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A fine predicament: Conditioning, compliance and consequences in a labeled cash transfer program

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ABSTRACT

The Kenya Cash Transfer Programme for Orphans and Vulnerable Children (CT-OVC) presents a valuable opportunity to examine the effects of imposing monetary penalties for noncompliance with conditions in cash transfer programs, in contrast to providing only guidance (or "labeling") for cash transfer use. We take advantage of random assignment to a conditional arm within the CT-OVC treatment locations to understand the impact of imposing conditions with penalties on program beneficiaries, as well as how this effect varies by household wealth. Program beneficiaries (orphans and vulnerable children) were expected to visit health facilities for immunizations, growth monitoring and nutrition supplements and to enroll in and attend school. We find little difference in program outcomes between households in the conditional treatment arm compared to those in the treatment arm with labeling only (in which information was provided about these expectations but compliance was not monitored). However, among the poorest CT-OVC beneficiaries, assignment to the conditional arm was associated with penalty fines and a significant decrease in non-food consumption. This suggests that in comparison to labeled cash transfers, conditional cash transfers may produce unintended, regressive policy effects for the most vulnerable participants.

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1. Introduction and background

Cash transfers are one of the most popular forms of aid interventions directed toward reducing poverty and the intergenerational transmission of poverty. More than a fifth of all countries have implemented a conditional cash transfer (CCT) program, including about one-third of developing and middle-income countries (Morais de Sá e Silva, 2017). Although most of the inaugural cash transfers programs and many subsequent program efforts have imposed conditions on households' receipt of cash transfers that prescribe how the monies should be used (Baird, Ferreira, Ozler, & Woolcock, 2013), unconditional cash transfer (UCT) programs are proliferating as well and are among some of the largest cash transfer programs today (e.g., China's dibao program with about 75 million beneficiaries) (Golan, Sicular, & Umapathi, 2015). In fact, because the implementation and enforcement of conditions requires substantial infrastructure and administrative capacity, the implementation of UCTs has become more commonplace in very low-income countries, and "labeled" cash transfer programs (LCTs), where guidance for spending the transfer is articulated but not monitored or enforced, have also been introduced (Benhassine, Florencia, Esther, Pascaline, & Victor, 2015).

In this research, we focus on an under-explored consequence of complying with conditions for households-the costs to them when financial penalties are incurred because of failure to comply with conditions. We undertake this analysis in the context of the Kenya Cash Transfer Programme for Orphans and Vulnerable Children (CT-OVC), a LCT that was noteworthy in its random assignment of health and schooling conditions *with penalties* (CCTs) to a subset of locations in the treatment group. We exploit the random assignment to the conditional treatment arm in the Kenya CT-OVC to explore the implications of penalty fines on household outcomes, given that the "labeling" of the cash transfers resulted in households in both treatment arms having similar beliefs regarding program rules and expectations. Our research, which shows how conditioning with penalties can unintentionally harm those most in need of assistance, has clear policy implications for the design and evolution of cash transfer programs and our understanding of how households respond to income shocks.

Although there is a very large literature on CCTs and UCTs, we identified only one prior study that compared a CCT version of a cash transfer program with an LCT, an evaluation by Benhassine

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et al. (2015) of the Tayssir cash transfer program in rural Morocco. The Tayssir program is distinct from the Kenya CT-OVC, in that it was a pilot program focused on school-aged children (6–15 years), with receipt of the cash transfers tied to a specific education goal (reductions in school absences), and it had a lower transfer amount as a fraction of baseline average household consumption than the CT-OVC (5% versus 23%).

Our analysis of the Kenya CT-OVC program produces four main findings. First, over a third of households in the conditional treatment arm were ever subjected to penalty fines, and the likelihood of being penalized was greatest for households with the lowest consumption at baseline. Second, we find that despite the high frequency of penalization, perceptions about the rules and requirements for receiving the transfer differed very little between treatment arms. Third, our results indicate that the conditioned-upon outcomes did not differ significantly between CCT and LCT treatment arms at follow-up. More specifically, although limits to our statistical power do not allow us to completely rule out the potential for meaningful differences in conditioned-upon outcomes, such as fewer days missed from school, we did not detect any statistically significant effects of assignment to the CCT arm (versus the LCT arm). Finally, we find that assignment to the CCT arm (versus the LCT arm) resulted in large decreases in non-food consumption at follow-up among households in the bottom quartile of baseline consumption, presumably as a result of the penalty fines. These findings affirm the conventional wisdom that penalties in cash transfer programs disproportionately harm those who are least able to respond to them.

In the following Section (2), we review the literature on conditional, unconditional and labeled cash transfer programs (including the Tayssir program), focusing on the types of conditions or guidance embodied in the programs, how they were implemented, and evidence on the relationship of conditions to program outcomes. In Section 3, we present background information on the Kenya CT-OVC program and the nature of the conditions, penalities and labeling of the cash transfers. We also describe the design of the experimental evaluation, data collected and measures, and the checks we perform for covariate balance and attrition. We then present our approach to the empirical analysis and the findings of our main analyses comparing the CCT and LCT treatment arms in section 4. We conclude with a discussion of the results in Section 5.

2. Literature review

One global estimate of the number of beneficiaries of cash transfer programs (Fiszbein, Kanbur, & Yemtsov, 2014) suggests that close to one billion people worldwide are receiving cash transfers as a form of social protection (i.e., social assistance for poor households). The implementation of many cash transfer programs has also been accompanied by rigorous evaluation efforts to identify their impacts, which has contributed to a growing evidence base on a wide range of potential program effects in education, health, labor, consumption, food security, asset building, risky behaviors and more (see:https://transfer.cpc.unc.edu/; Hidrobo, Hoddinott, Kumar, & Oliver, 2018; Ralston, Andrews, & Hsiao, 2017). In fact, after observing the positive findings of cash transfer programs on communities and households, some governments in poor countries are now implementing them as regular components of their economic development and social protection efforts (Bastagli et al., 2016).

As cash transfer programs have expanded to all regions of the world, variation in their implementation has spread as well, with tinkering typically around the designation and administration of conditions or rules of cash transfer receipt. Among the most common conditions are school enrollment and minimum attendance requirements for the child beneficiaries; regular health and wellness checks and immunizations for infants and young children, and health and nutrition training and information sessions for parents or caregivers of the beneficiaries. For example, two of the earliest and largest CCT programs, Mexico's Prospera program (previously named PROGRESA and Oportunidades), and Brazil's Bolsa Familia program, require households to enroll their children in school and the children to maintain 85 percent attendance rates, ensure that they get preventative healthcare (check-ups) and vaccinations, and participate in educational activities offered by health teams or attend monthly meetings to access health and education information, to receive the transfer (Fiszbein, Schady, Ferreira, Grosh, & Kelleher, 2009; Levy, 2006). While the marked success of these two CCT programs-including permanent increases in food consumption, reductions in chronic malnutrition, and increased school enrollment rates-galvanized the replication of this CCT model throughout Latin America and beyond (Fernald, Gertler, & Neufeld, 2008; Handa, Natali, Seidenfeld, Tembo, & Davis, 2018), the transmission of the conditionalities to other contexts has hit constraints.

The implementation and enforcement of conditions requires substantial infrastructure and administrative capacity. In Brazil, for example, local education departments are responsible for checking and reporting the school attendance rates of beneficiaries every two months through the (computerized) School Attendance Surveillance System, and principals are required to report the reasons for absences and take appropriate actions when the student attendance report is returned to the school. A separate computer system managed by the Ministry of Health, Sistema de Vigilância Alimentar e Nutricional is used by municipalities for reporting compliance with the health conditions, and municipalities are also required to verify access to quality health services for program beneficiaries. Furthermore, the direct costs of complying with conditions can be burdensome for beneficiaries and may also open the door for corruption in situations where those verifying conditions charge fees or demand payments for certifying compliance (de Brauw & Hoddinott, 2011; Heinrich & Brill, 2015).

2.1. Why condition?

Numerous works have articulated the arguments for and against the imposition of conditions (Baird, McIntosh, & Ozler, 2011; de Brauw & Hoddinott, 2011; Ferreira, 2008; Fiszbein et al., 2009), which we briefly review here. As Fiszbein et al. and Baird et al. point out, in ideal circumstances—where individuals are well-informed and make rational choices, governments are benevolent and operate efficiently, and markets function perfectly—unconditional cash transfers should be the preferred policy design from both public and private perspectives. However, if we are concerned that individuals lack information to make the most appropriate decisions for use of the transfers, the government can play a role in helping them to overcome these informational problems, e.g., conditioning receipt on uses that are believed to increase their net positive impacts. In other words, the conditions can induce a substitution effect (in spending) that enhances the overall effect of the cash transfers. Another set of arguments pertains to the political feasibility (or political benefits) of offering cash transfers, where public spending on the programs may be viewed as more palatable or popular if the cash transfers are conditioned on "good behavior" or if they are delivered as part of a "social contract" with the state that defines "co-responsibilities" (Fiszbein et al., 2009; Lindert, Anja, Jason, & de la Brière, 2007). In addition, de Brauw and Hoddinott (2011) note that if the conditions serve as a mechanism for increasing the effectiveness of the transfers and politicians and policy makers can take credit for the results, the conditions may be a useful tool for helping them to stay in office as well. Lastly, a third prevailing argument in support of CCTs is that the investments in human capital encouraged through conditioning generate positive externalities for the public, such as the benefits associated with immunization, which caregivers would not fully consider in their own decision making (contributing to underinvestments from a societal perspective).

These potential benefits have to be weighed, however, against the (public and private) costs of administering and complying with the conditions (Baird et al., 2011). There is very limited information available on the costs associated with implementing and monitoring compliance with conditions, largely because it is difficult to distinguish these costs from other administrative costs or to identify those that are imposed on health, education sector and other social welfare staff involved in delivering services. In a study comparing program costs across three Latin American CCTs, Caldes, Coady, and Maluccio (2006) estimated the costs of conditions-distributing, collecting, and processing registration, attendance, and performance forms to schools and healthcare providers (distinguishing them from overall program monitoring and evaluation costs)-and found that the conditions constituted nearly one quarter of the administrative costs in PROGRESA (in 2000). It is also challenging to fully account for the costs of meeting conditions that are imposed on the program beneficiaries—such as transportation and other transaction costs associated with accessing required services—and to assess who bears those burdens in the household. Of course, there are also direct costs to households of any fines or penalties imposed if they are found not to be in compliance. The research base generally finds that CCTs increase total household consumption and disproportionately affect food consumption in poor households, and that increases in food expenditures are typically directed at increasing quality (e.g., items rich in protein and fruits and vegetables) (Fiszbein et al., 2009; Hoddinott, Skoufias, & Washburn, 2000; Macours et al., 2008; Maluccio & Flores, 2005). If the households who find it most challenging to satisfy the conditions are among the poorest of program eligibles, this could unduly penalize household consumption among those most in need (de Brauw & Hoddinott, 2011; Heinrich & Brill, 2015; Rodríguez-Castelán, 2017). Indeed, research summarized by Handa, Seidenfeld, Davis, and Tembo (2016) suggests that UCTs (including Kenya's CT-OVC program) likewise have strong, positive effects on household consumption, and hence, any penalties associated with noncompliance in CCTs may be unjustifiably punitive.

2.2. Nature, role and effects of conditions in cash transfer programs

In the growing evidence base on CCTs, UCTs, and their program variants, researchers have sought to characterize the nature and role of conditions in implementation and to understand how they relate to program effectiveness (Morais de Sá e Silva, 2017). In their 2013 meta-analytic review of 35 studies of cash transfer programs focused on CCTs with at least one condition tied to schooling, Baird et al. conceded that the binary classification of CCTs vs. UCTs disregarded considerable variation in the nature and intensity of the conditions. In their analysis, they further categorized the cash transfer programs as having: (i) no schooling conditions, (ii) some schooling conditions with no enforcement or monitoring, and (iii) explicit schooling conditions that were monitored and enforced; within each of these categories, they attempted to capture variation in nature and intensity of the conditions. For example, Baird et al. describe both Bolsa Familia and PROGRESA as having "explicit conditions," but with imperfect monitoring and minimal enforcement. Other research similarly suggests that the distinction between the second and third categories may not always be precise; that is, there may be more of a gradation from monitoring and enforcement to no monitoring and enforcement in many programs, where the degree of "softness" is realized in implementation of the cash transfer programs (Fiszbein et al., 2009; Hidrobo et al., 2018; Ralston et al., 2017). Silva (2007), for instance, describes the Bolsa Familia conditions as a "soft type of conditionalities," where the sanctions imposed for not complying with conditions are moderate and implemented at different levels, ranging from a simple warning to temporary suspension of payments or definitive removal (following a progression of non-compliance), and take into consideration the reasons for non-compliance.

The more flexible approach to the implementation of conditions in Bolsa Familia reflects concerns that some families with a greater likelihood of non-compliance may be more economically vulnerable (and harmed by a financial penalty), and that weaknesses in infrastructure, such as resources and staff for meeting demand for education and health services (as well as in the administrative and financial capacities for managing the program), may limit the support families receive in attempting to meet the conditions. Prospera (in Mexico) likewise applies a multi-stage approach to fines or sanctions, with suspension of payments as a first step, indefinite suspension with the option of re-admittance as a second step, followed by permanent suspension. Other programs also allow exceptions or exemptions to the conditions and sanctions they impose, such as forgiving absences on grounds of illness, or in the case of Jamaica, granting waivers from attendance requirements for disabled children (Fiszbein et al., 2009; Mont, 2006). In contrast, the Chile Solidario program does not begin paying cash transfers until families have complied with the first criterion, and noncompliance results in an immediate termination of the transfers (Palma & Urzúa, 2005).

Somewhat distinct from cash transfer programs with a continuum of hard to soft conditions is the LCT, where the cash transfer is distributed to households with a "nudge" or "label" indicating its intended use, in contrast to a monetary carrot or stick to ensure compliance with specified uses (Benhassine et al., 2015). For example, if an LCT is to be spent exclusively on more nutritious food, program administrators would convey this through "loose guidance" to recipients when the cash transfer is received. Like Baird et al.'s first category (conditions with no enforcement or monitoring), no monitoring takes place to determine whether the recipients are following the guidance on how the money is to be spent. Benhassine et al.'s (2015) evaluation of the Tayssir (pilot) cash transfer program in rural Morocco compared a CCT version of the program with an LCT arm that portrayed the cash tranfers as an educational intervention. Monitoring of the enrollment of children ages 6–15 years was conducted at schools by headmasters, with receipt of the cash transfers tied to reductions in school absences, albeit without formal requirements for attendance or enrollment. Both the CCT and LCT had two variants: in one, the cash was transferred to the father, and in the other, the cash transfer went to the mother. More than 320 school sectors (with at least two communities in each) were randomly assigned to either a control group or one of these four program variants.

Benhassine et al.'s (2015) analysis of over 44,000 children in more than 4,000 households found significant impacts of the Tayssir cash transfers on school participation for each program variant they tested, and that these impacts did not differ significantly between the CCT and LCT. Interestingly, they also saw little difference between the LCT and CCT in how the program's intended uses were perceived, and parents' beliefs about the returns to education increased in both the LCT and CCT treatment arms. Benhassine et al. (2015) suggested that this is consistent with parents interpreting the intervention as a pro-education government program, regardless of whether they formally required regular school participation (through conditioning). They also found that dropouts related to the "child not wanting to attend school" and to "poor school quality" declined significantly in the LCT and CCT.

Similarly, Baird et al. (2013) found in their analysis-including 26 CCTs, five UCTs, and four studies that compared CCTs to UCTs-that both CCTs and UCTs significantly increased school enrollment, with the odds of a child being enrolled in school 41 percent higher in the CCTs and 23 percent higher in the UCTs (compared to no cash transfers). These differences in effects between the CCTs and UCTs were not statistically significant. However, they also compared cash transfer program effects across the three categories that included the middle design alternative (some schooling conditions with no enforcement or monitoring). When distinguishing between whether or not the schooling conditions were monitored and enforced, they did find that programs where the conditions were monitored and enforced had significantly higher odds of increasing children's enrollment than those with no conditions. At the same time, their own randomized controlled trial comparing conditional vs. unconditional cash transfers in Malawi (Baird et al., 2011) found that the largest effects of cash transfers on teenage pregnancy and marriage rates were among adolescent girls who had dropped out of school but continued to receive *un*conditional cash transfers; there were no statistically significant effects in the CCT arm of the experiment on teenage fertility or marriage. More generally, the implementation of program conditions (i.e., intensity of conditions) was the only measured design feature of the 35 cash transfer programs that significantly moderated the overall effect sizes of the programs.

We expand on this research in our analysis of the Kenya CT-OVC program, in which cash transfers were explicitly earmarked or "labeled" for spending on education and healthcare for orphans and vulnerable children in the household, but conditions with monitoring and penalties for noncompliance were assigned randomly to some districts and a sub-location within the treatment group (Hurrell, Ward, & Merttens, 2008). While as noted above, there are many studies in the literature assessing outcomes of CCTs and a few comparing CCTs and UCTs, the Benhassine et al. study is the only other we are aware of that employed a random assignment design to compare the outcomes between an LCT and CCT program.[1] In addition, the Benhassine et al. study focused on rural areas and school-aged children, with program conditions based only on school absences, whereas the Kenya CT-OVC program covered infants and preschool-aged children as well and included more geographic variation and a wider set of program expectations or conditions (i.e., program rules). Like Benhassine et al., we use detailed information on cash transfer recipients' understanding of the program rules, guidance, and consequences of failure to comply with conditions to understand the extent to which the imposition of conditions with penalties (vs. labeling *only* of cash transfers) influenced household responses and program outcomes. Based on existing research evidence (discussed above), we expect the costs of the CCT monetary penalties to be felt most immediately in terms of household consumption. Thus, our comparison of the CCT and LCT treatment arms focuses on households' total, food and non-food consumption, as well as the health and education outcomes conditioned upon by the program.

3. Program background, study design, data and measures

The CT-OVC program is the Kenyan government's primary intervention for social protection. The program provides a flat transfer equal to approximately 20 USD per month (in 2007 dollars, exchange rate: US$1: KSh 75) that is paid bi-monthly to the caregiver for the care and support of orphans and vulnerable children (OVCs) in the household (Handa, Halpern, Pettifor, & Thirumurthy, 2014). In terms of the average (per adult equivalent) consumption levels at baseline (2007), the monthly cash transfers represent about 23 percent of average monthly consumption. The CT-OVC began as a pilot program in 2004, and following a three-year demonstration period, the government formally approved its integration into the national budget and began rapidly expanding the program in 2007. By the end of the impact evaluation in 2011, the CT-OVC program was providing cash transfers to more than 130,000 households and 250,000 OVCs, with the aim to scale up coverage to 300,000 households (900,000 OVCs). As of fiscal year 2015–2016, approximately 246,000 households and nearly half a million children were benefitting from the cash transfer.[2]

We use data from an experimental evaluation of the Kenya CT-OVC program, mandated by the Government of Kenya, Department of Children's Services (in the Ministry of Gender, Children and Social Development), and undertaken by Oxford Policy Management with financial assistance from UNICEF. The baseline quantitative survey was conducted between March and August 2007 using questionnaires in Swahili, Luo and Somali, and follow-up surveys were administered in 2009 and 2011. The surveys collected information on household consumption expenditures, education and employment of adults, assets owned, housing conditions and other socio-economic characteristics, as well as information on child welfare measures such as anthropometric status, immunizations, illness, health-care seeking behaviour, school enrollment and attendance, child work and birth registration. As many of the outcome indicators of interest for the children are only available in the 2007 and 2009 data collections, we restrict our analysis to these two years. A total of 2,759 households were included in the 2007 baseline sample, and of these, 2,255 were interviewed at follow-up in 2009. As Handa et al. (2014) explain, the 17 percent attrition between baseline and the first follow-up was concentrated in Kisumu and Nairobi, where the turmoil of the disputed national elections in December 2007 caused the most unrest.

The evaluation of the Kenya CT-OVC was designed as a clustered randomized controlled trial (RCT) and took place in seven districts in the country (see Fig. 1 that illustrates the design).[3] Within each of the seven districts, two sub-locations out of four were randomly assigned to be treatment locations and two were randomly assigned to the control state (no cash transfer distribution). Households in treatment locations were eligible to receive cash transfers if at least one OVC resided in them, they met the designated poverty criteria, and the OVC(s) were not benefitting from any other cash transfer program. In treatment locations, a list was compiled containing the households eligible to receive the cash transfer, and households on the list were reportedly prioritized for treatment by several "vulnerability" criteria (Hurrell et al., 2008). These included the age of the caretakers of the OVCs, and the number of OVCs and chronically ill living in the household (in that order). Thus, within treatment locations, there was an intent to prioritize more "vulnerable" households for cash transfer receipt. We include these three prioritization criteria in all regressions to account for this selection. However, it is important to note that since our study focuses on comparing the two treatment arms to *one another*, this prioritization of vulnerable households into the treatment group has no effect on our main results.

[1] In some research publications on the Kenya CT-OVC, the program is described as a UCT or "social cash transfer" program, while at the same time acknowledging that it involves "social messaging" (Asfaw, Davis, Dewbre, Handa, & Winters, 2014, p. 1175).

[2] See the Kenyan government website:https://www.socialprotection.or.ke/social-protection-components/social-assistance/national-safety-net-program/cash-transfer-for-orphans-and-vulnerable-children-ct-ovc.

[3] During the time of the CT-OVC evaluation and prior to the new constitution in Kenya that became effective in 2013, Kenya was divided into eight provinces, which were further subdivided into 46 districts (excluding Nairobi) and are today recognized as semi-autonomous counties.

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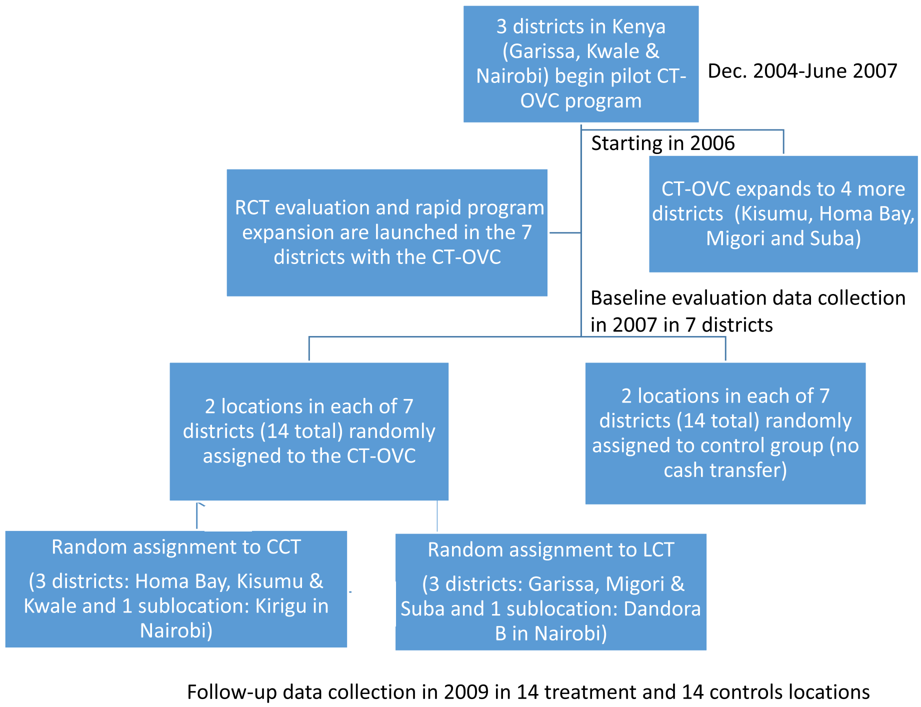

Fig. 1. RCT Design.

In every treatment location, beneficiary households were expected to comply with program guidance or expectations for how the cash transfers would be used. These included visits to health facilities for immunizations, growth monitoring and nutrition supplements, school enrollment and attendence, and caregiver "awareness" session (see Appendix A, Table A.1), although attendance requirements were waived for children deemed to be without access to schools or clinics (Government of Kenya, 2006). In half of these locations—all treatment locations in Homa Bay, Kisumu and Kwale districts and one sub-location in Nairobi (Kirigu)—households were randomly assigned to the CCT treatment arm, where the expected penalty for not following the program conditions was a deduction of KSh 500 from the transfer amount per infraction, and multiple infractions could result in ejection from the program. Treatment locations in the other districts and one sub-location—Garissa, Migori, Suba and the other Nairobi location (Dandora B)—were assigned to the labeling only (LCT) arm where non-compliance was not supposed to be penalized.

Centrally, the CT-OVC program was coordinated through the Department of Children's Services in the Ministry of Gender, Children and Social Development (MGCSD), but its implementation and monitoring was managed locally through District Children's Offices (DCO). The DCO, in turn, collaborated with committees of voluntary members, typically composed of community leaders. These "Beneficiary Welfare Committees (BWCs)" were charged with the responsibilities of general program operations, including promoting awareness, monitoring and supporting implementation, and addressing grievances. As a labeled cash transfer program, it was intended that all beneficiaries would be made aware of the expectations that the cash transfers should be spent on visits to health facilities and expenses associated with children's enrollment and attendance in school. In fact, the final operational and impact evaluation report (Ward et al., 2010) indicated that 84 percent of cash transfer recipients believed that they had to follow some sort of rules to continue receiving the cash transfers, although the report also noted that most beneficiaries were not aware of the full set of conditions with which they were expected to comply.

Qualitative research on the program's implementation revealed that largely because of the decentralized nature of administration and reliance on volunteers for its execution, monitoring of the conditions (and enforcement of the penalties in the CCT arm) lacked structure and was uneven across and within locations (FAO, 2014). In addition, monitoring and enforcement were hindered by onerous forms and logistical challenges. The community representatives responsible for communicating and checking on conditions were often informally appointed, and implementation of that role was highly dependent on a given community representative's knowledge, interpretation of their obligations, and activism. Two years after baseline, many beneficiaries in the CCT arm had not been reached with communications about the penalities (Ward et al., 2010; FAO, 2014). The literature on CCTs suggests that these types of challenges in implementing conditions are relatively common, and that they can delay actions to sanction noncompliance, which can weaken the "positive quid pro quo" effects of the conditions on program outcomes (Fiszbein et al., 2009).

3.1. Measures of treatment implementation

Following the baseline data collection and implementation of the cash transfer program, household surveys were conducted in 2009 to assess the receipt of cash transfers and how households used them. For all households that received the transfer, household members were asked about their perceptions of any conditions or obligations they faced in receiving the cash transfers and about any consequences they faced for noncompliance, as well as how they used the cash transfers. In addition, the household members were asked if they "have to follow any rules in order to continue receiving the program," and they were prompted to list the rules that they thought they had to follow "in order to receive the full payment from the OVC program." Furthermore, household members were asked if they knew which members of the household the

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rules applied to, if they knew what would happen if they did not follow the rules, and if they believed that anyone was checking on the conditions.

In regard to identifying the penalties that were applied in association with the conditional treatment arm, the 2009 household survey asked respondents if they had ever gone to the Post Office to collect their payment and "received less than 3000KSh for the payment cycle.[4]" The interviewer was instructed to look at all of the receipts the respondent provided and to identify cash transfer amounts of less than KSh 3000 to determine if a monetary penalty had been applied. Household respondents identified as having been fined were also asked if they knew why the payment was less than the full amount, and if they were aware of an appeal/complaints process they could pursue if they received less than 3000 KSh in a payment cycle. Appendix A, Tables A.2 and A.3 shows the survey questions that were used in constructing measures of program perceptions and implementation.

Because the implementation of conditions in the CCT arm was intended to impose concrete expectations for how households would spend the cash transfers and penalties for violations thereof, we hypothesize that households in districts and sub-locations randomly assigned to the conditional arm might differ in their perceptions, responses to, and uses of the cash transfer from those randomly assigned to the labeling only arm. Furthermore, because it is well-documented that taking a "hard line" on compliance with CCT conditions is likely to impose higher costs on the poorest and most vulnerable among those targeted for cash transfers—who, because of their greater need, also have less budgetary capacity to absorb the monetary loss—we expect there may be differential consequences of being penalized or fined for noncompliance by household baseline wealth.

3.2. Outcome measures

We evaluate the difference between CCT and LCT arms in the Kenya CT-OVC program on the following dimensions of household and child wellbeing: consumption (food and non-food), health, i.e., vaccinations (total doses and sequences completed) and receipt of vitamin A supplements, and schooling (enrollment and absences from school). Most of these outcomes are linked with the program conditions shown in Appendix A Table A.1, which are intended to promote children's nutrition, growth and immunizations through increased consumption and health facility visits and their enrollment and attendance of school. We include consumption outcomes in our analysis as proxies for overall household well-being and wealth[5]. The sample sizes in our regressions vary by outcome, primarily because the outcomes we focus on are measured for distinct groups receiving the cash transfers: households for consumption, children 0–7 years for health outcomes, and school-aged children (6–17 years) for education outcomes.

We follow The Kenya CT-OVC Evaluation Team, 2012 in adjusting consumption (reported at baseline in 2007) for household adult equivalents; children under age 15 were counted as three-quarters of an adult, and individuals aged 15 and over were counted as one adult. Consumption measured at follow-up (in 2009) was deflated to 2007 Kenya Shillings (KSh), following Ward et al. (2010), with separate price deflators for food and non-food items. These price adjustments were critical, given that the Kenyan post-election violence and world food crisis that occurred between baseline and follow-up each engendered upward pressures on the relative price of food and increased poverty among the beneficiary population as a whole (The Kenya CT-OVC Evaluation Team, 2012). Household expenditures (by broad household item groups) were combined into three main categories for our analysis: total household consumption, food consumption, and non-food consumption. Analyses by the Kenya CT-OVC Evaluation Team showed that none of the nine separate categories of household (food and non-food) expenditures were significantly different at baseline between CT-OVC treatment and control households, in spending levels, shares, or proportion of households reporting positive spending.

Children in the Kenya CT-OVC program (LCT and CCT arms) were expected to visit a health facility every two months and to receive vaccinations, vitamin A supplements and growth monitoring. According to the final operational and impact evaluation report, children 0–7 years were considered fully vaccinated if they had received (at a minimum) the following vaccinations: three DPT (diphtheria, pertussis, and tetanus) doses, three oral polio (OPV) doses, one BCG (bacille Calmette-Guerin, a vaccine for tuberculosis) and one measles (Ward et al., 2010). The household survey inquired about four OPV doses, which is recommended by the World Health Organization, thus, we consider an OPV sequence complete if four doses were received. The outcome measures we constructed to assess the impact of receiving a LCT or CCT on children's vaccinations included the total number of doses received (of all vaccinations recommended) and the number of vaccine sequences completed. For vitamin A supplements, the household survey recorded whether the child had received the supplement from a health worker within the last 6 months.

The third primary outcome we investigate, school attendance, was one element of the Kenya CT-OVC program's explicit goal to increase schooling (enrollment, attendance and retention) of children aged 6–17 years. At baseline (2007), about 95 percent of children aged 6–17 years in both treated and control households were enrolled in school, and the final impact evaluation report (Ward et al., 2010) did not find statistically significant impacts of the cash transfers on enrollment or attendance of *basic* schooling (although it did report statistically significant increases of 6–7 percentage points in enrollment in *secondary* schooling). The baseline (2007) data also show that children in our sample missed an average of 1.5 days of school in last month, and 10 percent of these children missed over five days in one month. We therefore focus our analysis on school attendance, which we measure as days missed from school during the school year (in 2007 and 2009). The education literature has also increasingly looked to attendance as a more informative measure of children's progress in schooling. Attendance rates have been linked to the development of important sociobehavioral skills such as motivation and self-discipline (Gershenson, 2016; Heckman, Stixrud, & Urzua, 2006) and to improved cognitive development (Gottfried, 2009), as well as to retention rates and increased educational attainment (Gershenson, Jacknowitz, & Brannegan, 2017; Nield & Balfanz, 2006; Rumberger & Thomas, 2000). In addition, existing research finds that the harm of absences, in terms of reduced academic achievement, is greater among low-income students (Gershenson et al., 2017; Gottfried, 2009), and that non-school factors, such as poverty, family emergencies and work obligations, are the primary determinants of attendance rates (Balfanz & Byrnes, 2012). If being fined reduces resources for poor families that enable them to overcome these non-school barriers to school attendance, we would expect assignment to the CCT arm to diminish the cash transfer program's impact on reducing student absences compared to the LCT arm.

[4] Payment cycles were two months in length. Since households were to receive 1500 KSh per month (if no fines had been applied), this translates to a transfer of 3000 Ksh each cycle.

[5] Deaton and Zaidi (2002) consider consumption data to be the "gold standard" for proxying wealth for several reasons. First, since consumption is presumed to be smoothed for households over periods of time, it provides a more accurate measure of wealth than income in short reference periods. Second, levels of income are often more difficult to assess in developing countries due to self- and informal sector employment.

3.3. Balance checks and attrition

To estimate the unbiased difference between the CCT and LCT treatment arms, we make the identifying assumption that assignment to either arm was independent of potential outcomes. To phrase this another way, we are assuming that randomization produced two statistically equivalent groups at the onset of the experiment. We verify that randomization was successfully implemented through a series of balance tests below. Furthermore, we also check for differential attrition by treatment status to verify that our results are not driven by changes in sample composition.

One methodological challenge to evaluating these data is the small number of randomization clusters in the experimental design. As described in Fig. 1, the districts Homa Bay, Kisumu, and Kwale and the sub-location Kirigu (in Nairobi district) were randomly assigned to administer a CCT to their transfer households. The remaining districts (Garissa, Migori, and Suba) and transfer sub-location in Nairobi (Dandora B) were assigned to the LCT. This produces eight randomization clusters in total. The traditional formula for consistently estimating clustered standard errors relies on the assumption that the number of clusters is sufficiently large to approximate asymptotic results, the minimum for which is 30–50 clusters. However, multiple methods now exist to produce consistent clustered standard errors when clusters are fewer than 30. The first is the wild cluster bootstrap, which, in Cameron, Gelbach, and Miller (2008), is shown to produce standard errors that are robust when the number of clusters is as few as six (as long as Webb weights are used). The second method is randomization inference. The main advantage to randomization inference in our context is that it allows us to conduct valid hypothesis tests even in the presence of small sample sizes, regardless of error structure (Young, 2019). Another advantage is that randomization inference acts as its own "placebo test". As the method consists of correlating "placebo" treatments with outcome values from the actual experiment, it verifies that treatment effects do not exist when they, in fact, should not (i.e., experimental outcomes are uncorrelated with re-randomized treatments). We report p-values produced by both of these methods in our analyses.

3.3.1. CCT versus LCT balance and attrition

In this section, we test our identifying assumption by assessing the comparability of the CCT and LCT treatment arms at baseline.[6] Accordingly, we present in Table 1 the results of our balance tests for the two arms by estimating Eq. (1) on the sample of households assigned to receive the cash transfer. Here, x_{ijk} refers to a baseline characteristic of household *i*. CCT_{jk} is a binary variable indicating if district *k* or sub-location *j* was assigned to the CCT (versus LCT). We include $carerIndex_{ijk}$, $totalOVC_{ijk}$, and $totalChronicallyIll_{ijk}$ to adjust for the transfer prioritization criteria. We also test if assignment to either treatment arm is predicted jointly by a vector of baseline characteristics, X_{ijk}, by conducting an F-test for joint orthogonality using the wild cluster bootstrap after estimating Eq. (2), where X_{ijk} contains all variables in Table 1 except for those with multicolinearity issues.[7] When running the joint test, we replace missing observations of the regressors in X_{ijk} with the variables' sample means. Additionally, we include a dummy for each regressor in X_{ijk} that equals 1 when the observation is missing and 0 otherwise. These dummies are specified as D_{ijk} in Eq. (2). The results in Table 1 do not indicate any statistically significant differences between households in the LCT and CCT arms across all t-test and the F-test, implying that the randomization within the transfer (treated) group was successfully executed.

$$x_{ijk} = \alpha + \delta CCT_{jk} + \gamma_1 carerIndex_{ijk} + \gamma_2 totalOVC_{ijk} + \gamma_3 totalChronicallyIll_{ijk} + e_{ijk} \quad (1)$$

$$CCT_{jk} = \alpha + X'_{ijk}\beta_1 + D'_{ijk}\beta_2 + \gamma_1 carerIndex_{ijk} + \gamma_2 totalOVC_{ijk} + \gamma_3 totalChronicallyIll_{ijk} + e_{ijk} \quad (2)$$

The existing evidence base suggests that imposing conditions on cash transfer receipt, accompanied by fines, could potentially change household responses to and use of the cash transfers. The literature also suggests that we should pay special attention to the heterogeneous effects of cash transfer receipt by baseline levels of household wealth (proxied by per adult-equivalent consumption in our study) (de Brauw & Hoddinott, 2011; Rodríguez-Castelán, 2017). Since we are interested in whether differences in outcomes between treatment arms vary by baseline household consumption, we must also show that the treatment arms are balanced on baseline characteristics across the consumption distribution. We do this by grouping households into bins by quintile of baseline consumption, estimating Eq. (1) separately within each bin, and conducting inference using both the wild cluster bootstrap and randomization inference. The full results from this analysis are available upon request. Testing the 26 baseline characteristics from Table 1 within each of 5 quintile groups results in 26 × 5 = 130 estimated differences and 130 × 2 = 260 seperate hypothesis tests. Of these 260 hypothesis tests, only 5 result in p-values less than 0.05[8]. Since this number of significant differences is no greater than what one would expect from chance, this provides more evidence that the treatment arms are also balanced at baseline across the consumption distribution.

Attrition would also be a concern in estimating the effects of the CCT versus LCT treatment arms of the program if the likelihood of attritting varied by CCT vs. LCT status. This would imply that the estimated parameter of interest would represent not only the effect of the treatment, but also differences in sample composition induced by treatment arm assignment. In our sample, attrition within the transfer group is about 19 percent. In Table B.2 Panel B, we report the results of a test to determine if households assigned to the CCT arm within the transfer group experienced a differential rate of attrition compared to the LCT arm. Differential attrition is low between the two treatment arms, at only slightly more than 3 percent. This difference is statistically insignificant according to both the wild cluster bootstrap and randomization inference. We also split the sample into bins by baseline consumption quintile, as we do when checking for balance, and test for differential attrition within each bin. None of the coefficients are significant at the 5% level for any of the bins, which leads us to conclude that attrition should not distort our comparison of outcomes between the CCT and LCT groups.

4. Results: CCT versus LCT Implementation and Impacts

We are primarily interested in how assignment to the conditional arm (versus labeling only) of the Kenya CT-OVC program affected household and children's outcomes, as well as how the effects varied based on the households' baseline wealth. In Appendix B, we present an analysis of how assignment to receive cash transfers in the CT-OVC program affected outcomes as a whole,

[6] The household characteristics we test are based on the balance test in Annex F of Ward et al. (2010).

[7] We exclude HH Owns Livestock, HH Food Consumption, and People Aged 0–5 in HH from X_{ijk} to avoid perfect multicolinearity.

[8] The wild cluster bootstrap produces two p-values less than 0.05: in the 40th to 60th percentile bin for "Years of Edu. of HH Head" and in the 80th to 100th percentile bin for "HH Receives Outside Transfer". Randomization inference produces three p-values less than 0.05: in the 80th to 100th percentile bins for "Poor Quality Floors" and "Rural", and in the 40th to 60th percentile bin for "HH Food Consumption".

Table 1
Balance Table: LCT vs CCT.

	(1) LCT	(2) CCT	(3) Bootstrap P-Value	(4) RI P-Value
Years of Edu. of HH Head	5.788	6.034	0.235	0.249
Sex of HH Head	0.355	0.348	0.877	1.000
HH Receives Labor Wages	0.038	0.029	0.855	0.901
HH Receives Outside Transfer	0.355	0.217	0.294	0.325
Poor Quality Walls	0.683	0.781	0.655	0.843
Poor Quality Floor	0.724	0.737	0.949	0.849
HH Owns Livestock	0.831	0.758	0.389	0.240
Cattle Owned	1.186	1.432	0.428	0.406
Poultry Owned	3.899	5.123	0.251	0.427
Owns Telephone	0.105	0.106	0.612	0.833
Owns Blanket	0.823	0.865	0.900	0.822
Owns Mosquito Net	0.647	0.551	0.184	0.293
Acres of Land Owned	1.387	1.866	0.373	0.383
Household in Rural Location	0.885	0.766	0.311	0.306
HH Total Consumption	1.668	1.521	0.485	0.568
HH Food Consumption	0.972	0.926	0.611	0.659
HH Non-food Consumption	0.695	0.595	0.478	0.350
Dietary Diversity Score	4.972	5.29	0.437	0.617
Size of the HH	5.409	5.489	0.689	0.738
Age of HH Head	56.498	60.046	0.860	0.843
People Aged 0–5 in HH	0.649	0.702	0.549	0.465
People Aged 6–11 in HH	1.261	1.180	0.875	0.955
People Aged 12–17 in HH	1.325	1.418	0.161	0.231
People Aged 18–45 in HH	1.136	1.120	0.709	0.792
People Aged 46–64 in HH	0.665	0.636	0.740	0.803
People Aged 65 + in HH	0.373	0.433	0.428	0.507
N: t-tests	609	483		
N: F-tests	1092		0.644	

Note: The p-values in column (3) are calculated with the wild cluster bootstrap procedure, and the p-values in column (4) are calculated with randomization inference. Clustering is done at the district level. All estimates are calculated conditioning on the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. The F-test for Joint Orthogonality regresses CCT assignment (versus LCT) on a vector containing baseline characteristics (excluding HH Owns Livestock, HH Food Consumption, and People Aged 0–5 in HH to avoid perfect multicolinearity) and the transfer prioritization criteria. The following variables have fewer observations than reported beside "N: t-tests" due to missing responses: Years of Edu. of HH Head, Cattle Owned, Poultry Owned, and Age of HH Head.

comparing outcomes of households in sub-locations randomized to receive the transfer (CCT and LCT arms pooled together) to the outcomes of households in sub-locations randomized to the control group. Consistent with the findings of The Kenya CT-OVC Evaluation Team (2012), we found that cash transfer receipt increased both food and non-food consumption in households, although the only conditioned-upon outcome that was affected by the CT-OVC program was school attendance conditional on enrollment. We keep these results in mind as we compare program outcomes across the CCT and LCT treatment arms by estimating Eq. (3). y_{lijk} represents the outcome of interest and which may vary at the level of the child l or the household i. X'_{1ijk} is a vector of household characteristics at baseline, which include: the gender of the household head, whether someone in the household earns wages from an outside job, total consumption, an indicator for owning livestock, acres of agricultural land owned, an indicator for being in a rural location, the number of households members, and the baseline level of the outcome (if the outcome varies at the household level). X'_{2lijk} is a vector that contains child-level controls, consisting of the child's age, gender, OVC status, and the baseline level of individual-varying outcomes. The vectors of controls also include a dummy that equals 1 if the baseline value of the outcome is missing and 0 otherwise.[9] This dummy could vary at the household- or individual-level, depending on the outcome.

[9] In order to retain observations with missing baseline values of the outcome, we employ a method described in McKenzie (2012). This method entails coding the missing values of baseline outcome variables as 0, and adding the dummy described in the text to the specification.

$$\begin{aligned} y_{lijk} = \; & \alpha + \delta CCT_{jk} + \gamma_1 carerIndex_{ijk} + \gamma_2 totalOVC_{ijk} \\ & + \gamma_3 totalChronicallyIll_{ijk} + X'_{1ijk}\beta_1 + X'_{2lijk}\beta_2 + e_{lijk} \end{aligned} \tag{3}$$

4.1. Enforcement and salience of conditions

We now show that the conditions and penalties were meaningfully implemented on the ground and that households were indeed at risk for being penalized. The estimation sample is the group of households assigned to received the transfer (in either the CCT or LCT arm). The outcome is set as an indicator for whether the households reported ever receiving less than their full transfer amount for at least one payment cycle by the time of the follow-up survey (two years later). We view this as an important test of the first stage that assignment to the CCT group was a meaningful treatment for households. Table 2 contains the results of estimating Eq. (3) with and without controls. Assignment to the CCT group increased the likelihood that a household was ever fined by about 34 percentage points (with and without controls), compared to a control mean of about 0.8 percent. The control mean is not zero because it appears as though a few households in the LCT arm were either fined by mistake, or misreported that they had experienced a transfer deduction. We interpret these results as evidence of a strong first stage, which in our context means that assignment to the CCT substantially increased households' likelihood of ever being fined. The results also provide some assurance that the survey data on fining do not suffer from substantial error or overreporting (particularly for the LCT arm). Lastly, the results imply that CCT households received less money in transfers overall than the LCT households due to the imposition of penalty fines. In Section 4.3.2, we will show that the magnitude of this effect varied by

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Table 2
Impact of Assignment to CCT on Ever Being Fined.

	No Controls			Controls		
	(1) Differential Effect	(2) Bootstrap P-value	(3) RI P-value	(4) Differential Effect	(5) Bootstrap P-value	(6) RI P-value
Assigned to CCT	0.341	0.002	0.000	0.344	0.002	0.000
LCT Mean	0.008			0.008		
N	1090			1090		

Note: The p-values in columns (2) and (5) are calculated with the wild cluster bootstrap procedure, and the p-values in columns (3) and (6) are calculated with randomization inference. Clustering is done at the district level. All estimates are calculated conditioning on the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent.

baseline household wealth and subsequently impacted downstream outcomes.

4.2. Perceptions of conditions and penalties

Within the transfer group, assignment to the CCT arm may have affected outcomes through two primary channels. The first is in how the penalty fines (deductions to transfers) directly reduced household income. The second is in how the potential for penalties (associated with conditions) might have affected household decision-making. Assignment to the CCT arm (versus the LCT arm) was only likely to have affected household decisions if it produced a different understanding of program rules and consequences (potential penalties) between treatment arms. In the Kenya CT-OVC, over one third of households in the CCT arm were fined at some point, which stands in contrast to the Tayssir program, in which CCT households rarely received penalty fines due to high rates of compliance with the program's single condition. Benhassine et al. (2015) also found that understanding of program conditions was somewhat poor among households overall and that the differences in knowledge between LCT and CCT groups were small, which the authors attributed to the infrequency of penalties. Furthermore, the Tayssir program's transfer was a smaller percentage of mean baseline consumption than the Kenya CT-OVC (5 percent versus 23 percent). Together, these factors suggest that a stronger feedback loop or greater incentive for CCT households to internalize the conditions in the Kenya CT-OVC relative to Tayssir may have been present. We can investigate this, and the degree to which perceptions of program rules and consequences differed across treatment arms, using the large battery of survey questions available in the Kenya CT-OVC evaluation of households' perceptions of conditions.

We once again estimate Eq. (3), but set the outcome variable as an indicator that equals 1 if the household purports to understand or perceive that a particular rule or operational detail of the program applied to them. Table 3 contains the results from these regressions. The first observation is that labeling alone leads over 73 percent of households to believe that they needed to comply with rules to receive the transfer. Households assigned to the CCT treatment arm were 13 percentage points more likely to believe this, although the difference is statistically insignificant. Households also did not differ significantly in their beliefs about the specific rules (or conditions) that they perceived they had to follow to continue receiving the cash transfers (enrollment/attendance in school, health facility visits, attendance at program awareness sessions).[10] In the last row of Panel A in Table 3, we show the results for a summary measure or "index of program understanding" that we created by adding together the five dummy variables for the specific rules households were expected to follow. The treatment effect for this index is small in magnitude and statistically insignificant. This suggests that assignment to the CCT arm did not affect the likelihood that households believed they had to follow rules to receive the transfer *and* to know what those specific rules were.

If households in both arms understood the program perfectly, we would have expected a large difference between treatment arms in their beliefs about having to follow these rules. This does not appear to have been the case. In fact, general understanding of the rules appears to have been low across both treatment arms, consistent with the pattern observed by Benhassine et al. (2015) in the Tayssir program, despite the much larger transfer amount and higher risk of being penalized in the Kenya CT-OVC program. Moreover, households in both the LCT and CCT arms of the Kenya CT-OVC program believed that they could be disbarred from the program if they did not follow the rules or guidance (see Panel B of Table 3), which did not apply to the LCT households. Taking our results with those in Benhassine et al. (2015), it appears as though households have a difficult time distinguishing labeling or guidance from conditions with penalties when program rules are explained to them.

At the same time, we do observe a statistically significant difference between treatment arms in household perceptions about program penalties. As shown in Panel B, CCT households were 17 percentage points more likely to believe they would receive a monetary fine on their transfer for each violation of the perceived rules. This finding is of note for two reasons. First, it provides additional (first stage) evidence that the conditions in the CCT arm were implemented successfully. Second, it indicates that "understanding of the penalties" might be the primary margin on which assignment to the CCT arm influences households' perceptions of program rules differentially from labeling (the LCT arm).

It is also possible that if the CCT group were more likely to believe they could penalized than the LCT group, they may have been more likely to act on their perceptions of the rules and penalties. The results of our exploration of this possibility are reported in Panel C of Table 3, which shows that there was no statistically significant difference between treatment arms in households' beliefs that someone was monitoring them. Lastly, Panel D considers what households believed about the rules or criteria for being ejected entirely from the program. Very few households in either treatment arm knew what the particular criteria were for total disbarment, and rates of understanding did not vary by treatment arm. Because the ejection criteria were more complex than the basic program rules, and ejection appears to have been a rare occurrence, this is not a surprising result. Overall, we conclude that households in both the CCT and LCT treatment arms appear to have had similar perceptions about the program rules and consequences, with the one exception being the beliefs of CCT households regarding monetary fines for violations of program rules.

4.3. Program compliance, outcomes, and heterogeneous effects

We have shown above that CCT households were no more likely than LCT households to believe they had to follow rules to receive

[10] If a household answered that they did not have to follow rules to receive the transfer, it was a "logical skip" in the survey that they did not have to answer questions about specific rules. Thus, we coded these households as not believing they needed to follow any of the specific rules.

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Table 3
Impact of Assignment to CCT on Perceptions of Conditions and Penalties.

	(1) LCT Mean	(2) CCT Differential Effect	(3) Bootstrap P-value	(4) RI P-Value	(5) N
Panel A: Understanding of Program Rules					
Believes HH Must Follow Rules to Receive Payments	0.733	0.132	0.324	0.612	1086
Enrollment/Attendance in Primary or Secondary School	0.294	0.004	0.936	0.946	1092
Visit Health Facility for Immunizations	0.154	0.054	0.528	0.463	1092
Visit Health Facility for Growth Monitoring	0.090	0.057	0.134	0.296	1092
Visit Health Facility for Vitamin A Supplement	0.059	−0.003	0.905	0.889	1092
Attendance at Program Awareness Sessions	0.043	0.036	0.201	0.269	1092
Index of Program Understanding	0.640	0.148	0.246	0.428	1092
Panel B: Understanding of Penalty for Violation					
Monetary Fine on Transfer	0.048	0.170	0.009	0.000	1092
Total Disbarment from Program	0.437	−0.007	0.938	0.942	1086
Panel C: Perceived Likelihood of Being Monitored					
Believes Someone is Checking if HHs are Following Rules	0.421	0.085	0.518	0.582	876
Panel D: Understanding of Ejection Criteria					
Claims to Know Specific Criteria for Ejection from Program	0.437	−0.007	0.865	0.946	1086
HH has no OVCs Below 18 Years Old	0.146	−0.025	0.558	0.645	1092
At Least One Program Rule is Ignored for Three Consecutive Pay Periods	0.189	0.098	0.522	0.505	1092
HH Moves to Non-Program District	0.018	−0.014	0.442	0.912	1092
HH Does Not Collect Transfer for Three Consecutive Pay Periods	0.011	0.000	0.999	0.946	1092

Note: The p-values in column (3) are calculated with the wild cluster bootstrap procedure, and the p-values in column (4) are calculated with randomization inference. Clustering is done at the district level. All estimates are calculated conditioning on control variables and the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. Note that if a household did not believe it needed to follow any rules to receive the transfer, it is also coded not to believe it had to follow the specific rules and not to believe it knew the penalty for violations. Similarly, a household that did not claim to know the criteria for ejection from the program is coded not to know the individual ejection criteria either.

the cash transfer or to believe that they were being monitored. These facts, combined with the observation that one third of CCT households were fined at least once, leads us to hypothesize that CCT households should have experienced similar, if not worse, downstream outcomes than LCT households. We further hypothesize that relatively poorer households (proxied by consumption) in the CCT group should have experienced the worst outcomes relative to similar households in the LCT group. This stands in contrast with the findings of Benhassine et al. (2015), in which very few CCT households in the Tayssir program ever had their transfers penalized, and downstream outcomes between the LCT and CCT did not differ significantly. We explore the average effects of assignment to the CCT in Section 4.3.1. Then, in Section 4.3.2, we explore the heterogeneous incidence of being fined across the household consumption distribution and its consequences for outcomes of interest, particularly consumption. Lastly, we draw upon the 2011 wave of the survey to provide some limited evidence on the longer-run differences in outcomes between the CCT and LCT arms in Section 4.3.3.

4.3.1. Average effects of assignment to CCT versus LCT

We now assess whether, on average, random assignment to the CCT versus LCT arm had any impact on the schooling, health, or consumption-related outcomes of interest described above. We do this by estimating equation (3) where the dependent variable is a child-level outcome, and present the results in Table 4.

We do not find any evidence that CCT households experienced different outcomes, on average, than LCT households. These results,

Table 4
Impact of Assignment to CCT versus LCT: Average Effects.

	(1) LCT Mean	(2) Assigned to CCT	(3) Bootstrap P-value	(4) Bootstrap CI	(5) RI P-Value	(6) N
Enrolled in School	0.937	0.000	0.964	[−0.017, 0.023]	0.943	2549
Days Missed from School	1.109	−0.067	0.510	[−0.366, 0.284]	0.761	2242
Total Doses of Vaccinations	7.429	−0.507	0.349	[−1.249, 0.954]	0.456	235
Number of Vacc. Sequences Completed	3.010	−0.235	0.303	[−0.707, 0.466]	0.395	235
Received Vitamin A Supplement Six Months	0.510	0.004	0.971	[−0.182, 0.364]	0.951	561
HH Total Consumption	2.107	−0.055	0.619	[−0.531, 0.227]	0.705	1092
HH Food Consumption	1.275	0.030	0.679	[−0.155, 0.180]	0.751	1093
HH Non-food Consumption	0.835	−0.083	0.175	[−0.355, 0.074]	0.251	1095

Note: The p-values in column (3) and confidence intervals in column (4) are calculated with the wild cluster bootstrap procedure. The p-values in column (5) are calculated with randomization inference. Clustering is done at the district level. All estimates are calculated conditioning on control variables and the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. The sample for both education variables consists only of children that were aged at 2 to 17 at baseline. Only children enrolled in school at baseline and follow-up are included in the sample for Days Missed from School, and the reference period is the past 2 months. Vaccinations include BCG (1 dose sequence), DPT (3 doses), OPV (4 doses), and measles (1 dose). Only children aged seven or under at follow-up are included in the sample for the vaccination variables and the vitamin A supplement, the reference period for the latter is the past 6 months.

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and especially those for education outcomes, are consistent with the findings of Benhassine et al. (2015), despite the fines levied on CCT households in our study. It is important to note that the precision of our results do not completely rule out the presence of economically meaningful effects for some outcomes. For example, the 95% confidence interval on the point estimate for Days Missed from School is [−0.366, 0.284] according to the wild cluster bootstrap, which are 33% and 26% of the control mean, respectively. However, the overall takeaway is that even in the presence of a larger cash transfer amount than the Tayssir program, coupled with a higher probability of being penalized for noncompliance, we still cannot detect statistically significant average effects of assignment to the CCT group (versus the LCT arm) on conditioned-upon outcomes.

4.3.2. Heterogeneous effects of assignment to CCT versus LCT

One possible explanation for these null effects is that while CCT households may have been more motivated to comply with the conditions, the penalty fines created financial constraints that prevented them from doing so. As we suggest in Section 4.1, a common concern about CCTs is that they may be least beneficial for vulnerable households that have trouble complying with conditions. This appeared to have been the case in Baird et al. (2011), in which girls who dropped out of school (thus breaking the conditions) suffered worse marital and fertility outcomes in the CCT group than the UCT group. Furthermore, resource-constrained households that cannot comply with conditions are also likely to be the ones who potentially benefit most from cash transfer programs. Thus, there is reason to believe that a household's likelihood of being fined, and its ability to cope with said fines, may vary according to baseline household We analyze the heterogeneous effects of assignment to the CCT arm versus the LCT arm on likelihood of being fined by estimating Eq. (4) which adds an interaction term between the assignment variable and baseline per capita consumption (our proxy for wealth). The main effect for baseline consumption, $cons_{ijk}$, is contained in the vector X'_{1ijk}, as described above.

$$y_{lijk} = \alpha + \delta_1 CCT_{jk} + \delta_2 CCT_{jk} \times cons_{ijk} + \gamma_1 carerIndex_{ijk} + \gamma_2 totalOVC_{ijk} + \gamma_3 totalChronicallyIll_{ijk} + X'_{1ijk}\beta_1 + X'_{2lijk}\beta_2 + e_{lijk} \quad (4)$$

We plot these results in Fig. 2, which includes randomization inference p-values and wild cluster bootstrapped confidence intervals. According to the linear specification, assignment to the CCT arm increased relatively poorer households' likelihood of ever being fined more than other (wealthier) groups. These results suggest that CCT households' burden of fines on transfer income varied by baseline consumption on the extensive margin. These findings motivate us to re-estimate equation (4) with our downstream outcomes of interest on the left-hand side. We report these results in Table 5a.

Across most outcomes and consumption percentiles, we find little difference in outcomes between CCT and LCT arms. One exception, however, is that households at and below the 25th percentile of consumption reported significantly lower non-food consumption at follow-up in the CCT arm than in the LCT arm. In particular, according to the wild cluster bootstrap, the 95% confidence intervals for these differences are [−0.309, −0.041] and [−0.308, −0.052] at the 25th and 10th consumption percentiles, respectively. It appears as though the poorest households in the CCT arm may have been substantially affected by the penalty fines, leading them to reduce their non-food (i.e., likely less essential) consumption. Note that these results withstand significance tests using randomization inference p-values, which means that they are likely not to be driven by only one or two of the CCT clusters,

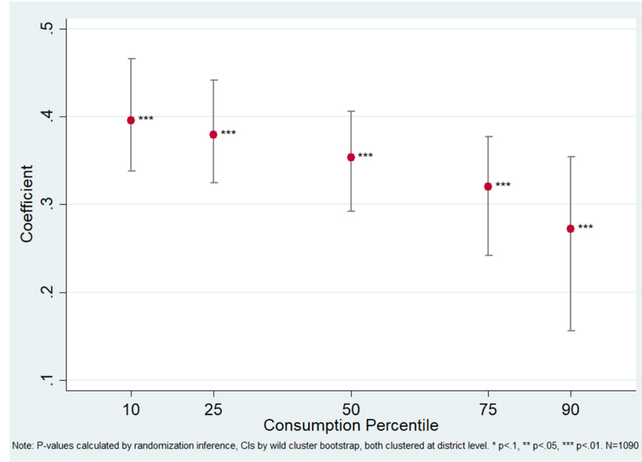

Fig. 2. Impact of Assignment to CCT on Ever Being Fined by Consumption Percentile.

thereby increasing our confidence in their validity. Although we attribute this reduction in non-food consumption primarily to the penalty fines, we do not claim that they are the only mechanism at work here. Lastly, we do not want to overstate the precision of the null results in Table 5b because, like in Section 4.3.1, the confidence intervals on the point estimates are quite wide. For example, the effect of being assigned to the CCT on Days Missed from School for children in the 10th percentile of household consumption has a point estimate of −0.129 and a 95% confidence interval of [−0.643, 0.312]. This interval contains effect sizes that range from 58% to 28% of the control mean, which are of substantial magnitude in both directions. Thus, we cannot claim that being assigned to the CCT versus the LCT had no effect on outcomes apart from non-food consumption, just that they were not statistically detectable in this analysis.

Overall, these findings imply that assignment to the CCT arm within the transfer group of the Kenya CT-OVC did not result in statistically significant improvements in the conditioned-upon outcomes. If anything, it appears as though receiving the CCT instead of the LCT resulted in reductions in non-food consumption among the poorest households, likely due to the burden (cash loss) imposed by the penalty fines. As a robustness check, we estimate Eq. (4) on downstream outcomes while controlling for the false discovery rate (FDR) to adjust for multiple hypothesis testing.[11] This produces q-values-or the lowest FDR that would allow us to reject the null hypothesis for a given p-value-that are below 0.05 for our main results, non-food consumption for households at the 25th percentile of consumption and below (when using p-values from the wild cluster bootstrap).We present our full re-analysis in Appendix C, accompanied by a more detailed description of the false discovery rate and q-values.

4.3.3. Longer-run effects

In addition to the follow-up survey that was conducted in 2009, a second set of follow-up data were collected in 2011 that potentially allow us to study the longer-run effects of the Kenya CT-OVC. These data and our analysis of them come with several important caveats. The first is that we were unable to find program documentation that addressed whether the conditionality continued to be meaningfully implemented and enforced between 2009 and 2011. Additionally, the second follow-up survey asked few

[11] We control for the false discovery rate using the publicly available Stata code posted by Michael Anderson, as described in Anderson (2008).

Table 5a
Impacts of Assignment to CCT versus LCT: Heterogeneous Effects.

	(1) Enrolled in School	(2) Days Absent from School	(3) Total Doses of Vaccinations	(4) Number of Vacc. Sequences Completed
Effects by Baseline Consumption Percentile				
Percentile: 10	0.013	−0.129	−0.586	−0.289
	(0.312)	(0.507)	(0.443)	(0.387)
	[0.357]	[0.568]	[0.549]	[0.351]
Percentile: 25	0.009	−0.109	−0.561	−0.272
	(0.325)	(0.443)	(0.404)	(0.348)
	[0.367]	[0.618]	[0.501]	[0.351]
Percentile: 50	0.002	−0.076	−0.521	−0.244
	(0.782)	(0.460)	(0.341)	(0.303)
	[0.763]	[0.719]	[0.456]	[0.403]
Percentile: 75	−0.007	−0.035	−0.469	−0.209
	(0.652)	(0.792)	(0.328)	(0.317)
	[0.745]	[0.793]	[0.509]	[0.443]
Percentile: 90	−0.019	0.024	−0.395	−0.158
	(0.587)	(0.937)	(0.278)	(0.445)
	[0.560]	[0.952]	[0.550]	[0.591]
LCT Mean	0.937	1.109	7.429	3.010
N	2549	2242	235	235

Note: P-values calculated with the wild cluster bootstrap procedure are in parentheses, and p-values calculated with randomization inference are in brackets. Clustering is done at the district level. All estimates are calculated conditioning on control variables and the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. The sample for both education variables consists only of children that were aged at 2 to 17 at baseline. Only children enrolled in school at baseline and follow-up are included in the sample for Days Missed from School, and the reference period is the past 2 months. Vaccinations include BCG (1 dose sequence), DPT (3 doses), OPV (4 doses), and measles (1 dose). Only children aged seven or under at follow-up are included in the sample for the vaccination variables and the vitamin A supplement, the reference period for the latter is the past 6 months.

Table 5b
Impacts of Assignment to CCT versus LCT: Heterogeneous Effects.

	(1) Received Vitamin A Supplement	(2) HH Total Consumption	(3) HH Food Consumption	(4) HH Non-food Consumption
Effects by Baseline Consumption Percentile				
Percentile: 10	0.025	−0.215	−0.050	−0.161
	(0.870)	(0.176)	(0.526)	(0.005)
	[0.854]	[0.201]	[0.582]	[0.000]
Percentile: 25	0.018	−0.165	−0.025	−0.137
	(0.901)	(0.230)	(0.738)	(0.004)
	[0.951]	[0.251]	[0.690]	[0.042]
Percentile: 50	0.007	−0.085	0.015	−0.098
	(0.949)	(0.422)	(0.833)	(0.070)
	[0.951]	[0.600]	[0.794]	[0.204]
Percentile: 75	−0.007	0.018	0.066	−0.047
	(0.959)	(0.862)	(0.338)	(0.467)
	[1.000]	[0.900]	[0.443]	[0.508]
Percentile: 90	−0.028	0.165	0.139	0.025
	(0.840)	(0.393)	(0.141)	(0.804)
	[0.850]	[0.395]	[0.245]	[0.796]
LCT Mean	0.510	2.107	1.269	0.838
N	561	1092	1093	1095

Note: P-values calculated with the wild cluster bootstrap procedure are in parentheses, and p-values calculated with randomization inference are in brackets. Clustering is done at the district level. All estimates are calculated conditioning on control variables and the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. The sample for both education variables consists only of children that were aged at 2 to 17 at baseline. Only children enrolled in school at baseline and follow-up are included in the sample for Days Missed from School, and the reference period is the past 2 months. Vaccinations include BCG (1 dose sequence), DPT (3 doses), OPV (4 doses), and measles (1 dose). Only children aged seven or under at follow-up are included in the sample for the vaccination variables and the vitamin A supplement, the reference period for the latter is the past 6 months.

questions about program implementation and did not collect data on whether households experienced penalty fines on their transfers. Moreover, since the Kenya CT-OVC evaluation team did not specify how they calculated the 2009 price deflators, we are unable to use the same methods to create an updated pair of deflators for the food and non-food components of the 2011 consumption variables. Instead, we draw upon the country-wide inflation rate of the KSh, compiled by the International Monetary Fund between 2009 and 2011, and use that information to deflate the 2011 values to 2007 shillings. Since this only gives us a single value for inflation, we apply it to both food and non-food consumption. Additionally, there are no 2011 data on Vitamin A usage among children, and the data on Days Missed from School are only for a two-week time window. Lastly, there was additional attrition between the 2009 and the 2011 surveys, further reducing the sample size.

With these limitations in mind, we estimate the longer-run impacts of the transfer by estimating appendix equation (B.1) with the 2011 outcome values on the left-hand side. These results are presented in Appendix D, Table D.1.[12] Most of the significant effects from before are either insignificant or marginally significant when

[12] Since the specification includes the indicator for missing baseline values of the outcome on the right-hand side, we adjust the variable as needed when we substitute the 2011 outcomes for the 2009 outcomes.

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using 2011 data. The most notable of these changes are the newly null effects on all of the consumption variables, which were previously quite robust. Next, to obtain the long run effects of being assigned to the CCT arm versus the LCT arm, we estimate Eq. (4) using the 2011 outcomes and display the results in Table D.2. Given the insignificant effects of the pooled transfer, it is unsurprising that there are no significant differences in outcomes between the CCT and LCT arms. This holds true when we are looking at both average and heterogeneous effects by baseline consumption. One potential explanation for this is that inflation had greatly eroded the value of the transfer. By 2011, the monthly transfer was only worth 991 KSh in 2007 shillings, about two-thirds of the real value from four years earlier. This erosion was acknowledged by the program coordinators and the transfer was increased from 1500 to 2000 KSh in the 2011–12 fiscal year. However, according to transfer receipts collected by the enumerators from participants, this increase appears not to have been effective until late in the 2011 data collection period.[13]

5. Policy implications and conclusion

In a 2013 blog post,[14] Berk Ozler characterized efforts to describe or define cash transfer programs as "an unconditional mess," arguing that the distinctions between CCTs and UCTs were "too blurry" and that interested stakeholders (donors, policymakers) would be better off thinking about them along a "continuum from a pure UCT to a heavy-handed CCT". Our research further suggests that a particular cash transfer program, such as the Kenya CT-OVC program, may not correspond to a single point along such a continuum. Indeed, our examination of the Kenya CT-OVC program shows that where it fits along a continuum from fully unconditional to "hard" conditions may depend on the implementation of the program as experienced and understood by households. And as Ozler opined and we found in this research, there are tradeoffs for household outcomes in terms of how the conditions (or lack thereof) are implemented. Our findings show that the imposition of conditions in a CCT arm of the Kenya CT-OVC program-i.e., a "heavy-handed" implementation that monetarily penalized families for their failure to comply with program conditions-did not improve children's outcomes relative to the LCT arm and had tradeoffs for household non-food consumption that varied by baseline poverty or wealth. These findings are consistent with prior literature showing that the effects of CCTs on household consumption vary according to household baseline wealth (or depth of poverty) (Fiszbein et al., 2009; Hoddinott et al., 2000; Macours et al., 2008; Maluccio & Flores, 2005). Indeed, one of the more compelling aspects of our estimates showing that the consumption of poorer households may be harmfully reduced is that they are largely consistent with what development practitioners and researchers have long suspected (even if debate in the literature is ongoing).

Having a program where households face penalities for not complying with expectations to spend cash transfers wisely (or for the benefit of the children) is a potentially promising way to achieve the broader goals of cash transfers programs, that is, to reduce not only poverty but also the intergenerational transmission of poverty. But it also creates more administrative burdens and costs for program implementation in the monitoring of household compliance with program conditions and enforcement of penalties. Furthermore, researchers and practitioners have long been concerned about the undue burdens that conditional cash transfers also place on the poorest of poor households. Not only is complying with rules more challenging for them, but penalizing their transfers may cut them off from purchasing basic necessities that their more meager budgets barely afford. Regrettably, this is what appears to have happened in the case of the Kenya CT-OVC program. These concerns are underscored by the fact that our analysis was not able to detect significant effects of assignment to the CCT on outcomes that were conditioned-upon by the program. However, we have also acknowledged that the few randomization clusters in this experiment prevent us from completely ruling out the possibility that the CCT arm experienced *any* change in outcomes relative to the LCT arm. In fact, the confidence intervals on our estimates are sufficiently wide such that even moderately-sized effects on other outcomes could have occured. What we can say with confidence, though, is that the negative effects on non-food consumption were sufficiently large that even an analysis with limited power (such as ours) was able to detect them.

If the insignificant differences between the CCT and LCT arms are to be taken at face value (i.e., if one overlooks their precision), then the policy implications of our results for a hypothetical cash transfer program depend on the said program's stated objectives. In a program that is purely concerned with improving conditioned-upon outcomes at the lowest cost, then the choice of CCT versus LCT hinges upon the cost of implementing and enforcing the conditions relative to the forecasted savings in transfer money withheld from households in the form of fines. If the program is concerned about overall household wellbeing (instead of primarily conditioned-upon outcomes), then those who are planning the program should also take into account how the imposition of fines on transfers reduces consumption for households at the lower end of the wealth distribution. Put another way, planners would have to weigh the costs of implementing the CCT with the money saved in the form of withheld transfer payments *and* the fines' negative effects on household consumption, which complicates the analysis. Finally, to add further complexity, these calculations change if one relaxes the assumption that assignment to the CCT arm produces the same conditioned-upon outcomes as assignment to the LCT arm. The width of our confidence intervals suggests that the effects of being assigned to the CCT on these outcomes could have been large enough to change this cost-benefit analysis substantially in either direction. Future work in this area could explore how CCTs and LCTs compare in the context of a well-powered experiment, in which the transfer constitutes a large fraction of households' baseline consumption (as in the Kenya CT-OVC). Only through further research, coupled with detailed data on implementation costs, can the relative benefits of CCTs versus LCTs be more clearly established, and even then the conclusions may be tempered by contextual factors in implementation.

Surprisingly, given the expansive literature that has emerged over time on CCTs and UCTs (and the nascent literature on LCTs), we found little empirical exploration of the consequences of experiencing financial penalties (or suspension or termination of benefits) for households and children receiving cash transfers. The random assignment between CCT and LCT arms in the Kenya CT-OVC may have allowed us a unique opportunity to examine the consequences of financial penalties in CCTs in terms of household and children's outcomes. That said, while we believe that we have presented compelling evidence on the differential impacts of CCTs and LCTs, our study is not without limitations. As noted above, our data on penalty fines are only for a two-year window of program implementation, and we do not have detailed data to identify the frequency or timing of penalties on households at all. Ideally, we would have had better data to explore a fuller range of impacts of being fined on household and children's well-being, but we are constrained by sample sizes within the CT-OVC treatment group and by the fact that many outcomes were measured only for age-appropriate subgroups. We hope this research will spur

[13] In the 2011 wave of data collection, households were asked to hand enumerators their most receipt transfer receipts. Of the households that could supply this information, approximately 10% of them reported having received the increased transfer amount (4000 Ksh per payment cycle versus 3000 KSh).

[14] https://blogs.worldbank.org/impactevaluations/defining-conditional-cash-transfer-programs-unconditional-mess.

further interest in "labeling" or other behavioral nudges in cash transfer programs that can offset the welfare costs inherent in traditional CCTs, as we observed in the Kenya CT-OVC program.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgements

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Appendix A. CT-OVC program guidance and conditions

Table A.1(A)

Kenya CT-OVC Program Conditions and Compliance Monitoring

Children aged one year and under should:

- Attend the health facility for immunizations, growth monitoring and vitamin A supplement
    - Frequency of required compliance: six times per year
    - Frequency of compliance monitoring: every two months.

Children aged between one and five years should:

- Attend the health facility for growth monitoring and vitamin A supplement
    - Frequency of required compliance: twice per year
    - Frequency of compliance monitoring: every six months.

Children aged between six and 17 years should:

- Enroll in school
    - Frequency of required compliance: once per academic year
    - Frequency of compliance monitoring: every 12 months.

- Attend basic education institutions
    - Frequency of required compliance: 80 per cent attendance of effective days
    - Frequency of compliance monitoring: every two months.

One adult parent or caregiver should:

- Attend awareness sessions
    - Frequency of required compliance: once per year
    - Frequency of compliance monitoring: every 12 months.

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Table A.1(B): Household Survey Questions on Knowledge of Program Rules and Conditions.

Survey questions
Do families participating in the OVC cash transfer programme have to follow any rules in order to continue receiving payments? 1 = Yes 2 = No 98 = Don't Know
Can you please list the rules that you think cash transfer families have to follow in order to receive the full payment from the OVC programme? A = Enrolment / attendance in primary school only B = Enrolment / attendance in primary and secondary schools C = Attendance to health facility for immunizations D = Attendance to health facility for growth monitoring E = Attendance to health facility for vitamin A supplement F = Adequate food and nutrition for children G = Clean and appropriate clothing for children H = Attendance at OVC Programme community awareness sessions I = Birth certificate for children J = Other, specify ___________________________ 98 = Don't Know
Which household members do these rules apply to? 1 = All children in the household 2 = Only to orphans and vulnerable children 3 = Other, specify______________________________ 98 = Don't know
Do you know what will happen if cash transfer families do not follow the rules? 1 = Yes 2 = No
What will happen to a cash transfer family if they do not follow all of the rules? 1 = Nothing 2 = Kicked out of the programme 3 = Go to jail 4 = A penalty fine will be deducted from the next payment – but do not know the amount 5 = A penalty fine will be deducted from the next payment – 500KS for every rule that is not followed 6 = Other ___________________________
Is anyone checking to see if cash transfer families are following the rules? 1 = Yes 2 = No 98 = Don't know

Table A.1(C): Household Survey Questions on Knowledge of Program Rules and Conditions.

Survey questions
Can you please list the reasons why a cash transfer family would be asked to leave the OVC cash transfer programme? A = After being in the programme for 5 years B = The household no longer has orphans or vulnerable children below 18 years old C = Household members do not follow all of the rules of the OVC Programme for 3 consecutive periods D = The household moves to another district where the OVC Programme is not operating E = The household caregiver has presented false information related to the eligibility for the Programme F = The household does not collect the payment for 3 consecutive collections G=Misuse of the money, specify ____________________ H=Neglect of the OVC, specify _____________________ I = Other, *specify* _________________________ 98 = Don't know
Have you ever gone to the Post Office to collect your payment and received less than 3000KS for the payment cycle? 1 = Yes 2 = No **Interviewer:** Look at all of the receipts provided the respondent and look for cash transfer amounts of less than KS 3000.
For the last time you received less than 3000KS for your payment, do you know why you received less? 1 = Yes 2 = No
Do you know if there is an appeal/complaints process if you ever receive less than 3000 KS in a payment cycle? 1 = Yes 2 = No

Appendix B. CT-OVC overall program impacts

In order to examine how cash transfer receipt in the CT-OVC program affected outcomes as a whole, we first assess the comparability of the transfer and control groups at baseline as shown in Appendix Table B.1. Instead of using traditional clustered standard errors, we conduct inference using p-values generated from the wild cluster bootstrap and randomization inference (discussed in

Table B.1
Balance Table: Cash Transfer vs. Control Group.

	(1) Control	(2) Transfer	(3) Bootstrap P-Value	(4) RI P-Value
Years of Edu. of HH Head	6.748	5.902	0.682	0.535
Sex of HH Head	0.411	0.352	0.448	0.386
HH Receives Labor Wages	0.080	0.034	0.402	0.130
HH Receives Outside Transfer	0.199	0.294	0.411	0.205
Poor Quality Walls	0.849	0.726	0.123	0.044
Poor Quality Floor	0.824	0.730	0.182	0.041
HH Owns Livestock	0.797	0.799	0.757	0.640
Cattle Owned	1.606	1.289	0.174	0.017
Poultry Owned	6.080	4.413	0.054	0.009
Owns Telephone	0.164	0.105	0.570	0.386
Owns Blanket	0.872	0.842	0.500	0.229
Owns Mosquito Net	0.703	0.604	0.254	0.157
Acres of Land Owned	2.32	1.599	0.089	0.010
Household in Rural Location	0.700	0.832	0.955	0.945
HH Total Consumption	1.640	1.603	0.590	0.372
HH Food Consumption	0.925	0.952	0.404	0.198
HH Non-food Consumption	0.715	0.651	0.876	0.808
Dietary Diversity Score	5.513	5.114	0.150	0.021
Size of the HH	5.703	5.444	0.736	0.709
Age of HH Head	48.249	58.067	0.513	0.366
People Aged 0–5 in HH	0.833	0.672	0.814	0.752
People Aged 6–11 in HH	1.320	1.225	0.455	0.245
People Aged 12–17 in HH	1.374	1.366	0.130	0.045
People Aged 18–45 in HH	1.514	1.129	0.574	0.563
People Aged 46–64 in HH	0.427	0.652	0.005	0.000
People Aged 65 + in HH	0.235	0.399	0.067	0.005
N	438	1092		

Note: The p-values in column (3) are calculated with the wild cluster bootstrap procedure, and the p-values in column (4) are calculated with randomization inference. Clustering is done at the district level. All estimates are calculated conditioning on the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. Some variables have fewer observations than given in the final row due to missing responses.

more detail in Section 3.3). While most of the differences in treatment versus control group means were statistically insignificant at the 5 percent level according to the wild cluster bootstrap, there were additional statistically significant differences when using randomization inference. These differences appear to be driven by the prioritization of households with very old caregivers of OVCs, as evidenced by the imbalance in household composition of older members. Although we controlled linearly for the prioritization criteria when comparing means, these differences persist. We also looked at attrition by treatment status between the cash transfer and control groups (overall about 24 percent), and there did appear to be some differential attrition between these groups, as households that received transfers were 10 percentage points less likely to attrit than control households (see Appendix Table B.2). However, since the main focus of this study is on comparing the randomly assigned treatment arms to one another (CCT versus LCT), we were not overly concerned about the imperfect balance and presence of differential attrition between the pooled transfer and control groups.

We estimate the overall impact of the CT-OVC cash transfer program using equation (B.1) below. The variable y_{lijk} refers to the outcome measures for child l (school enrollment/attendance, immunizations and vitamin A supplementation) and household i (consumption outcomes) in sub-location j and district k. $transfer_{jk}$ indicates random assignment of sub-location j to receive the cash transfer. The variables $y_{lijk}, X'_{1ijk}, X'_{2lijk}, carerIndex_{ijk}$, $totalOVC_{ijk}$, and $totalChronicallyIll_{ijk}$ have the same meanings as described in the main text.

$$\begin{aligned} y_{lijk} = \; & \alpha + \delta transfer_{jk} + \gamma_1 carerIndex_{ijk} + \gamma_2 totalOVC_{ijk} \\ & + \gamma_3 totalChronicallyIll_{ijk} + X'_{1ijk}\beta_1 + X'_{2lijk}\beta_2 + e_{lijk} \end{aligned} \tag{5}$$

The results from these estimates are given in Appendix Table B.3 and suggest that assignment to the transfer increased both food and non-food consumption. These results are consistent with the findings of The Kenya CT-OVC Evaluation Team (2012) in their differences-in-differences impact analysis, which indicated that CT-OVC cash transfer receipt was associated with increases in household consumption of both food and non-food items and a reduction in poverty levels by about 13 percentage points. However, it appears as though the only conditioned-upon outcome that was affected by the cash transfer was school attendance conditioned on enrollment. The Kenya CT-OVC Evaluation Team reported impacts on secondary school enrollment, but similar to what we find in our analysis, they found no overall impacts on child health indicators. One exception to this pattern of findings on child health was reported by Huang et al. (2017), who identified a reduction in the incidence of illness (fever and hot body symptoms) among children 0–7 years in the CT-OVC.

Appendix C. Correcting for multiple hypothesis testing

When testing for the significance of many coefficients, as we do with this paper, it is possible to find significant treatment effects purely by chance even when the true effects are zero. In order correct for this, we re-analyze the data while controlling for the false

Table B.2
Differential attrition by treatment status.

Panel A: Cash Transfer vs. Control Group	(1) Control Mean	(2) Transfer Differential Effect	(3) Bootstrap P-Value	(4) RI P-Value	(5) N
Assigned to Transfer	0.298	−0.107	0.003	0.000	1978
Panel B: CCT vs. LCT	(1) LCT Mean	(2) CCT Differential Effect	(3) Bootstrap P-Value	(4) RI P-Value	(5) N
Assigned to CCT	0.171	0.038	0.266	0.183	1351
Percentile: 1–20	0.222	0.040	0.064	0.108	310
Percentile: 20–40	0.149	0.017	0.643	0.637	291
Percentile: 40–60	0.156	−0.008	0.886	0.960	259
Percentile: 60–80	0.143	0.074	0.147	0.099	251
Percentile: 80–99	0.176	0.072	0.177	0.266	240

Note: The p-values in column (3) are calculated with the wild cluster bootstrap procedure, and the p-values in column (4) are calculated with randomization inference. Clustering is done at the sub-location level in Panel A and distict level in Panel B. All estimates are calculated conditioning on the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. Percentiles refer to households' places in the baseline consumption distribution.

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Table B.3
Impact of Assignment to Cash Transfer versus Control.

	(1) Control Mean	(2) Transfer Differential Effect	(3) Bootstrap P-value	(4) RI P-Value	(5) N
Enrolled in School	0.935	0.006	0.570	0.392	3716
Days Missed from School	1.195	−0.417	0.024	0.008	3294
Total Doses of Vaccinations	7.375	−0.366	0.361	0.378	371
Number of Vacc. Sequences Completed	3.000	−0.219	0.198	0.216	371
Received Vitamin A Supplement	0.477	0.099	0.213	0.146	895
HH Total Consumption	2.021	0.342	0.006	0.002	1530
HH Food Consumption	1.211	0.207	0.003	0.002	1531
HH Non-food Consumption	0.810	0.136	0.034	0.014	1535

Note: The p-values in column (3) are calculated with the wild cluster bootstrap procedure, and the p-values in column (4) are calculated with randomization inference. Clustering is done at the sub-location level. All estimates are calculated conditioning on control variables and the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. The sample for both education variables consists only of children that were aged at 2 to 17 at baseline. Only children enrolled in school at baseline and follow-up are included in the sample for Days Missed from School, and the reference period is the past 2 months. Vaccinations include BCG (1 dose sequence), DPT (3 doses), OPV (4 doses), and measles (1 dose). Only children aged seven or under at follow-up are included in the sample for the vaccination variables and the vitamin A supplement, and the reference period is the past 6 months.

Table C.1
(A): Impacts of Assignment to CCT versus LCT: Heterogeneous Effects (Controlling for FDR).

	(1) Enrolled in School	(2) Days Absent from School	(3) Total Doses of Vaccinations	(4) Number of Vacc. Sequences Completed
Effects by Baseline Consumption Percentile				
Percentile: 10	0.014	−0.174	−0.586	−0.290
	(1.000)	(1.000)	(0.802)	(0.802)
	[1.000]	[1.000]	[1.000]	[1.000]
Percentile: 25	0.010	−0.150	−0.561	−0.273
	(1.000)	(1.000)	(0.802)	(0.802)
	[1.000]	[1.000]	[1.000]	[1.000]
Percentile: 50	0.003	−0.112	−0.520	−0.245
	(1.000)	(1.000)	(0.802)	(0.802)
	[1.000]	[1.000]	[1.000]	[1.000]
Percentile: 75	−0.006	−0.064	−0.469	−0.210
	(1.000)	(1.000)	(0.802)	(0.802)
	[1.000]	[1.000]	[1.000]	[1.000]
Percentile: 90	−0.019	0.006	−0.394	−0.160
	(1.000)	(1.000)	(0.832)	(0.832)
	[1.000]	[1.000]	[1.000]	[1.000]
LCT Mean	0.937	1.109	7.429	3.010
N	2549	2242	235	235

Table C.1
(B): Impacts of Assignment to CCT versus LCT: Heterogeneous Effects (Controlling for FDR).

	(1) Received Vitamin A Supplement	(2) HH Total Consumption	(3) HH Food Consumption	(4) HH Non-food Consumption
Effects by Baseline Consumption Percentile				
Percentile: 10	0.022	−0.206	−0.041	−0.163
	(1.000)	(0.732)	(1.000)	(0.039)
	[1.000]	[1.000]	[1.000]	[0.001]
Percentile: 25	0.016	−0.156	−0.015	−0.139
	(1.000)	(0.852)	(1.000)	(0.039)
	[1.000]	[1.000]	[1.000]	[0.417]
Percentile: 50	0.005	−0.075	0.026	−0.100
	(1.000)	(1.000)	(1.000)	(0.436)
	[1.000]	[1.000]	[1.000]	[1.000]
Percentile: 75	−0.009	0.028	0.079	−0.050
	(1.000)	(1.000)	(1.000)	(1.000)
	[1.000]	[1.000]	[1.000]	[1.000]
Percentile: 90	−0.029	0.177	0.154	0.022
	(1.000)	(1.000)	(0.732)	(1.000)
	[1.000]	[1.000]	[1.000]	[1.000]
LCT Mean	0.510	2.107	1.269	0.838
N	561	1092	1093	1095

Note: The FDR q-values contained in parentheses are based on wild cluster bootstrap p-values, and those in brackets are based on randomization inference p-values. Clustering is done at the district level. All estimates are calculated conditioning on control variables and the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. The sample for both education variables consists only of children that were aged at 2 to 17 at baseline. Only children enrolled in school at baseline and follow-up are included in the sample for Days Missed from School, and the reference period is the past 2 months. Vaccinations include BCG (1 dose sequence), DPT (3 doses), OPV (4 doses), and measles (1 dose). Only children aged seven or under at follow-up are included in the sample for the vaccination variables and the vitamin A supplement, the reference period for the latter is the past 6 months.

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Table D.1
Impact of Assignment to Cash Transfer versus Control: Longer-Run Effects.

	(1) Control Mean	(2) Transfer Differential Effect	(3) Bootstrap P-value	(4) RI P-Value	(5) N
Enrolled in School	0.930	0.018	0.207	0.088	3447
Days Missed from School	0.655	−0.152	0.421	0.058	1681
Total Doses of Vaccinations	7.759	0.325	0.479	0.582	109
Number of Vacc. Sequences Completed	3.070	0.150	0.638	0.691	104
HH Total Consumption	2.459	0.036	0.841	0.819	1380
HH Food Consumption	1.512	0.083	0.543	0.467	1384
HH Non-food Consumption	0.957	−0.048	0.531	0.425	1382

Note: The p-values in column (3) are calculated with the wild cluster bootstrap procedure, and the p-values in column (4) are calculated with randomization inference. Clustering is done at the sub-location level. All estimates are calculated conditioning on control variables and the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. The sample for both education variables consists only of children that were aged at 2 to 17 at baseline. Only children enrolled in school at baseline and follow-up are included in the sample for Days Missed from School, and the reference period is the past 2 weeks in the 2011 data. Vaccinations include BCG (1 dose sequence), DPT (3 doses), OPV (4 doses), and measles (1 dose). Vitamin A supplement data were not available for the 2011 follow-up period.

Table D.2
(A): Impacts of Assignment to CCT versus LCT: Longer-Run Effects.

	(1) Enrolled in School	(2) Days Absent from School	(3) Total Doses of Vaccinations	(4) Number of Vacc. Sequences Completed
Panel A: Average Effects				
Assigned to CCT	−0.007	0.011	−0.333	−0.083
	(0.591)	(0.878)	(0.641)	(0.846)
	[0.619]	[1.000]	[0.649]	[0.792]
Panel B: Effects by Baseline Consumption Percentile				
Percentile: 10	−0.008	−0.049	−0.513	−0.135
	(0.606)	(0.619)	(0.615)	(0.830)
	[0.633]	[0.558]	[0.844]	[0.848]
Percentile: 25	−0.008	−0.029	−0.425	−0.110
	(0.578)	(0.717)	(0.606)	(0.828)
	[0.586]	[0.850]	[0.795]	[0.794]
Percentile: 50	−0.007	0.004	−0.283	−0.070
	(0.586)	(0.956)	(0.618)	(0.856)
	[0.660]	[1.000]	[0.740]	[0.844]
Percentile: 75	−0.006	0.047	−0.100	−0.018
	(0.658)	(0.577)	(0.784)	(0.933)
	[0.674]	[0.948]	[0.901]	[0.907]
Percentile: 90	−0.005	0.107	0.162	0.056
	(0.775)	(0.457)	(0.758)	(0.858)
	[0.841]	[0.542]	[0.904]	[0.851]
LCT Mean	0.936	0.612	7.773	3.047
N	2361	1137	79	74

Note: P-values calculated with the wild cluster bootstrap procedure are in parentheses, and p-values calculated with randomization inference are in brackets. Clustering is done at the district level. All estimates are calculated conditioning on control variables and the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. The sample for both education variables consists only of children that were aged at 2 to 17 at baseline. Only children enrolled in school at baseline and follow-up are included in the sample for Days Missed from School, and the reference period is the past 2 weeks in the 2011 data. Vaccinations include BCG (1 dose sequence), DPT (3 doses), OPV (4 doses), and measles (1 dose). Vitamin A supplement data were not available for the 2011 follow-up period.

discovery rate (FDR) (Benjamini & Hochberg, 1995). The FDR is the expected proportion of null hypothesis rejections that are Type I errors (false rejections). By implementing the procedure detailed in Benjamini, Benjamini, Krieger, and Yekutieli (2006), each p-value from our previous analysis is assigned a "q-value, or the lowest FDR that would allow us to reject the null hypothesis for the given p-value. These q-values are separately calculated for each family of outcomes, or a group of outcomes for which p-values are expected to be positively correlated. Our study defines four families of outcomes: educational outcomes (Days Missed from School, Enrollment), vaccination outcomes (Total Doses of Vaccines, Completed Vaccination Sequences), Vitamin A Supplement Received, and consumption outcomes (HH Total Consumption, HH Food Consumption, HH Non-food Consumption). We calculate two sets of q-values for each family, one for each method of conducting inference (wild cluster bootstrap and randomization inference), and report them in Appendix Table C.1.

See Appendix Table C.1.

Appendix D. Longer-run CT-OVC program impacts

Appendix Table D.1, D.2, D.2.

Appendix E. Supplementary data

Supplementary data associated with this article can be found, in the online version, athttps://doi.org/10.1016/j.worlddev.2020.104876.

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Table D.2
(B) Impacts of Assignment to CCT versus LCT: Longer-Run Effects.

	(1) HH Total Consumption	(2) HH Food Consumption	(3) HH Non-food Consumption
Panel A: Average Effects			
Assigned to CCT	0.092	0.062	0.024
	(0.641)	(0.643)	(0.781)
	[0.698]	[0.691]	[0.734]
Panel B: Effects by Baseline Consumption Percentile			
Percentile: 10	−0.033	0.038	−0.081
	(0.871)	(0.774)	(0.296)
	[0.903]	[0.799]	[0.354]
Percentile: 25	0.006	0.046	−0.048
	(0.974)	(0.732)	(0.502)
	[0.948]	[0.799]	[0.538]
Percentile: 50	0.069	0.058	0.005
	(0.711)	(0.668)	(0.931)
	[0.856]	[0.792]	[0.948]
Percentile: 75	0.150	0.074	0.073
	(0.500)	(0.599)	(0.411)
	[0.450]	[0.582]	[0.426]
Percentile: 90	0.266	0.097	0.171
	(0.313)	(0.560)	(0.117)
	[0.284]	[0.595]	[0.189]
LCT Mean	2.066	1.321	0.745
N	985	987	985

Note: P-values calculated with the wild cluster bootstrap procedure are in parentheses, and p-values calculated with randomization inference are in brackets. Clustering is done at the district level. All estimates are calculated conditioning on control variables and the transfer prioritization criteria: Carer Age Index, Chronically Ill Members of HH, and Number of OVCs in HH. Consumption variables are in terms of 1000 KSh per adult-equivalent. The sample for both education variables consists only of children that were aged at 2 to 17 at baseline. Only children enrolled in school at baseline and follow-up are included in the sample for Days Missed from School, and the reference period is the past 2 weeks in the 2011 data. Vaccinations include BCG (1 dose sequence), DPT (3 doses), OPV (4 doses), and measles (1 dose). Vitamin A supplement data were not available for the 2011 follow-up period.

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