Conditioning out the Poor? Consumption Inequality and the Design of Cash Transfer Programs*

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Abstract

Conditionality can prevent poor households from receiving cash transfers. Re-analyzing five randomized evaluations of conditional cash transfers, we find: (1) non-compliers — households that do not meet education conditions — are common, representing 4.4% to 37% of eligible households; (2) non-compliers often have lower baseline consumption than compliers; (3) under standard social welfare function assumptions, a budget-neutral switch to unconditional cash transfers can raise the welfare gains from increased consumption by up to 46%, depending on the context. Our results suggest that conditionality exacerbating consumption inequality can be quantitatively important for the overall welfare impacts of cash transfer programs.

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1 Introduction

Hundreds of millions of households worldwide receive cash transfers annually. During the Covid pandemic, an estimated 1.36 billion households, or 17% of global households, received a cash transfer (Banerjee et al., 2024). A key policy question in designing these programs is whether to make transfers conditional on recipient behavior. Most conditional cash transfer (CCT) programs have two main conditions: the household must i) have income below a certain threshold, and ii) meet specific behavioral conditions, such as enrolling children in school.

If excluded households are poorer than those meeting the conditions, the transfer program effectively targets richer households and increases inequality among those eligible. Under standard social welfare functions that value progressive redistribution, this diminishes the program's welfare gains. To our knowledge, no existing work quantifies the welfare consequences of conditionality that exacerbates consumption inequality; this is the focus of our paper.[1]

Theoretically, it is unclear whether poorer or richer households are more likely to meet conditions. Poorer households have more to gain from a transfer due to the declining marginal utility of consumption. However, they may also face higher opportunity costs, as meeting schooling requirements could mean foregoing child labor. Additionally, they might struggle more with administrative hurdles associated with the program. Lastly, poor households may, correctly or incorrectly, perceive lower returns to schooling for their children.

We analyze data from five randomized evaluations of CCTs in Malawi (SIHR), Mexico (Progresa), Morocco (Tayssir), Nicaragua (RPS), and Tanzania (TASAF). We present four results for each context. First, we report the fraction of compliers and non-compliers by child age and/or grade. Non-compliers are eligible households that do not meet their program's most basic educational condition: school enrollment. Among eligible households assigned to a CCT treatment, non-compliance rates are 36.8% in Malawi, 19.7% in Mexico, 20.8% in Morocco, 15.5% in Tanzania, and 4.4% in Nicaragua. Non-compliance

[1] A large literature estimates the effects of CCTs on education, health, labor supply, and other outcomes. See Banerjee et al. (2024) for a survey.

rates are higher in certain groups. For example, children in Mexico who completed only primary school (6th grade) have a 56.3% non-compliance rate. Overall, these sizable non-compliance rates suggest that conditionality may have substantial welfare consequences through our inequality mechanism.[2]

Second, we compare the baseline (before transfers start) consumption distributions for compliers and non-compliers. In Malawi, non-complier households have, on average, 26% lower consumption. In Mexico, Tanzania, and Morocco, non-compliers are also poorer than compliers, but differences in averages are more modest (between 4%-5%). In Nicaragua, the average consumption of non-compliers is 16% higher, but this number should be interpreted with caution given the small number of non-compliers (33) in this sample. In no context do we find clear evidence of non-compliers having substantially larger consumption. Overall, conditionality may lead to ex-ante poorer households not receiving transfers, but magnitudes depend on the context.

Third, we calculate counterfactual welfare gains from switching from the implemented CCT to a hypothetical unconditional cash transfer (UCT) that removes conditionality in a budget-neutral manner, providing payments to all eligible households. The goal of this exercise is to determine whether conditionality exacerbating consumption inequality has quantitatively important welfare consequences. It thus considers only the transfers' impact on consumption in a static approach and abstracts from the administrative costs of conditionality and its effects on human capital accumulation. We discuss both issues in Section 3, but note here that there is uncertainty about whether conditionality induces its intended behaviors. A review of 35 cash transfer studies (Baird et al., 2013) finds no statistically significant difference between the effects of CCTs and UCTs on school enrollment.

We follow a social welfare formulation from Alatas et al. (2019) and Hanna and Olken (2018), using a CRRA utility with a coefficient of relative risk aversion $\rho = 3$ for our baseline calculations.[3] We find welfare consequences are substantial in some contexts but

[2] We considered a larger set of 19 programs, but only these five met our requirements for program design and data availability. In the case of Morocco, compliance is defined as being enrolled in school and attending at least once in the reference month. See Section 2 for further details.

[3] We follow these papers since they study transfer recipients in a developing country (Indonesia). Studies focusing on developed countries use similar formulations (e.g., Finkelstein et al., 2019). The choice of ρ in the social insurance literature usually ranges from one to four (Chetty and Finkelstein, 2013). We also provide estimates for other ρ values in this range.

more muted in others. In Malawi, we estimate the CCT delivers only 69.4% of the welfare gains that an equally budgeted UCT program would deliver. In other words, it suggests a change to a UCT would increase welfare by 46%. In Mexico, the CCT delivers 71.3% of the benefit of a UCT when focusing on children who are likely to be non-compliers (those who only finished 6th grade). In Morocco, the CCT delivers 91% of the UCT's welfare gains (when focusing on children older than ten, which are more likely the non-compliers). In Tanzania, the same figure is 95%, and in Nicaragua, the welfare gains of the CCT and a UCT are similar.

What explains variation in the CCT-relative-to-UCT welfare gains? Intuitively, three key ingredients lower the relative CCT gains: larger transfer sizes, higher shares of non-compliers, and non-compliers having lower consumption than compliers. All these factors are substantial in Malawi. For Mexicans who only finished 6th grade, transfers and the share of non-compliers are large. In Morocco, the transfers are small, while in Tanzania non-compliers are not substantially poorer. In Nicaragua, there are few non-compliers.

Fourth, for each context, we decompose the CCT-relative-to-UCT welfare gains stemming from two mechanisms. The first relates to compliers and non-compliers having different baseline consumption (the "complier selection effect"). The second relates to UCT providing payments to all eligible households, which increases welfare even if non-compliers and compliers have the same baseline consumption distribution (the "unequal payments effect"). In Malawi, Morocco, and Tanzania, we estimate that approximately half the welfare gain comes from the unequal payments effect and the remaining from the complier selection effect. In Mexico, almost all of the welfare gains from a CCT-to-UCT switch come from the unequal payments effect, which is expected given that the consumption distributions for compliers and non-compliers are similar in this context.

We contribute to the literature on cash transfer programs—see Banerjee et al. (2024) for a review. A major motivation for our study is the finding from Baird et al. (2011) that a UCT reduced marriage and pregnancy among girls in Malawi, whereas a CCT did not have a detectable effect. This differential is driven by school dropouts (i.e., non-compliers), who benefit from transfers in the UCT but not in the CCT. We re-analyze their

data from the perspective of inequality more broadly, and indeed find the never-takers were poorer on average prior, and that overall welfare benefits of the UCT are higher than a CCT. We find similar, although more muted, evidence of this effect in other contexts. We believe our paper, by providing a strategy to quantify such welfare consequences and documenting that it is substantial in some contexts, can be used to inform the design of cash transfer programs, in particular their conditionality.

Our paper also relates to the public finance literature studying the welfare and inequality consequences of taxes and transfers, particularly those intended to induce specific behaviors. For example, Kolsrud et al. (2024) studies the design of retirement pensions and finds that reduced payments for early retirees (intended to increase retirement ages) have substantial inequality and welfare consequences since retirement age and consumption are negatively correlated. Allcott et al. (2019) and Lockwood et al. (2021) explore related issues regarding the regressivity of "sin taxes" and state-run lotteries.[4]

The remainder of this paper is organized as follows. Section 2 describes the contexts and data from the five evaluations we study. Section 3 presents the findings, and Section 4 concludes with a discussion of these results.

2 Experiments' Descriptions

The five experiments analyzed in this paper were selected as follows. We began with the 18 trials considered by Banerjee et al. (2017) study of cash transfers' effects on labor supply. We added Robertson et al. (2013), as it is one of the three experiments we are aware of that includes both CCT and UCT treatment arms.

Although our analysis does not directly use the random assignment of cash transfers, we focus on experiments because they provide both baseline (pre-treatment) consumption and endline (post-treatment) compliance status for the same household, which is not available in cross-sectional surveys.

To be included, an experiment must meet three criteria: it must evaluate a CCT (e.g.,

[4] Although not using similar social welfare calculations as in this paper, Björkegren et al. (2022) and Haushofer et al. (2022) investigate the social welfare goals of cash transfer programs.

not only compare a UCT to a control), provide baseline (before CCT payments) micro-data on program eligibility and consumption, and have post-treatment data on enrollment to measure compliance. Of the 19 experiments considered, seven were excluded for only evaluating UCTs, three for lacking consumption data, one for missing eligibility information, and one for lacking school enrollment data. Data was not available for the two other studies. See Appendix Table A.1 for details.

Four of the programs are operated by governments (except for Malawi). A systematic review (Baird et al., 2013) classifies the Malawi, Morocco, and Nicaragua programs as having "*explicit schooling conditions monitored and enforced*" and the Mexican Progresa as having "*explicit conditions, (imperfectly) monitored, with minimal enforcement*." The Tanzania program is not included in Baird et al. (2013), but Evans et al. (2014) reports that conditions were monitored and enforced.

We describe each of the five experiments below and conclude this section by describing how data across contexts is harmonized and discussing the measurement of compliance.

Malawi (SIHR). The source paper for this experiment is Baird et al. (2011). The experiment occurred in 176 enumeration areas (EAs) in the Zomba district. The study focused only on never-married women aged 13-22. Randomization was at the EA level: 46 were assigned to a CCT arm, 27 to a UCT arm, and 15 to control. Our sample includes 470 eligible girls from the CCT arm sampled by the evaluators.

The CCT arm involved two types of payments. First, parents of girls attending school received 4, 6, 8, or 10 USD per month. The amount was randomly determined at the EA level. Second, girls received an individual payment of 1, 2, 3, 4, or 5 USD per month. The amount was determined by a public lottery within the community. Payments were conditional on school enrollment and attendance greater than 80% of school days.

We use consumption data from the baseline survey collected between October 2007 and January 2008. Specifically, we use the household's response to the question "What were your total monthly household expenditures last month?"

Table 1 shows the number of compliers and non-compliers by age within the CCT arm

of the Malawi experiment. We define compliance as being enrolled in school in Term 1, 2010. The fraction non-complying increases substantially with age, from 23% for 14-year-olds up to 65% - 82% for 18- to 20-year-olds. The average payment size is substantial (close to 25% of average per capita consumption).[5]

Mexico (Progresa). The Progresa CCT has been studied by several papers. See Parker and Todd (2017) for a survey. For the experimental evaluation, 506 rural communities from seven Mexican states were selected in 1997, with 320 randomly assigned to treatment (receiving treatment starting in May 1998) and the other 186 to control (receiving benefits starting in January 2000). Our sample includes 11,093 eligible children in treatment communities

Payments were conditional on a child aged 7-18 enrolling in school between 3rd and 9th grade and on attendance greater than 85% of school days. Payments depended on the grade of enrollment.[6]

We use consumption data from the March 1998 ENCEL survey, before payments started. Specifically, we use the household's response to the question "Generally, how much money do you have available for total household expenses per week?"

Table 1 shows the number of compliers and non-compliers by grade among eligible children in treated communities, by grade of enrollment. We define compliance as being enrolled in school in the May 1999 ENCEL survey. The survey does not explicitly collect data on current grades. We infer this by adding one to the last completed grade to determine the grade a child should be enrolling at. A substantial amount of students drop out at the end of primary school (6th grade in Mexico). Thus the share of non-compliers is large (56%) for those labeled as "7th grade" but smaller (10% or lower) for other grades. In subsequent analysis, we will focus both on the entire sample of eligible children and also on the subsample of those that only completed sixth grade (the "7th-grade" subsample,

[5] Baird et al. (2011) also uses non-enrollment in Term 1, 2010 to define non-compliers in their analysis of effects on pregnancy and marriage.

[6] For primary school students, it varied from 75 Mexican pesos (8.2 USD) per month for 3rd grade to 150 pesos (16.4 USD) for 6th grade. For secondary school, payments also depended on grades and were larger for girls than boys. It varied from 220 Mexican pesos (24 USD) for boys in 7th grade to 285 pesos (31.1 USD) for girls in 9th grade. Families also received yearly payments of 135 Mexican pesos (15 USD) for each child in primary school and 170 Mexican pesos (18.6 USD) for each child in secondary school.

henceforth). The average payment size is substantial (approximately 22% of per capita consumption).

Morocco (Tayssir). The source paper for this experiment is Benhassine et al. (2015). The program targeted children of primary school age (6 to 15). The experiment was implemented in 320 "school sectors" in the five poorest regions of Morocco. Randomization was at the sector level: 60 were assigned to a control group and the remaining 260 randomized to the four following treatments: a CCT group where the payment was made to the father (90 sectors), a CCT group where the payment was made to the mother (90 sectors), a "labeled" cash transfer group where the payment was made to the father (40 sectors), and a "labeled" transfer where the payment was made to the mother (40 sectors). The purpose of the "labeled" transfer group is to test the effects of conditionality on school participation. Our sample includes 6,203 children from both the CCT to fathers and CCT to mothers arms.[7]

Payments were 60 Moroccan dirhams (8 USD) per month for grades 1 and 2 (6 to 7-year-olds), 80 dirhams (10 USD) for grades 3 and 4 (8 to 9-year-olds), and 130 dirhams (13 USD) for children 10 to 15-years-old. Parents in all treatment arms had to enroll their children in the program, at their school, annually to receive payments. The enrollment process in the CCT groups made clear that payment receipt was conditional on enrollment and "no more than four absences in the month." Program take-up was 97%.

Our consumption variable is taken directly from the source paper's replication files. We use monthly per capita consumption consumption from the June 2008 baseline survey, reported in Table 2 of Benhassine et al. (2015).

Table 1 shows the number of non-compliers and compliers by age. We define compliance using the main school participation outcome in the source paper: whether a child was reported as being enrolled and having attended school at least once in "*end of program year 2*" (May 2010). The share of non-compliers is low (below 4%) for those aged 6-10, but substantially larger (ranging from 12% to 77%) for those aged 11-15. In subsequent analysis, we focus both on the entire sample of eligible children and the subsample aged 11-15.

[7] One CCT-to-fathers sector could not be reached due to flooding during the baseline survey, leaving 89 sectors in this arm.

A key feature of the Morocco experiment, also noted in Benhassine et al. (2015), is the relatively small payment size, close to 3% of per capita consumption (compared to 25% in Malawi and 22% in Mexico).

Nicaragua (RPS). The source paper for this experiment is Maluccio and Flores (2005). The Red de Protección Social (RPS) pilot took place in two of Nicaragua's poorest departments (Madriz and Matagalpa). It targeted 42 *comarcas*, randomly assigning 21 to the treatment and 21 to the control group. Our sample includes 714 eligible children in the treated *comarcas*.

RPS featured two core components. First, the education component provided an annual C$1,440 (1112 USD) per child aged 7-13, conditional on enrollment and maintaining at least 85% attendance, plus an additional C$275 (21 USD) for school supplies. Second, a food security, health, and nutrition component offered C$2,880 (224 USD) per household each year on the condition that parents attend bi-monthly health workshops and bring children under five for routine checkups.

We use consumption data from the baseline survey (before treatment) conducted in August and September 2000. The survey included detailed itemized questions on the consumption of several food and non-food items. We use these questions to construct total household consumption.

Table 1 shows the number of compliers and non-compliers by age for eligible households in treated *comarcas*. We define compliance as being enrolled in school in the endline survey conducted in October 2002. Compared to other contexts, the share of non-compliers in the RPS program is small, at only 4.4%. Moreover, our sample only includes 33 non-compliers, making comparisons less precise. The payment size relative to per capita consumption levels is small at 5.6%.

Tanzania (TASAF). The source paper for this experiment is Evans et al. (2014). The TASAF experiment took place in three Tanzanian districts. The program targeted 80 villages, randomly assigning 40 to the treatment and 40 to the control group. Eligibility was based on locally defined criteria for vulnerability, such as orphanhood and chronic illness

within the family. Our sample includes 753 eligible children in the treated villages.

Payments were determined by household composition. Each child aged 7–15 received USD$1.5 per month (approximately 50% of the food poverty line), conditional on compliance. Children were required to be enrolled in school and maintain at least 80% school attendance.

We use consumption data from the baseline survey (before treatment) conducted in early 2009. The survey included detailed itemized questions on consumption of several food and non-food items, including the value of own crops that were consumed and gifts. We use these questions to construct total household consumption.

Table 1 shows the number of compliers and non-compliers by age. We define compliance as being enrolled in school in the endline survey conducted in late 2012. Non-compliance is high among children aged 7 but decreases with age until rising again for those 13 and older. Thus, differently from other contexts, non-compliance also includes a margin of delaying when younger children start school. Payment sizes are substantial (close to 17% of per capita consumption), but not as much as in Malawi and Mexico.

Data harmonization. In all five contexts we study, the sample consists only of children who are eligible to receive a CCT (e.g., we exclude those in non-CCT and control arms, with income above the eligibility cutoff, and children of age below or above eligibility for payments). We always measure compliance in the period that the source papers label as the "endline."[8]

Our consumption variables are measured *before* CCT payments start (e.g., in a baseline survey before treatment). We adjust consumption variables so they measure the logarithm of monthly total household consumption per capita. We adjust to per capita terms by calculating total household size as the number of adults plus 3/5 the number of children, where a child is defined as younger than 12 years old (Deaton, 1997). Observations with missing data on consumption and/or compliance are excluded from the analysis.

[8] In the Mexican case, we treat the May 1999 ENCEL survey as the endline since it is the last survey before the control group starts receiving treatment.

Measuring compliance. Our measure of compliance is school enrollment (except in Morocco, where it is enrollment and attending at least once in May 2010). However, enrollment may be part of a larger set of conditions, including attendance and health components such as vaccinations. We focus on enrollment for four reasons. First, school enrollment is the condition most likely to be enforced in practice (Baird et al., 2013). Second, it provides a comparable definition of compliance across our contexts. Third, data on meeting other conditions is often unavailable. Fourth, in all the contexts we study, micro-data explicitly detailing which households were excluded from payments for non-compliance is also unavailable. Even if such data were available, the enforcement of conditions in a pilot evaluation may differ from when the program is operating at scale, and enrollment can potentially be a better predictor of not receiving the transfers at this stage.

Moreover, only data reported by households in a survey is available to measure enrollment. This raises the potential issue of measurement error in defining compliers in our analysis. We highlight three points on this issue. First, the source papers (and several others, in the Mexican case) use the same enrollment variables as their main outcomes to evaluate CCT's effects on human capital investments. Second, analysis in the source papers for Malawi and Morocco suggests little measurement error on average. In Malawi, Baird et al. (2011) measures enrollment using school surveys for a subsample of participants. Mean self-reported and teacher-reported enrollment rates are virtually the same in the CCT arm, which is the focus of our paper. In Morocco, Benhassine et al. (2015) estimates the effects on enrollment and dropout behavior using both household surveys and school records and finds similar results using both sources. Third, if measurement error in the enrollment variable is independent of baseline consumption, our analysis would be unaffected. We are not aware of papers documenting how misreporting of enrollment in surveys correlates with income or consumption.[9]

[9] We do not analyze data from school surveys from Malawi since this data is not available. Moreover, only 43% of participants were randomly chosen to have their enrollment data collected in such surveys. We cannot use the data from school records in Morocco in our analysis since the replication files do not allow us to link it to the household survey on consumption (understandably, given privacy considerations).

3 Results

Consumption differences between compliers and non-compliers. Figure 1 presents the consumption distributions for compliers and non-compliers across our five contexts. Specifically, it provides histograms of the logarithm of baseline consumption per capita (before payments start). In Mexico and Morocco, we focus on the previously discussed subsamples with higher shares of non-compliers (those expected to enroll in 7th grade and those aged 11-5, respectively).[10] We treat each child as if she comes from a unique household - i.e., we do not consider potential correlations in non-compliance within households.

In Malawi, the non-compliers' distribution is clearly shifted to the left of the compliers' and mean consumption for non-compliers is 26% lower than for compliers. In Morocco, the non-complier distribution also appears shifted to the left, but by a more modest magnitude (differences in averages are 4%). In Mexico, Nicaragua, and Tanzania, there are no clearly visible differences between the distributions. On average, non-compliers consume 4% and 5% less than compliers in Mexico and Tanzania, respectively. In Nicaragua, non-compliers consume 16% *more* on average. However, we caveat that the sample of non-compliers here is small at only 33 households.

Welfare comparisons: empirical strategy. We compare the welfare gains from switching from the implemented CCT to a hypothetical UCT that removes conditionality and provides payments to all households that were eligible for the CCT. The counterfactual UCT has the same aggregate transfer budget. We implement this by scaling down transfers: if t^{cct} is the monthly transfer paid to compliers in a CCT and s is the share of compliers in the program, we set the UCT transfer t_i^{uct} equal to $s \cdot t_i^{cct}$.[11]

Let c_i^{cct} denote the household i's per capita consumption when enrolled in the CCT program. Empirically, we set $c_i^{cct} = c_i^{baseline}$ if i is a non-complier and $c_i^{cct} = c_i^{baseline} + t_i^{cct}$

[10] Appendix Figure A.1 provides similar figures for the entire samples.

[11] Transfers have an i subscript since, in Mexico and Morocco, CCT payments become larger with age. We calculate UCT counterfactual payments assuming that older children also get larger transfers: payments are all scaled down by the same factor. The Mexican program provides larger payments for girls than boys. We again maintain the same proportional difference by gender in our counterfactual UCT program.

if i is a complier. $c_i^{baseline}$ is the baseline monthly household per capita consumption we directly observe. Let c_i^{uct} be i's consumption in the counterfactual UCT program. We set $c_i^{uct} = c_i^{baseline} + t_i^{uct}$ to all i in our samples (which only includes eligible households). Let $u(c_i)$ denote i's utility. Its change from the transition from the CCT to the UCT, $u(c_i^{uct}) - u(c_i^{cct})$, is always positive for non-compliers, as they go from not receiving any transfers to receiving one, and always negative for compliers, as the size of their transfer payment decreases.

Note our approach is static and abstracts from borrowing and saving (i.e., the marginal propensity to consume is one).[12]

Our key measure is the ratio between the CCT welfare gain (relative to no transfers) and the UCT utility gain (relative to no transfers):

$$\frac{\sum_{i=1}^{N} u(c_i^{cct}) - \sum_{i=1}^{N} u(c_i^{baseline})}{\sum_{i=1}^{N} u(c_i^{uct}) - \sum_{i=1}^{N} u(c_i^{baseline})} \tag{1}$$

where N is the number of households in a sample.

Equation (1) has four desirable properties. First, it provides an intuitive interpretation. If it equals one, it indicates the CCT delivers the same aggregate utility gains as the UCT. A value of, say, 0.5 would indicate that the CCT program delivers only half the welfare benefit of a UCT program that has the same aggregate transfer budget. Second, it provides a comparable metric for each context we study. Third, it is unaffected by any positive affine transformation of $u(\cdot)$. Fourth, our measure uses information on the entire distribution of consumption in complier and non-complier households (and not only their mean differences) without making assumptions about the shape of such distributions.

The goal of our exercise is to focus on the welfare consequences of conditionality exacerbating consumption inequality. It thus abstracts from conditionality's impact on human capital investments and its monitoring and enforcement costs. However, as these are key parts of a full-fledged cost-benefit analysis, we briefly discuss them below.

There are three reasons further supporting not incorporating conditionality's effects

[12] The evaluations we study provide limited information on borrowing and saving and also do not collect consumption data over enough periods to further study its dynamics.

on human capital in our analysis. First, there is uncertainty about whether conditionality induces its intended behaviors. A Campbell systematic review of 35 cash transfer studies (Baird et al., 2013) finds no statistically significant difference between the effects of CCTs and UCTs on school enrollment.[13] Second, even if such effects were known, the long-run welfare consequences depend on other hard-to-predict factors (e.g., returns to education and discount rates). Third, conditionality can have effects on multiple outcomes, including those with difficult-to-determine welfare effects. For example, Baird et al. (2011) finds that a UCT lowers teenage marriage and pregnancy compared to a CCT.

The costs of monitoring and enforcing conditionality can be sizable and thus likely to "tilt the scales" towards UCTs' effectiveness. Caldés et al. (2006) estimate that conditionality costs alone (i.e., separately from other administrative costs such as determining eligibility) account for almost 20% of Progresa's administrative costs and equal 2% of the program's transfer budget. However, one may consider that transfers themselves are not a social cost (and only the deadweight loss of raising the funds for them is a cost). Benhassine et al. (2015) indicates the administrative costs of Tayssir's CCT are 30% higher than the labeled cash transfer and provides further discussion of how administrative costs make standard CCT programs expensive.

Our method also assumes that a child's school enrollment implies the household receives the transfer. If the program is less strict and provides payments to non-enrolled children, our results would overestimate the benefits of switching to a UCT for poorer non-compliers. This discrepancy also affects the interpretation of the original RCTs on CCT programs, as households may initially comply, expecting enforcement, but later adjust their behavior upon learning conditions are not strictly applied.

Welfare comparison: results. Table 2 presents the ratio from equation (1) for different contexts. Our baseline estimates follow the parametrization of $u(\cdot)$ from Alatas et al.

[13] The same review concludes that both UCTs and CCTs have a positive effect on enrollment relative to a pure control. However, the relevant comparison for our welfare calculations is between CCTs and UCTs. To our knowledge, three randomized trials include both a CCT and UCT arm. Two find that CCT's effect on enrollment is 40% larger than the UCT's effect (Baird et al., 2011; Akresh et al., 2020), and one finds essentially no difference in their effects (Robertson et al., 2013). Benhassine et al. (2015) compares a CCT to a labeled cash transfer (with no conditionality, but labeled as meant to support education) and finds that conditionality "*made almost no difference in our context*."

(2019), Hanna and Olken (2018), and Finkelstein et al. (2019): a constant relative risk aversion (CRRA) utility with a coefficient of risk aversion $\rho = 3$. To probe the sensitivity of results to the concavity of the utility function, we also provide estimates using ρ equal to one ("ln utility"), two, and four. As before, we treat each child as if she comes from a unique household.

Our baseline estimates indicate the Malawi CCT only delivers 68.4% of the utility gain of an equally budgeted UCT program. In other words, the budget-neutral switch to a UCT would increase welfare by 46.2% ($1/0.684 - 1$). In Mexico, when focusing on the 7th-grade subsample, a CCT only delivers 71.3% of the UCT gains. The corresponding number using the entire Mexico sample is 94.8%. The CCT-to-UCT welfare gains ratios for Morocco and Tanzania range between 90.8%-96.0%, and in Nicaragua it is close to one. We further discuss comparisons across contexts in Section 4.

As expected, ratios become closer to one as we consider utility specifications with less curvature. However, they remain substantial in the cases of Malawi and the 7th-grade subsample in Mexico. For example, with $\rho = 2$, the Malawi CCT delivers only 74.9% of the welfare gains of the UCT, and with ln utility, 85.3% of the gain.

Decomposing mechanisms: empirical strategy. Two mechanisms can affect the CCT-to-UCT welfare gains ratio. The first are differences in the consumption distribution of compliers and non-compliers. If compliers have higher baseline consumption, CCT payments will target the richer households with lower marginal utility of consumption. We label this the "complier selection effect." Second, even if the baseline consumption distribution for complier and non-complier households is exactly the same, the ratio in equation (1) would be smaller than one. This is because the CCT only targets a share of eligible households (the compliers) while the UCT provides payment to all of them. Given a concave utility function, the gains using the same budget to provide transfers to all households are thus larger. We label this as the "unequal payments effect" and define it as the ratio in equation (1) that would occur if the distribution of baseline consumption

for compliers was the same as for non-compliers.[14]

We decompose our observed CCT versus UCT gains into complier selection and unequal payments effects as follows. Let s be the share of non-compliers and N the number of eligible households in a given context. We randomly draw sN households, "pretend" these households are non-compliers, and recalculate the ratio in equation (1). We replicate this process 1,000 times. In *expectation*, the consumption distributions of those randomly labeled as non-compliers are the same. Thus, the *average* of the 1,000 simulated CCT-to-UCT welfare gains ratios is driven only by the unequal payments effect. Since our main results in Table 2 include both effects, comparing them to this average simulated ratio provides a decomposition.

Decomposing mechanisms: results. Figure 2 presents the results using our baseline utility specification (CRRA with $\rho = 3$). Each panel shows in blue the distribution of the ratio between the simulated CCT-to-UCT welfare gains ratio across the 1,000 draws. The dashed vertical line shows the mean of this distribution, and the solid line shows the "observed ratio", which is based on the actual compliers observed in the data and reported on Table 2).[15]

In Malawi, the average simulated ratio is 0.809, indicating that, even if there was no difference between the consumption distribution of compliers and non-compliers, a CCT would only deliver 80.9% of the welfare gains of a UCT given the unequal payments mechanism. Comparing it to the observed ratio from Table 2 of 0.684 indicates the unequal payments mechanism accounts for approximately two-thirds of the gap between the welfare gains that a CCT and a UCT provide, with the complier selection mechanism accounting for the other third. For Morocco and Tanzania, the average simulated ratio is roughly is closer to one than to the observed ratio and one, indicating the unequal payments mechanism approximately accounts for one-third of the CCT-to-UCT welfare gains

[14] A simple example illustrates the unequal payments effect. Assume two households have the same baseline consumption c. Providing a transfer of t to one increases aggregate utility by less than providing $t/2$ to both if there is diminishing marginal utility: $u(c+t) - u(c) < 2[u(c+t/2) - u(c)]$.

[15] Note the distribution can have support above one (the simulated CCT to delivers higher gains the UCT) since some draws may have (randomly assigned) non-compliers with systematically higher baseline consumption than compliers.

ratios. In Mexico, almost all the CCT-to-UCT welfare gains ratio can be accounted for by the unequal payments effect. This is expected given that non-compliers' and compliers' consumption are similar in this context. In the Nicaragua case, there is essentially no utility difference to decompose (both simulated and observed ratios are close to one).[16]

4 Discussion

We conclude by discussing why the CCT-to-UCT welfare gains ratios vary across the five contexts we study. Intuitively, three ingredients can reduce the relative gains of a CCT. The first is the transfer amount relative to baseline consumption. The second is the share of non-compliers. The third is the extent the non-complier consumption distribution is "shifted to the left" of the complier distribution.

Malawi combines all three ingredients. It has the largest transfers, the highest share of non-compliers, and the clearest difference in consumption distributions among all five contexts. Not surprisingly, it has the smallest CCT-to-UCT welfare ratio, at 68.4%.

Mexico, which also involves large transfer sizes, illustrates the role of the share of non-compliers. In the subsample where non-compliance is substantial (those expected to enroll in 7th grade), the CCT only delivers 71.3% of a UCT's welfare gains, but incorporating the remainder of the sample, where compliance is larger, moves the ratio closer to one. Moreover, the decomposition analysis highlights that, even when the consumption distributions of compliers and non-compliers are similar, the welfare losses from a CCT exacerbating inequality can be substantial due to the "unequal payments effect" if the share of non-compliers is large.

Morocco illustrates the role of transfer sizes. As they are small (on average 3% of baseline per capita consumption), the CCT-to-UCT welfare ratio is not substantially far from one, regardless if one focuses on a subsample where non-compliers are more common (those aged 11-15, where the CCT-to-UCT welfare ratio is 91%) or the entire sample that has a smaller share of non-compliers (where the ratio is 96%).

[16] As before, the Mexican and Moroccan cases focus on the subsamples with higher shares of non-compliers. Appendix Figure A.2 presents the same analysis for the full samples.

In Tanzania, both the transfer size and share of non-compliers are smaller than in Malawi and Mexico, and thus the CCT-to-UCT welfare ratio is closer to one. The ratio is essentially one in Nicaragua; this can be attributed to its small transfer size and share of non-compliers, but we reiterate the caveat of the small sample of non-compliers.

Our results suggest that conditionality requirements that exacerbate consumption inequality among eligible households are quantitatively important for the overall welfare impacts of cash transfer programs. We believe the issue has been overlooked in previous literature and hope the type of assessment we provide here can be used to inform the design of cash transfers.

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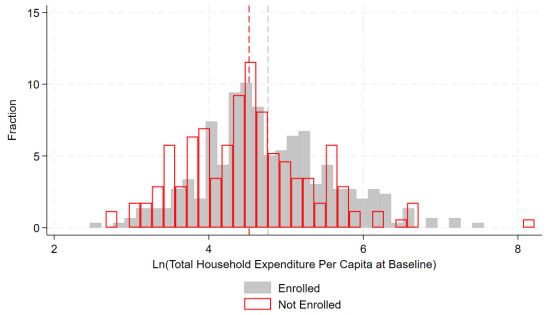
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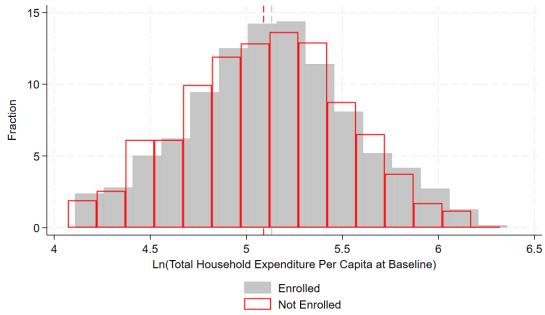
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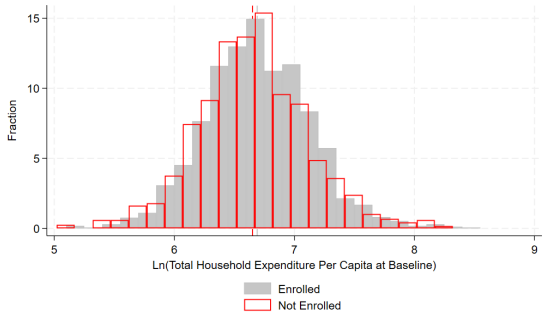
Figure 1: Baseline Consumption Distribution by Complier Status



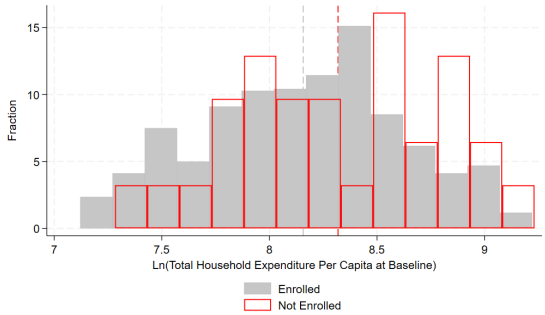
(a) Malawi: 14-20 Year Olds
Complier mean: 4.77, Non-complier mean: 4.51



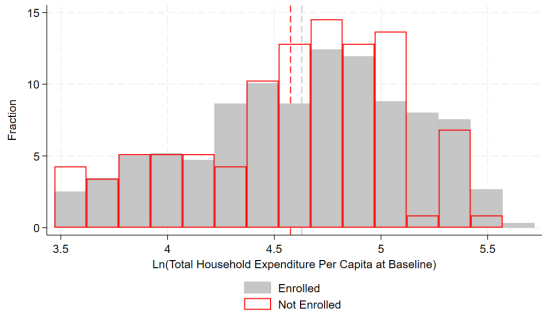
(b) Progresa - 7th Grade
Complier mean: 5.13, Non-complier mean: 5.09



(c) Morocco: 11-15 Year Olds
Complier mean: 6.69, Non-complier mean: 6.65



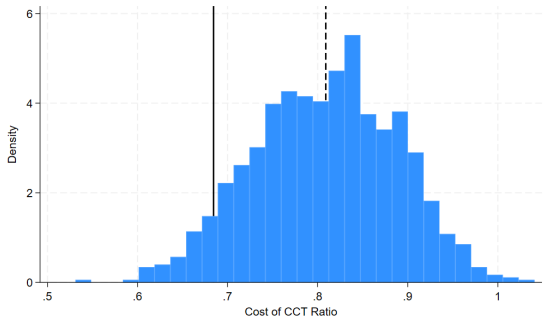
(d) Nicaragua: 7-13 Year Olds
Complier mean: 8.16, Non-complier mean: 8.32



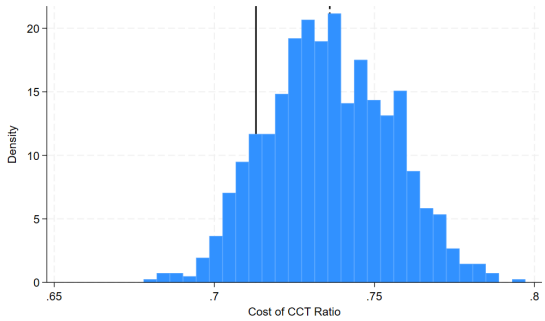
(e) Tanzania: 7-15 Year Olds
Complier mean: 4.63, Non-complier mean: 4.58

Each sub-figure shows, for a given context, histograms of the logarithm of total household consumption per capita at baseline (i.e., before CCT payments start). Solid gray-colored bars provide the distribution for compliers and the red-bordered bars do so for non-compliers. Vertical lines indicate the mean consumption for each group. The sample only includes eligible children in the CCT arm of their respective experiment. Compliance is defined as being enrolled in school (and attending at least once a month in the case of Morocco).

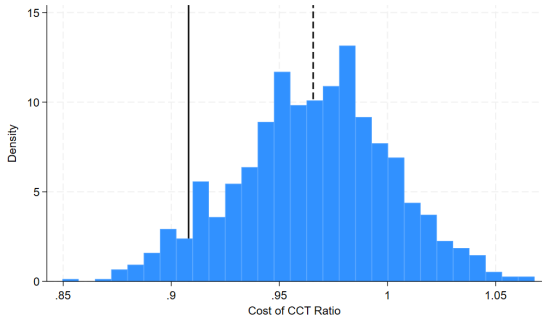
Figure 2: Decomposing the CCT-to-UCT Welfare Gains Ratios



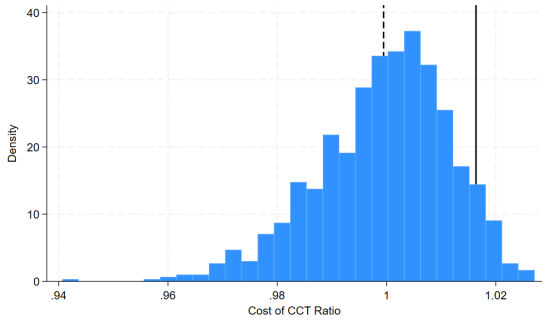
(a) Malawi: 14-20 Year Olds
Obs. Ratio: 0.684, Avg. Simul. Ratio: 0.809



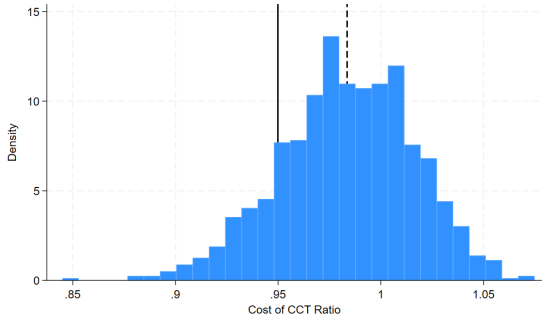
(b) Mexico - 7th Grade
Obs. Ratio: 0.713, Avg. Simul. Ratio: 0.737



(c) Morocco: 11-15 Year Olds
Obs. Ratio: 0.908, Avg. Simul. Ratio: 0.964



(d) Nicaragua: 7-13 Year Olds
Obs. Ratio: 1.016, Avg. Simul. Ratio: 1.000



(e) Tanzania: 7-15 Year Olds
Obs. Ratio: 0.950, Avg. Simul. Ratio: 0.984

Each sub-figure shows, for a given context, the results from randomly reassigning households' complier status and recalculating the CCT-to-UCT welfare gains ratio from equation (1). We replicate this process 1,000 times, creating a distribution of ratios depicted in the histogram (blue bars). The dashed vertical line provides the mean value of this distribution ("average simulated ratio"). It provides an estimate of the part of the CCT-to-UCT welfare gains ratio stemming from the "unequal payments effect." The solid vertical line is the observed ratio based on the actual compliance from Table 2 (the "observed ratio"). All calculations assume a CRRA utility with $\rho = 3$. See Section 3 for further information.

Table 1: Non-Complier vs. Complier Summary Statistics

	# Non-Compliers	# Compliers	Share Non-Compliers	Payment \Avg. Consumption	
				Girls	Boys
	(1)	(2)	(3)	(4)	(5)
Panel A: Malawi (SIHR)					
Age=14	20	67	0.230	0.260	-
15	28	79	0.262	0.241	-
16	38	35	0.521	0.279	-
17	30	30	0.500	0.318	-
18	20	11	0.645	0.235	-
19	7	3	0.700	0.176	-
20	14	3	0.824	0.293	-
Unknown	16	69	0.188	0.195	-
All Ages	173	297	0.368	0.252	-
Panel B: Mexico (Progresa)					
Grade=3	99	1634	0.057	0.119	0.119
4	147	1583	0.085	0.141	0.141
5	117	1432	0.076	0.174	0.174
6	113	1347	0.078	0.223	0.223
7	1518	1175	0.564	0.336	0.318
8	111	979	0.101	0.367	0.346
9	75	763	0.089	0.397	0.355
All Grades	2180	8913	0.197	0.229	0.221
Panel C: Morocco (Tayssir)					
Age=6	11	412	0.026	0.021	0.021
7	5	486	0.010	0.019	0.019
8	9	654	0.014	0.025	0.025
9	24	752	0.031	0.026	0.026
10	34	819	0.040	0.034	0.034
11	101	719	0.123	0.033	0.033
12	174	478	0.267	0.033	0.033
13	283	312	0.476	0.033	0.033
14	349	191	0.646	0.032	0.032
15	299	91	0.767	0.033	0.033
All Ages	1289	4914	0.208	0.030	0.030
Panel D: Nicaragua (RPS)					
Age=7	13	89	0.127	0.056	0.056
8	6	128	0.045	0.056	0.056
9	2	131	0.015	0.056	0.056
10	3	119	0.025	0.056	0.056
11	1	83	0.012	0.056	0.056
12	4	69	0.055	0.056	0.056
13	2	62	0.031	0.056	0.056
All Ages	33	681	0.044	0.056	0.056
Panel E: Tanzania (TASAF)					
Age=7	22	61	0.265	0.169	0.169
8	14	64	0.179	0.169	0.169
9	8	71	0.101	0.169	0.169
10	8	87	0.084	0.169	0.169
11	4	65	0.058	0.169	0.169
12	12	112	0.097	0.169	0.169
13	11	77	0.125	0.169	0.169
14	15	61	0.197	0.169	0.169
15	23	38	0.377	0.169	0.169
All Ages	117	636	0.156	0.169	0.169

The table presents, for each context: (1) the number of eligible non-complier households, (2) the number of complier households, (3) the share of households that are non-compliers, (4) the size of the CCT payment for girls as a share of average per capita household consumption, and (5) the same as (4) for boys. The sample only includes eligible children in the CCT arm of their respective experiment. Compliance is defined as being enrolled in school (and attending at least once a month in the case of Morocco). See Section 2 for further information.

Table 2: CCT-to-UCT Welfare Gains Ratios

	Ratio of CCT Utility Gain to UCT Utility Gain			
Sample	Ln Utility	CRRA ($\rho = 2$)	**CRRA ($\rho = 3$)**	CRRA ($\rho = 4$)
Malawi	0.853	0.749	**0.684**	0.660
Progresa - 7th Grade	0.868	0.787	**0.713**	0.648
Progresa - All	1.009	0.978	**0.948**	0.921
Morocco (ages 11-15)	0.990	0.956	**0.908**	0.841
Morocco - All	0.996	0.981	**0.960**	0.932
Nicaragua	1.003	1.011	**1.016**	1.020
Tanzania	0.986	0.970	**0.950**	0.928

The table presents the CCT-to-UCT welfare gains ratio: the ratio between the welfare gains from the observed CCT and a counterfactual UCT program that uses the same budget to provide payments to all eligible households. In particular, it provides estimates of equation (1) discussed in Section 3. Different columns provide estimates based on different curvatures for the assumed CRRA utility function. Our baseline is the case with $\rho = 3$, in bold.

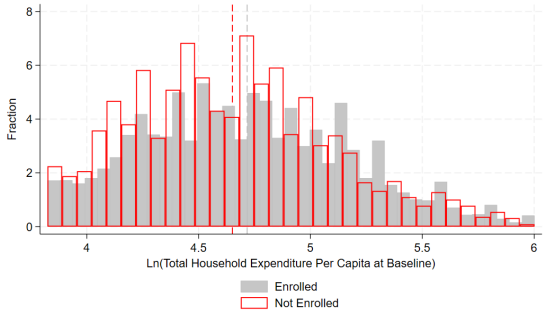
Online Appendix - Supplemental Tables and Figures

Table A.1: Programs Considered for Analysis

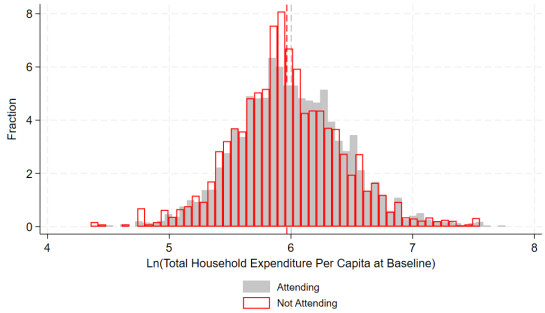
Country	Program Name	Reason for Non-Inclusion	Source Paper
Included in our analysis:			
Malawi	SIHR	-	Baird et al. (2011)
Mexico	Progresa	-	Parker and Todd (2017)
Morocco	Tayssir	-	Benhassine et al. (2015)
Nicaragua	Red de Protección Social (RPS)	-	Maluccio and Flores (2005)
Tanzania	TASAF	-	Evans et al. (2014)
Not included in our analysis:			
Burkina Faso	NCTPP	Data not available	Akresh et al. (2020)
Indonesia	Program Keluarga Harapan (PKH)	No baseline eligibility data	World Bank Office Jakarta (2011)
Nicaragua	Atención a Crisis	Enrollment data not available	Macours et al. (2012)
Honduras	Programa de Asignación Familiar II (PRAF II)	No consumption data	Galiani and McEwan (2013)
Philippines	Pantawid Pamilyang Pilipino Program (PPPP)	No consumption data	Chaudhury et al. (2013)
Colombia	SCAE	No consumption data	Barrera-Osorio et al. (2011)
Uganda	Cash transfer for pre-school	Unconditional	Gilligan and Roy (2013)
Kenya	GiveDirectly	Unconditional	Haushofer and Shapiro (2013)
Mexico	Programa de Apoyo Alimentario (PAL)	Unconditional	Skoufias et al. (2013)
Ecuador	BDH	Unconditional	Edmonds and Schady (2012)
Kenya	CT-OVC	Unconditional	Asfaw et al. (2014)
Malawi	SCT program	Unconditional	Covarrubias et al. (2012)
Zambia	Child Program	Unconditional	American Institutes for Research (2013)
Zimbabwe	Manicaland CCT/UCT Experiment	Data not available	Robertson et al. (2013)

The papers listed above are the 18 trials considered by Banerjee et al. (2017) study of cash transfers' effects on labor supply with the addition of Robertson et al. (2013), as it is one of the experiments we are aware of that includes both CCT and UCT treatment arms. See Section 2 for further discussion.

Figure A.1: Baseline Consumption Distribution by Complier Status - Additional Figures



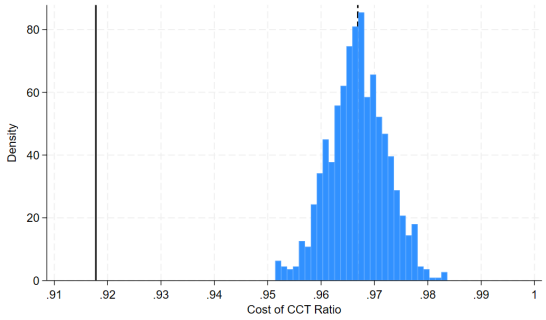
(a) Mexico - All Grades
Complier mean: 4.71, Non-complier mean: 4.65



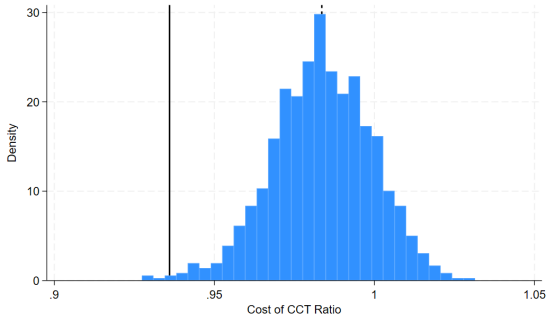
(b) Morocco - All Ages
Complier mean: 6.00, Non-complier mean: 5.96

This figure replicates Figure 1 using the entire sample for Mexico and Morocco (as opposed to only the 7th grade and ages 11-15 subsample, respectively). Each sub-figure shows, for a given context, histograms of the logarithm of total household expenditure per capita at baseline (i.e., before CCT payments start). Solid gray-colored bars provide the distribution for compliers and the red-bordered bars do so for non-compliers. Vertical lines indicate the mean consumption for each group. The sample only includes eligible children in the CCT arm of their respective experiment. Compliance is defined as being enrolled in school (and attending at least once a month in the case of Morocco).

Figure A.2: Decomposing the CCT-to-UCT Welfare Gains Ratio - Additional Figures



(a) Mexico: All Grades
Obs. Ratio: 0.917, Avg. Simul. Ratio: 0.966



(b) Morocco: All Ages
Obs. Ratio: 0.936, Avg. Simul. Ratio: .983

This figure replicates Figure 2 using the entire sample for Mexico and Morocco (as opposed to only the 7th grade and ages 11-15 subsample, respectively). Each sub-figure shows, for a given context, the results from randomly reassigning households' complier status and recalculating the CCT-to-UCT welfare gains ratio from equation (1). We replicate this process 1,000 times, creating a distribution of ratios depicted in the histogram (blue bars). The dashed vertical line provides the mean value of this distribution ("average simulated ratio"). It provides an estimate of the part of the CCT-to-UCT welfare gains ratio stemming from the "unequal payments effect." The solid dashed vertical line is the observed ratio based on the actual compliance from Table 2 (the "observed ratio"). All calculations assume a CRRA utility with $\rho = 3$. See Section 3 for further information.