	-0.0191‡‡	0.72 (0.40)	0.006 (0.013) [0.963]	0.031 SDs
Government should see to it everyone gets job	-0.0290‡‡‡	0.73 (0.40)	0.012 (0.013) [0.568]	0.037 SDs
Income should be unequal as incentive for effort	0.1673‡‡‡	5.70 (2.56)	-0.150 (0.080)* [0.343]	0.006 SDs
Increase funding for gov't programs for poor and unemployed, even if it would raise taxes	-0.0256‡‡‡	0.74 (0.39)	0.018 (0.013) [0.359]	0.043 SDs
Increase taxes on those making over \$250k/yr	0.0046	0.73 (0.40)	0.005 (0.013) [0.973]	0.030 SDs
Individuals should take more responsibility than government for whether people are cared for	0.2371‡‡‡	4.52 (2.59)	0.008 (0.081) [1.000]	0.166 SDs
<u>Social Liberalism</u>			0.035 (0.019)* [0.214]	0.073 SDs
Abortion should always be allowed as a matter of personal choice	-0.0105‡‡‡	0.74 (0.41)	0.023 (0.011)** [0.343]	
Same sex couples should be allowed to marry	-0.0072	0.77 (0.39)	0.005 (0.011) [0.963]	0.026 SDs
Support for increasing immigration	0.0126	0.47 (0.45)	0.017 (0.015) [0.568]	0.047 SDs

Notes: This table reports estimated treatment effects of the transfer payments on outcomes listed in the rows. The family-level effect is reported in bold at the top of the table. Underlined outcomes represent components that aggregate individual level outcomes listed below them into a single index; when no outcomes are listed below, this component was measured with the single item listed only. In instances when there is more than one outcome related to the component topic, these are measured in standard deviations. The column "Gradient" shows the size of the effect a \$11,400 increase in annual income would be predicted to have based solely on the pre-treatment correlation. The column "Can Reject Effect Greater Than" shows the effect size that can be ruled out with a two-sided test based on the 95% confidence interval of the estimate. See text for further details. ‡ denotes the significance level of the test with which we can reject the size of the baseline gradient. ∗ denotes traditional significance levels. † denotes significance levels based on q-values adjusted to control the false discovery rate. In all cases, three symbols denotes significance

Table OA12: Estimated Effects on Other-Regarding Preferences

	Gradient	Control Mean and SD	Estimated Effect	Can Reject Effect Greater Than:
Other Regarding Family			**0.106 (0.020)*****††† **[0.001]**	
Today you unexpectedly received $1,600. How much of this amount would you donate to a good cause?	-9.1057‡‡‡	226.49 (248.73)	46.944 (8.669)***††† [0.001]	
Volunteer Hrs/Yr (conditionally winsorized)	3.6519	21.80 (55.20)	0.723 (2.034) [0.525]	4.709 SDs
How much money participant donated to charity in the past year (conditonally winsorized)	113.0000‡	545.16 (1550.22)	29.622 (44.340) [0.525]	116.528 SDs
Pro-social behaviors index	0.0334	8.26 (3.47)	0.132 (0.102) [0.298]	0.333 SDs

Notes: This table reports estimated treatment effects of the transfer payments on outcomes listed in the rows. The family-level effect is reported in bold at the top of the table. Underlined outcomes represent components that aggregate individual level outcomes listed below them into a single index; when no outcomes are listed below, this component was measured with the single item listed only. In instances when there is more than one outcome related to the component topic, these are measured in standard deviations. The column "Gradient" shows the size of the effect a $11,400 increase in annual income would be predicted to have based solely on the pre-treatment correlation. The column "Can Reject Effect Greater Than" shows the effect size that can be ruled out with a two-sided test based on the 95% confidence interval of the estimate. See text for further details. ‡ denotes the significance level of the test with which we can reject the size of the baseline gradient. ∗ denotes traditional significance levels. † denotes significance levels based on q-values adjusted to control the false discovery rate. In all cases, three symbols denotes significance at the 1% level; two denote significance at the 5% level; one denotes significance at the 10% level of the test.

Table OA13: Estimated Effects on Attitudes Towards Work

	Gradient	Control Mean and SD	Estimated Effect	Can Reject Effect Greater Than:
Work Attitudes Family			**0.033 (0.018)*†** **[0.097]**	0.069 SDs
Explanation For Economic Inequality			-0.004 (0.020) [0.385]	0.035 SDs
Contextual Index of Inequality	-0.0064	3.43 (0.77)	0.007 (0.026) [1.000]	0.058 SDs
Dispositional Index of Inequality	0.0178	3.51 (0.87)	0.001 (0.026) [1.000]	0.053 SDs
Intrinsic Importance of Work Index	0.0869	3.16 (0.76)	0.053 (0.022)**† [0.050]	

Notes: This table reports estimated treatment effects of the transfer payments on outcomes listed in the rows. The family-level effect is reported in bold at the top of the table. Underlined outcomes represent components that aggregate individual level outcomes listed below them into a single index; when no outcomes are listed below, this component was measured with the single item listed only. In instances when there is more than one outcome related to the component topic, these are measured in standard deviations. The column "Gradient" shows the size of the effect a $11,400 increase in annual income would be predicted to have based solely on the pre-treatment correlation. The column "Can Reject Effect Greater Than" shows the effect size that can be ruled out with a two-sided test based on the 95% confidence interval of the estimate. See text for further details. ‡ denotes the significance level of the test with which we can reject the size of the baseline gradient. ∗ denotes traditional significance levels. † denotes significance levels based on q-values adjusted to control the false discovery rate. In all cases, three symbols denotes significance at the 1% level; two denote significance at the 5% level; one denotes significance at the 10% level of the test.

C Estimated Effects Over Time

Figure OA5: Estimated Effects on Political Participation Over Time

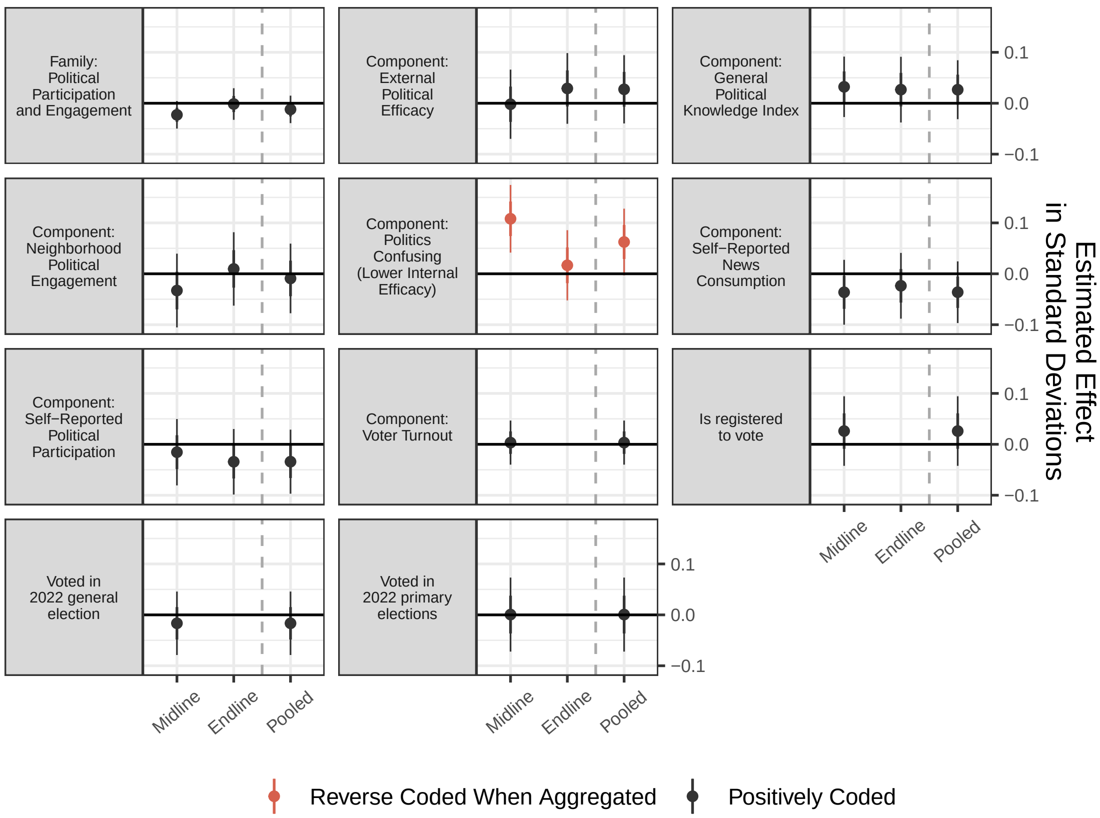


Figure OA6: Estimated Effects on Intergroup Attitudes Over Time

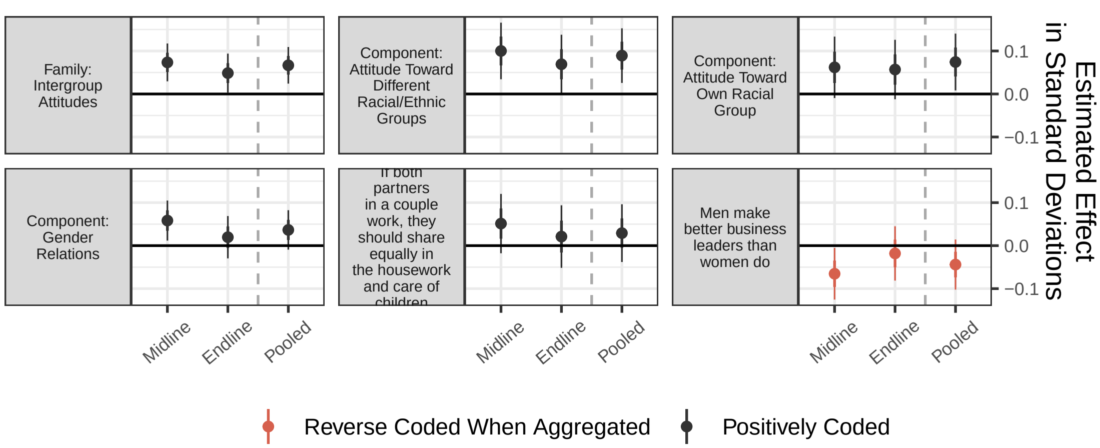


Figure OA7: Estimated Effects on Other-Regarding Preferences Over Time

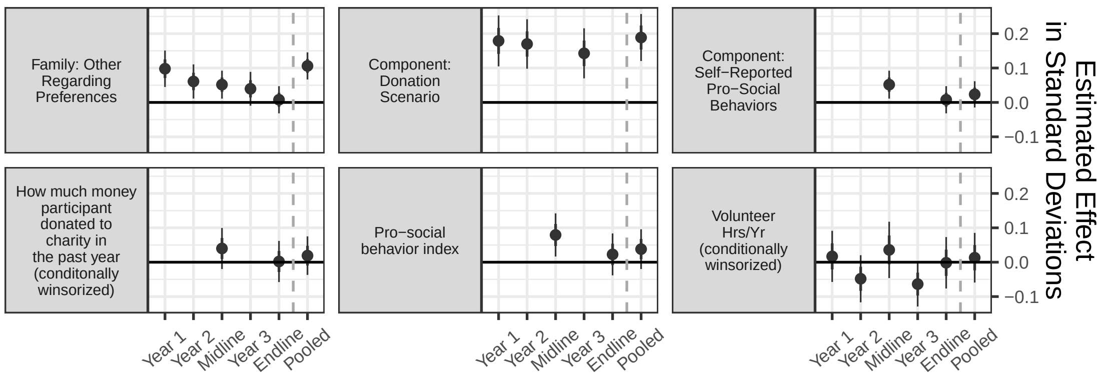


Figure OA8: Estimated Effects on Political Preferences and Attitudes Over Time

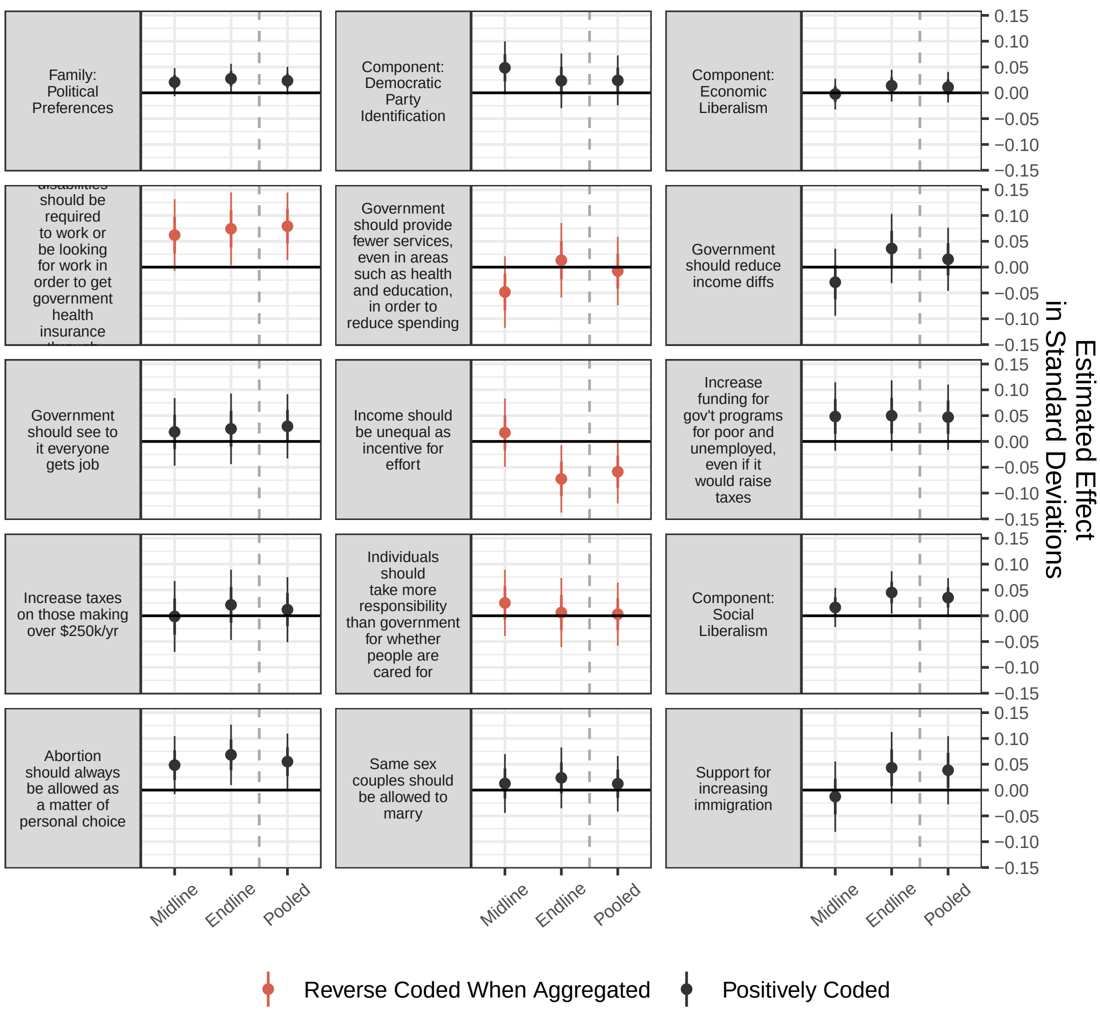


Figure OA9: Estimated Effects on Political Trust and Polarization Over Time

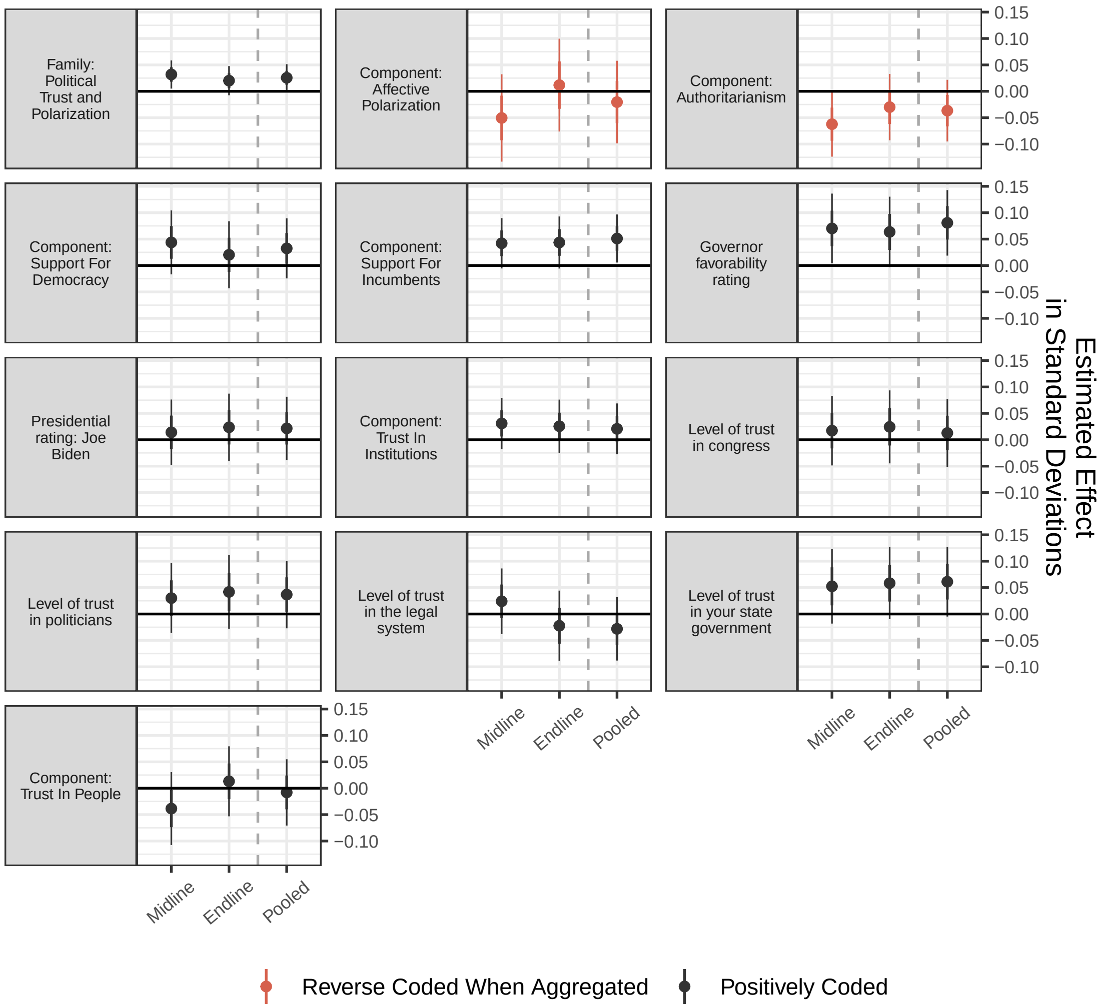
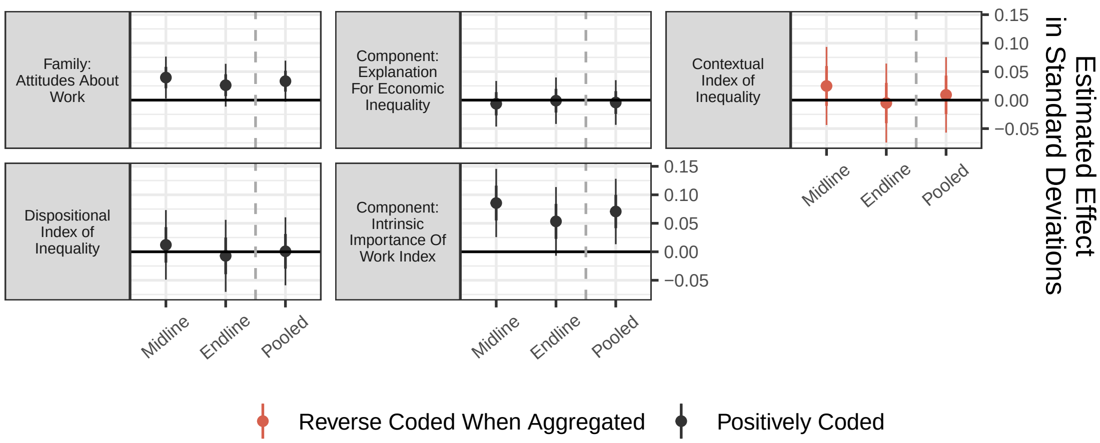


Figure OA10: Estimated Effects on Attitudes Towards Work Over Time



D Amendments to original pre-analysis plan

Over the several years long period of the intervention and analysis, we made several small amendments to our original pre-analysis plan. These amendments were due to feedback we received and our evolving understanding about how best to analyze, structure, and present our results. The following were made prior to the receipt of the enumerated midline data, and before any analysis of the data had occurred:

- In our updated pre-analysis plan, we changed our approach to multiple hypothesis testing from calculating family-wise error rate adjusted p-values to presenting tiered false discovery rate q-values.

Additionally, we made additional changes following the midline survey, although most of these were implemented before we had computed any treatment effects.

- Our pre-analysis plan specified that, in pooling items across time, we would impute any time periods for which an item was missing with the treatment group specific mean at that time period, and consider the pooled item as non-missing as long as the outcome was observed for at least one time period. In the current version, we do not perform such an imputation, and instead average over non-missing time periods. Results are similar if we instead use the original version of the imputation.
- We did not originally anticipate the need to winsorize some outcomes.
- The pre-analysis plan specified that a robustness check using median regression would be reported for outcomes potentially susceptible to outliers (e.g., those based on expenditures). However, given that this concern affects only very few outcomes, and the fact that we are already reporting a large number of tables, we opted the skip this robustness check in the interest of space.

E Random assignment simulation results

As mentioned in the main text, we used a re-randomization procedure to increase balance on key covariates. In particular, we imposed a p-value floor, with covariates we deemed to be more important assigned a higher floor; these floors were determined ex ante. We rejected any randomization where the p-value on a t-test for difference across treatment arms was below the p-value floor for any of the selected variables and re-randomized, using a procedure similar to the one described in Zhao and Ding (2024). We also conducted an F-test for the joint significance of all of the same set of pre-treatment variables by outcome area and rejected a randomization if the p-value on the F-test was over 0.25.

If there were large outliers in the data, imposing balance in this way may generate a situation where some participants were more likely to be assigned to treatment than others. To examine this, we conducted 1,000 simulations and verified that this procedure resulted in all observations having a 1/3 probability of being assigned to the treatment group. We could not reject that the simulated distribution of treatment assignments was significantly different from what we would observe from a Bernouilli distribution with a one third probability of success. Furthermore, no baseline characteristics predicted the average probability over these 1,000 simulations that any participant received treatment. The remainder of this section provides more discussion and the results of this simulation.[26]

We assessed the validity of our random assignment procedure by re-running the procedure 1,000 times to obtain 1,000 counterfactual treatment assignments. Then, we analyzed the distribution of these treatment assignments to assess whether they were consistent with each participant

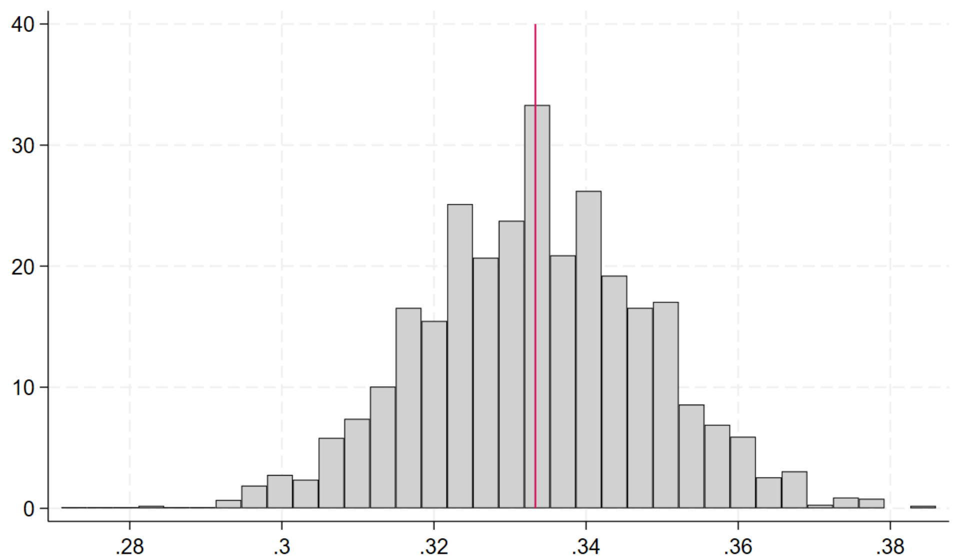
[26]Early in the study, one participant who was randomized into the treatment group was removed from the program by the non-profit partner, but continued to participate in the research activities. Another participant was assigned to treatment but initially declined, and was replaced from the waitlist. However, this participant changed their mind and ended up accepting the treatment. We use the original treatment assignment to calculate treatment effects, with 1001 participants assigned to treatment and 1000 actually receiving the cash transfers. Our estimates are therefore, technically intent-to-treat (ITT) effects. However, given that the first stage effect of treatment assignment on program participation exceeds 0.999, the local average treatment effects are essentially indistinguishable from the ITT.

having a one third probability of being assigned to the treatment group. Our analysis of these 1,000 permutations indicated our procedure was valid.

First, we examined the distribution of treatment probabilities for each participant to ensure it was centered on one third. Appendix Figure OA11 shows a histogram of assignment probabilities. The mean and median treatment probability are 0.333. We also compared the observed distribution of average treatment assignments to what we might expect from a Bernouilli distribution with a one third probability of success. A quantile-quantile (QQ) plot comparing this distribution to our observed distribution of treatment under our 1,000 simulations is shown in Appendix Figure OA12. Most points fall on the 45 degree line, and a Kolmogorov-Smirnov test of equality of these distributions fails to reject that they are the same ($p = 0.52$).

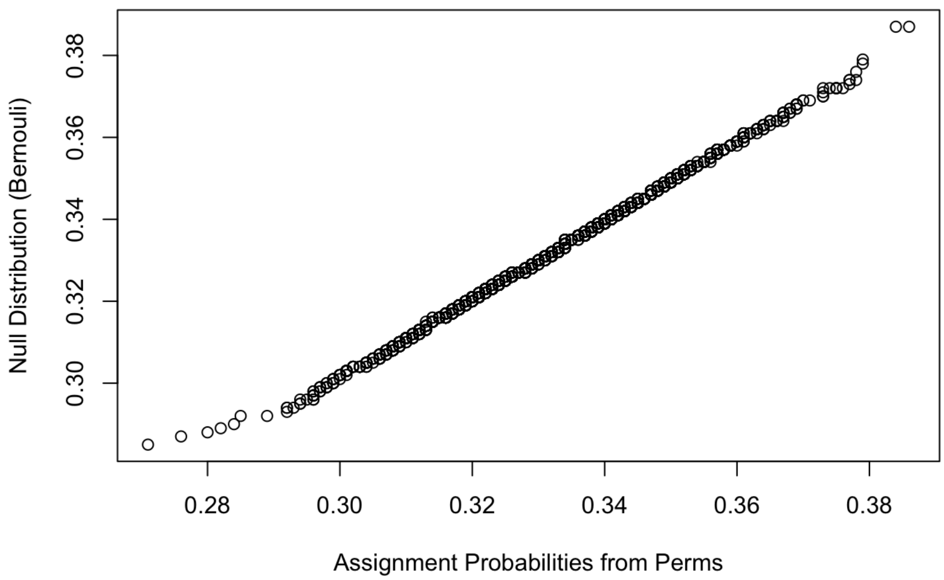
Second, we examined whether participants' baseline characteristics could predict the probability the participant received treatment under our randomization procedure. We regressed the average probability of being assigned to treatment across our 1000 simulations for each participant with all baseline characteristics listed in Table 1. We also included variables that were relevant in the randomization: the number of individuals in the participants' cluster and whether the participant was at the Texas site rather than the Illinois site. None of these variables is significantly correlated with the probability of being assigned to the treatment group, and all coefficients are very small. These combined analyses reassure us that our randomization procedure produced a treatment assignment that is uncorrelated with any participant unobservables.

Figure OA11: Histogram of treatment assignment probabilities



Note: This figure shows a histogram of average treatment assignment for each participant calculated after 1,000 simulations of the treatment assignment procedure. Vertical line indicates 0.33333.

Figure OA12: QQ-plot of treatment probability against Bernoulli distribution with one third success probability



Note: This figure compares the distribution of treatment assignments (quantiles plotted on x-axis) to what we would expect from a random one-third probability of assignment (quantiles plotted on y-axis). A Kolmogorov-Smirnov test fails to reject the null hypothesis that these distributions are the same (p=0.5226).