POVERTY SOLUTIONS
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THE COVID CASH TRANSFER STUDIES: KEY FINDINGS AND FUTURE DIRECTIONS

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INTRODUCTION

In March 2020, the COVID-19 pandemic hit the United States and resulted in businesses, schools, and other non-essential operations being shut down. In response, the federal government passed and administered a series of historically robust cash transfer programs, including a series of Economic Impact Payments (EIPs, know as stimulus checks), expanded unemployment insurance, and an expanded child tax credit. Studies find these measures helped low- and middle-income families build their savings, and protected many families from rising poverty and hardship (Chetty et al., 2020; Board of Governors of the Federal Reserve System, 2020; Evangelist, Wu and Shaefer, 2021).

Many non-governmental charitable organizations also responded to the economic crisis in significant ways. GiveDirectly began providing individuals receiving federal food assistance (Supplemental Nutrition Assistance Program, or SNAP benefits) with a one-time, unconditional cash transfer of $1,000 during spring 2020. Our research team was invited to study the effects of these transfer payments using a randomized controlled trial experiment in May 2020 (RCT1), and then again in fall 2020 (RCT2).

Despite widespread interest in cash transfers as a means to alleviate poverty and hardship in recent years, many questions regarding the best approaches, optimal amounts, and effects remain unanswered, especially in the U.S. Most studies of unconditional cash transfers have been conducted outside the U.S., provide recurring cash payments, or involve transfer amounts that would be prohibitively expensive to replicate in the U.S. Few, if any, rigorous experiments have been conducted in the U.S. to examine one-time, modest cash transfers and their impact on well-being. This evaluation offered an opportunity to do so.

BASIC FACTS ABOUT THE EVALUATION

For in-depth information on the two studies, see Pilkauskas, Jacob, Rhodes, Richard, and Shaefer, 2022 (RCT1), and Jacob, Pilkauskas, Rhodes, Richard, and Shaefer, 2022 (RCT2). In brief, the study samples were recruited from individuals using a free mobile application called Providers (formerly called Fresh EBT) that helps users manage their public assistance benefits, most commonly food assistance from the Supplemental Nutrition Assistance Program (SNAP). Providers serves over 5 million SNAP beneficiaries. At the time of our study, Providers served approximately 4 million SNAP beneficiaries, and today they serve nearly one-quarter of all SNAP households. In both studies, individuals were randomly assigned to treatment and control groups.

The first study, RCT1, began in May 2020 and enrolled 9,433 low-income families with children across 12 states (4,068 control and 5,365 treatment group members, 99 percent of whom took up the treatment). A total of 6,848 families responded to a one-month follow-up survey (4,070 treatment and 2,778 control). Families were asked about five outcomes: material hardship, mental health, parenting, child behavior, and relationship quality. In September 2020, we conducted the second study (RCT2), which included a broader set of material hardship and mental health measures. This second study also expanded the sample to include lower-income households with and without children in 49 states (as well as D.C. and Puerto Rico), focusing on 1,265 high-poverty zip codes, with a total 13,755 participants (6,720 in the control group and 7,035 in the experimental group, 4,825 of whom claimed the transfer). In contrast to RCT1, only 68.6% of the treatment group in RCT2 took up the treatment. This was due to a change in the way families were recruited into the study. In RCT1, families were randomly assigned only after expressing interest and answering a set of screener questions. In RCT2, families were randomly assigned prior to any contact. After random assignment, families were shown a banner and if they clicked on the banner, they had the opportunity to obtain the transfer.

In both studies, the treatment consisted of an offer of a one-time, unconditional, lump-sum $1,000 cash payment. As a

one-time, lump-sum transfer, the payment did not impact recipients' eligibility for other public assistance benefits. Our primary outcomes from both studies come from a survey of study participants fielded roughly four to six weeks following the cash transfer. In both studies, the characteristics of the treatment and control groups who completed the survey were generally balanced. The surveys asked respondents a variety of questions, including questions about basic demographics, participation in various social programs, experiences related to economic and psychological well-being, and expenditures.

Our primary outcome measures in both studies were survey-based indexes of material hardship and mental health outcomes. Following the approach used in prior research, the surveys asked respondents to indicate whether they experienced various types of hardship during the past month, ranging from not being able to pay the full amount of rent, utilities, phone or internet to whether they worry about running out of food to whether they have skipped needed medical care because of the cost. Mental health challenges are measured by an index that includes validated, reliable measures of anxiety and depression (see papers for more details). For all outcomes we create composite measures, following the approach outlined in Anderson (2008) to calculate a weighted mean of the standardized survey items within the domain. We also analyzed all of the individual components of each index. In the other outcomes, we also used validated measures of partner conflict, child behavior problems, and parenting problems. See the working papers for more detail.

Based on prior research, we expected to see discernable impacts of the $1,000 cash transfers across hardship domains, but we expected impacts to be modest and rates of hardship among the disadvantaged sample to remain high. We further anticipated that any effects would fade over time. In RCT1, we conducted a one-month and three-month follow-up survey; in RCT2 we conducted one follow-up survey 30-45 days after treatment.

MAIN RESULTS

In RCT1, we find that the intervention did not affect material well-being in the full sample, but did reduce material hardship for a subsample of very low-income households (defined as those reporting <$500 in earned monthly income, approximately the bottom 50% of the income distribution based on household monthly income). There is some suggestive evidence that the transfer improved mental health among the very low-income sample, although this finding is not robust to tests accounting for multiple hypothesis testing. The magnitude of the impacts on the very low-income households that we measured were roughly in line with our expectations going into the study. In the second study, we find no discernable impacts on any of the outcomes we measure for either the full sample or any of the pre-specified subgroups (very low monthly income, low annual income, unemployed). Our large sample size allows us to rule out even small effects on material hardship and mental health, both for the full sample and the very low-income sample.

Table 1, panel A presents results for our five primary (pre-specified) outcome measures for RCT1 from the one-month follow-up surveys for the full sample: material hardship, mental health challenges, partner conflict, child behavior problems and parenting problems. As described earlier, each outcome is a standardized composite index of survey items and should be interpreted in standard deviation units above or below the control mean. We find no significant effects of the treatment on any of the composite outcomes. Although the material hardship estimate is marginally significant, it is not robust to multiple hypothesis testing. Panel B shows results restricted to a sample of respondents with less than $500 in monthly earnings in the previous month (the very low monthly income sample). We find that receipt of the $1,000 cash transfer was associated with a 0.166 standard deviation (SD) decrease in the composite material hardship measure. We also find that the transfer reduced mental health challenges by 0.076 SD at the one-month survey for this group; however, this effect is not robust to multiple hypothesis testing. For partner conflict, child behavior problems, and parenting problems we observe no effects that are robust to multiple hypothesis testing.

Table 2, panel A presents results for RCT2 on the effects of the cash transfers for our two primary outcome measures (material hardship and mental health challenges). This time we present both Intent-to-Treat (ITT) (including all members of the treatment group who both took up the transfer and who did not) and Local-Average Treatment Effects (LATE) among those who took up the transfer, since take-up of the benefit in RCT2 was lower. Panel B shows estimates for a similarly defined "very low-income sample" which, like RCT1, includes approximately the bottom half of the reported income distribution. The standard errors shown in parentheses adjust for clustering at the zipcode level. The p-values shown in square brackets are based on the randomization inference method described in more detail in the paper. We do not adjust for multiple hypothesis testing as we find no significant results.

All of the estimates in both the full and very low-income samples are small and not statistically different from zero, suggesting the cash transfer did not have a discernible impact on either our measures of material hardship or mental health challenges (the same is true for the other outcomes we measured).

TABLE 1: RCT1 INTENT-TO-TREAT EFFECTS ON MAIN OUTCOMES ONE MONTH POST CASH TRANSFER

	MATERIAL HARDSHIP	MENTAL HEALTH CHALLENGES	PARTNER CONFLICT	CHILD BEHAVIOR PROBLEMS	PARENTING PROBLEMS
PANEL A: FULL SAMPLE					
Treatment	-0.041+	-0.01	-0.071	0.011	-0.024
(SE)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)
Observations	6520	6299	2139	6375	6363
R2	0.106	0.060	0.049	0.067	0.026
Control Mean	0.002	0.000	0.005	-0.001	-0.001
Control SD	0.998	1.000	1.005	1.001	1.001
Bonferroni	0.448	1.000	0.448	1.000	1.000
PANEL B: VERY LOW-INCOME SAMPLE					
Treatment	-0.166***	-0.076*	-0.166+	-0.026	-0.044
(SE)	(0.04)	(0.04)	(0.09)	(0.04)	(0.04)
Observations	2969	2843	722	2902	2885
Control Mean	0.248	0.098	0.135	0.040	-0.060
Control SD	1.042	1.048	1.120	1.049	1.032
Bonferroni	0.000	0.247	0.289	2.555	1.296

Note: One-month survey respondents. Outcomes are standardized. Treatment is the $1,000 cash transfer. All analyses include the full set of controls. Standard errors are robust. Bonferroni p-values adjust for five tests in each sample split.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

TABLE 2: RCT2 TREATMENT EFFECTS OF ON MAIN OUTCOMES FOUR-TO-SIX WEEKS POST CASH TRANSFER

	MATERIAL HARDSHIP		MENTAL HEALTH	
	(1)	(2)	(3)	(4)
PANEL A: FULL SAMPLE	ITT	LATE	ITT	LATE
Treatment	0.005	0.007	-0.018	-0.026
Zipcode Cluster Robust SE	(0.016)	(0.023)	(0.018)	(0.026)
Randomization Inference P Value		[0.99]		[0.99]
Observations	13738	13738	13718	13718
Control Mean	0.000		0.000	
Control Complier Mean		0.039		0.025
Bonferroni	0.448	1.000	0.448	1.000
PANEL B: VERY LOW-INCOME SAMPLE	ITT	LATE	ITT	LATE
Treatment	0.012	0.017	-0.011	-0.017
Zipcode Cluster Robust SE	(0.023)	(0.034)	(0.024)	(0.035)
Randomization Inference P Value		1.000		[0.99]
Observations	7260	7260	7242	7242
Control Mean	0.121		0.030	
Control Complier Mean		0.170		0.046

Note: Columns 1 and 3 present the effects of Intent-to-Treat (ITT) specifications, while columns 2 and 4 show Local Average Treatment Effects (LATE) obtained by instrumenting for treatment assignment with an indicator equal to one if a user successfully claimed their cash transfer. All analyses include the full set of controls. Standard errors in parentheses and are clustered at the zipcode level. See RCT2 working paper for the full technical details.

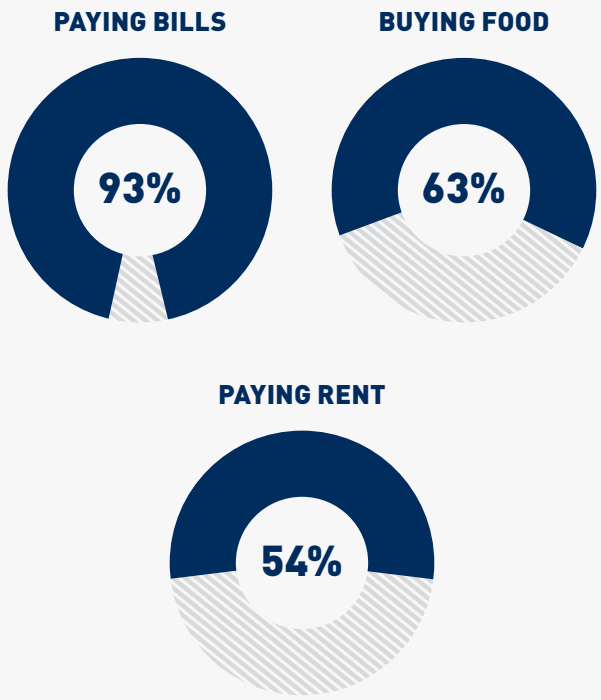
FIGURE 1: COMMENTS ON TRANSFER PAYMENTS FROM RCT1 RESPONDENTS

> "I WAS ABLE TO PAY THE BILLS AND COULD RELAX AND ACTUALLY FEEL LIKE OUR FAMILY IS GOING TO BE OKAY."

> "I WAS ABLE TO BREATHE FOR A FEW WEEKS."

> "IT HELPED ME FEED MY FAMILY."

FIGURE 2: HOW RCT2 GROUP SPENT THE TRANSFER MONEY



Taken together, the results of both studies do not provide evidence that a one-time, $1,000 cash transfer can impact the short-term material hardship of a sample of low-income households receiving SNAP. In one study, we see robust impacts among very low-income households with kids. Still, both samples consist of households who are quite poor and report high levels of underlying material hardship. In the following sections, we describe in more detail what we can say based on our data, and then offer some suggestions for future research on cash transfers in the United States.

HOW DID THE TREATMENT GROUP SPEND THE MONEY?

In RCT1, we invited respondents to tell us how they spent the money they received from GiveDirectly in an open-ended item. Findings from our open-ended questions indicate that the cash transfer was well-received by beneficiaries (see Appendix Table 10 in RCT1 for more details). We coded the responses we received for content and found that the top three responses included paying bills (68%), buying food (28%), and paying rent (27%). Between 12 and 15 percent of respondents reported spending some of the money on house items or repairs, car-related expenses and clothes, or other items for their children. Very few respondents reported saving any of the money (3%). RCT2 asked a similar close-ended question about spending, where 93% said they used the transfer to pay bills, 63% said they bought food, and 54% said they paid rent. Another 5.5% reported saving some of the money and 9.9% reported paying loans (note that the differences in the rates between the two studies is likely a result of the different ways the questions were asked). Although the top categories are consistent with other research on cash transfers during the COVID-19 pandemic, it is also worth noting that respondents listed a wide range of expenditures, reflecting the flexibility of cash.

While it is useful to look at self-reports from families who received the funds, we know that cash is fungible so that the transfer payment could have influenced the spending of families in other ways. For example, if a family used the transfer to pay off bills, they might have been able to allocate more of other resources to pay for food or transportation. In order to understand these types of effects, we would need to account for the complete range and amounts of expenditures for both treatment and control households. Our survey asked individuals to report spending on 18 different categories, including rent, utilities, transportation, childcare, food and education. In the first study, we simply asked respondents to report any spending in these categories. In the second study, we asked students to provide an estimate of the amount they spent. Unfortunately, prior research has found that individuals are not very good at recalling or reporting this information, and we believe that our data suffered from similar problems. In RCT2, for example, nearly 20% of respondents indicated spending nothing at all in the prior month, and 30% indicated that they spent money in three or fewer categories in the prior month, which seems implausible as many potential expenditures were common basic needs.

We also have a good sense from open-ended comments received from RCT1 that the cash transfer was well-received by beneficiaries. One recipient said "It helped me feed my family." Another reported that because of the transfer they were "able to get my medicine and keep our house for our children." Many said paying down bills relieved their stress, like a respondent who reported, "I was able to pay the bills and could relax and actually feel like our family is going to be okay." Another respondent said, "I was able to breathe for a few weeks;" and another that said the transfer "allowed me to be able to do what I need to do because I can drive my car again, literally made me functional again." Finally others said it normalized life for their family, including one who said it "was great to be able to put a smile on my children's face with snacks and toys that don't happen often." These quotes, and many others like them, offer a notable contrast to the empirical results from our two RCT evaluations.

WHAT CAN EXPLAIN OUR RESULTS?

The results from these studies do not lead to a straightforward conclusion about the efficacy of a modest, one-time cash transfer in mitigating hardship and improving mental health or the other secondary outcomes we examined. It remains possible that a one-time cash transfer of $1,000 is not enough to move our outcomes of interest even in the short-term. It is also possible that the effects of the one-time transfers were diluted because of the unprecedented expansion of cash transfers by the federal government in response to the pandemic. Finally, it may be that the cash transfers did provide true benefits to families, but our survey measures were not able to capture these effects.

SUBSTITUTIONS

One possible explanation for our results is that families substituted the cash transfer for other resources. For example, it is possible a cash transfer could have no impact on material hardship if recipients reduce labor supply as a result. Our results do not support that explanation, as we find no effect of the transfer on employment or earnings in either study. A second potential explanation is substitution of the transfer for public benefits. If households do not pursue existing public benefits because they received the cash transfer, it could reduce the impact of the transfer, making them no better off financially. Our data also are not consistent with this explanation as we see no reduction in public benefits receipt among the treatment group compared to the control. We also do not see movement in other hardship avoidance techniques, such as dipping into savings, selling things, cutting spending, borrowing money, or relying on private charity. We do see that very low-income cash transfer recipients in RCT1 may have reduced reliance on food banks and reduced borrowing from friends and family, but this is the only sub-group across both studies to register an effect.

STATISTICAL POWER AND SURVEY RESPONSE BIAS

Both of our studies benefit from very large sample sizes that allow us to detect even very small effects. In both studies, the experimental and control groups in follow-up surveys are well-balanced across a variety of measures. Thus, we believe we can largely rule-out the concern that we could not see impacts because of a lack of statistical power. One explanation that we cannot rule out is that our findings are driven by who responded to our follow-up surveys. The study team went to considerable lengths to increase the survey response rate in both cases—the RCT1 response rate at the one-month follow-up survey was 60% among the treatment group and 59% among the control group; the RCT2 response rate at the follow-up survey was 31% among the treatment group and 29% among the control group. (The differences in these rates are attributable to the differences in our randomization process summarized above). Still, many among the treatment and control groups did not respond. It is possible—even likely—that there are meaningful differences between those who responded to the survey and those who did not. This is a big part of why we believe future research would benefit from outcomes that do not rely on survey responses.

HISTORIC TIMES

One possibility that we cannot rule out is that our findings are a product of the unusual time during which the evaluation took place. Both studies were undertaken during a time of unprecedented use of cash transfers by the federal government to stem the tide of the economic crisis that accompanied the onset of the COVID-19 pandemic—and considerable research finds that these government transfers had major impacts on the well-being of low-income households. A large fraction of both the experimental and treatment groups received EIP payments and expanded unemployment assistance, which had a value much larger than $1,000. Thus, the effects of the additional $1,000 cash transfer may have been diluted to such a degree as to not register impacts.

TREATMENT NOT OF SUFFICIENT SIZE

It is possible more broadly that, in the U.S. context, a one-time $1,000 cash transfer would never be of sufficient size to see discernable impacts 30-45 days after it was received. This could mean either that $1,000 would never register impacts, even in the days following the transfer, or that impacts might dissipate after a short period of time (say, five to ten days), so that by the time we conducted our survey they were no longer detectable. Perhaps the transfer must be significantly larger in order to register changes in the outcomes we selected. Experimental studies in low- and middle-income countries tend to offer much larger transfers as a fraction of household income. Evidence in the international context suggests that beneficiaries disproportionately spend one-time transfers on capital investments (assets), and allocate recurring transfers

to core household expenses. Research should explore the possibility that recipients of cash transfers in the U.S. might behave similarly.

SENSITIVITY OF MEASURES

It is also possible that the measures we selected are not sensitive enough to pick up true changes in outcomes. Our material hardship measures have been used extensively in existing literature, with known associations with income public benefits in non-experimental studies. Yet, it is also true that there are very few experimental studies that have found short-term impacts of income support on these measures. To address this issue, we tested the inclusion of measures of hardship that addressed the timing of hardships, as it may be difficult to recall the exact timing, and whether the transfer was not sufficient to allow families to pay their full rent, but perhaps to pay part of their rent or back owed rent. However, even with these more sensitive measures we failed to find hardship effects. Importantly, material hardship measures are by nature subjective. It is possible that the treatment group really was materially better off after 30 days, but they did not answer the questions we used in a way that showed this due to some psychological phenomenon of recall.

HOW THE FUNGIBILITY OF CASH MAY IMPACT OUTCOME MEASUREMENT

A big challenge in the evaluation of cash transfers is that the fungibility of cash makes it challenging to measure the right outcomes across heterogeneous populations. Usually social intervention experimental studies evaluate interventions targeted at a very clear goal—does an educational program improve test scores? Does rental assistance improve housing security? Yet the flexibility of cash transfers allow recipients to allocate funds to a wide range of uses, and even spread a transfer over many different expenses. Furthermore, cash transfers distributed to a heterogenous sample are likely to elicit heterogenous spending outcomes. If so, the impact of the cash transfer in any one domain may be muted. If beneficiaries spread their transfer over many different uses, this might mean we would not see an impact, and this poses a challenge especially for untargeted, relatively small cash transfers.

RECOMMENDATIONS FOR FUTURE RESEARCH

There are many studies of cash transfers—experimental and non-experimental—being undertaken right now. While our results do not offer a simple conclusion about the efficacy of a modest, one-time cash transfer in mitigating hardship and improving mental health in the United States, the results do offer some insights for these ongoing and future studies.

The first is that we encourage those who design programs and those who evaluate them to think deeply about what outcomes we would expect cash transfers of various sizes and forms (one-time or recurring) to impact, and what magnitude of an impact would be expected. Researchers and program administrators should think about the target populations and what outcomes might be most relevant to them and then the size and frequency of the transfer that would be most likely to generate the intended outcomes.

Taking this a step further, given the flexibility of cash transfers, we propose that an "adaptive outcomes" approach might be useful. That is, if the argument for cash is that households can allocate the cash towards their greatest need and that this process will lead to heterogeneous spending patterns, then researchers might seek out ways to assess what sample members identify as their area of greatest need, and then assess progress in meeting that need following the transfer for both the experimental and control groups.

In addition, we think it will be necessary for evaluators to explore the possibility of outcome measures that move beyond subjective reflections on well-being because of concerns related to survey response rates and potential bias described above. Some forms of administrative data can be linked to the records of study participants with their permission and followed over the long-term without the need for participants to respond to surveys. Another idea is to track how treatment members spend the money. Yet another idea is to ask respondents to provide real time feedback on well-being.

Future research should consider the timing of when researchers would expect to see impacts and by what time they would fade. Our study examined outcomes at 30-45 days post transfer. To understand the possibility of fade out, we would urge researchers to measure outcomes very soon after treatment (e.g., one-week post transfer) as well as at later times (e.g., one month, three month, six months, etc.)

Finally we encourage researchers to test transfers of different sizes, and one-time versus recurring cash transfers. Ideally, a study could test the impact of different sizes of cash transfers, such as a $1,000 cash transfer as compared to a much larger transfer. Doing so could shed light on the question of whether $1,000 was big enough to register outcomes. In addition, future studies should also look at the relative impacts of one-time lump sum transfers and recurring transfers of similar size.

Thus, in our view, the ideal next study of the efficacy of cash transfers would concurrently test a variety of types of transfers and measure a wider variety of outcomes using varied sources of data. For example, one treatment arm might receive a $6,000 lump sum while a second treatment arm might receive a $1,000 payment made over 6 months. Another treatment arm might include a mix of lump sum payments

followed by recurring payments to test whether the two forms of transfer work better in tandem. A final treatment arm might replicate the transfer we evaluate here: $1,000 one time.

This is an exciting time for the evaluation of cash transfer programs in the United States. There is considerable, non-experimental research that supports the conclusion that cash transfers can be an effective way to address poverty and hardship in the U.S., and a small number of experimental studies that support that conclusion, most recently a recurring cash transfer payment for families with infants (Troller-Renfree et al., 2022). Yet the findings from our partnership with GiveDirectly suggests that there is a long way to go yet in the understanding of the best ways to evaluate cash transfers experimentally, and the most efficacious forms they might take. We hope these studies provide some insights to those who design such programs and those who evaluate them.

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