A Fine Predicament: Conditioning, Compliance and Consequences in a Labeled Cash Transfer Program*

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Abstract

As conditional cash transfer (CCT) and unconditional cash transfer (UCT) programs have matured as development tools, attention has turned to improving the effectiveness of "second generation" CCT and UCT programs. Of particular interest is the role of conditions and their implementation in CCTs, such as compliance monitoring and penalties for non-compliance, and how they affect program outcomes for households and children. The Kenya Cash Transfer Programme for Orphans presents a valuable opportunity to examine the effects of imposing monetary penalties on cash transfers to poor households, in contrast to providing only guidance or "labeling" for their intended use. We take advantage of the fact that "hard" conditions were assigned randomly within the treatment group to estimate the impact of fines imposed on program beneficiaries. We also conduct a marginal analysis of the effects of being penalized by household wealth (proxied by baseline consumption). We find that comparatively wealthier households that get fined not only have more resources to avoid negative effects, but they also undertake preventative measures to avoid being fined in the future. Alternatively, for comparatively poorer households, getting fined is associated with a decrease in consumption of about one-third the size of the cash transfer. If the poorer among beneficiary households have fewer means for fully complying with conditions and avoiding the penalties, and penalizing their transfers constrains their purchases of basic necessities, the imposition of fines under hard conditions could have lasting, harmful effects on such households—an unintended, regressive policy effect.

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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). Unconditional cash transfer programs are profilerating 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 et al., 2015). One global estimate of the number of beneficiaries of cash transfer programs (Fiszbein et al., 2014) suggests that close to one billion people worldwide are now 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 et al., 2018; Ralston et al., 2017). In fact, 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).

Most of the inaugural cash transfers programs, as well as many subsequent program efforts, have imposed conditions on households' receipt of cash transfers that prescribe how the monies should be used (Baird et al., 2013). Among the most common of these 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 PROGRESA (Programa de Educación, Salud, y Alimenación) program, later renamed 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 (Levy, 2006; Fiszbein et al., 2009). 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 et al., 2008), 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. In a study comparing program costs across three Latin American CCTs, Caldes et al. (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). 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 and Hoddinott, 2011; Heinrich & Brill, 2015). For these and related reasons, the implementation of unconditional cash transfers (UCTs) has become more commonplace in very low-income countries, and intermediate program models, where guidance for spending the transfer is articulated but not monitored or enforced–sometimes described as "labeled" cash transfer programs–have also been introduced (Benhassine et al., 2013).

In this research, we focus on a less 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 labeled cash transfer program that was distinct in its random assignment of "hard conditions" (conditions with penalties) within locations that were randomly selected to receive cash transfers. In the following section (2), we review the literature on conditional, unconditional and "labeled" cash transfer programs, 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. We next present background information on the Kenya CT-OVC program and the nature of the conditions, penalities and labeling of the cash transfers (section 3), and we also describe the design of the experimental evaluation and data collected that we draw on in this study. In section 4, we introduce the methods we employ in investigating how imposing hard conditions (vs. providing guidance or labeling alone for cash transfer use) influences households' understanding of program rules and their behavioral responses, as well as three domains of household and children's outcomes (consumption, dietary diversity and schooling). We present the findings of our analyses in section 5 and conclude with a discussion of the results in section 6. We find that the effect of receiving a monetary penalty (that reduces a household's cash transfer amount) differs greatly by baseline household consumption. In fact, while comparatively poorer households endure long-lasting penalties to consumption after being fined, comparatively wealthier households respond by increasing spending on food quantity and variety, perhaps in the effort to avoid being fined in the future. These findings affirm conventional wisdom that penalties in cash transfer programs disproportionately harm those who are least able to meet them.

2 Literature Review

2.1 Why condition?

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. Numerous works have articulated the arguments for and against the imposition of conditions (Ferreira, 2008; Fiszbein et al., 2009; de Brauw and Hoddinott, 2011), which we briefly review here. As Fiszbein 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 et al., 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.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. Grosh et al. (2008) assembled data on the combined administrative costs of targeting and paying transfers, monitoring compliance, and related program management for 10 CCT programs and estimated a range from 4 to 12 percent of total program costs. 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 collecting the transfers and 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. If the households who find it most challenging to satisfy the conditions are among the poorest of program eligibles, this could not only reduce the targeting effectiveness of the CCT (de Brauw and Hoddinott, 2011) but also unduly penalize those most in need of financial support (Heinrich & Brill, 2015). How large and important are the informational problems and externalities of CCTs (and the role of conditions in addressing them) relative to the public and private costs they engender is still an open question, and one where the answer surely varies considerably across program and country contexts.

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 (Fizbein et al., 2009; Ralston et al., 2017; Hidrobo et al., 2018). 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. This 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. Opportunidades (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 & Shady, 2009; Mont, 2006). In contrast, the Chile Solidario program does not begin paying cash transfers until families have complied with the first criteria, 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 concept of a "labeled" cash transfer program (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 (Behassine et al., 2013). 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. In Behassine et al.'s evaluation of the Tayssir cash transfer program in Morocco, a CCT version of the program was compared with the LCT, where the LCT arm portrayed the cash tranfers as an educational intervention. Enrollment for the Tayssir LCT was conducted at schools and by headmasters, thereby tying receipt of the cash transfers to an education goal, 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.

Behassine et al.'s (2013) 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. Interestingly, they 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. Behassine et al. 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 UCT.

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% higher in the CCTs and 23% 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. 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 size of the programs.

We expand on the research of Behassine et al. and Baird et al. 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 "hard" conditions (with monitoring and penalties for noncompliance) were assigned randomly to some districts and sublocations within the treatment group (Hurrell, Ward & Merttens, 2008). We also have detailed information on cash transfer recipients' understanding of the program rules and guidance (under the different treatment conditions) and their perceptions of the consequences they believed that households would face if they did not comply with the rules and expectations. We use the random assignment of hard conditions (i.e., the potential for financial penalties), and the information on households' perceptions of them, to understand the extent to which the imposition of "hard" conditions and associated penalties (vs. labeling of cash transfers) influences household responses and program outcomes. We expect that the costs of monetary penalties would be felt most immediately in terms of household consumption, thus, our analysis focuses primarily on estimating the impact of fines on households' total, food and non-food consumption, as well as their dietary diversity. At the same time, given the emphasis on schooling for children in the "labeling" of Kenya CT-OVC program, we also examine whether the imposition of fines affects children's (OVC) school attendance (absences).

3 Program Background, Study Design, Data and Measures

The Kenya CT-OVC program is the government's primary intervention for social protection in Kenya. 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 the OVC (Handa et al., 2014). In terms of the average (per adult equivalent) consumption levels at baseline (2007), the monthly cash transfers represent about 22 percent of average 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).

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 most unrest was experienced following the turmoil of disputed national elections that occurred in December 2007.

The evaluation of the Kenya CT-OVC was designed as a clustered randomized controlled trial (RCT) and took place in seven of 70 districts in the country (see Figure 1 that illustrates the design). Within each of the seven districts, two sub-locations were randomly assigned to be treatment locations and two were randomly assigned to the control state (no cash transfer distribution). Households in the 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 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 basic education institution attendence, and caregiver "awareness" session (see Appendix Table A.1). However, in four districts—Homa Bay, Kisumu and Kwale and one sub-location in Nairobi (Kirigu)—households were randomly assigned to a "hard conditions" CCT treatment arm, where the stated penalty for not following the program conditions was a deduction of KSh 500 from the transfer amount per infraction. The other districts and one sub-location—Garissa, Migori, Suba and the other Nairobi location (Dandora B)—were assigned to the LCT arm where non-compliance was not penalized. More than a third of households subject to hard conditions were fined within the first two years of cash transfer receipt, although in practice, there was considerable variation in the implementation and enforcement of the conditions within and across locations (which we discuss further below). In addition, attendance requirements were waived for children deemed to be without access to schools or clinics (Government of Kenya, 2006).

In treatment locations, a list was compiled containing the households eligible to receive the cash transfer, and households on the list were prioritized for treatment by several "vulnerability" criteria. These include 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 somewhat poorer households for cash transfer receipt, but this contributes to only one systematic difference in household characteristics between the study treatment and control groups at baseline once standard errors are clustered at the level of treatment (sub-location) (see Appendix Table A.2). We account for these selective differences between the treatment and control groups in our estimation of program impacts (discussed in Section 4).

3.1 Treatment measures

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 the households in the 14 treatment districts or sublocations in this study, 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 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 the section of the survey on households' use of the cash transfers, they were also asked to indicate whether the cash transfer payments for the OVC(s) were kept separate from the rest of the household's income sources, and who in the household benefitted from the cash transfer.

In regard to the penalties associated with hard conditions, 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." 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 ever received less than 3000 KSh in a payment cycle. In our study sample, about 37 percent of the households subject to the hard conditions were reported to have received a fine in the two years since becoming CT-OVC beneficiaries. Table 1 (A) and (B) show all of the survey questions that we used in constructing measures of the treatment as implemented, perceived and used.

Because the implementation of "hard conditions" imposed concrete expectations for how households would spend the cash transfers and penalties for their failure to comply, we hypothesized that households in districts and sublocations randomly assigned to hard conditions might differ in their perceptions, responses to and uses of the cash transfer from those randomly assigned to the control state or status quo of "labeling," i.e., instructions for how to use the cash transfers but without penalities. Furthermore, we also expect there to be heterogeneity in responses to the hard conditions among those randomly assigned to this treatment arm, given the variation observed in how those conditions were implemented within sites. The final operational and impact evaluation report (Ward et al., 2010) indicated that 84 percent of the beneficiaries believed that they had to follow some sort of rules to continue receiving the cash transfers, but the report also noted that most beneficiaries were not aware of the full set of conditions with which they were expected to comply. Monitoring and enforcement of the hard conditions within and across locations was hindered by onerous forms and logistical challenges, which the literature suggests can impact poorer families disproportionately (Heinrich & Brill, 2015; Heinrich, 2016). In addition, community representatives charged with the role of communicating and checking on conditions were typically informally appointed and lacked remuneration, and implementation of that role was highly dependent on a given community representative's knowledge, interpretation of their obligations, and activism. Two years after random assignment, many beneficiaries had not been reached with communications about the penalities, and where penalties were imposed, those affected often did not understand the reason for the decrement in their transfer (Ward et al., 2010; FAO, 2014).

The literature on CCTs suggests that these types of program capacity constraints in implementing conditions and verifying compliance are relatively common. Fiszbein and Shady (2009) point out that these constraints can delay actions to sanction noncompliance, even in established programs such as Opportunidades in Mexico. They also argue (p. 89) that longer lag times between household noncompliance and the reduction of cash transfer program benefits are likely to weaken the “positive quid pro quo” effects of the conditions on program outcomes. 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 budetary capacity to absorb the monetary loss—we expect there to be differential effects of being penalized or fined for noncompliance by household baseline need and consumption levels.

3.2 Outcome measures

We evaluate the impact of being fined (penalized for noncompliance) in the Kenya CT-OVC program on the following dimensions of household and child wellbeing: consumption (food and non-food), nutrition and dietary diversity, and schooling. The sample sizes in our analysis vary by outcome, primarily because the outcomes we focus on are measured for distinct groups receiving the cash transfers: households for consumption and the dietary diversity score, and absences from school for school-aged children (6-17 years) (0-5 years).

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 (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 nonfood 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.

The second dimension that we examine reflects the broader program goal of increasing food security and dietary diversity in OVC households. A highly consistent finding among cash transfer program evaluations is their effectiveness in reducing hunger and food insecurity, given the monetary resources newly made available to households for meeting their basic consumption needs (Devereux & Coll-Black, 2007; Fernald et al., 2008). The final impact evaluation report (Ward et al., 2010) described increases in food expenditure and dietary diversity associated with cash transfer receipt, with significantly increased frequency of consumption within five food groups: meat, fish, milk, sugar and fats. Ward et al. (2010) also reported an increase of 15 percent (from baseline) in the dietary diversity score; this is consistent with the findings of Lopez-Arana et al. (2016), who found a 16.5 percent increase in the purchase of protein-rich foods among families benefitting from Colombia's CCT program. Asfaw et al. (2012) also evaluated the average difference between the treatment and control households in the Kenya CT-OVC program in terms of different components of food consumption expenditure and found positive and statistically significant impacts of the program on consumption of animal products (e.g., dairy, eggs, meat and fish) and fruits. In our analysis, we use as an index of dietary diversity that mirrors that of Hurrell et al. (2008), tallying the number of different food groups from which the household ate in the past week.

The third 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 six to 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 (Gernshenson, 2016; Heckman, Stixrud & Urzua, 2006) and to improved cognitive development (Gottfried, 2009), as well as to retention rates and increased educational attainment (Gershenson et al., 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, 2011), and that non-school factors, such as poverty, family emergencies and work obligations, are the primary determinants of attendance rates (Balfanz & Byrnes, 2012; Ladd, 2012). If being fined reduces resources for poor families that enable them to overcome these non-school barriers to school attendance, we would expect being fined to potentially diminish the cash transfer program's impact on reducing student absences.

4 Methods and Estimation Strategy

The primary objective of our analysis is to estimate the impact of a monetary penalty, or *fine*, imposed on CT-OVC program recipients, focusing on its effects on the household's per capita consumption and dietary diversity, as well as children's schooling. The fine is imposed sometime between the time the household is randomly assigned to receive the cash transfer in 2007 and the time when the household is interviewed two years after random assignment. For the purposes of strong identification, the ideal (but impractical and wholy unethical) experiment for identifying the impact of fines would be to randomly assign fines to households receiving cash transfers and then measure how household and children's outcomes change in response to the financial penality. We instead adopt an instrumental variables approach, in which we exploit the random assignment to hard conditions (by district/sub-location) and use it as an instrument for households being fined in the CT-OVC program. One might expect that in many cash transfer programs, using random assignment to hard conditions as an instrument for being fined would not satisfy the exclusion restriction, given that the mere threat of a fine for non-compliance would likely alter households' behavioral responses to treatment. The Kenya CT-OVC program, however, is exceptional in this regard, owing in part to how its hard conditions were implemented. As we will show explicitly below (Section 4.3), random assigment to hard conditions had little effect on household behaviors beyond increasing their probabilities of being fined. As we explicate in the following sections, we believe our estimation approach, relying on multistage random assignment (first to the cash transfer program and then to hard conditions) with strong first-stage results, gets us very close to the ideal experiment.

In the remainder of this section, we first lay out our empirical specification for estimating the impact of being fined on household per capita consumption and dietary diversity and school absences (for children aged 6-17 years). Next, we present our arguments for why random assignment to hard conditions is a credible instrument for a household getting fined in the CT-OVC program. Specifically, we provide evidence for its exogeneity, the strength of the first-stage estimation, and its excludability.

4.1 Empirical specification

As stated above, the primary objective of this analysis is to estimate the impact of receiving a fine (monetary penalty) on household consumption, dietary diversity and schooling outcomes. We employ a two-stage least squares (2SLS) IV approach, in which we instrument for being fined by households' random assignment to either the hard conditions or the labeled treatment arm. Our sample for this estimation is comprised entirely of households i that received the cash transfer in the Kenya OVC-CT program[1]. The specification for the structural equation of interest is as follows:

$$\begin{aligned} y_{i,2009} = \alpha_1 + \delta_{1,1} y_{i,2007} + \delta_{1,2} fined_i + \delta_{1,3} totalcons_{i,2007} \\ + \delta_{1,4} fined_i * totalcons_{i,2007} + X'_{i,2009} \beta_1 + e_i \end{aligned} \tag{1}$$

The variable $y_{i,2009}$ represents the follow-up (2009) survey value of our outcome variables, $y_{i,2007}$ represents the baseline outcome values, and $fined_i$is an indicator for whether a house-

[1]Note that there were a few housedholds that received the transfer despite not being assigned to treatment in the CT-OVC program. They are retained in these regressions, and we control for their presence.

hold ever experienced a monetary deduction from its cash transfer between 2007 and 2009. The existing evidence base (discussed above) suggests that we should pay special attention to the heterogeneous effects of being fined, particularly according to baseline household wealth. For this reason, we also conduct a marginal analysis of the effects of being fined on our outcomes; we include an interaction term between being fined and baseline total household consumption (our proxy for wealth) and also control for baseline total consumption independently. Lastly, $X_{i,2007}$ is a vector of baseline household demographic variables, including measures used by program officials to prioritize households for cash transfers among CT-OVC treatment households and an indicator for being assigned to the transfer.

As $fined_{i,2009}$ and $fined_{i,2009} * totalcons_{i,2007}$ are likely both endogenous, we need to choose appropriate instruments for our first-stage equations to avoid weak identification. We use random assignment to hard conditions as an instrument for being fined, and a natural candidate for the other endogenous predictor is the interaction between hard conditions and baseline total consumption: $hard_i * totalcons_{i,2007}$. Below are the two resulting first stage specifications:

$$\begin{aligned} fined_i = \alpha_2 + \delta_{2,1} y_{i,2007} + \delta_{2,2} hard_i + \delta_{2,3} totalcons_{i,2007} \\ + \delta_{2,4} hard_i * totalcons_{i,2007} + X'_{i,2009}\beta_2 + \varepsilon_i \end{aligned} \tag{2}$$

$$\begin{aligned} fined_i * totalcons_{i,2007} = \alpha_3 + \delta_{3,1} y_{i,2007} + \delta_{3,2} hard_i + \delta_{3,3} totalcons_{i,2007} \\ + \delta_{3,4} hard_i * totalcons_{i,2007} + X'_{i,2009}\beta_3 + u_i \end{aligned} \tag{3}$$

The Kenya CT-OVC Evaluation Team (2012) found in their differences-in-differences impact analysis that being randomly assigned to the CT-OVC cash transfer was associated with increases in household consumption of both food and non-food items. We look to replicate their findings with our 2SLS IV modeling approach, while also extending our analysis to examine the impacts of being fined on consumption, dietary diversity and schooling outcomes. Our specification for the estimation of CT-OVC cash transfer program impacts on household consumption is shown below, where CT_i indicates that a household was assigned to receive the cash transfer. We also conduct a marginal analysis by baseline levels of total consumption.

$$y_{i,2009} = \mu + \gamma_1 y_{i,2007} + \gamma_2 CT_i + \gamma_3 totalcons_{i,2007} + \gamma_4 CT_i * totalcons_{i,2007} + X'_{i,2009}\beta_4 + v_i \quad (4)$$

4.2 Exogeneity

The first condition or key assumption that we make in IV estmation is that our instrumental variable is not correlated with any unobserved factors that affect our outcome variables; that is, its assignment must be as-good-as-random. In the Kenya CT-OVC program, assignment to the treatment arm with hard conditions was done randomly at the district level, with the exception of the Nairobi District, where it was conducted at the sub-location level. If random assignment to hard conditions worked as intended, we would expect it to produce two statistically equivalent groups of program beneficiaries (treated with and without hard conditions) at baseline. Table 2 presents the results of our tests for equality of means between these two groups for various household demographic characteristics at the outset of the experiment. The sample used for these comparisons is the same sample we use in estimating our models of the impact of being fined on program outcomes. The results in Table 2 indicate that balance is achieved between these groups, that is, there are no statistically significant differences in means (at the 5% level) in their observable characteristics at baseline. This result holds regardless of whether we cluster the standard errors at the district, sub-location, or community level. While this gives us confidence that random assignment to hard conditions achieved the intended result, we still adjust (control) for characteristics such as rural location and agricultural land ownership in our main specifications to improve the efficiency of our estimation (Gennetian et al., 2006).

4.3 Exclusion restriction

In many cash transfer program evaluations, we would *not* expect to be able to use random assignment to hard conditions or a CCT arm as an instrument for being fined due to noncompliance with program rules. This is because we would expect that a household's assignment to hard conditions would alter the household's incentives and decisions regarding how to spend the transfer or what it chooses to consume for fear of being fined. In other words, this would violate the exclusion restriction and lead to inconsistent estimates of the effects of being fined. In the case of the Kenya CT-OVC program, however, we have compelling evidence that random assignment to the hard conditions arm within the cash transfer program did not affect households' responses to or use of the cash transfers. As described earlier, the imposition and enforcement of hard conditions was uneven and inconsistent across treated locations, and once we condition on covariates that reflected policy or institutional decisions about which households were neediest (and should be prioritized for the program), assignment to hard conditions is not associated at follow-up with households' beliefs about how the CT-OVC program works. That is, in the context of this "labeled" cash transfer program, households in both treatment arms (with and without hard conditions) conveyed the same beliefs about the program rules and penalties, leading us to feel confident that these households' behavioral responses to cash transfer receipt did not differ in any meaningful or systematic ways based on conditionality. In the remainder of this sub-section, we present evidence to convince our readers that this was the case.

The Kenya CT-OVC Impact and Operational Evaluation Team produced two reports on the Kenya OVC-CT program evaluation, the second of which focuses on the 2009 follow-up data and includes a qualitative assessment of the implementation of hard conditions (Ward et al. 2010). This qualitative assessment was based on fieldwork (primarily focus group discussions with program participants) conducted across multiple program regions in two rounds. The latter round included several "semi-structured" interviews with officials who were responsible for completing compliance forms at school and clinics as part of the process of monitoring households with hard conditions (Ward et al. 2010). An important takeaway from this qualitative assessment is that, while only households in the hard conditions arm were supposed to be penalized for breaking the rules, "in practice, recipients were told everywhere that they needed to do certain things in order to receive the transfer," consistent with the "labeled" cash transfer program design (Ward et al. 2010, p. 102). These "certain things" often did not reflect the actual program rules, however, and administrators appeared just as likely to threaten households in the group not assigned to conditions with penalties as they were those in the hard conditions group. For example, in the Nairobi sub-location assigned to no conditions, recipient households were told that their children had to attend school and health clinics or else "you [the household] will answer for this" (Ward et al. 2010, p. 102). Thus, it is not surprising that many of the households that *were* fined did not know why the deduction occurred. One possible explanation for these seemingly contrary actions in program implementation relates to the fact that, as discussed above, many of the program administrators were local officials or community representatives who likely had public service motivations for promoting these behaviors (Vandenabeele, 2007). In addition, as has been observed in other cash transfer program evaluations (Heinrich, 2016), heavy administrative burdens and discretion afforded to localities in implementation also likely contributed to inconsistent enforcement of penalties for violations of the program rules. In summary, the CT-OVC program rules were unevenly communicated and rarely understood, and both households without conditions, as well as those facing hard conditions, believed that they faced penalities for non-compliance.

These insights from the qualitative research are confirmed in our analysis of household responses to the questions from the 2009 follow-up survey that assess households' beliefs about the program design. Specifically, we implement several placebo tests to examine whether assignment to hard conditions affected households' understanding of the program rules and penalties. The results from these tests are displayed in Table 3, which we divide into panels by the category of survey questions. Each variable in the left-most column of Table 3 is a binary indicator of what the household believed about the program operations. As in our balance table, columns (1) and (2) are the mean affirmative response rates for these beliefs, divided by treatment arm (assignment to hard conditions versus labeling). Column (3) contains the "controlled" difference in the rates of beliefs between the treatment households with and without hard conditions, along with the standard errors.[2]These results mostly conform to the implications of the qualitative findings. On the whole, assignment to hard conditions did not significantly (differentially) affect households' understanding of the program rules, their perceived likelihood of being fined, or even their understanding of the criteria for suspension or expulsion from the program. Moreover, there were only a few exceptions to this pattern.

At the outset, one would expect any exceptions to this pattern to be associated with household knowledge about the imposition of hard conditions. That is, only households in locations

[2]Notice that the point estimate in column (3) is not the difference between columns (1) and (2). The values in column (3) are estimated using OLS, where the row variable is regressed on the regressors in equation (2). The point estimate is thus the coefficient on $hard_i$.

randomly assigned to hard conditions were supposed to be told that they could be fined for noncompliance; it was otherwise not on a program administrator's script. Furthermore, administrators in the locations without conditions were not empowered to impose fines, and thus, even if they threatened households with penalties for noncompliance (which they were not supposed to do but seemed to do anyway), it should not have been in the form of a fine[3]. This expectation is consistent with what we see in Table 3. The largest (and statistically significant) difference in beliefs between the hard conditions vs. no conditions groups is for the item "Believes Fining is a Punishment". There is also a minimally significant difference (at $\alpha < 0.10$) in the rate at which households believed that they needed to follow rules to continue receiving payments. Apart from another small difference in households' understanding about growth monitoring requirements (which is unlikely to meaningfully affect household consumption or children's schooling), these are the only two statistically significant differences in beliefs about the program between these treatment arms. We argue that this confirms that any differences in beliefs about the potential to be fined in the Kenya CT-OVC program likely arose mechanically from the program design, which was well-documented by Ward et al. (2010). Most importantly, it also implies that such differences are unlikely to be bellwethers of other unobserved violations of the exclusion restriction; rather, they would be self-contained, predictable divergences owing to program design that we can readily control for in our models.

Given that Ward et al. (2010) explained in detail the specific things that cash transfer program administrators conveyed *only* to the hard conditions group, we can control explicitly for the few ways in which their administrator scripts differed between the hard and labeled transfer groups. In doing so, we are able to alleviate a fundamental concern about potential differences in incentives between these treatment arms (grounded in the program design). In the appendix, we present the results of regression models that we estimated to provide further evidence that the exclusion restriction holds. Specifically, Table A.3 shows that if we control for "Believes Fining is a Punishment", we find that assignment to hard conditions no longer has a statistically significant association with the household belief that "No One is Checking if HHs are Following Rules". Furthermore, when we include these variables in our main

[3]As discussed in Section 4.4, some households in locations without hard conditions were, in fact, fined, but only in a few isolated cases

specifications along with the measure that indicates households believed that visits to health facilities for growth monitoring were required, we find that none of the three are statistically significant predictors in either the first or second stages. Because these variables do not explain any variation in the outcomes and only add noise to the coefficient estimates, we exclude them from our preferred specifications (main results).

Lastly, Panel E in Table 3 tests whether assignment to hard conditions is significantly related to a summary index of household perceptions and understanding of program rules that was created based on of all of these variables tested in the same table. The point estimate of the average difference in this scalar measure (between households with hard conditions vs. labeling) is small in magnitude and statistically insignificant. The index's distribution is also visibly similar between treatment arms, which we show in Figure 1. To summarize, the most apparent threat to the exclusion restriction in our IV strategy concerned how it might change the incentives of households to respond to or spend the cash transfers. Our analyses presented in this subsection demonstrate that, for the most part, households facing hard conditions had similar beliefs about program rules and penalties as did as households with labeling only, and when we control for the few exceptions, we find that they are small and statistically insignificant in the first and second stages of our impact estimation models. We submit that the exclusion restriction in our IV strategy is not violated and proceed with our analysis.

4.4 First-stage estimation

Our first-stage estimation confirms that assignment to hard conditions is a very strong, statistically significant predictor for a CT-OVC household being fined. This result follows from the fact (discussed above) that households in the hard conditions treatment arm were the only households that were intended to be fined for noncompliance with the program rules[4]. We present the results of our first-stage estimation, estimated using linear probability models (LPM), for our total household consumption (at follow-up) outcome model[5]. In the first LPM estimated, we omitted the interaction term (shown in equation 3, Section 4.1) from the first

[4]In practice, program administrators mistakenly fined a few households in the no conditions treatment arm as well. Despite such administrative errors, households in the hard conditions arm were still much more likely to experience a fine as measured in the follow-up survey

[5]Results are identical if we include baseline values for any of the other outcomes in our analysis

stage and only regressed being fined on assignment to hard conditions; this plainly shows the predictive power of the instrument. We present these results in Table 5. In the second LPM estimated, we add this interaction term back into the model and estimate equations (2) and (3), with results reported in Table 6. For both sets of estimations, we report the relevant test statistics.

Table 5, which contains the results for the first set of models, reports a very strong first stage. The point estimates of the coefficient on "Hard Conditions" indicate that being randomly assigned to the hard conditions state is associated with an increase in the likelihood of being fined between baseline and follow-up by around 33 percent. This estimate is highly stable across specifications and the addition of covariates, which we would expect given that households in the soft conditions arm were not supposed to be fined at all. Indeed, only about one percent of households assigned to soft conditions were fined sometime before the follow-up survey. Our preferred specifications cluster standard errors at sub-location level, which reflects the random assignment process for hard conditions (though our results are nearly identical when clustering at the district level). This produces F-statistics in columns (1)-(4) that are far above the rule-of-thumb threshold suggested by Staiger and Stock (1997) for avoiding weak instruments. However, since there are only 15 sublocations over which to cluster, one may be concerned that this number is too small to satisfy the conditions of the standard formula laid out in White (1984). In the interest of being as conservative as possible in our estimates of standard errors, we estimate them in column (5) using the wild bootstrap procedure detailed in Cameron, Gelbach, and Miller (2008). This procedure produces consistent estimates of clustered standard errors and allows the number of clusters to be as few as six. Although the procedure has difficulty precisely estimating t- and F-statistics for coefficients when the associated p-value is less than 0.001, the results indicate that a lower bound for the F-statistic on Hard Conditions is at most 10.48 (see table notes for details) when standard errors are clustered at either the sub-location or district level[6].

Next we estimate the first-stage models for our main specification. We present the key results from estimating equations (2) and (3) in Table 6. At the bottom of the table, we also

[6]These technical difficulties also imply that we cannot use the estimates from this bootstrap in the second stage, so it is purely for illustrative purposes

present the results from a variety of tests for weak instruments. Conducting these tests is more complicated when the structural equation contains more than one endogenous variable, -and in this case, we have two: getting fined and the interaction between being fined and baseline total household consumption. There is currently a lack of agreement in the literature about the best way to test for weak instruments in this context. The first attempt was made by Stock and Yogo (2005), who developed a set of critical values to use with the Cragg-Donald (CD) (1993) test statistic for assesing the strength of instruments. However, this procedure can only test the model as a whole for weak identification (rather than endogenous variable by endogenous variable), and it relies on the assumption of i.i.d. error terms. The Kleibergen-Paap (KP) Test rectified this latter deficiency with a statistic robust to conditionally heteroskedastic data. Even though this statistic allows for violations of the i.i.d. assumption of Stock and Yogo (2005), the KP statistic is typically evaluated relative to their critical values for determining weak identification. Angrist and Pischke (AP) (2009) subsequently developed a conditional F-statistic that, although similarly dependent on i.i.d. data, is able to test for the weak identification of individual endogenous variables when there are more than one of them. And more recently, Sanderson and Windmeijer (SW) (2016) improved on Angrist and Pischke's statistic by adjusting its asymptotic distribution, making it the preferred statistic for testing identification of a single endogenous variable in a model with at least two such variables. The SW test also uses the Stock and Yogo (2005) critical values.

In the single endogenous variable case, the typically cited rule-of-thumb for avoiding weak instruments is having an F-stat greater than 10 on the excluded instruments. We need a comparable cut-off value for the two-endogenous variable case, and Stock and Yogo (2005) provide two choices of criteria to select this value. The first is based on the relative bias of the 2SLS estimates compared to OLS. This is the authors' preferred criterion, but the critical values for the test statistic are only available for use when the model contains at least three excluded instruments. An alternative criterion is based on the maximal size of a 5% Wald test of the second stage coefficient estimates. Specifically, the cut-off value for the statistic corresponds to when the maximal size of the 5% Wald test is at most 15% (Stock and Yogo, 2005). The value depends on the number of endogenous variables, n, and number of excluded instruments, K_2. When $n = 1$ and $K_2 = 2$, the critical value for avoiding weak identification is 11.59. In our model, this is the relevant critical value for the SW statistic. Next, when $n = 2$ and $K_2 = 2$ the critical value for avoiding weak identification is 4.58. We use this critical value in conjunction with the CD and KP statistics. Stock and Yogo do not prefer using the size criterion over the one based on 2SLS bias because of how large the critical values become when the number of instruments are high (around 30). However, not only would this imply that these tests may be biased conservatively (if at all), given that we only use two IVs, we should be able to circumvent this problem.

The results from the first-stage estimations of our main specification are shown in Table 6. Columns (1) and (2) report the first stages with no control variables (except for household consumption in 2007). Columns (3) and (4) add in the rest of the controls, although they hardly alter the precision our estimates. When the standard errors are clustered at the sublocation level, as they are in our preferred specification and in Table 6, the statistics for the weak instruments tests are all estimated to be above their relevant thresholds (as denoted in the paragraph above). When the standard errors are clustered by district instead, the SW statistic for the models in columns (2) and (4) is 10.68, just below the cut-off of 11.59 for this statistic. However, this is most likely explained by the colinearity problem we discuss in the next paragraph. The rest of the test statistics remain above their relevant thresholds, which diminishes our concern, but one might interpret the results clustered by district with some additional caution.

We also want to address the fact that it may seem problematic that in columns (2) and (4), the coefficient on the interacted IV term is not statistically significant, and the dependent variable in those columns appears to be mostly identified by the variation from assignment to hard conditions. Tackling the first concern, the coefficient on the interacted IV term is likely only insignificant due to its high colinearity with the baseline consumption measure that is also (necessarily) included in the model. Indeed, the point estimates and standard errors on these two variables are very close. If we omit baseline consumption from these specifications and keep the interaction term, we find that the coefficient on the interaction has a t-statistic of over six. This most likely explains why the SW statistic on these estimations is low compared to its value in columns (1) and (3). However, because the inclusion of baseline consumption is important for identification of the marginal effects of being fined, we do not omit it in our main specification. We also do not see it as a problem that assignment to hard conditions affects the outcomes through both being fined and the interaction of being fined with baseline consumption. In fact, it makes intuitive sense, in that the dependent variable in columns (2) and (4) is mechanically related to being fined (as discussed earlier). We also know that assignment to hard conditions is not related baseline consumption by the results in Table 3. Thus, this implies that it is only through its correlation with being fined that assignment to hard conditions is also correlated with the interaction between being fined and baseline consumption. This suggests that, in an economic sense, the exclusion restriction still holds.

5 Impact Estimation and Results

In this section, we first undertake an intent-to-treat (ITT) analysis of assignment to the CT-OVC program (on all of our outcome measures) and conduct a marginal analysis based on baseline consumption. Next, we estimate a naive OLS model to measure the impact of being fined, which establishes a baseline for assessing potential bias in our model results. Finally, we estimate our second stage 2SLS (IV) models that tell us the impact of being fined on our household and child outcomes. We present two sets of results for the second stage, one with standard errors clustered at the sub-location level and the other with standards errors clustered at the district level. The two estimates are largely the same, with a few minor differences.

Before examining how getting fined affects consumption and dietary diversity, we must first show that being randomly assigned to receive cash transfers in the CT-OVC program affects these household outcomes. Recall that assignment to the cash transfer program was random at the sub-location level; we compare households randomly assigned to receive the cash transfer to those selected as controls at this level. Like the Kenya CT-OVC Evaluation Team (2012), we find that cash transfer receipt was associated with significant increases in both food and non-food consumption. However, while food consumption increased fairly evenly across the income distribution, the only statistically significant increases in non-food consumption were seen at the wealthier end of this distribution. On the contrary, with respect to the dietary diversity score, the only (marginally) significant effects of cash transfer receipt on this outcome were observed among the very poor. We also find no impact of the cash transfer on days missed form school by children. We present these estimated impacts in Table 6. These results clearly indicate that being assigned to receive the cash tranfer was associated with increased levels on consumption for everyone, although responses in terms of the types of consumption varied by wealth level.

Next we show the estimated impacts of being fined in the CT-OVC program from the naive OLS model (see Table 7). These results imply that, only except for the wealthiest of households in the sample, being fined is associated with reductions in consumption across the baseline consumption distribution. While food consumption is reduced roughly evenly across the five percentile groups, reductions in non-food consumption are most heavily concentrated at the lower end of the distribution. Lastly, there appear to be no impacts of being fined on the dietary diversity score or days missed from school for any income group. Of course, our rationale for pursuing an IV approach to this estimation is that we expect that these parameter estimates are likely biased due to the presence of unobserved confounders. For example, it could be that households that live further away from town centers with schools and medical centers have a harder time meeting program requirements, and these greater distances also make purchasing food more costly (leading them to purchase less of it). In such circumstances, the coefficient estimate of being fined in the naive OLS model may very well be biased, due to its correlation with distance from the town center (presumed to be negatively correlated with consumption).

To mitigate these concerns about omitted variable bias, we turn now to our 2SLS estimates of the impact of being fined. We begin with our preferred specification, the second stage estimates of which are given in Table 8. The most striking differences between these estimates and those of the OLS model is that getting fined is no longer negatively associated with the four outcomes across all income groups. In fact, comparatively wealthier households experienced increases in food consumption and dietary diversity of quite high magnitudes after being fined. Indeed, getting fined appears to have led households in the 90th percentile of baseline consumption to add four additional food groups to their diet. Relatively poorer households still faired quite badly, although it appears they tried to bear the burden of the fines mostly through reductions in nonfood consumption. Getting fined was associated with with steep decreases in non-food consumption among the poorer households, and negative, but statistically insignificant, changes in food consumption. These effects can be seen graphically in Figures [2]-[6]. When standard errors are clustered by district, as the are in Table 9, the results are largely the same. More generally, the implications seem to be that households across the distribution attempt to respond to the punitive action of being fined, but poorer households, with less capacity to maintain or improve the quality of food consumption, suffer harsher consequences that they bear largely in nonfood consumption reductions.

The results of our 2SLS estimation of the impact of being fined on children's absences from school (see Table 8) do not identify any statistically significant impacts of the financial penalties on this schooling outcome. This suggests that the nonfood consumption reductions experienced by the relatively poorer families who were fined were unlikely to have pertained to items that affected children's ability to get to school. This finding is consistent not only with the heavy emphasis and labeling of the CT-OVC as a program to support the education and welfare of orphans in the household, but also with the households' apparent efforts to buffer the impacts of the fines on household consumption of necessities like food and nutrition. At the same time, our analysis only spans a two-year period (between baseline and follow-up), and thus, we do not claim that there would be no longer-term effects on children's educational attainment or related outcomes if poorer households continued to suffer penalties and additional budgetary constraints.

6 Conclusion

In a 2013 blog post[7], 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 the Kenya OVC-CT program shows that where it fits along a continuum from fully

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

unconditional to "hard" conditions may depend on the implementation of the program as experienced 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 hard conditions in the Kenya CT-OVC program–i.e., a "heavy-handed" implementation of CCTs that monetarily penalized families for their failure to comply with program conditions–had tradeoffs for the well-being of targeted families, depending on their baseline poverty or wealth. One of the more compelling aspects of our estimates showing that the consumption of poorer households may be harmfully reduced, while that of better-off families may be improved through the imposition of fines, 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. Our findings show that when comparatively wealthier households get fined for noncompliance, not only do they have more cash on hand to circumvent any immediate consumptions losses that the fine might present, but they also use these resources to undertake what appear to be preventative measures to avoid being fined in the future. While providing adequate food and nutrition was not an explicit program rule of the CT-OVC program that was punishable (if violated) by a fine, over 70 percent of households assigned to hard conditions thought that it was a formal requirement. In fact, far more household respondents believed that it was a program rule than the number who knew the correct rules (about schooling and health conditions), as we showed in Table 3 in section 4.3. We argue that this gives credibility to the noticeable pivot toward higher food consumption and diet diversity among households who had the resources to respond accordingly after receiving a fine.

On the other hand, researchers and practitioners have long been concerned about the undue burdens that conditional cash transfers place on the poorest of the poor. Not only is complying with rules more challenging for them, but penalizing their transfers may cut them off from purchasing the most basic of necessities that their more meager budgets afford. Regrettably, this is what appears to have happened in the case of the Kenya OVC-CT program, where poorer households experienced lasting decreases in consumption after getting fined. We found that, for comparatively poor households, getting fined is associated with a decrease in consumption at follow-up of about 500 KSH (about 1/3 of the size of the transfer), estimated to equal about 25% of average monthly (total) consumption. In this particular case, it appears that while imposing monetary penalties for not following program rules provided some additional motivation for comparatively wealthier households to use their resources to the greater benefit of their household members, it came at the cost of regressive impacts on the very poor that may have contributed to lasting harms.

Surprisingly, given the expansive literature that has emerged over time on CCTs and UCTs (and now 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 Kenya CT-OVC program evaluation design–and the implementation of the program that supported our confidence in the exclustion restriction, which is critical to our identification of impacts of being fined–may have allowed us a unique opportunity to examine the consequences of being fined in terms of household and children's outcomes. That said, our study is not without limitations. We are only examining the impact of fines on households within a two-year window of program implementation, and we do not have detailed data to identify the frequency or timing of penalties that households experienced in this program. 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. Lastly, while we believe that we have presented compelling evidence to argue that our identification of the impacts of being fined in the Kenya CT-OVC program are plausibly causal, we tip our hat to the statistician, George Box (1976: 792), who articulated the view that "all models are wrong."

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Appendix

Figure 1

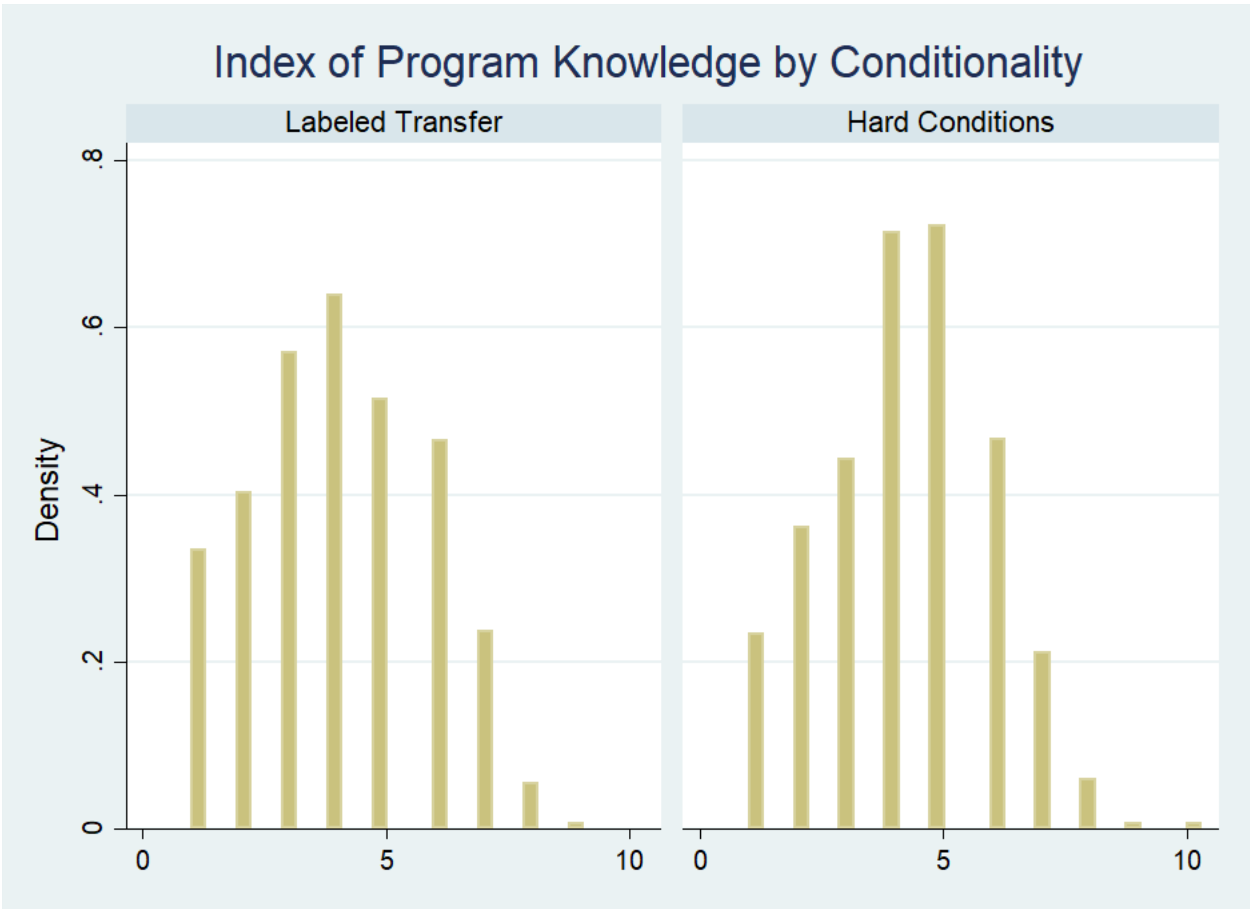


Table 1: (A)

Kenya CT-OVC Program Evaluation (2009) Household Survey Questions on

Knowledge of Program Rules and Conditions - Part 1

Survey questions	Scalar measure components
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	Indicator variable =1 if CT-OVC families believe the program has rules
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	Indicator variables (=1) for each rule CT-OVC families correctly believe they have to follow, in particular: - conditions for school enrollment and attendance in primary schools (B) - conditions for going to health facilities for immunizations and health monitoring (C-E) - conditions for adequate food and nutrition for children (F) - requirements to attend community awareness sessions and present a birth certificate for children (H, I)
Do you know what will happen if cash transfer families do not follow the rules? 1 = Yes 2 = No	Indicator variable: CT-OVC families believe there are consequences if they do not follow the rules
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 ___________________________	Indicator variables (=1) if CT-OVC families know what the consequences are if they do not follow the rules, specifically: - could get kicked out (2) - could pay a penalty fine (4 and 5)
Is anyone checking to see if cash transfer families are following the rules? 1 = Yes 2 = No 98 = Don't know	Indicator variable (=1) if CT-OVC families believe that someone is checking on compliance with the rules

Table 1: (B)

Kenya CT-OVC Program Evaluation (2009) Household Survey Questions on

Knowledge of Program Rules and Conditions - Part 2

Survey questions	Scalar measure components
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	Indicator variables (=1) for each correct rule, specifically: - A, B, C, D, E and F
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	

Table 2: Balance Table of Baseline Household Characteristics

	(1) Soft Conditions	(2) Hard Conditions	(3) Difference
Years of Edu. of HH Head 2007	5.820	6.043	0.251
	(2.727)	(3.143)	(0.495)
Sex of HH Head	0.353	0.340	-0.013
	(0.478)	(0.474)	(0.024)
HH Receives Labor Wages 2007	0.044	0.025	-0.016
	(0.206)	(0.157)	(0.024)
HH Owns Livestock in 2007	0.823	0.752	-0.078
	(0.382)	(0.432)	(0.128)
Acres of Land Owned 2007	1.420	2.055	0.622
	(1.926)	(4.907)	(0.481)
Household in Rural Location	0.882	0.761	-0.130
	(0.323)	(0.427)	(0.188)
HH Cons. Per Adult Equiv. 2007 (KSh)	1.649	1.526	-0.124
	(1.018)	(0.878)	(0.134)
Dietary Diversity Score 2007	4.975	5.300	0.331
	(1.506)	(1.500)	(0.297)
Size of the HH in 2007	5.456	5.550	0.103
	(2.386)	(2.953)	(0.526)
People Aged 0-5 in HH 2007	1.600	1.708	0.102
	(0.901)	(1.128)	(0.137)
People Aged 6-11 in HH 2007	1.645	1.760	0.114
	(0.814)	(0.982)	(0.092)
People Aged 12-17 in HH 2007	1.736	1.738	0.003
	(0.961)	(0.961)	(0.083)
People Aged 18-45 in HH 2007	1.874	2.030	0.170
	(1.054)	(1.313)	(0.242)
People Aged 46-64 in HH 2007	1.111	1.100	-0.011
	(0.314)	(0.300)	(0.020)
People Aged 65+ in HH 2007	1.129	1.105	-0.024
	(0.350)	(0.308)	(0.032)
Observations	634	476	1,110

[1] Standard deviations in parentheses in columns (1) and (2). Standard errors, clustered at sub-location level, in parentheses in column (3). * p < .01, ** p < .05, *** p < .01

[2] Consumption is in terms of 1000 KSh.

Table 3: Placebo Tests Justifying Excludibility of 'Hard Conditions'

	(1) Soft Conditions	(2) Hard Conditions	(3) Controlled Diff.	(4) Obs.
Panel A: Understanding of Program Rules				
Enrollment/Attendance in Primary or Secondary School	0.290	0.307	0.004	1110
	(0.454)	(0.462)	(0.039)	
Visit Health Facility for Immunizations	0.155	0.221	0.050	1110
	(0.362)	(0.415)	(0.032)	
Visit Health Facility for Growth Monitoring	0.091	0.149	0.059**	1110
	(0.289)	(0.357)	(0.027)	
Visit Health Facility for Vitamin A Supplement	0.058	0.057	-0.003	1110
	(0.235)	(0.232)	(0.020)	
Adequate Food and Nutrition for Children	0.599	0.718	0.113	1110
	(0.490)	(0.450)	(0.088)	
Attendance at Program Awareness Sessions	0.044	0.076	0.031	1110
	(0.206)	(0.265)	(0.020)	
Panel B: Perceived Likelihood of Punishment				
Believes HH Must Follow Rules to Receive Payments	0.733	0.901	0.150*	1110
	(0.443)	(0.299)	(0.084)	
Believes No One is Checking if HHs are Following Rules	0.420	0.490	0.078	893
	(0.494)	(0.500)	(0.076)	
Panel C: Understanding of Punishments				
Believes Fining is a Punishment	0.050	0.218	0.176***	1110
	(0.219)	(0.414)	(0.020)	
Believes HHs can be Ejected from Program for Disobedience	0.431	0.445	0.002	1110
	(0.496)	(0.498)	(0.065)	
Claims to Know Specific Criteria for Ejection from Program	0.461	0.500	0.027	1110
	(0.499)	(0.501)	(0.058)	
Panel D: Understanding of Ejection Criteria				
HH has no OVCs Below 18 Years Old	0.142	0.118	-0.023	1110
	(0.349)	(0.323)	(0.027)	
At Least One Program Rule is Ignored for Three Consecutive Pay Periods	0.188	0.279	0.096	1110
	(0.391)	(0.449)	(0.073)	
HH Moves to Non-Program District	0.019	0.004	-0.014	1110
	(0.136)	(0.065)	(0.009)	
HH Does Not Collect Transfer for Three Consecutive Pay Periods	0.011	0.013	0.001	1110
	(0.105)	(0.112)	(0.005)	
Panel E: Summary Test				
Index of Knowledge and Understanding[3]	4.011	4.231	0.247	893
	(1.828)	(1.738)	(0.223)	

[1] Standard errors in parentheses, * p < .1, ** p < .05, *** p < .01, † p < .001 .
[2] Standard errors clustered at the sub-location level. If clustered at district level, all differences become significance except for that of "Knows About Fining as Punishment".
[3] This index is an unweighted linear combination of all of the above variables, except for "Adequate Food and Nutrition for Children", which means its support is from 0 to 14.

Table 4: Effect of Controlling for "Believes Fining is a Punishment" on the Relationship Between Hard Conditions and Belief About Following Rules

Dep. Variable:	Believes HH Must Follow Rules to Receive Payments
Hard Conditions	0.120 (0.0807)
Believes Fining is a Punishment	0.175*** (0.0443)
Assigned to Transfer	-0.285*** (0.0904)
HH Carer Age Score 2007	1.070** (0.482)
Carer Age Score Sq	-0.898** (0.383)
Total Chronically Ill in HH 2007	0.0629 (0.0528)
Sex of HH Head	0.0338 (0.0312)
HH Receives Labor Wages 2007	0.0701 (0.0814)
HH Cons. Per Adult Equiv. 2007 (KSh)	0.00489 (0.0153)
HH Owns Livestock 2007	-0.0981** (0.0410)
Acres of Land Owned 2007	0.00803 (0.00764)
Household is in Rural Location	0.0390 (0.0763)
Size of the HH 2007	-0.00703 (0.00663)
Constant	0.790† (0.0928)
Observations	1110

[1] Standard errors in parentheses, * p < .1, ** p < .05, *** p < .01, † p < .001 . Standard Errors clustered at the sub-location level and robust to clustering at district level.
[2] Consumption is in terms of 1000 KSH.

Table 5: Impact of Assignment to Hard Conditions on Likelihood of Being Fined

	(1)	(2)	(3)	(4)	(5)
Hard Conditions	0.338†	0.338†	0.338†	0.341†	0.341†
	(0.0195)	(0.0196)	(0.0274)	(0.0275)	(.)[3]
Assigned to Transfer		0.0111	0.0111*	0.0249	0.0249
		(0.132)	(0.0055)	(0.0464)	(0.0511)
HH Carer Age Score 2007				0.356*	0.356*
				(0.189)	(0.198)
Carer Age Score Sq				-0.381*	-0.381*
				(0.198)	(0.206)
Total Chronically Ill in HH 2007				0.0180	0.0180
				(0.0310)	(0.0289)
Sex of HH Head				-0.0156	-0.0156
				(0.0134)	(0.0132)
HH Receives Labor Wages 2007				0.101	0.101
				(0.0654)	(0.0798)
HH Cons. Per Adult Equiv. 2007 (KSh)				-0.00587	-0.00587
				(0.0103)	(0.0118)
HH Owns Livestock 2007				-0.0112	-0.0112
				(0.0244)	(0.0287)
Acres of Land Owned 2007				-0.00269	-0.00269
				(0.00214)	(0.00328)
Household is in Rural Location				0.0397	0.0397
				(0.0655)	(0.0854)
Size of the HH 2007				0.00803*	0.00803*
				(0.00395)	(0.00436)
Constant	0.0105	-1.39e-14	-1.39e-14	-0.138**	-0.138**
	(0.0115)	(0.122)	(.)	(0.0564)	(0.0664)
Observations	1321	1241	1241	1110	1110
F	347.6	341.51	133.46	153.24	.[3]

[1] Standard errors in parentheses, * p < .1, ** p < .05, *** p < .01, † p < .001 . Consumption is in terms of 1000 KSh.

[2] Standard errors clustered at the sub-location level in columns (3) and (4). Standard errors in column (5) are clustered using the wild bootstrap procedure from Cameron, Gelbach, and Miller (2008). Results are robust to clustering at district level.

[3] The F-statistic and standards errors on Hard Conditions in column (5) cannot be reported since Cameron, Gelbach, and Miller's wild bootstrap procedure cannot precisely estimate a t-statistic when the associated p-value is less than 0.001. However, a p-value of <0.001 translates to a t-statistic of >3.3, which has an associated F-statistic of greater than 10.89 at 1101 degrees of freedom . This passes the standard rule-of-thumb that the F-statistic on the excluded instruments should be at least 10 to be considered valid. This may be taken as a strict lower-bound for the F-statistic on Hard Conditions.

Table 6: Impact of Assignment to Cash Transfer on Outcomes

Dep. Variable:	(1) HH Cons. 2009	(2) HH Food Cons. 2009	(3) HH Non-food Cons. 2009	(4) Dietary Div. Score 2009	(5) Days Missed 2009
Panel A: Average Effects					
Assigned Transfer	0.375**	0.251**	0.116	0.713*	-0.0947
	(0.168)	(0.0935)	(0.103)	(0.353)	(0.347)
HH Cons. 2007	0.204***	0.0304	-0.0145	0.190*	0.0547
	(0.0666)	(0.0476)	(0.0468)	(0.104)	(0.182)
Assigned Transfer × HH Cons. 2007	-0.0296	-0.0321	0.00768	-0.239**	-0.178
	(0.0730)	(0.0471)	(0.0421)	(0.104)	(0.190)
Panel B: Marginal Effects by HH Cons. 2007					
Percentile: 10	0.355**	0.229***	0.121	0.551*	-0.215
	(0.131)	(0.069)	(0.081)	(0.296)	(0.254)
Percentile: 25	0.346***	0.220***	0.123	0.483*	-0.266
	(0.118)	(0.061)	(0.073)	(0.274)	(0.225)
Percentile: 50	0.333***	0.206***	0.126*	0.376	-0.346
	(0.103)	(0.053)	(0.064)	(0.243)	(0.201)
Percentile:75	0.317***	0.188**	0.130**	0.246	-0.443
	(0.098)	(0.054)	(0.058)	(0.214)	(0.219)
Percentile: 90	0.292**	0.162**	0.137**	0.049	-0.589
	(0.118)	(0.075)	(0.066)	(0.193)	(0.316)
Controls	X	X	X	X	X
Observations	1555	1555	1555	1555	2768

[1] Standard errors in parentheses, * p < .1, ** p < .05, *** p < .01, † p < .001 . Standard clustered at the sub-location level.
[2] The standard errors on the marginal analysis are estimated via the delta-method applied to the formula applied of the response and variance-covariance estimator of the preceding estimation.
[3] Household Consumption is measured in terms of 1000 Kenyan Shillings (KSh) per adult-equivalent.

Table 7: Impact of Being Fined, OLS

Dep. Variable:	(1) HH Cons. 2009	(2) HH Food Cons. 2009	(3) HH Non-food Cons. 2009	(4) Dietary Div. Score 2009	(5) Days Missed 2009
Fined	-0.471†	-0.169**	-0.301***	-0.253	0.221
	(0.0964)	(0.0642)	(0.0888)	(0.457)	(0.467)
HH Cons. 2007	0.164***	0.000708	-0.0246	-0.0565	-0.0480
	(0.0477)	(0.0609)	(0.0265)	(0.0713)	(0.0690)
Fined × HH Cons. 2007	0.118**	0.0150	0.102**	0.0939	-0.161
	(0.0533)	(0.0451)	(0.0425)	(0.213)	(0.222)
Panel B: Marginal Effects by HH Cons. 2007					
Percentile: 10	-0.392†	-0.159***	-0.232***	-0.190	0.113
	(0.072)	(0.045)	(0.064)	(0.328)	(0.344)
Percentile: 25	-0.359†	-0.155***	-0.203***	-0.164	0.069
	(0.065)	(0.041)	(0.055)	(0.278)	(0.299)
Percentile: 50	-0.305†	-0.148***	-0.157***	-0.121	-0.006
	(0.061)	(0.043)	(0.043)	(0.207)	(0.242)
Percentile: 75	-0.240***	-0.140**	-0.101**	-0.070	-0.094
	(0.068)	(0.056)	(0.038)	(0.164)	(0.221)
Percentile: 90	-0.0143	-0.127	-0.017	0.007	-0.226
	(0.095)	(0.086)	(0.054)	(0.232)	(0.299)
Controls	X	X	X	X	X
Observations	1110	1110	1110	1110	1903

[1] Standard errors in parentheses, * p < .1, ** p < .05, *** p < .01, † p < .001. Standard errors clustered at the sub-location level.
[2] The standard errors on the marginal analysis are estimated via the delta-method applied to the formula applied of the response and variance-covariance estimator of the preceding estimation.
[3] Household Consumption is measured in terms of 1000 Kenyan Shillings (KSh) per adult-equivalent.

Table 8: Impact of Being Fined, Second Stage - SEs Clustered at Sub-District Level

Dep. Variable:	(1) HH Cons. 2009	(2) HH Food Cons. 2009	(3) HH Non-food Cons. 2009	(4) Dietary Div. Score 2009	(5) Days Missed 2009
Panel A: Average Effects					
Fined	-1.430*	-0.612	-0.809**	-1.379**	-1.045
	(0.735)	(0.413)	(0.398)	(0.682)	(0.884)
HH Cons. 2007	0.0555	-0.0804	-0.0603	-0.285	-0.179*
	(0.0716)	(0.0606)	(0.0438)	(0.206)	(0.100)
Fined × HH Cons. 2007	0.903*	0.551**	0.348	1.938***	0.613
	(0.492)	(0.280)	(0.292)	(0.724)	(0.633)
Panel B: Marginal Effects by HH Cons. 2007					
Percentile: 10	-0.825*	-0.243	-0.576**	-0.082	-0.634
	(0.446)	(0.255)	(0.231)	(0.481)	(0.527)
Percentile: 25	-0.573	-0.089	-0.479***	0.459	-0.464
	(0.348)	(0.205)	(0.180)	(0.522)	(0.416)
Percentile: 50	-0.161	0.162	-0.320**	1.345*	-0.184
	(0.274)	(0.174)	(0.162)	(0.718)	(0.371)
Percentile: 75	0.333	0.463*	-0.130	2.405**	0.152
	(0.390)	(0.243)	(0.256)	(1.045)	(0.562)
Percentile: 90	1.073	0.915**	0.154	3.994**	0.654
	(0.734)	(0.436)	(0.469)	(1.597)	(1.015)
Controls	X	X	X	X	X
Observations	1110	1110	1110	1110	1903

[1] Standard errors in parentheses, * p < .1, ** p < .05, *** p < .01, † p < .001. Standard errors clustered at the sub-location level.

[2] The standard errors on the marginal analysis are estimated via the delta-method applied to the formula applied of the response and variance-covariance estimator of the preceding estimation.

[3] Household Consumption is measured in terms of 1000 Kenyan Shillings (KSh) per adult-equivalent.

Table 9: Impact of Being Fined, Second Stage - SEs Clustered at Sub-District Level

Dep. Variable:	(1) HH Cons. 2009	(2) HH Food Cons. 2009	(3) HH Non-food Cons. 2009	(4) Dietary Div. Score 2009	(5) Days Missed 2009
Fined	-1.430**	-0.612**	-0.809**	-1.379	-1.045
	(0.604)	(0.286)	(0.328)	(0.842)	(0.781)
HH Cons. 2007	0.0555	-0.0804**	-0.0603	-0.285	-0.179
	(0.0620)	(0.0367)	(0.0401)	(0.250)	(0.119)
Fined × HH Cons. 2007	0.903***	0.551†	0.348	1.938**	0.613
	(0.329)	(0.109)	(0.238)	(0.907)	(0.576)
Panel B: Marginal Effects by HH Cons. 2007					
Percentile: 10	-0.825*	-0.243	-0.576**	-0.082	-0.634
	(0.415)	(0.225)	(0.202)	(0.614)	(0.439)
Percentile: 25	-0.573	-0.089	-0.479***	0.459	-0.464
	(0.347)	(0.202)	(0.166)	(0.675)	(0.326)
Percentile: 50	-0.161	0.162	-0.320**	1.345*	-0.184
	(0.270)	(0.169)	(0.158)	(0.929)	(0.277)
Percentile: 75	0.333	0.463*	-0.130	2.405**	0.152
	(0.274)	(0.144)	(0.231)	(1.342)	(0.473)
Percentile: 90	1.073	0.915**	0.154	3.994**	0.654
	(0.448)	(0.148)	(0.398)	(2.032)	(0.903)
Controls	X	X	X	X	X
Observations	1110	1110	1110	1110	1903

[1] Standard errors in parentheses, * p < .1, ** p < .05, *** p < .01, † p < .001. Standard errors clustered at the district level.
[2] The standard errors on the marginal analysis are estimated via the delta-method applied to the formula applied of the response and variance-covariance estimator of the preceding estimation.
[3] Household Consumption is measured in terms of 1000 Kenyan Shillings (KSh) per adult-equivalent.

Figure 2

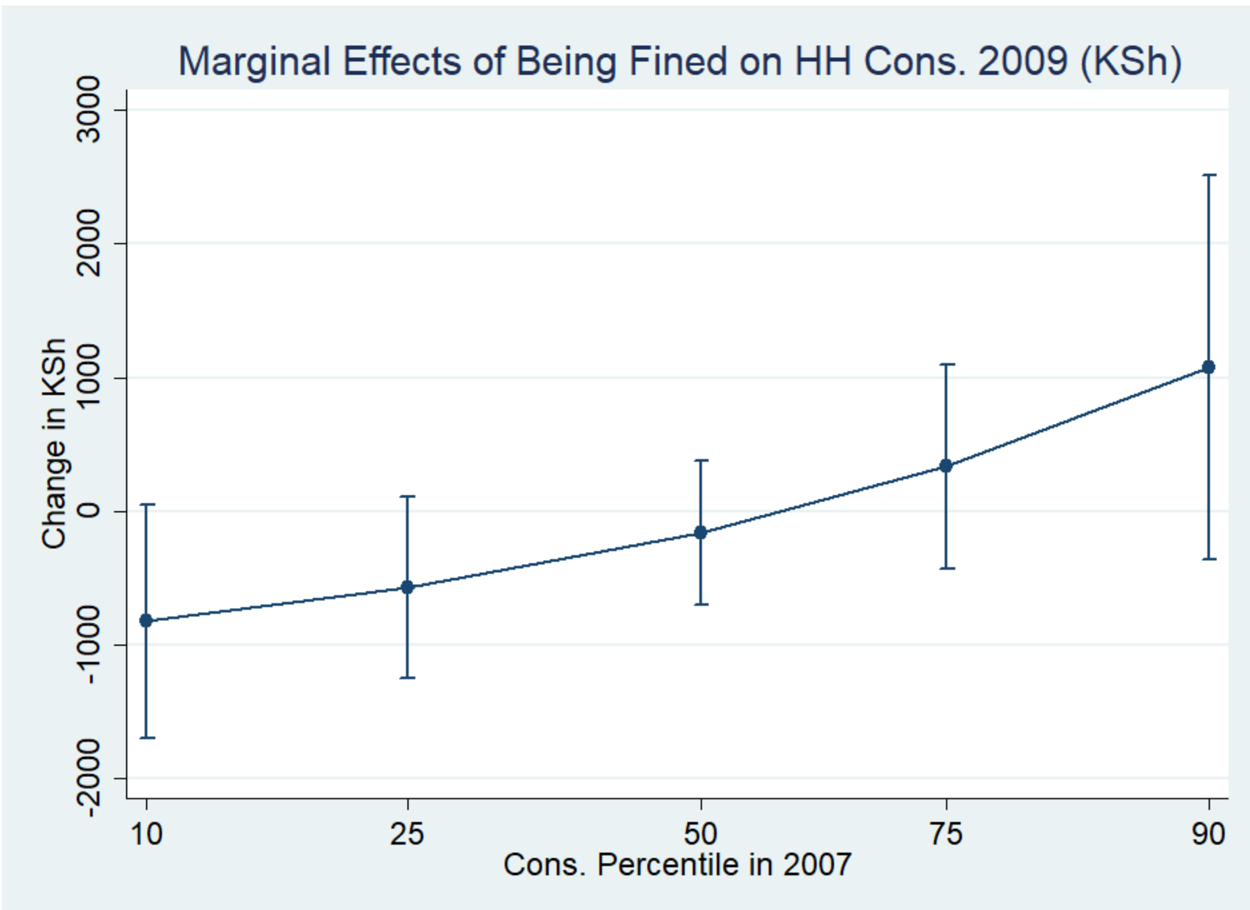

Marginal Effects of Being Fined on HH Cons. 2009 (KSh)
Change in KSh
-2000
-1000
0
1000
2000
3000
10
25
50
75
90
Cons. Percentile in 2007

Figure 3

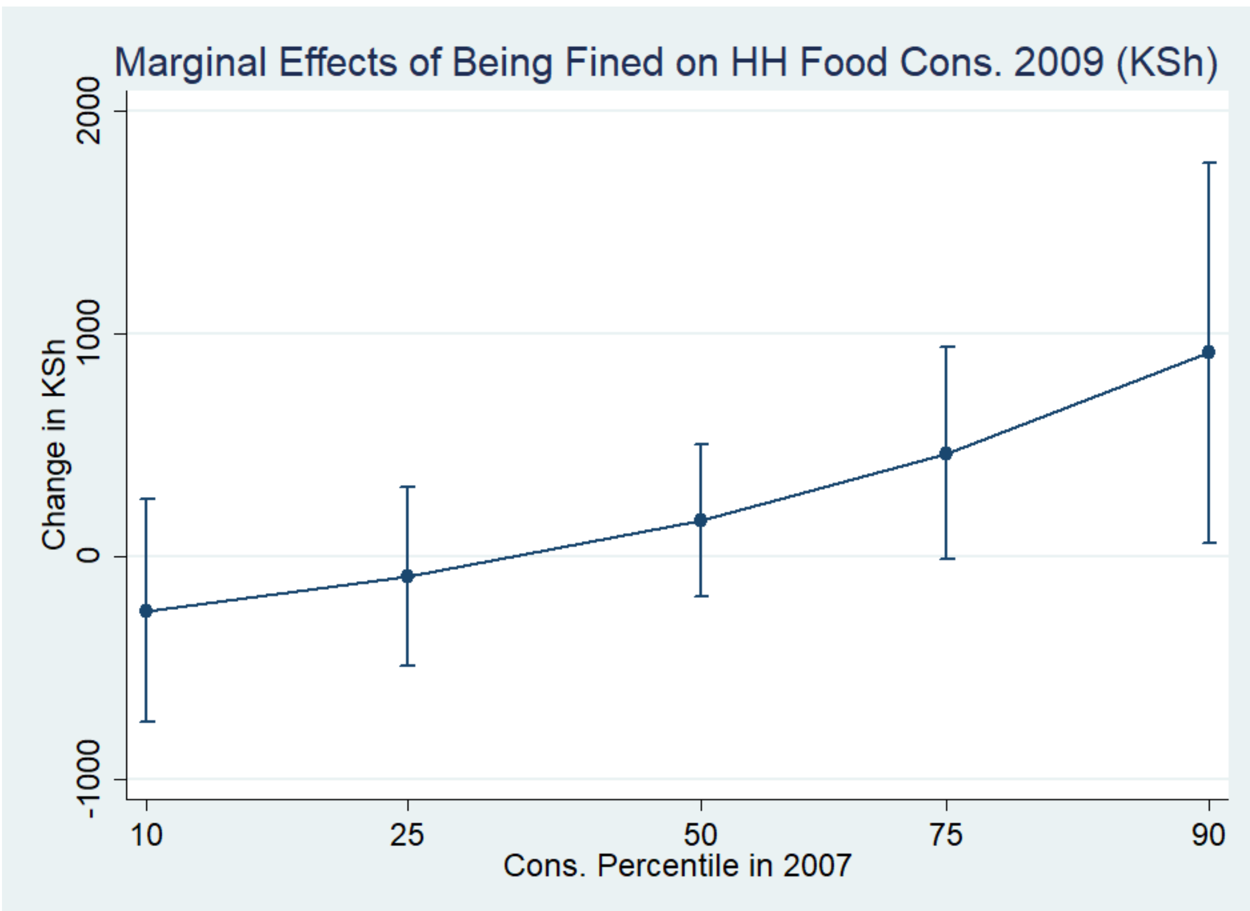
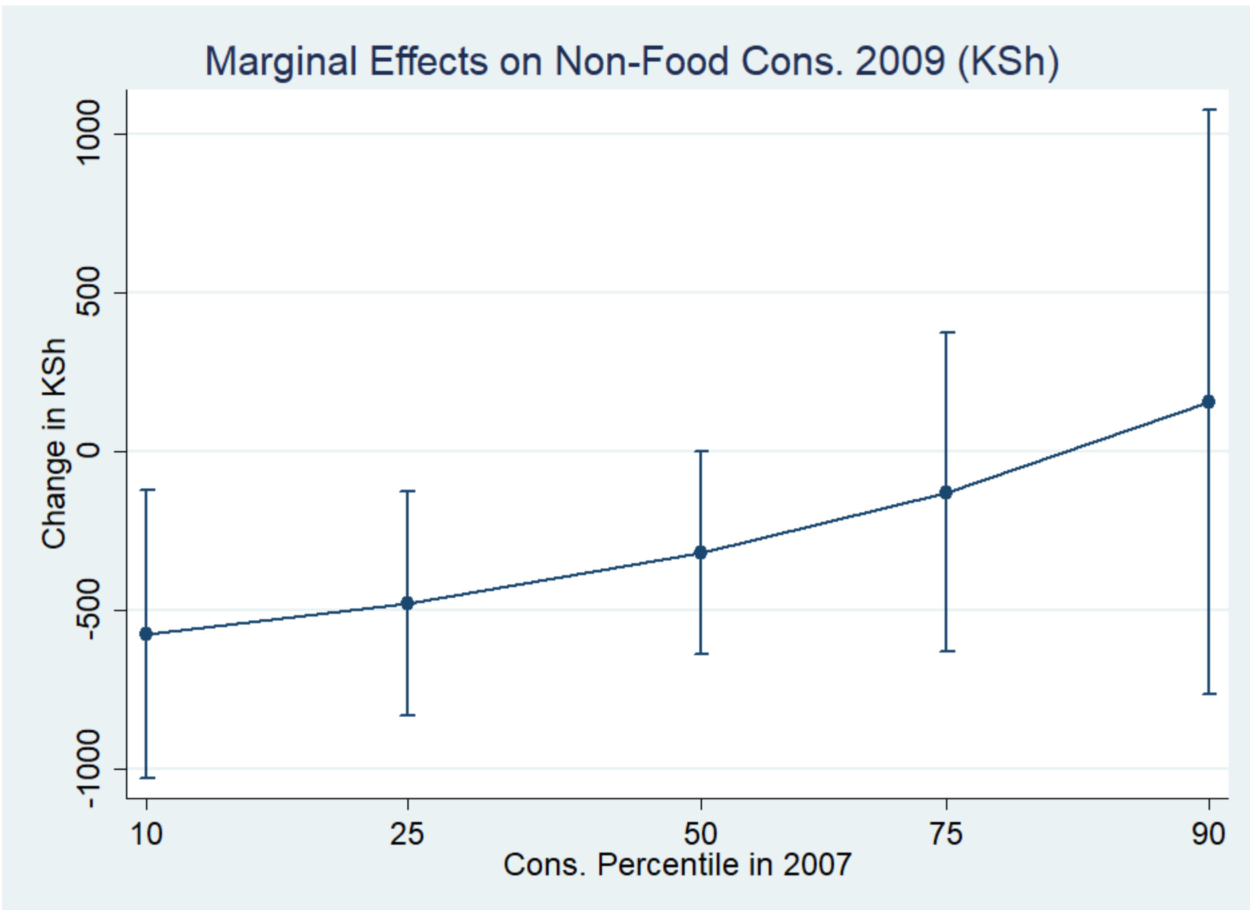

Marginal Effects of Being Fined on HH Food Cons. 2009 (KSh)
Change in KSh
-1000
0
1000
2000
10
25
50
75
90
Cons. Percentile in 2007

Figure 4



Marginal Effects on Non-Food Cons. 2009 (KSh)
Change in KSh
1000
500
0
-500
-1000
10
25
50
75
90
Cons. Percentile in 2007

Figure 5

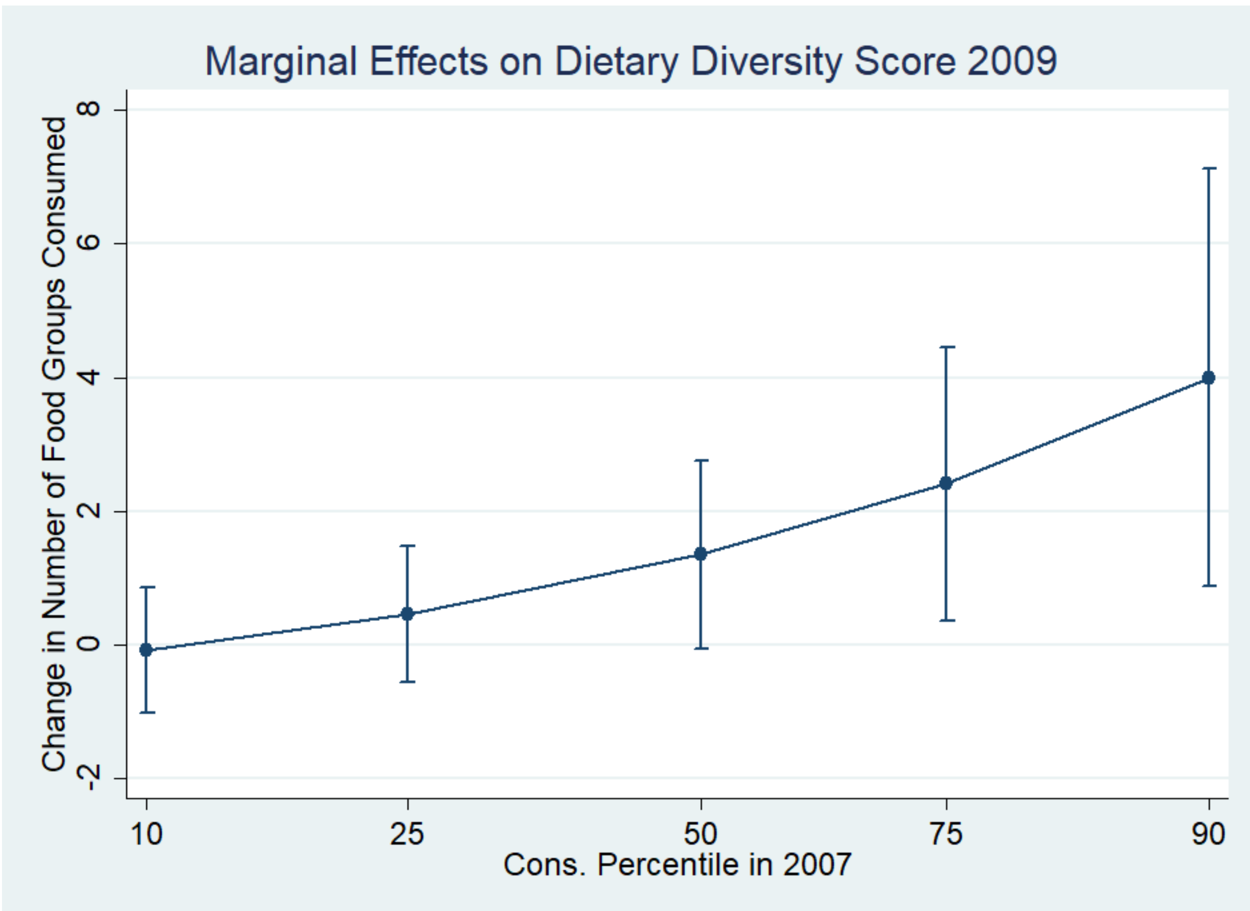
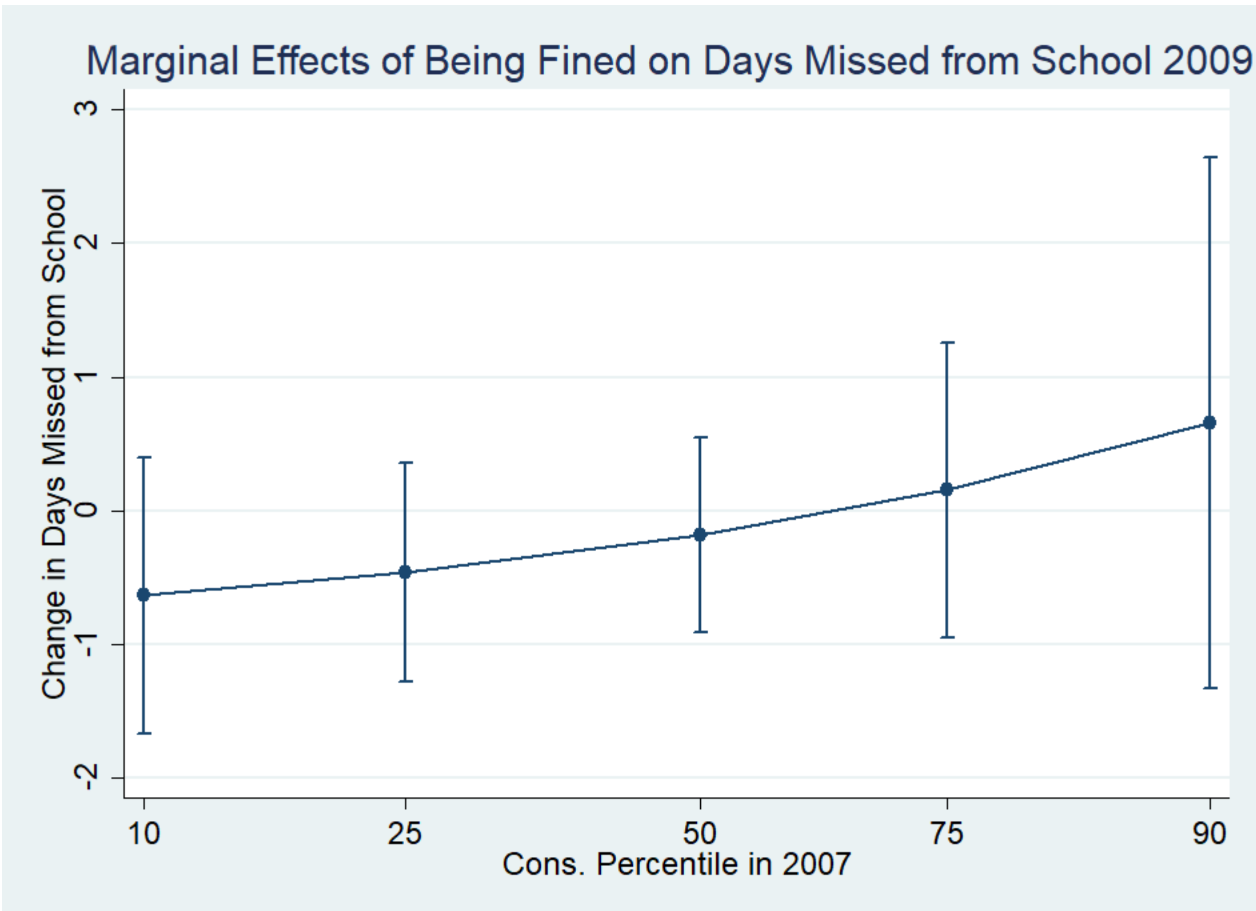

Marginal Effects on Dietary Diversity Score 2009
Change in Number of Food Groups Consumed
-2
0
2
4
6
8
10
25
50
75
90
Cons. Percentile in 2007

Figure 6



Marginal Effects of Being Fined on Days Missed from School 2009
Change in Days Missed from School
3
2
1
0
-1
-2
10
25
50
75
90
Cons. Percentile in 2007

Table A.1

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.

Table A.2: Balance Table Across Treated and Control Groups

	(1) Control	(2) Treatment	(3) Difference
Sex of HH Head	0.413	0.347	-0.067**
	(0.493)	(0.476)	(0.028)
HH Receives Labor Wages 2007	0.079	0.034	-0.044
	(0.269)	(0.182)	(0.033)
HH Owns Livestock in 2007	0.793	0.796	0.003
	(0.405)	(0.403)	(0.079)
Acres of Land Owned 2007	2.318	1.703	-0.615
	(6.422)	(3.548)	(0.496)
Household in Rural Location	0.804	0.833	0.029
	(0.397)	(0.373)	(0.128)
HH Cons. Per Adult Equiv. 2007 (KSH)	1.634	1.602	-0.032
	(0.982)	(0.976)	(0.129)
Dietary Diversity Score 2007	5.530	5.116	-0.414
	(1.442)	(1.512)	(0.245)
Size of the HH in 2007	5.780	5.486	-0.293
	(2.473)	(2.640)	(0.296)
People Aged 0-5 in HH 2007	1.668	1.651	-0.017
	(0.817)	(1.009)	(0.087)
People Aged 6-11 in HH 2007	1.797	1.692	-0.105
	(0.941)	(0.885)	(0.097)
People Aged 12-17 in HH 2007	1.698	1.738	0.040
	(0.833)	(0.962)	(0.052)
People Aged 18-45 in HH 2007	1.960	1.926	-0.034
	(1.199)	(1.160)	(0.124)
People Aged 46-64 in HH 2007	1.147	1.105	-0.042
	(0.355)	(0.307)	(0.029)
People Aged 65+ in HH 2007	1.175	1.118	-0.058
	(0.382)	(0.330)	(0.041)
Observations	445	1,110	1,555

[1] Standard deviations in parentheses in columns (1) and (2). Standard errors, clustered at sub-location level, in parentheses in column (3). * p < .01, ** p < .05, *** p < .01

[2] Consumption is in terms of 1000 KSh.