Journal of Public Economics 228 (2023) 105004

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Short communication

Encouragement and distortionary effects of conditional cash transfers[☆]

Gharad Bryan[a], Shyamal Chowdhury[b], Ahmed Mushfiq Mobarak[c,d,*], Melanie Morten[e], Joeri Smits[c,f]

[a] London School of Economics, United Kingdom
[b] University of Sydney, Australia
[c] Yale University, USA
[d] Deakin University, Australia
[e] Stanford University, USA
[f] Harvard University, USA

ARTICLE INFO

JEL classification:
I38
O15
J48

Keywords:
Conditional cash transfers
Distortion
Seasonal migration

ABSTRACT

Conditional cash transfer (CCT) programs aim to reduce poverty or advance social goals by *encouraging* desirable behavior that recipients under-invest in. An unintended consequence of conditionality may be the *distortion* of recipients' behavior in ways that lower welfare. We first illustrate a range of potential distortions arising from CCT programs around the world. We then show that in the simple case where a CCT causes low return participants to select into a behavior, and social returns and private perceived returns are aligned, transfer size plays an important role: the larger the transfer, the stronger the distortion becomes, implying that (i) there is an optimal transfer size for such CCTs, and (ii) unconditional cash transfers (UCTs) may be better than CCTs when the transfer amount is large. We provide empirical evidence consistent with these claims by studying a cash transfer program conditional on seasonal labor migration in rural Indonesia. In line with theory, we show that when the transfer size exceeds the amount required for travel expenses, distortionary effects dominate and migration earnings decrease.

1. Introduction

Conditional Cash Transfer (CCT) programs, started in the late 1990s in Latin America, have become the anti-poverty program of choice in many developing countries. In 1997 three countries had such programs, but by 2014 sixty-four non-OECD countries had programs (Honorati et al., 2015; Medgyesi and Temesváry, 2013). CCTs can be useful compared to unconditional cash transfer (UCT) programs, which have also grown in popularity. A CCT makes its payment conditional on completion of a behavior. Examples of common conditions include school enrollment and attendance, health checkup visits of children and their vaccination (Dearden et al., 2009; Macours et al., 2012; Attanasio et al., 2015; Cahyadi et al., 2020). Other programs encourage positive environmental actions, such as leaving forest intact or planting new trees (Jayachandran et al., 2017; Jack and Jayachandran, 2019). A CCT can have a greater positive welfare impact than a UCT if the encouraged behavior has a greater social benefit than its social cost, but would not be undertaken in the absence of the conditionality.[1] Unconditional cash transfers (UCTs) are cheaper to deliver and administer because no monitoring of conditions is required. This leads to a fundamental trade-off that policymakers designing transfer programs must grapple with: is adding conditions to transfer programs and monitoring adherence worth it? Do CCTs improve welfare beyond UCTs?

Economic theory cautions that CCTs can distort choices leading individuals or households to engage in behavior that has a higher social cost than benefit. We aim to shed light on one small but important dimension of the choice between UCT and CCT: that larger CCTs are more likely to distort choices. To fix ideas, we first use a simple model to show that under a natural ordering condition which states that those with the highest social benefit also have the highest perceived private benefit, combined with the assumption that the optimum does not involve all households taking the action, then the benefit that a CCT has over a UCT reaches a maximum and then decreases, potentially becoming negative. This non-monotonicity result is an important element for understanding the potential scope of CCTs: in the presence of a well understood market failure, a small CCT likely dominates a UCT,

☆ We acknowledge funding from the Australian Department of Foreign Affairs and Trade (DFAT) and Evidence Action. We thank J-PAL Southeast Asia for their collaboration in the fieldwork. We also thank Berk Özler, participants at the 2019 MWIEDC, and seminar participants at Yale, UC Davis and the University of Sydney, for useful comments. The Yale IRB protocol number is 2000024824, and the trial is registered at the AEARCTR (# 0003045).

* Correspondence to: 165 Whitney Avenue, New Haven, CT 06520-8200, USA.
E-mail address: ahmed.mobarak@yale.edu (A.M. Mobarak).

[1] Both types of transfer programs typically target a subset of the population, which distinguishes them from Universal Basic Income (UBI) programs.

https://doi.org/10.1016/j.jpubeco.2023.105004
Received 13 January 2022; Received in revised form 27 August 2023; Accepted 26 September 2023
Available online 15 November 2023

but this presumption does not apply as the size of the transfer increases. This may put limits on the possible use of CCTs to achieve the kind of transformative changes seen recently in papers on UCTs and in-kind transfers (Egger et al., 2019; Balboni et al., 2021).

We then review the empirical literature on distortions from CCTs, finding several instances suggestive of behavioral responses to CCTs that may indicate distortions. However, the evidence is not conclusive because few papers control effectively for income effects from CCTs. To provide an empirical test of our non-monotonicity claim, we designed and implemented a simple experiment among would-be Indonesian seasonal labor migrants. Building on the work of Bryan et al. (2014) we conjecture that there is under-investment in migration so that a CCT that conditions a transfer on the act of migration might increase household incomes, but that not all households ought to send a migrant. Under-investment might result from a variety of sources, including the information frictions highlighted by Baseler (2020), missing insurance markets as in Bryan et al. (2014), externalities on origin labor market as in Akram et al. (2017) or more behavioral mechanisms as alluded to in Bryan et al. (2014). Our treatment arms consist of a UCT and a CCT of the same size, as well as a CCT that is twice as large. The CCT arms require that the household sends a seasonal migrant to receive the transfer, and half the transfer is allocated only at the migration destination. Further, half of the households assigned to the small CCT are randomly 'surprised' when collecting the second part of their transfer, and in fact receive a larger amount such that their total transfer equals that of the large CCT, thus holding constant the income effects of the larger CCT.

We find that the small CCT that covers the cost of travel induces migration, and also increases peak migration season income relative to the UCT benchmark treatment. However, when the size of the transfer is increased beyond what is needed to cover migration travel expenses, distortionary effects prevail. For example, people who do not have the skills to succeed at the destination may travel mainly to collect the CCT payment and to visit relatives or friends. Larger transfers induce negative selection into migration of low-return types, and peak migration season income deteriorates. While our case is a very simple one, we think it illustrates the concerns that we wish to highlight.

The remainder of this paper is organized as follows. The next section reviews the literature on distortionary effects of CCTs. Section 3 provides a conceptual framework linking the transfer size to encouraging and distortionary effects of CCTs. Section 4 describes an experimental design that tests this framework's predictions, and our implementation of the design in a seasonal migration CCT in Indonesia. Section 5 presents the experimental results, and Section 6 discusses implications and concludes.

2. Literature review

When markets operate without friction, UCTs will dominate CCTs on efficiency grounds, since imposing a condition or constraint can only make the beneficiary (weakly) worse off. The rationale for tying conditions to cash transfers is the presence of market failures (other than credit constraints) that lead individuals to under-invest in certain profitable and/or socially desirable behaviors. Policymakers impose conditions presumably to correct the market failure:[2] by reducing the price of the conditioned action, the condition mitigates the underinvestment. However, if a policymaker sets the size of a CCT too high, this could cause households to over-invest in the conditioned behavior. These encouragement effects and distortionary effects are formally defined in Section 3.

Consider an action such as sending children to school or migrating to a city that households under-invest in due to a market failure like downward-biased beliefs about the returns to that activity (Jensen, 2010). Such information failures may arise if knowledgeable people have reason to systematically under-report their returns to friends and family (Baseler, 2020). An optimally calibrated transfer is sized such that the sum of the income effect that relaxes a credit constraint (i.e., the cash component) and the substitution effect (stemming from the conditioning of the transfer) offsets the amount of underinvestment. A further increase in the transfer size beyond that point would lead to overinvestment: households invest in the encouraged behavior more than they would have under no market failures other than credit constraints (Fiszbein and Schady, 2009).

Over-investment at the extensive margin may take the form of adverse selection, which can be illustrated with the case of an education CCT. If individuals select into a CCT at least in part on the basis of their expected returns to the induced investment (e.g., the expected effect on wages), then the children who can expect to benefit the most from schooling generally will enroll first (Heckman et al., 2006). Marginal children brought into school by cash transfers that condition on school attendance, may thus be drawn disproportionately from the left-hand side of the ability distribution (Fiszbein and Schady, 2009). This negative sorting on returns, and the associated decline in the average ability of the student pool, may limit the extent to which additional schooling translates into more learning.

Filmer and Schady (2009) found evidence for this in a scholarship program in Cambodia: by lowering the cost of education for scholarship recipients, some of the lower-ability children who under normal circumstances would have dropped out now stay in school, but their test performance was not improved by their additional schooling.[3] Negative selection on returns was also observed by Heckman et al. (2007) in a voucher program to attend private schools in Chile. These results do not go as far as showing a negative effect of a CCT on returns *on net*, as we show in our empirical application, but they are suggestive that a larger CCT may start to distort behavior. For a large enough transfer, a UCT could welfare-dominate a CCT of the same size given such distortions. We then proceed to make a stronger claim: that the effect of a CCT could in theory even be negative relative to the world where no cash transfer program exists. In the next Section, we further dissect the case of negative selection into CCT programs on the basis of returns to the induced investment, and Sections 4 and 5 describe the novel case of a seasonal labor migration CCT.

There have been several recent theoretical contributions to the literature on the role of conditions in cash transfers. Martinelli and Parker (2003) show that if family decisions are the result of (generalized) Nash bargaining between two parents, and if bequests are zero, then a CCT for children welfare-dominates a UCT. Mookherjee and Napel (2021) model theoretically the implications of the design of education CCTs in terms of Pareto efficiency and distributional effects compared to either a UCT, a UBI, or laissez-faire. Bergstrom and Dodds (2020) consider targeting benefits of CCTs over UCTs. Baird et al. (2011) shows that a conditionality can prevent the most needy from benefiting from the program is adhering to the condition is prohibitively costly for poor households.

Empirical analyses of CCT-UCT comparisons make at least three distinct contributions. First, several papers find that adding the condition is necessary to encourage the desired behavior. Akresh et al. (2013) finds that a CCT was significantly more effective than a UCT in improving the enrollment of "marginal children" in Burkina Faso who are otherwise less likely to go to school, such as girls, younger

[2] Political economy considerations may also favor conditional over unconditional transfers: taxpayers may be more likely to support transfers to the poor if they are linked to efforts to overcome poverty in the long term, particularly when the efforts involve actions to improve the welfare of children (Fiszbein and Schady, 2009).

[3] Gazeaud and Ricard (2021) also find negative effects of a CCT on student test scores. The poor program impact on test scores appears (at least in part) due to increases in class size, suggesting that the program constrained learning by putting additional pressure on existing resources in beneficiary areas.

children, and lower ability children. A condition in a Colombian CCT requiring preventive health visits induce these visits, which in turn improves children's health (Attanasio et al., 2015). Second, papers compare CCTs and UCTs in terms of spillover effects on other outcomes not targeted directly by the condition, such as effects of a schooling CCT on adolescent schoolgirls' pregnancy and marriage rates (Baird et al., 2011), or their mental health (Baird et al., 2013). Third, other papers show that in trying to meet the condition, recipients substitute away from other positive behaviors. For example, if the condition targets the schooling of a specific child, siblings may suffer (Barrera-Osorio et al., 2011). Or, women substitute away from formal employment when meeting time-consuming CCT health checkup requirements for their children (De Brauw et al., 2015). An unintended consequence of adults' participation in a public works program is to increase children's domestic work to compensate for their parent's absence, thereby reducing their school enrollment (Shah and Steinberg, 2019). However, lacking a UCT comparison group, this third set of papers fails to dispositively demonstrate a distortion generated by the conditionality rather than a response to the cash component of the program.

Other researchers have also assessed empirically the size of cash transfers, including Filmer and Schady (2011), who find no difference in school attendance between a smaller and larger education CCT. Baird et al. (2013) find that an education CCT improved psychological well-being among adolescent female beneficiaries in Malawi (compared to both the control group and UCTs) when the transfer amount is small. However, doubling the transfers wipes out the beneficial effects.

It should be noted that over the long-term, conditions that aim to increase lifelong opportunity may generate aggregate lifetime net present value of benefits (for recipients and their children) that are so large that they may dominate any distortionary effects for transfer sizes in the range considered by policymakers. Araujo and Macours (2021) found that short-term impacts on schooling of differential exposure to Progresa during early childhood were sustained in the long-run and manifested themselves 20 years later in larger labor incomes, more geographical mobility including through international migration, and later family formation. Likewise, a CCT in Honduras conditioning on primary school attendance increased secondary school completion, the probability of reaching university, as well as the probability of international migration for young men (Millán et al., 2020). And Hamory et al. (2021) found a 14% gain in consumption expenditures and 13% increase in hourly earnings among individuals twenty years after they had received two to three additional years of childhood deworming (a condition in some CCTs, e.g. Ahmed et al., 2022).

3. Theory: Non-monotonic welfare impacts of CCTs

To fix ideas, and provide clear definitions of encouragement and distortionary effects, we provide a simple model. We also use the model to show that, in the most empirically relevant case, the welfare impacts of a CCT are likely to be an inverted U-shape in the size of the CCT: the CCT will initially increase welfare through encouragement effects and then eventually it will create distortionary effects which could potentially eliminate all the welfare gains. The model is presented in terms of migration choice, as that is our primary empirical example, but it could be applied to any binary choice. The model could easily be extended to allow for more continuous choices, such as days of schooling, and this would not affect the main ideas.

Households in a community ($i \in N$) are each characterized by a pair $\{z_i, \hat{z}_i\}$. z_i captures the net increase in societal welfare if household i were to migrate, measured in money. Societal benefit is a combination of the private benefit of migration, and any external effects. z_i could be a fixed number, or it could be a function of the number of migrants, or even the names of other migrants. From a societal perspective it is optimal if, in equilibrium, all households with $z_i \geq 0$ migrate, and that none of those households with $z_i < 0$ migrate. $\hat{z}_i$ captures the perceived private benefit of migration. Household i will migrate, in equilibrium, if and only if $\hat{z}_i \geq 0$. Clearly, $\hat{z}_i$ need not equal z_i. This discrepancy could be because of external or internal effects. For example, z_i would be greater than $\hat{z}_i$ if migration out of the rural community increases output per worker in the origin (an external effect), if potential migrants are misinformed about returns to migration (an internal effect), or if procrastination leads potential migrants to miss the opportunity to migrate as in Duflo et al. (2011) (another internal effect).[4] This simple setup allows us to provide clear definitions of under- and over-investment without taking a stance on the exact source of under-migration.

Definition: Under-investment. We say that i *under-invests* in migration if $z_i \geq 0$ but $\hat{z}_i < 0$. That is, household i should migrate in order to maximize social welfare, but does not.

Definition: Over-investment. We say i *over-invests* in migration if $z_i < 0$ but $\hat{z}_i \geq 0$. That is, household i should *not* migrate in order to maximize social welfare, but does migrate.

As noted above, this simple model could capture a number of possible reasons for under-investment. For example, it may be that both $\hat{z}$ and z are fixed numbers, but $\hat{z}_i < z_i$ because of a behavioral bias, such as projection bias. Alternatively, the model can capture a constant positive externality at the destination so that $z_i - \hat{z}_i = \kappa \ \forall i$ is equal to the difference between social and private marginal benefits of migration. The model can also capture more complex externalities by allowing the z's to depend on the number or name of households that migrate. Our empirical setting is one in which underinvestment is the result of an internal effect, for example, lack of information.

As with the z's we assume that the $\hat{z}$'s are measured in dollars, and that there are no income effects on either z's or $\hat{z}$'s. We can then consider the impact of conditional and unconditional cash transfers. Consider first a conditional cash transfer that makes payment of size m to a household if and only if it migrates. In the event that $\hat{z}_i < 0$ and $\hat{z}_i + m \geq 0$ household i will migrate if and only if offered this conditional cash transfer. We can now provide clear definitions of encouragement and distortionary effects of CCTs.

Definition: Encouragement effect. We say that a migration CCT of size m has an *encouragement effect* for household i if household i under-invests in the absence of the CCT, but migrates with the CCT. That is, if $\hat{z}_i < 0$ and $z_i \geq 0$, but $\hat{z}_i + m \geq 0$.

If household i is the only recipient of a CCT, and i receives an encouragement effect from the CCT, then a CCT increases societal welfare more than a UCT of the same size. To see this, observe that $m + z_i$ is the gain in societal welfare from the CCT, while m is the gain from the UCT and that $m + z_i \geq m$ so long as $z_i \geq 0$. In this calculation we have ignored the welfare loss for the organization paying for the cash transfers, which we think is appropriate in this context, but nothing would change if we simply subtract m from both welfare gains.

Definition: Distortionary effect. We say that a CCT of size m has a *distortionary effect* for household i if household i over-invests in the presence of the CCT, but did not over-invest in the absence of the CCT. That is, if $\hat{z}_i < 0$ and $z_i < 0$, but $\hat{z}_i + m \geq 0$.

Following the same logic as above, a CCT that targets only household i and that has a distortionary effect is dominated by a UCT.

In any given community of N households it is possible that there are both households that over-invest and also households that under-invest. It is also possible that any given CCT of size m leads to both encouragement and also distortionary effects. Hence, it is an empirical question whether a given CCT increases societal welfare more or less

[4] It is important to note that the theory requires z_i and $\hat{z}_i$ to be measures of welfare, rather than a narrower measure, say income. In our empirical work we will not show that welfare is non-monotonic, but rather that peak migration season income is non-monotonic. To the extent that peak migration season income is a good measure of welfare in our context we believe this is strong evidence in favor of our basic claim.

than a UCT, regardless of its impact on the targeted behavior, in this case migration. Despite this, there is a quite compelling simplification of the model which delivers some clear predictions. In particular, in the most likely case, welfare gains from a CCT form an inverted U-shape in the size of the transfer. In the following discussion we always assume that total expenditure on a compared CCT and UCT is held constant, and the quasi-linearity of our utility function allows us to ignore the question of who receives the transfers.

Claim: Monotonicity of CCT welfare effects. Consider a community of N households, ordered such that $\hat{z}_1 < \hat{z}_2 < \cdots < z_N$ and a CCT of size m. So long as the z_i are similarly ordered so that $z_1 < z_2 < \cdots < z_N$ then societal welfare gain from a CCT, relative to a UCT, will either be (i) monotonically increasing in m, (ii) monotonically decreasing in m, or (iii) an inverted u shape in m.

We think of case iii to be the most empirically relevant, for reasons we discuss below. To see why this claim is true, consider Fig. 1. The black line denotes the $\hat{z}_i$, so that all those to the right of point A will migrate. The dotted red line shows the impact of a CCT of size m, which leads all those to the right of B to migrate, increasing the migration rate. The blue line shows one possible configuration of the z_i in which all those to the right of C *ought* to migrate. Every possible z_i configuration that is ordered the same as $\hat{z}_i$ forms an upward sloping line in this space, and hence identifies a single cutoff point c with those to the right of c being those who ought to migrate. It is then easy to see why the claims above are true. Assuming the cutoff point c is to the left of point A then those between A and c are underinvesting. A CCT will initially encourage these under-investing households to migrate, increasing welfare (relative to a UCT to hold income effects constant). Welfare will reach a maximum at the point where all those to the right of c migrate, and none to the left. Beyond this point, further increasing the size of the CCT will create a distortionary effect and total welfare will start to fall (again, relative to a UCT). In the extreme, the CCT may create a strong enough distortionary effect that all the initial gains from the encouragement effect are reversed and total welfare is lower in the case of a CCT than a similar sized UCT. This is case (iii) in the claim above (an inverted U-shaped welfare function), and seems the most likely outcome: the CCT is in place because we have strong a priori reason to believe that there is under-investment by some households, but surely it is not the case that all households should migrate. Case (i) (monotonically increasing welfare) occurs where there are no households that should not migrate, so c lies on the y-axis. Case (ii) (monotonically decreasing welfare) occurs where point c lies to the right of A, so there are no under-investing households, and the CCT can only create distortionary effects.

These welfare gains (or losses) from a CCT can then be compared to a UCT of the same cost. A CCT that has only encouragement effects dominates a UCT of the same size (although it will tend to target different households). A CCT that has only distortionary effects will be dominated by a UCT (although again, it will target different households). Finally, for a CCT that has both encouragement and distortionary effects there is no clear comparison between a UCT and a CCT; part (iii) of our claim suggests that it is likely that a small CCT will initially dominate a UCT and then as m rises it will eventually be dominated by a UCT. It is important to note that our simple model does not capture all of the welfare relevant distinctions between CCTs and UCTs. For example, an important observation (for which we thank a referee) is that monitoring costs may mean that small CCTs are not economical. In our framework, this may create a goldilocks zone for CCTs: small enough to avoid distortion but large enough to be worthwhile given monitoring costs. There are also other benefits of CCTs. For example, a CCT can help with targeting funds to those that are willing to undertake a task as highlighted in the micro-ordeals literature (e.g., Alatas et al., 2016).

Our experimental design will randomly vary the size of the (conditional) transfer to induce people to migrate, say m and m', with $m' > m$.

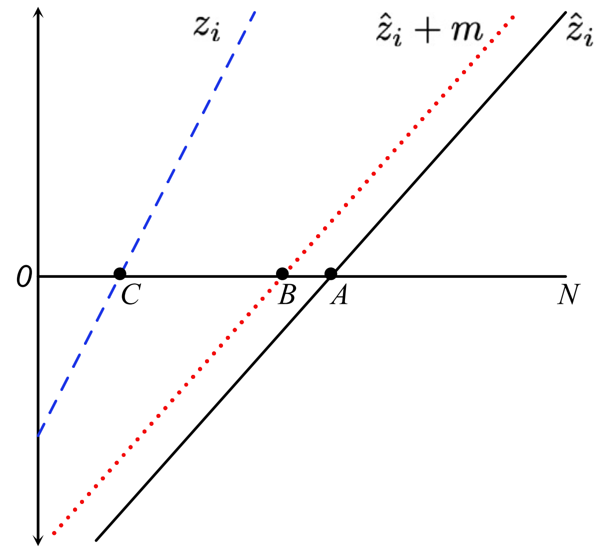

Fig. 1. Non-monotonicity of CCT Welfare Impacts.

We will test whether the larger transfer m' induces migration among those for whom the act of migration has negative returns on migration season earnings. This could be because they may migrate without any intention of finding work, simply to collect the large transfer m', or because m' is large enough to induce people whose skills are not well matched to the destination and for whom the opportunity costs of moving away from the village are large. Under what conditions would such a finding confirm our basic claim that a CCT of too large a size can create distortionary effects? If the under-investment is caused by an internal effect (for example, a lack of information on private financial returns) and peak migration season income is a good measure of welfare, then our experiment will allow a test of the ordering assumption and directly reveal non-monotonicity and distortion. At the other extreme if under-migration is caused by an external effect, then the experiment reveals a non-monotonicity in private returns, which would imply a non-monotonicity in social returns so long as the externality is not too large and the ordering assumption holds. We are not aware of any direct tests of the ordering assumption but it seems reasonable to us in many settings, for example external effects at the origin are likely stronger for those who have higher private returns. We also recognize that it need not hold, for example, those with higher private returns may be less likely to be excluded from markets.

4. Data and experimental design

In this section, we apply the conceptual framework introduced in Section 3 to design a CCT for seasonal migration. Our experimental design varies the size of CCTs and compares them to a UCT benchmark as a way to quantify the encouragement-distortion trade-off embedded in conditionalities.

4.1. Experimental context

In agrarian areas around the world, labor demand and wages fall during the pre-harvest season, and the prices of staples tend to rise while the economy waits for the new crop to grow. These combine to produce pre-harvest seasonal poverty and hunger (known as the 'lean season') in many poor rain-fed parts of the world (Bryan et al., 2014; Dercon and Krishnan, 2000; Jalan and Ravallion, 2001; Khandker and Mahmud, 2012; Macours and Vakis, 2010; Paxson, 1993; Fink et al., 2020). Rural areas of Eastern Indonesia experience such seasonal deprivation. In West-Timor the pre-harvest period is known as 'musim lapar biasa' (ordinary hunger period), which sometimes turns into famine-like conditions (known locally as 'paceklik') (Basu and Wong, 2015). Some rural households send seasonal migrants to cities to cope with this seasonal income shortfall. Given a missing insurance market however, the risk of failed migration (migrating and not finding a job) may be preventing some households close to subsistence from migrating (see Appendic C). Our experiment is designed to test whether more households would benefit from employing the migration strategy, but are currently constrained from doing so. Appendic A provides additional details about the setting and the experiment, including the cropping calendar in West-Timor and the timing of our intervention and data collection activities.

4.2. Sampling

Five villages in Timor Tengga Utara (TTU) Regency in West-Timor were sampled in July to early August 2017 based on poverty incidence and seasonality. Please see Appendic A for details on village selection. Out of 869 sampled households in these five villages, 855 gave consent to be interviewed and 775 of them satisfied the eligibility criteria of (i) having at least one household member aged 21 or above; and (ii) not owning land exceeding 200 Are (2 Hectare). Out of the baseline sample, 708 households (91.5%) were re-interviewed at endline, which took place from December 2017 until February 2018. Sample attrition does not differ statistically significantly across treatment arms, and is not statistically significantly predicted by baseline covariates, (Tables Appendix C1 and Appendix C2).

4.3. Experimental design

Randomization was done at the household level. First, households were randomized into either a UCT or a CCT treatment (Table 1). If a UCT-assigned household took up the offer, it received IDR 150,000 (~USD 11.25 in July 2017 by nominal exchange rate; ~USD 32 using the 2017 PPP OECD, 2021),[5] and no condition was imposed. Households assigned to the CCT arm had the choice to take up the offer and migrate (to a destination of their choice within West-Timor), or to not take up the offer. The baseline (and endline) survey was administered, and the offers made, by local NGO Kopernik, coordinated by J-PAL South East Asia (SEA). Kopernik also had check-in officers in the major towns in West-Timor (Kefa, Belu, So'e, Kupang). The amount was carefully calibrated to cover the cost of transport to common migration destinations (cities in West-Timor) plus the opportunity cost of moving away from the village. The CCT payment was divided into two installments: Half paid at the village of origin after the offer is accepted, and the other half collected at the destination city after "checking in" with a program officer. This helped us monitor adherence to the conditionality. The first-half payment was issued in advance to address any liquidity constraints preventing potential migrants from traveling. It was made clear to beneficiaries that this first half-payment was still conditional on the household sending a migrant.

The CCT arm was further split into three groups with varying transfer amounts, to understand distortionary effects through differential selection of migrants. A group we label 'CCT-high' received IDR 150,000 at the origin, and an additional IDR 150,000 after checking in at the destination (IDR 300,000 total, which was ~USD 22.50 by the July 2017 nominal exchange rate, and ~USD 64 using 2017 PPPs). People randomized into a 'CCT-low' group received IDR 75,000 at the origin, and they were told they would get an additional IDR 75,000 upon checking in at the destination. Hence, their total disbursement of 150,000 equaled that of the UCT group. This CCT-low group was further split into two, whereby half of the households assigned to CCT-low at baseline who check in at a destination, were 'surprised' upon checking in to receive a second subsidy of IDR 225,000 rather than IDR 75,000. We label this group 'CCT-low+'. They also received IDR 300,000 in total, like the CCT-high group. The amount of IDR 75,000 was carefully calibrated so as to cover the full cost of migration, including transport and subsistence upon arrival. Most people only spend IDR 50,000 or less on a one-way trip (see Appendic C2), but we wanted to ensure that migrants could leave some money behind for their families, as insurance against the departure of the family member primarily responsible for livelihood generation.

Table 1
Treatment arms (amounts in Rp.). In 2017, Rp. 150,000 ≈ USD 32 in PPP (OECD, 2021).

	CCT			UCT (D)
	High (A)	Low (B)	Low with surprise (C)	
1st disbursement at the origin	150,000	75,000	75,000	150,000
2nd disbursement at the destination	150,000	75,000	(75,000 +150,000) =225,000	0
Total subsidy	300,000	150,000	300,000	150,000

Doubling the promised transfer from IDR 150,000 to IDR 300,000 changes the selection of households who migrate, and that is the potential distortion that our research design was meant to capture. However, the larger transfer may also cause ex-post actions (e.g., searching longer for a job due to having a larger buffer, investing in household enterprises), which confounds the selection effect we are interested in. This is why the 'surprise' component in CCT-low+ is useful: The total transfer was IDR 300,000, so it controls for the direct income effect, but since those households did not know that they were going to receive this when they made their offer take-up and migration decisions, we are able to capture the pure effect of the selection or distortion.[6] We do so by comparing the effect of assignment to CCT-high or CCT-low+ on ex-post outcomes such as peak migration season household income. The ex-post 'surprise' treatment arm was inspired by Karlan and Zinman (2009), who offered different interest rates before and after loan applications are made to experimentally disentangle adverse selection from moral hazard in a credit market.

Table Appendix C1 shows that the treatment arms were generally balanced at baseline, but we show results with and without controlling for baseline covariates. Appendix B also contains a description of the construction of the variables used in our analysis.

5. Results

5.1. CCT versus UCT

Table 2 presents results on three key outcome variables: acceptance of the treatment (cash transfer) offer, checking in at migration destinations to receive the second part of the transfer (for the CCT assigned subsample only), and household income at the peak of the migration season — the first two weeks of September. Unsurprisingly, the takeup of the UCT is highest (92.8%).[7] The UCT-CCT low contrast identifies

[5] For reference, Indonesia's poverty line in 2015 was 302,735 (US$25) per month per person-about 82 cents a day, and about 20% of East Nusa Tenggara province (NTT, which contains West-Timor) lived below that poverty line.

[6] The experimental design was focused on cleanly identifying the selection into migration; the framing of the paper around distortion related to tying conditions to cash transfers, was ex-post.

[7] The uptake of the UCT is less than 100%, which might be due to some risk averse households who might have thought they will be asked to reciprocate in some form in the future (but we lack data to assess this). We do not find statistically significant differences in take-up determinants between CCT and UCT (Table Appendix D5), except that female-headed households may be less likely to take-up the CCT offer (statistically significant only at the 10% level).

the impact of the imposition of the migration conditionality, since both arms receive the same total amount of money (Table 1). The takeup of the CCT low is (92.8%–40.3%) = 52.5% (column (2)). Despite the lower takeup, the intent-to-treat effect on peak migration season income of the CCT is at least Rp. 300,000 higher compared to the UCT of the same transfer size (columns (9)–(10)). Respondent households assigned to the CCT are more likely to report having spent it on seasonal migration (to adhere to the condition), whereas the UCT subsidy is more likely to be spent on non-farm capital and food consumption (Appendix C1). These facts combined imply that the labor migration was a profitable investment compared to alternative investments enabled by the UCT. Second, the lower takeup but higher ITT on peak migration season income of the CCT-low treatment (compared to the UCT) jointly imply that the CCT-low strongly dominates the UCT, if peak migration season income is a good proxy for benefits. As randomization was done at the household level, we cannot rule out spillover effects from CCT to UCT-assigned households (for example, by inducing some UCT assigned neighbors to (co-)migrate). Appendix D.5 examines the heterogeneity of treatment effects on income as a function of the number of other villagers also assigned to CCT treatment, and this suggests that spillover effects, if anything, are negative. If that is the case, the treatment effects of CCTs reported in Table 2 provides a lower bound of the true effect.

5.2. Testing for distortion

From the perspective of the recipient household, the CCT low and CCT low+surprise treatments do not look any different from each other until a household member checks in at a destination. Reassuringly, there is no takeup difference between these arms (columns (1)–(2): p = 0.776 without covariates; p = 0.428 with covariates). Hence, we merge the CCT-low and low+ arms in columns (3) and (4) to increase power. The CCT high induces an additional 15%–20% of households to take up the CCT offer, as compared to the CCT-low/low+ arms (columns (3)–(4)).[8] We asked migrants in the CCT treatment to "check in" with a program officer at their migration destination. Hence, the CCT-low is the left-out category in columns (5)–(6). Reassuringly, migrants in the CCT-low arm checked in at the same rate as migrants in the CCT-low+ arm (column (5)–(6)), so we merge the CCT low and low+ groups in columns (7) and (8). The large transfer induces 11–14 percentage points more migrants to check in, compared to the check-in rate of 20.7% in the CCT low/low+ arms (columns (7)–(8); an increase of 52%–66%), and this difference is statistically significant.

The key question for us is whether this additional migration induced by the CCT-high treatment is "distortionary" in the sense that it induces a set of people to migrate for whom this is not really a productive activity. The transfer amounts were such that this could be a problem, in principle. Migrants report their transportation expenditures, so we know that the CCT-low subsidy amount suffices for transport to common migration destinations (Table Appendix D2 and Figure Appendix C2). In other words, the extra transfer received by CCT-high households exceeds their transport expenditure requirement, so it could induce recipients to travel despite not being especially well-suited or eager to search for and secure well-paying jobs at the destination. This would imply a worsening of the composition of the pool of migrants in terms of their returns, as discussed in Section 3. We recognize that there is some slippage between our results, which concern income, and the theory which discusses welfare. Despite this slippage we believe our results are strongly supportive of the claim that CCTs can lead to distortions and that our ordering assumption is likely correct in this setting.[9]

Columns (9) and (10) in Table 2 are indicative of such distortion. Even though the verified migration (destination check-in) rate is higher among CCT-high recipients, they report significantly lower peak migration season income compared to CCT-low+ households. The CCT-high versus CCT-low+ is the most relevant experimental comparison, because we hold the ultimate size of the transfer constant in these two groups, and only vary the selection process. Column (10) shows that households that received the CCT-high offer have about IDR 35,000 lower peak migration season income (p-value = 0.002) compared to households that received the CCT-low+ offer.[10]

The CCT-low+ group ultimately received the same-sized transfer as CCT-high, so we are holding constant any income effects from the monetary transfer, and only focusing on differences based on who selects in under each treatment. We verify that the difference in peak migration income between CCT-high and CCT low+ is not driven by outliers (Figure Appendix D2). The differences in take-up, check-in rates, and peak migration season income between CCT-high and CCT-low treatments that we highlight remain statistically significant even after accounting for multiple hypothesis testing using strategies outlined in Anderson (2008) (Table Appendix D1). However, we cannot rule out that the 'surprise' element of the CCT-low+ may have helped that group, if people make more prudent decisions when they are not expecting external financial support.

Why do CCT-high households have lower peak migration season income than CCT-low households? This is because they appear to sort into lower-paying occupations. We find no statistically significant difference in the odds of (salaried) employment between the CCT arms (Table 3, columns (1)–(4)). To explore whether there are differences between the treatment arms in terms of migrants' sector of employment, we first regress migration season earnings on indicators for the sector in which the migrant works, along with some other control variables (Table Appendix D3). Next, we use the ranking of the coefficients on the sectors of occupation in the aforementioned regression to construct an ordinal variable — 'pay rank', that takes on 0 for the lowest-paying sector (fisheries), 1 for the second lowest-paying sector (trade/retail), and so forth, up to 7 for the highest-paying sector (manufacturing). Columns 9 and 10 of Table 3 shows that CCT-low(+) migrants sort into higher-paying occupations compared to the CCT-high migrants. Compared to CCT-high migrants, CCT-low/low+ migrants appear a bit more likely to be employed in manufacturing jobs, which pay higher wages, and less likely to employed in trade/retail jobs, where wages are lower (column (5)–(8) of Table 3), although not all pairwise comparisons are statistically significant.[11]

Other than peak migration season income, we also checked with the CCT-high versus CCT-low treatments produce any differential effects on food insecurity during the hungry season. We do not find statistically significant effects of any treatment on household food security during the hungry season (Table 3, column (11)–(12)). This is possibly because the hungry season does not coincide with the migration season in West-Timor (as described in Appendix A and Appendix B), unlike other regions like northern Bangladesh, where seasonal hunger has also been documented.

8 Without covariates, the effect size is (0.422–0.366)/0.366 = 15.3% larger (column (3)); with covariates, the effect size is (0.427–0.355)/0.355 = 20.3% larger (column (4)).

9 The ordering assumption which implies that the ranking of social returns aligns with the ranking of perceived returns is more plausible in environments where individuals, despite not knowing their exact returns to migration, have at least some information about the types of individuals (ages, skills, sectors) who migrate and seem to have some success. There is reason to believe this in our context: among the migrants (we only asked these questions to the migrants in our sample), 65.3% had ever migrated before the baseline survey, 81.4% knew someone at the destination before migrating, and 75.9% had contact with their employer before migrating (Table Appendix D2).

10 If households who accepted their CCT offer but did not check in over-reported their peak migration season income, then this would work against our key findings that a large CCT reduces migration earnings, and that migration earnings are non-monotonic in the transfer size.

11 Figure Appendix D1 and Table Appendix D4 provide further details.

Table 2
Impact (intent-to-treat effects) of assigned treatments on take-up of the treatment offer, checking in at a destination, and household income during the peak of the migration season (the first two weeks of September).

Dependent variable:	Accepting cash transfer offer				Check-in at a destination (CCT subsample)				Migration season income (Rp. 10k)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CCT high	−0.366***	−0.355***	−0.366***	−0.355***	0.114*	0.137**	0.107*	0.137**	12.233	5.176
	(0.055)	(0.065)	(0.055)	(0.066)	(0.047)	(0.042)	(0.047)	(0.047)	(9.904)	(11.064)
CCT low	−0.414***	−0.403***							45.282**	31.946*
	(0.050)	(0.061)							(11.793)	(14.347)
CCT low+	−0.430***	−0.449***			0.013	0.000			47.476**	40.926**
	(0.055)	(0.069)			(0.026)	(0.027)			(11.313)	(12.505)
CCT low/low+			−0.422***	−0.427***						
			(0.046)	(0.059)						
F-test, p-values:										
Low = low+	0.776	0.428								
High = low/low+			0.258	0.053						
High = low+									0.002	0.002
High = low									0.019	0.053
High = low, high = low+									0.003	0.005
E(Y | UCT)	0.928	0.928	0.928	0.928					134.056	134.056
E(Y | CCT low)					0.191	0.191				
E(Y | CCT-low/low+)							0.207	0.207		
Controls		✓		✓		✓		✓		✓
Observations	775	708	775	708	526	474	526	474	708	708
R^2	0.210	0.249	0.210	0.248	0.110	0.164	0.110	0.164	0.060	0.114

* p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors clustered at the household level in parentheses. Peak migration season household income is winsorized at the 99th percentile to ameliorate the undue influence of outlying observations. Controls include the household head's gender, age, and years of education, household size, the number of adults aged >=21, the average age of those adults, a socio-economic status (SES) index, the household's number of different income sources, and indicators for which protein source the household reported to have consumed most in the year preceding the baseline. All estimations include village, Rukun Warga (one administrative level below village), and enumerator fixed effects. Details on the outcomes and control variables can be found in Appendix B.

Table 3
ITT estimates on employment, including self-employment (i.e., not being idle (column (1)–(2)) and salaried employment (i.e., working for others (column (3)–(4)) during the peak of the migration season; differences across treatment arms in terms of migrants' sector of employment (columns (5)–(10)); and ITT estimates on the food security index (columns (11)–(12)).

Dep. var.:	Work (any)		Salaried work		Sector: Trade/retail		Sector: Manufacturing		Sector ranked by average earnings[a]		Food security index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
CCT high	0.067**	0.071**	0.103*	0.080*	0.090**	0.086*	−0.017	0.011	−0.222	0.052	−0.095	−0.090
	(0.024)	(0.021)	(0.039)	(0.033)	(0.021)	(0.034)	(0.022)	(0.033)	(0.297)	(0.310)	(0.111)	(0.102)
CCT low	0.028	0.026	0.088**	0.057	0.059	0.053	0.068**	0.091**	0.018	0.328	−0.021	−0.041
	(0.031)	(0.035)	(0.031)	(0.030)	(0.064)	(0.050)	(0.023)	(0.025)	(0.333)	(0.306)	(0.125)	(0.144)
CCT low+	0.047*	0.049	0.068	0.045	0.014	0.011	0.077	0.088**	0.549**	0.632**	−0.045	0.009
	(0.021)	(0.025)	(0.046)	(0.042)	(0.046)	(0.036)	(0.042)	(0.031)	(0.152)	(0.191)	(0.078)	(0.080)
F-test (p-values):												
Low = low+	0.626	0.633	0.282	0.397	0.653	0.584	0.811	0.934	0.173	0.341	0.833	0.713
High = low/low+	0.427	0.342	0.520	0.534	0.056	0.119	0.177	0.192	0.028	0.019	0.942	0.515
High = low+	0.504	0.488	0.491	0.361	0.078	0.059	0.138	0.193	0.021	0.069	0.744	0.516
Controls		✓		✓		✓		✓		✓		✓
E[Y]	0.898	0.898	0.216	0.216	0.106	0.106	0.053	0.053	2.767	2.767	−0.000	−0.000
(st. dev.)	(0.302)	(0.302)	(0.412)	(0.412)	(0.308)	(0.308)	(0.224)	(0.224)	(1.810)	(1.810)	(1.001)	(1.001)
N	708	708	708	708	227	227	227	227	227	227	686	686
R^2	0.050	0.080	0.048	0.134	0.116	0.137	0.144	0.233	0.149	0.230	0.048	0.104

[a] See the description in the text regarding the construction of this outcome variable.

* p < 0.1, ** p < 0.05, *** p < 0.01; robust standard errors clustered at the household level in parentheses. UCT is the left-out category/arm. The estimates of columns (5)–(10) are on the subsample of migrants. Controls include the household head's gender, age, and years of education, household size, the number of adults aged >=21, the average age of those adults, a socio-economic status (SES) index, the household's number of different income sources, indicators for which protein source the household reported to have consumed most in the year preceding the baseline, and the week of the baseline survey. All estimations include village, Rukun Warga (one administrative level below village), and enumerator fixed effects. Details on the outcomes and control variables can be found in Appendix B.

6. Discussion

Policymakers face many choices about whether to condition transfers on socially desirable behaviors, about the type of conditions imposed, and about the transfer size. Theoretical considerations and credible empirical evidence can guide such decisions. A comprehensive accounting of the relative merits of CCTs and UCTs would have to include the aggregate lifetime net present value of benefits of adherence to the condition that corrects the targeted market failure to households (and their children) — both recipients and non-recipients, income effects, distortions created by the imposed condition, the cost of transfers and monitoring costs, distributional effects, and spillover effects on treated and untreated households. This article focused on understanding distortions in the behavior of targeted households. While there have been other suggestions of distortions in the CCT literature, we provide a careful experimental design to isolate the distortionary effect while controlling for the income effect that can confound the empirical identification of distortions.

Our experimental design highlights the role of the size of the CCT transfer in creating distortions. Simple theory predicts that welfare effects will often be an inverted U-shaped function of the transfer amount. We experimentally vary the transfer size designed to encourage seasonal migration in Indonesia, while holding the income effect fixed using two-stage randomization where some migrants are surprised with a larger transfer at the destination *after* their migration decision is already made. We find that larger CCTs generate distortionary effects, which may lower the benefits generated by the program.

These findings have several implications for the design of CCT programs and the design of evaluation studies. First, ex-ante theorizing about possible distortions generated by CCTs can inform the design of the condition and nudge us to collect data on unintended distortionary consequences. Second, the inclusion of a UCT comparison group allows a comparison of the extent to which observed behavioral responses reflect distortion due to the conditionality, rather than generic behavioral responses to the cash component of the program. Third, by experimenting with the transfer size, policymakers can calibrate the CCT amount that maximizes program benefits given the encouragement-distortion tradeoff highlighted in this paper. Policymakers will also have to factor in the costs of monitoring adherence to the CCT's condition when evaluating this tradeoff.

Declaration of competing interest

No author has any material financial interests linked to the results of the paper.

Data availability

Data will be made available on request.

Acknowledgments

We acknowledge funding from the Australian Department of Foreign Affairs and Trade (DFAT) and Evidence Action. We thank J-PAL Southeast Asia for their collaboration in the fieldwork. We also thank Berk Özler, participants at the 2019 MWIEDC, and seminar participants at Yale, UC Davis and the University of Sydney, for useful comments. The Yale IRB protocol number is 2000024824, and the trial is registered at the AEARCTR (# 0003045).

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jpubeco.2023.105004.

References

Ahmed, A., Aune, D., Vineis, P., Pescarini, J.M., Millett, C., Hone, T., 2022. The effect of conditional cash transfers on the control of neglected tropical disease: a systematic review. Lancet Glob. Health 10 (5), e640–e648.

Akram, A.A., Chowdhury, S., Mobarak, A.M., 2017. Effects of emigration on rural labor markets. Technical report, National Bureau of Economic Research.

Akresh, R., De Walque, D., Kazianga, H., 2013. Cash Transfers and Child Schooling: Evidence from a Randomized Evaluation of the Role of Conditionality. The World Bank.

Alatas, V., Purnamasari, R., Wai-Poi, M., Banerjee, A., Olken, B.A., Hanna, R., 2016. Self-targeting: Evidence from a field experiment in Indonesia. J. Polit. Econ. 124 (2), 371–427.

Anderson, M.L., 2008. Multiple inference and gender differences in the effects of early intervention: A reevaluation of the abecedarian, perry preschool, and early training projects. J. Am. Stat. Assoc. 103 (484), 1481–1495.

Araujo, M.C., Macours, K., 2021. Education, income and mobility: Experimental impacts of childhood exposure to progresa after 20 years.

Attanasio, O.P., Oppedisano, V., Vera-Hernández, M., 2015. Should cash transfers be conditional? Conditionality, preventive care, and health outcomes. Am. Econ. J.: Appl. Econ. 7 (2), 35–52.

Baird, S., De Hoop, J., Özler, B., 2013. Income shocks and adolescent mental health. J. Hum. Resour. 48 (2), 370–403.

Baird, S., McIntosh, C., Özler, B., 2011. Cash or condition? Evidence from a cash transfer experiment. Q. J. Econ. 126 (4), 1709–1753.

Balboni, C.A., Bandiera, O., Burgess, R., Ghatak, M., Heil, A., 2021. Why do people stay poor?. Technical report, National Bureau of Economic Research.

Barrera-Osorio, F., Bertrand, M., Linden, L.L., Perez-Calle, F., 2011. Improving the design of conditional transfer programs: Evidence from a randomized education experiment in Colombia. Am. Econ. J.: Appl. Econ. 3 (2), 167–195.

Baseler, T., 2020. Hidden income and the perceived returns to migration: Experimental evidence from Kenya. Available at SSRN 3534715.

Basu, K., Wong, M., 2015. Evaluating seasonal food storage and credit programs in east Indonesia. J. Dev. Econ. 115, 200–216.

Bergstrom, K., Dodds, W., 2020. The targeting benefit of conditional cash transfers. World Bank Policy Research Working Paper, (9101).

Bryan, G., Chowdhury, S., Mobarak, A.M., 2014. Underinvestment in a profitable technology: the case of seasonal migration in Bangladesh. Econometrica 82 (5), 1671–1748.

Cahyadi, N., Hanna, R., Olken, B.A., Prima, R.A., Satriawan, E., Syamsulhakim, E., 2020. Cumulative impacts of conditional cash transfer programs: Experimental evidence from Indonesia. Am. Econ. J.: Econ. Policy 12 (4), 88–110.

De Brauw, A., Gilligan, D.O., Hoddinott, J., Roy, S., 2015. Bolsa Família and household labor supply. Econom. Dev. Cult. Chang. 63 (3), 423–457.

Dearden, L., Emmerson, C., Frayne, C., Meghir, C., 2009. Conditional cash transfers and school dropout rates. J. Hum. Resour. 44 (4), 827–857.

Dercon, S., Krishnan, P., 2000. In sickness and in health: Risk sharing within households in rural ethiopia. J. Polit. Economy 108 (4), 688–727.

Duflo, E., Kremer, M., Robinson, J., 2011. Nudging farmers to use fertilizer: Theory and experimental evidence from Kenya. Am. Econ. Rev. 101 (6), 2350–2390.

Egger, D., Haushofer, J., Miguel, E., Niehaus, P., Walker, M.W., 2019. General equilibrium effects of cash transfers: experimental evidence from Kenya. Technical report, National Bureau of Economic Research.

Filmer, D., Schady, N., 2009. School enrollment, selection and test scores. World Bank Policy Research Working Paper, (4998).

Filmer, D., Schady, N., 2011. Does more cash in conditional cash transfer programs always lead to larger impacts on school attendance? J. Dev. Econ. 96 (1), 150–157.

Fink, G., Jack, B.K., Masiye, F., 2020. Seasonal liquidity, rural labor markets, and agricultural production. Amer. Econ. Rev. 110 (11), 3351–3392.

Fiszbein, A., Schady, N.R., 2009. Conditional Cash Transfers: Reducing Present and Future Poverty. World Bank Publications.

Gazeaud, J., Ricard, C., 2021. Conditional cash transfers and the learning crisis: evidence from tayssir scale-up in Morocco.

Hamory, J., Miguel, E., Walker, M., Kremer, M., Baird, S., 2021. Twenty-year economic impacts of deworming. Proc. Natl. Acad. Sci. 118 (14), e2023185118.

Heckman, J.J., Schmierer, D., Urzua, S., 2007. Testing for essential heterogeneity.

Heckman, J.J., Urzua, S., Vytlacil, E., 2006. Understanding instrumental variables in models with essential heterogeneity. Rev. Econ. Stat. 88 (3), 389–432.

Honorati, M., Gentilini, U., Yemtsov, R.G., 2015. The State of Social Safety Nets 2015. World Bank Group, Washington, DC.

Jack, B.K., Jayachandran, S., 2019. Self-selection into payments for ecosystem services programs. Proc. Natl. Acad. Sci. 116 (12), 5326–5333.

Jalan, J., Ravallion, M., 2001. Behavioral responses to risk in rural China. J. Dev. Econ. 66 (1), 23–49.

Jayachandran, S., De Laat, J., Lambin, E.F., Stanton, C.Y., Audy, R., Thomas, N.E., 2017. Cash for carbon: a randomized trial of payments for ecosystem services to reduce deforestation. Science 357 (6348), 267–273.

Jensen, R., 2010. The (perceived) returns to education and the demand for schooling. Q. J. Econ. 125 (2), 515–548.

Karlan, D., Zinman, J., 2009. Observing unobservables: Identifying information asymmetries with a consumer credit field experiment. Econometrica 77 (6), 1993–2008.

Khandker, S.R., Mahmud, W., 2012. Seasonal hunger and public policies: evidence from Northwest Bangladesh. The World Bank.

Macours, K., Schady, N., Vakis, R., 2012. Cash transfers, behavioral changes, and cognitive development in early childhood: Evidence from a randomized experiment. Am. Econ. J.: Appl. Econ. 4 (2), 247–273.

Macours, K., Vakis, R., 2010. Seasonal migration and early childhood development. World Dev. 38 (6), 857–869.

Martinelli, C., Parker, S.W., 2003. Should transfers to poor families be conditional on school attendance? a household bargaining perspective. Internat. Econom. Rev. 44 (2), 523–544.

Medgyesi, M., Temesváry, Z., 2013. Conditional cash transfers in high-income OECD countries and their effects on human capital accumulation. AIAS, GINI Discussion Paper, 84.

Millán, T.M., Macours, K., Maluccio, J.A., Tejerina, L., 2020. Experimental long-term effects of early-childhood and school-age exposure to a conditional cash transfer program. J. Dev. Econ. 143, 102385.

Mookherjee, D., Napel, S., 2021. Welfare rationales for conditionality of cash transfers. J. Dev. Econ. 151, 102657.

OECD, 2021. Purchasing power parities (PPP) (indicator). http://dx.doi.org/10.1787/1290ee5a-en.

Paxson, C.H., 1993. Consumption and income seasonality in Thailand. J. Polit. Economy 101 (1), 39–72.

Shah, M., Steinberg, B.M., 2019. Workfare and human capital investment: Evidence from India. J. Hum. Resour. 1117–9201R2.