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Nudging, Teaching, or Coercing?: A Review of Conditionality Compliance Mechanisms on School Attendance Under Conditional Cash Transfer Programs

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Abstract

Conditional cash transfer programs are widely used and widely researched social protection systems deployed in developing countries. A notable, yet important, gap in research on this topic arises from limited consideration over the role of conditionality compliance mechanisms in improving health and education outcomes, especially given the administrative and financial costs associated with monitoring and enforcement systems, which face significant operational constraints in African nations. This paper looks specifically at primary and secondary school attendance rates under 27 and 24 programs respectively, and uses random-effects meta-regression techniques to determine, all else constant, the contribution of heterogeneity in compliance mechanisms to variation in attendance rates. The results of this exercise show a highly statistically significant 1.8% increase in primary attendance per additional category of severity, with insignificant coefficients for secondary attendance. Baseline enrolment figures and presence of supply components are also found to be highly significant. Programs that use workshops and information sessions are identified as being the most appropriate choice for African nations. African nations, by introducing CCTs, are also predicted to experience 3.1% and 11.7% gains in primary and secondary attendance, as they have low baseline enrolment figures.

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List of Abbreviations, Figures, and Tables

Abbreviations

CCT – conditional cash transfer
LAC – Latin America or Caribbean (dummy)
LCT – labelled cash transfer
MDGs – Millennium Development Goals
NGO – non-governmental organisation
OLS – ordinary least squares (regression)
PRAF and RPS – CCT program acronyms
RCT – randomised controlled trial
REMR – random effects meta-regression
SDGs – Sustainable Development Goals
UCT – unconditional cash transfer

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1.Introduction

Development theorists and practitioners alike are dubious of the existence of ‘panaceas’ in the fight against poverty and underdevelopment. But if there were one tool in the public sector arsenal to come close to this status, it would likely be the conditional cash transfer (CCT). CCTs are government programs in developing countries that identify poor households and deliver cash transfers to alleviate poverty. The transfers differ from other social assistance programs by conditioning payments on changing behaviours and human capital investments of households’ in the health and education of their children; mothers must attend pre and post-natal clinic appointments, and children must be immunised, enrolled in school, and achieve a minimum attendance rate. CCTs have today spread to over 80 countries worldwide, including 14 in Africa, where administrators seek guidance setting up programs of their own from managers in Brazil and Mexico (Parker and Vogl, 2018). But two of their typical features - information and enforcement systems - are very expensive and capacity-intensive, and raise questions around the transplanting of Latin American program design to the African context. The merits of simpler and cheaper programs must be explored.

Prior to the mid 1980’s, education had been seen mainly in line with the Mincer earnings function (Mincer, 1958); as a highly personally remunerative investment, but with little value beyond the individual, since the Solow model orthodoxy at the time saw economic growth as a function only of technological progress and basic inputs (Solow, 1956). But the rise of endogenous growth theory (Romer, 1986; Lucas, 1988) placed a new and paramount emphasis on the role of education in aggregate economic outcomes by proposing that knowledge spillovers and externalities associated with higher human capital had lasting benefits through the increased generation, assimilation and spread of ideas and skills within a population. They argue that growth is therefore, in part, a function of education policy. This emphasis and orthodoxy on the economic returns to supply-side education investments largely persists today, shored up by the MDGs and SDGs, domestic social policy agendas, NGO work, and donor preferences (McCowan and Unterhalter, 2015). But this emphasis doesn’t consider the flipside - what if demand for education is low, because household incentives aren’t aligned with human capital investments? Conditionalities attached to cash transfers have seen massive success in bridging this issue by increasing enrolment and attendance rates in the last twenty years**.**

The literature and data around CCTs is rich and broad, consisting of randomised controlled trials (RCTs), natural experiments, grey literature analyses, and government surveys.

Researchers have discussed and trialled at length the optimal forms of transfer and proxy means tests (Baird, McIntosh and Ozler, 2011: Azevedo and Robles, 2013), short, medium and long term impacts of unconditional cash transfers (UCTs) versus CCTs on poverty and health and education outcomes (Gertler, 2004: Baird et al. 2013), spillovers and gender effects (Chioda, 2013: Handa et al. 2009), intergenerational mobility (Baez and Camacho, 2011), and so on. There is notably very little research, however, into what kinds of conditionality enforcement are optimal for incentivising households into complying with education investments. Compliance under CCT programs represents a classic principal-agent problem; the program manager (principal) wants to change household (agent) behaviours and human capital investments, but the two parties have asymmetric information on compliance (the state struggles to perfectly observe actions) and incentives may be misaligned (parents may wish to retain child labour for domestic production). To overcome this problem, programs marshal a range of tools; many use monitoring and penalty systems that vary in their leniency and severity, while others try to align household and state incentives by changing social norms through education and participatory workshops, and a final, more contemporary category are experimenting with 'nudging' through explicitly labelling but not enforcing conditionalities (Fiszbein and Schady, 2009: Benhassine et al., 2015: Lindert, 2014). The purpose of this paper is to elaborate on whether, all else constant, households respond better to strict or lenient coercion, or educating or nudging, and if there is a significant trade-off between attendance rates and enforcement capacity. The research questions are as follows:

1. *Do households in receipt of conditional cash transfers respond to attendance conditionalities more stringently when subject to stricter compliance mechanisms?*

2. *What are the implications for African program design?*

This thesis employs meta-regression techniques on a sample of 27 primary and 24 secondary attendance program effect sizes, examining the effect of program-level covariates and heterogeneity in conditionality and transfer design on attendance rates, in answering these two questions. All else constant, stricter compliance mechanisms are associated with a 1.8% increase in primary attendance rates per additional category of severity, and this is highly statistically significant, even following a permutation test. No statistically significant relationship between secondary attendance rates and compliance severity is found. Programs with low baseline enrolment rates and supply-side investment components are found to exhibit substantially higher attendance rates, although permutation tests in some instances bring the validity of these results into question. Theoretical discussion follows in chapter 5.

2. Literature Review

This paper borrows from and contributes to a number of literatures in public and behavioural economics, and African social policy studies. Firstly, it builds on the debate around the use of cash transfer programs to increase household education investments, and whether conditionalities are needed to overcome the principal-agent problem associated with household decision-making (Baird et al., 2013). Secondly, it contributes to the behavioural economics literature on 'nudging', and examines whether designing CCTs with subtle nudges like 'labelling', can be a cost effective solution to expensive information and enforcement systems (Benhassine et al, 2015). Thirdly, the paper's main contribution is to a much smaller, emergent literature on conditionality compliance in CCT programs, and how attendance rates respond to the type of enforcement and penalty in place (Brollo et al., 2017: de Brauw and Hoddinott, 2010). To this end, the author constructs an index of four kinds of enforcement inspired primarily by Fiszbein and Schady (2009, p.88-91) and Lindert (2014, pp. 27-33), among others**.** Lastly, given the question of financial and practical viability associated with the African context, a discussion of the small literature around the administrative costs of enforcement is appropriate (Caldes et al., 2006: Grosh, 2008).

2.1: Cash or condition?

Perhaps the most widely researched question in the cash transfer literature is whether paternalistic conditions are required to improve education outcomes, or whether households would typically prefer to have their children in school anyway, but lack the financial means to cut back on domestic labour - in which case conditions would have no additional benefit than a UCT. This section will see a discussion of the conceptual rationale for conditioning transfers based around three theories in public and behavioural economics; internalities, externalities, and the political economy of redistribution, before supporting these with a review of the evidence from the quantitative literature reviewed in Baird et al., 2013.

Firstly, economic agents typically are not the rational self-interested 'homo-economicus' that appear in neoclassical models - they are myopic, they have time-inconsistent preferences, they are loss-averse, they have irrational biases, and so on. Collectively these are known as 'internalities' - where an individual's consumption and investment decisions are privately suboptimal, because they contradict their long-term preferences (Reimer and Houmanfar, 2017). There are two kinds of internality likely at play within the context of typical poor households in developing countries, that may result in privately sub-optimal education

investments: misinformation, and **'**incomplete altruism' (Fiszbein and Schady, 2009 p.57). Misinformation, in this context, is a situation in which households underestimate the returns to education because of inaccurate beliefs born from insufficient information - they may, for instance, believe that social connections are more important than formal education, or that schooling is only for those with very high natural aptitudes (Attanasio and Kaufmann, 2017; Jensen, 2010). Secondly, 'incomplete altruism' refers to potential conflicts of interest within a household, where parents may be aware of the returns to education, but value their own, or unitary household utility, over education investments in the child; creating a mismatch between the child's optimal private level, and the parent's investment choices (Birdsall, Levine and Ibrahim, 2005). In other words, parents may prefer to keep children in domestic production to improve the household's financial situation, rather than investing in their long term prospects of upward mobility. The presence of these internalities and distortions to decision making provides a clear rationale for conditioning cash transfers on education attendance, where, at least for many households, UCTs would likely only increase consumption, but have little effect on education investments.

Secondly, in line with endogenous growth theory discussed in section 1, education investments have positive human capital externalities in the economy that will not factor into household decision making, and should therefore be corrected through demand-side interventions (i.e. conditionalities akin to a Pigouvian tax) to increase attendance and enrolment, to complement the emphasis on supply-side investments in donor orthodoxy of the last few decades.

The final theoretical rationale for conditioning arises from the political economy of redistributive policy; conditions represent a fair compromise between the left and right of the political spectrum, and garner greater, more broad-based support than their unconditional counterparts. A UCT, for example, could be optimally designed and targeted with the potential to massively reduce poverty and inequality within a population, but could be rejected in parliament or congress if it is seen by politicians or voters as creating labour market distortions, giving undeserved handouts, or rewarding the lazy poor. Conditionalities, in contrast, are viewed more favourably across the spectrum as partnerships with co-responsibilities, as evidence from Brazil demonstrates (Lindert and Vincensini, 2010 p. 62). These preferences and attitudes are best understood through the behavioural economics literature on inequity aversion, particularly the work of Fehr and Schmidt (1999). They find that in contrast to traditional game theory, where entirely self-interested agents seek to maximise their personal utility, many individuals are altruistic 'rewarders' or 'punishers' who are willing to forego some

income to reward behaviour they see as socially fair (redistribution), or to punish those they see as undeserving (ibid, p. 853). In addition to financial and administrative capacity, it is these political economy elements of redistributive preferences, ideology and social norms that drive heterogeneity in compliance mechanisms, although this avenue of discussion is beyond the scope of analysis in this paper.

Moving on to the quantitative evidence, Baird et al. (2013) conduct a systematic review of schooling outcome data in 25 countries, from five UCTs, 26 CCTs, and four RCTs containing both UCT and CCT treatment arms. The aggregate available data shows a 23% uptake in enrolment rates for UCT's, versus a 41% uptake under CCTs, while effect sizes on attendance are always higher for CCT programs. This discrepancy demonstrates the existence of a principal agent problem between the state and the household, as a result of one or both of the aforementioned internalities - either some parents are ignorant of the value of schooling, or they have preferences which prioritise the household over the child's long term prospects. The primacy of CCTs as the unit of analysis in this paper, as well as the general cash transfer literature, is justified on these theoretical and statistical grounds.

2.2: Nudging

As discussed in section 2.1, the presence of internalities in behaviour and externalities in the economy, cause individuals to make privately and socially suboptimal health and education investments. A recent phenomenon in the behavioural economics literature called 'nudge theory' presents a potential cost-effective solution to this problem. In contrast to other, more explicit, mechanisms designed to maximise compliance - like education, law and penalties - nudging involves subtly influencing behaviour by changing aspects of the context in which choices take place, to promote optimal decisions. Or, in the words of Thaler and Sunstein - 'A nudge... is any aspect of the choice architecture that alters people's behaviour in a predictable way without forbidding any options or significantly changing their economic incentives. To count as a mere nudge, the intervention must be easy and cheap to avoid.' (2008, p.6). One frequently cited success story - among many others - is the use of organ donor default options to increase donor rates - where, by making organ donation the default option when applying for a driving licence (rather than an opt-in process requiring an additional form), national organ donor rates typically rise to 99% (Whyte et al. 2012).

Nudging has two main advantages over other compliance mechanisms used in public policy. Firstly, they carry fewer distortions (like tax evasion, labour market effects, and price effects)

than mandates and legislation because individual's change their behaviour in line with the policy-makers' intentions, rather than as an unintended consequence of tax or social policy, that can carry with them distortions (Gruber, 2011). Secondly, nudging is relatively cheap and requires little administrative capacity; the only costs involved are associated with the design, research, and roll-out of the new policies, which are usually conducted in partnership with donors - they require no or very little continuing budgetary commitments, or information and enforcement technologies. This last point makes nudging particularly attractive for use in low-income countries and rural regions beyond the capacity of the state, and could represent a cost-effective alternative in African policy design.

Benhassine et al. (2015) assess the application of nudge theory to education conditionality compliance in a randomised pilot program in rural Morocco. They run an RCT of 320 school sectors with 60 in the control group, 180 in two CCT groups (90/90 mother/father), and 80 in two 'labelled cash transfer' (LCT) groups (40/40 mother/father). Households in the 'labelled' cash transfer group received a transfer that wasn't conditional on enrolment or attendance, but was made salient as an effort to promote education and came labelled from the Ministry of Education. The rationale behind this is that explicit framing will promote greater understanding of the returns to education by influencing household choices through positive signalling from an authority promoting the value of education, without compromising autonomy. After two years, the LCT had decreased drop-out rates by 76%, increased attendance by 82%, and enrolment by 31% when compared with the control group, while the CCT had strong effects on enrolment, but conditionalities on attendance led to an increase in the drop-out rate. This paper contributes to the nudge literature by expanding on this study to examine the aggregate contribution of labelling to increasing attendance outcomes, using a meta-dataset.

2.3: Compliance mechanisms and index

This paper's main contribution is to the small literature on CCT compliance mechanisms, through the creation and meta-regression of an index of varieties on school attendance rates. While direct studies into enforcement are limited to a handful of papers, the topic appears a number of times in the general CCT literature. Fiszbein and Schady (2009 p. 88-91) document heterogeneity in both the frequency of monitoring (none/annual/<quarterly), and the degree of penalties (none/light/full), and sort 22 programs into these categories. They place eight in the 'full penalties' category; where noncompliance is met in the first instance with a 'temporary reduction or suspension of all or part of the benefit', with eventual termination for repeat

offenders (ibid, p.89), eleven in the 'light penalties' category; where non-compliance is met first with written warnings and social worker visits, followed eventually by a blockage of the transfer until compliance is met, at which point the full withheld amount is transferred, and three in the 'no penalties' category; where conditionalities are announced but not monitored or enforced - equating to the 'labelling' seen in Benhassine et al. (2015). Lindert (2014 p.27) also notes a 'spectrum of hard and soft conditionalities', and divides CCT programs into four varieties; those with educational and participatory elements to correct parental misinformation around the value of schooling, those with conditionalities announced but not enforced (again akin to labelling), those with light penalties, and those with full penalties.

Regarding the direct literature on conditionality enforcement, Brollo et al. (2017) test how households respond to the threat of punishment after non-compliance with education attendance co-responsibilities in the context of Brazil's *Bolsa Familia.* By exploiting random variation in the day of penalisation, the authors observe increasing attendance following punishment, which rises in proportion with the severity of the penalty, with attendance rates also responding to the experience of enforcement by peers or siblings within and between households - showing evidence of spillover effects and learning. De Brauw and Hoddinott (2010) conduct a similar study with non-experimental data in Mexico while investigating attendance rates under PROGRESA. By exploiting the fact that some households under the CCT were unmonitored due to an administrative error, the authors were able to observe large decreases in attendance rates for children in these households.

It is from these direct and general literatures, combined with insights from Benhassine et al. (2015), that a clear pattern of four analytically distinct compliance mechanisms emerges; labelled cash transfers, cash transfers that try to target misinformation (rather than nudging or coercion) to change social norms around compliance, conditionalities with lenient enforcement, and conditionalities with strict enforcement.

2.3.1 Labelled cash transfers

Expanding on the analysis in section 2.2, LCTs are cash transfers that aim to nudge people into compliance through positive signalling and learning, by explicitly labelling the transfer as part of an education program. For the purposes of this index, CCT programs in the dataset that, for whatever reason, have conditionalities but no way to enforce or monitor them, will be included in the LCT category, because having an unenforced CCT is synonymous to an LCT due to the fact that both categories aim to achieve compliance via the same mechanism - the

labelling of conditions in a context where penalties aren't enforced, and funds received by households are fully fungible with no strings attached, with the only compliance effects coming from signalling and learning. Subsequently, an LCT is defined here as any cash transfer program that has conditionalities or is labelled, but is absent enforcement, and analytically distinguished from an unconditional cash transfer via these characteristics.

2.3.2 Compliance by targeting misinformation

Drawing on theory from the 'misinformation' internality outlined in section 2.1, Lindert (2014, p. 25) and Fiszbein and Schady (2009, pp. 54-55), this program category aims to increase compliance by correcting invalid beliefs around the returns of education, through conditionalities requiring parental participation in workshops and information sessions. Programs in this category may have either lenient enforcement or be unenforced, but always contain a substantial emphasis on targeting misinformation through participatory social programs for parents as the main compliance mechanism. An illustrative archetypal program is Nicaragua's *Sistema de Atención en Crisis;* where investment behaviours are targeted through social marketing on education, participation in local talks and events on education and vocational training, and complementary workshops to increase financial management skills to lessen the impact of negative shocks on education attendance (Macours and Vakis, 2009, p. 9). As noted in Fiszbein and Schady (2009), participatory workshops may have an edge over labelling, as passively received information may not be sufficient to change behaviour, while organised participation and social interaction are more likely to tackle misinformation, and have additional learning and spillover effects.

2.3.3 Compliance with lenient penalties

The third category of compliance mechanism involves full monitoring combined with lenient penalties that increase slowly in severity, sometimes resulting in termination as a last resort, once significant efforts have failed to aid households in becoming compliant again. Conditionalities under these systems are seen simply as encouraging households to pursue their human rights to free education and healthcare, and noncompliance as being the result of some obstacle (a health shock, for example), rather than unwillingness to comply. Brazil's *Bolsa Familia,* for instance, employs lenient sanctions beginning with a written warning and visit from a social worker to see if there is some obstacle that can be overcome to increase compliance, followed by a set of incrementally more severe penalties in two month intervals

with benefit accumulation until the fourth instance, and termination of benefits only after five instances (nine months) of non-compliance.

2.3.4 Compliance with strict penalties

The final compliance mechanism involves stricter and more immediate penalties and sanctions, culminating in a permanent termination of benefits. An archetype of this category is Mexico's Oportunidades; where benefits are suspended and lost for one month following the first offence, and then terminated after four continuous months of under-85% school attendance. Analytically, this category of compliance mechanism has the following features that distinguish it from other types: a temporary suspension with un-accumulated benefit after the first infraction, followed by permanent termination of benefits after 2-4 instances of non-compliance, and none of the social worker visits or exceptions to penalties that characterise the lenient enforcement category.

2.3.5 Finished compliance index

	LCT	Information	Lenient	Strict
Description	No monitoring or enforcement, conditionalities announced or programs explicitly 'labelled'	May have lenient enforcement, but main emphasis on changing household attitudes around the value of education, through workshops and participation	Full monitoring, lenient penalties begin after second or third warning and/or social worker visit. Penalties increase in severity and may eventually culminate in termination	Immediate penalties and transfer suspension without benefit accumulation, termination after 2-4 instances of non-compliance

Table 1: Compliance Mechanism

Combining the compliance mechanism index proposed above with the dataset outlined in section 3.1 (via examination of each CCT study source identified in appendix table 11 results in the categorisation below:

	LCT	Information	Lenient	Strict
Programs	• Bangladesh Shombhob • DR PS •Ghana LEAP • Honduras PRAF • Indonesia JPS • Kenya OVC • Morocco Tayssir	• Burkina Faso OVC •Macedonia CCT • Nicaragua SAC • Paraguay Tekopor • Zimbabwe Manicaland	• Argentina AUHPC • Brazil PETI • Cambodia JFPRS • Cambodia Scholarship • Colombia FEA • Malawi CCT •Peru JUNTOS • Tanzania CCT	• Cambodia ESSS • Colombia SCB (x3) • Indonesia KH • Jamaica PATH • Mexico Oportunidades • Mexico Progresa • Nicaragua RPS • Philippines Pantawid

Table 2: Programs by Compliance Mechanism

2.4: Administrative costs of programs

Lastly, this paper draws on insights from the small literature on the administrative costs of CCTs associated with complex monitoring and enforcement systems, and contributes with a discussion of optimal program design in Africa. Caldes et al. (2006) note important data constraints in carrying out large cross-country panel studies on cash transfer accounting that explain the limited amount of studies, with only 32 of 111 programs having any kind of cost information, and these having little cross-comparability due to heterogeneity and ambiguity in time periods covered and types of cost incurred. Quantitative investigation into the administrative costs of the enforcement index proposed in this paper is therefore currently difficult, and the author's contribution in this field is limited to a discussion based on insights from the available evidence. Refuting the simplistic accounting seen in Grosh (2008), Caldes, et al. (2006) conduct a cost-efficiency comparison of Mexico's PROGRESA, Honduras' PRAF, and Nicaragua's RPS by looking at the ratio of administrative costs to transfers, and accounting for start-up, targeting and conditionality compliance costs that affect finances and outcomes. They find that PROGRESA performs up to four times better than PRAF and up to five times better than RPS, but chalk this up to economies of scale, age of the program (absent start-up costs), and lack of supply-side interventions. Start-up costs are therefore included as a covariate later in meta-analysis. But ultimately, their message is twofold; poor data and cost-transfer ratios obscure program features and make cross-comparisons of efficiency very difficult, and that targeting, monitoring, and enforcement features are complex and expensive.

The literature on the spread and constraints of CCT programs in Africa draws similar conclusions about program design. Akinola (2016), for instance, notes two problems in this area; firstly, targeting and monitoring features require massive technical and administrative capacity, synergy between a range of actors, and skilled and experienced state personnel that are often absent in African departments, where community leaders, poorly trained officials and ineffective and uncommunicative ministries have little capacity to enforce conditionalities. Secondly, African CCTs suffer from insufficient and unsustainable funding by donors (rather than domestic tax systems as in Latin America and Asia), and therefore largely lack the finance or stability to support information, monitoring, and penalty systems. Schubert and Slater (2006, p. 575) also cite administrative constraints, poor skills, perverse incentives and weak ministries as the main reason why African CCT programs struggle to enforce conditionalities. Given these common problems, African CCT programs typically resort to announcing but not enforcing conditionalities (labelling), such as in Kenya and Morocco, or by using participation

and workshops to correct internalities, with or without lenient conditionalities attached, as in Zimbabwe and Burkina Faso (Garcia and Moore, 2012 pp. 120-121).

3. Methodology

3.1: Data

The author employs an adapted version of the meta-dataset produced by Saavedra and Garcia (2017). The original dataset contains average effect sizes for education enrolment, attendance, and dropout rates in 94 studies of 47 CCT programs in 29 countries, in addition to a large number of categorical and numerical variables on the characteristics of each program and transfer (220 in total). The key variable of interest in the dataset is the effect size, which is a figure that quantifies the average size of the variation between two groups (households at baseline and endline in this case) regardless of sample size and differing scales, making them easily comparable across studies, and therefore especially useful in meta-analyses to quantify the relative effectiveness of each program (Coe, 2002). Saavedra and Garcia (2017) create the meta-dataset from all relevant and suitable studies to test the cost-effectiveness and impact of program characteristics on education outcome effect sizes, against predictions from a constructed model of household-decision making. They present a unitary household model where parents face three constraints; budget (income from labour of each member and a fraction of children's' adult earnings potential), human capital production function (time of child and mother, and human capital investments), and adult earnings production function, and allocate child time, parent time, and investments to equalise the marginal costs of each, producing six predictions. They are that education outcomes will correlate positively with: increasing transfer size, national rather than pilot programs, achievement conditions, low baseline enrolment, mother as recipient, and more frequent payments.

Notably however, their model and identification strategy omit to test for the impact of compliance mechanisms or severity of enforcement, thus presenting an incomplete vision and prediction of household decision-making, to which a fourth constraint must be added; threat of benefit reduction and adherence to compliance. The following sections will test for the influence and inclusion of this constraint, and its effects on Saavedra and Garcia's (2017) meta-regression results, by adding an additional hypothesis; *stricter compliance mechanisms will positively correlate with increased attendance, as households seek to retain transfer access.* To the author's knowledge, the only other meta-dataset on cash transfers and education outcomes can be found in Baird et al. (2013), and while they do make a clear attempt at examining outcomes when grouped into severity of conditionality, they fail to examine the effects of LCT's and targeting misinformation, and employ odds ratios in favour

of effect sizes, which are more problematic for cross-comparison (Davies et al., 1998). Additionally their data includes UCT's that are irrelevant to analysis in this paper, and is less suitable for meta-regression as it omits many important covariates that can be found in the Saavedra and Garcia dataset, such as baseline enrolment and achievement conditionalities.

As enrolments and drop-outs are one-off events that don't give the opportunity to study the continual and aggregate effects of compliance mechanisms on education over a time period with multiple observations (i.e. instances of non-compliance at the household level over a study), attendance rate effect sizes will instead be analysed, as these are constructed from panel data on households over time, and therefore can be better used to estimate how parents respond to different compliance mechanisms. Consequently, all observations concerning enrolment and drop-out rates have been dropped, leaving the list of program studies seen in appendix table 11. The following covariates were also retained or added, given they all have a plausible impact on attendance rate effect sizes (see appendix table 12 for an explanation of each relationship):

- Africa dummy
- Average size of transfer in 2015 dollars
- Baseline enrolment
- Conditional on achievement
- Frequency of transfer
- Latin America and Caribbean dummy
- Length of treatment
- Meets evidence standards
- Mother dummy
- National (vs pilot) dummy
- Start-up dummy
- Supply side interventions

The data-set has average primary and secondary attendance effect sizes of 2.7% and 5.75% for 27 and 24 studies respectively, with substantial heterogeneity between programs within both categories. Summary statistics and forest plots sorted by compliance category can be seen in appendix tables 13 and 14, and figures 3-6.

3.2: Model

The author employs a random-effects meta-regression (REMR) model to analyse heterogeneity in effect sizes when moderated by compliance mechanism and the program and transfer variables listed in section 3.1. Meta-regression is a tool used in meta-analysis to relate statistical heterogeneity in effect sizes to categorical and continuous variation in program and study characteristics, and quantify the impact these covariates have on the relative score of each treatment. Much like OLS, meta-regression predicts the impact of a one unit increase in the explanatory variable on the outcome variable (effect size) and allows for the effects of multiple moderators to be run simultaneously, where linear sub-group analysis such as in section 4.1 can't account for other covariates. Meta-regression has two advantages over OLS in meta-analysis; it accounts for within-study heterogeneity (effect size standard errors) and allocates weights to studies accordingly, and it allows for the incorporation of 'residual heterogeneity' - i.e. effect size variation not explained by the covariates - to give wider confidence intervals by estimating the mean of a distribution of effects across studies, rather than estimating the assumed common effect (see Thompson and Higgins, 2002 for an elaboration of this method).

Higgins and Thompson (2004) however, elaborate on the main problem associated with meta-regression; spurious conclusions from misleading false-positive results. They cite small sample sizes and large numbers of covariates (typical of meta-regression) as a particular problem for spurious but statistically significant correlations. They demonstrate how adding three additional covariates to a sample size of ten studies results in an increase of 5%, 14.3% and 22.6% respectively to the false-positive rate - a measure of the probability of falsely rejecting the null hypothesis. The true p-value in meta-regression is a complex function of study *n*, effect size heterogeneity, covariate collinearity, and study weights (ibid, p.1673). Analysis in this paper with 13 covariates and 24 and 27 studies for primary and secondary attendance respectively therefore represents a real risk of false-positive correlations. To mitigate this problem, the authors recommend a Monte Carlo permutation test to adjust p-values to their truer significant levels.

REMR can be used to extend meta-analysis to test the coefficients of study-level covariates using the equation overleaf from Harbord and Higgins (2008, p. 494):

$$y_i = x_i \beta + u_i + \varepsilon_i$$

Where y_i is a study effect size, $x_i\beta$ is a vector of study covariate coefficients, u is the residual heterogeneity not explained by the covariates, and ε is the error term.

Applied for our purposes, the model above converts to:

$$Attendance\ effect\ size_i = \beta Compliance\ Severity_i + \chi_i + u_i + \varepsilon_i$$

Where i is a program, Compliance Severity is the categorical covariate of interest with values 1-4 for LCT, Information, Lenient and Strict respectively, χ is a vector of coefficients for study-level covariates, u represents residual heterogeneity, and ε is the error term.

4. Results and Hypothesis Tests

This section presents results in four parts: linear regression and sub-group analysis, followed by primary and then secondary attendance multiple meta-regression with permutation tests, then hypothesis testing against the predictions of Saavedra and Garcia (2017) with the inclusion of a compliance severity variable. A theoretical discussion with implications for policy and future research will follow in chapter 5.

4.1 Linear results and sub-group analysis

Figures 1 and 2 overleaf display graphically the linear regression between effect size and compliance category (rising with severity) seen in tables 3 and 4, while tables 5 and 6 show descriptive statistics for effect sizes when sub-grouped by compliance mechanism, and appendix figures 5 and 6 show forest plot effect size distributions in percentage points with their standard errors when grouped by compliance category. The fitted values on the scatter plot display coefficients of 0.280 and 0.06 and p-values of 0.4 and 0.967 for primary and secondary attendance respectively as enforcement severity increases. The coefficients are modestly positive, but statistically insignificant due to large errors arising from both limited sample size and heterogeneity in effect sizes in each category, itself the result of variation in program, transfer and country characteristics. Table 7 shows increasing mean primary attendance effect sizes of 2.9%, 4.7%, 5.7% and 7.3% respectively for programs that employ LCT, Strict, Lenient and Information compliance mechanisms. While table 8 displays a different order of performance for secondary attendance scores, with Information, Strict, LCT and Lenient scoring 5.2%, 6.4%, 6.6% and 9.8% respectively. Both tables are plagued by large standard deviations, biased in some cases by outliers like the Cambodia Scholarship Program in the Lenient category with effect sizes of 32.5% and 31.3% respectively, so no claims around causation can be made. In contrast to the preceding sub-group analysis, meta-regression is employed in the following sections as a tool to make more robust causal claims about the nature of the relationship between conditionality enforcement and attendance rates, as well as their impact on other covariates, through the inclusion of covariates that explain the large heterogeneity in effect sizes.

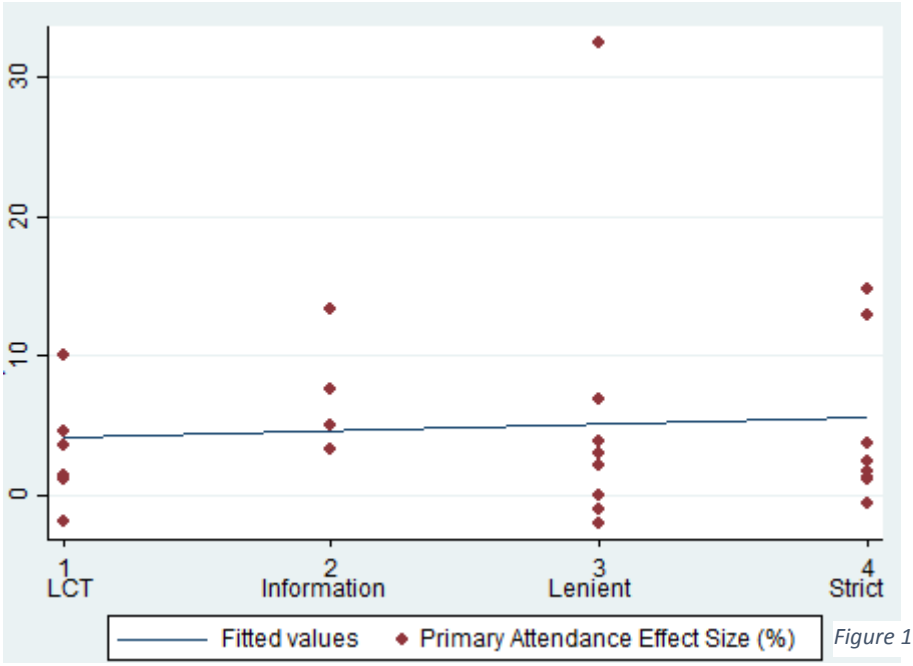

Figure 1 Linear regression graph primary

Primary Attendance ES	β	St.Err	t-value	p-value
Compliance Severity	0.280	0.709	0.40	0.696
Constant	2.755	2.089	1.32	0.199

Table 3 Linear meta-regression primary

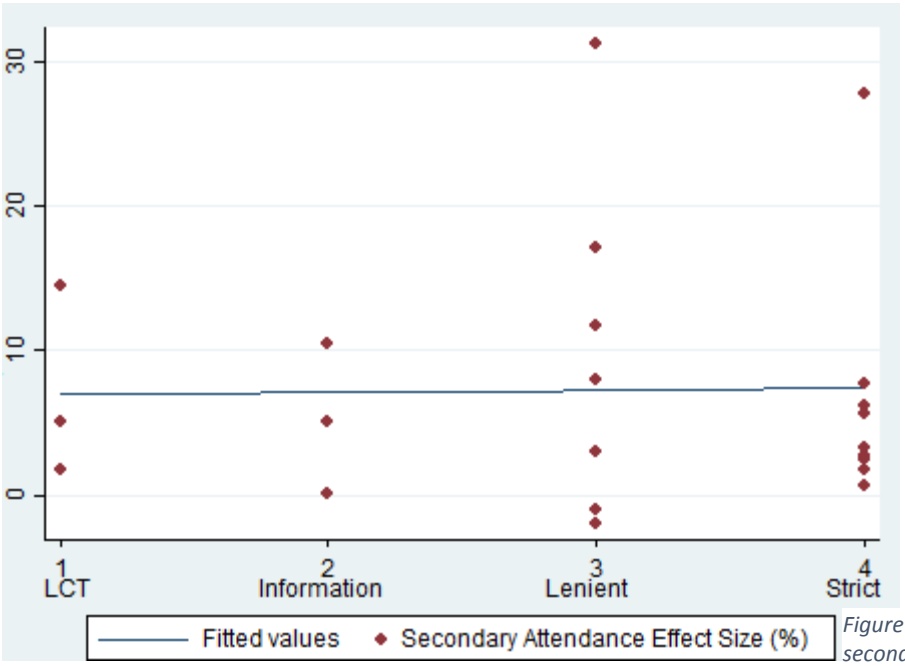

Figure 2 Linear regression graph secondary

Secondary Attendance ES	β	St.Err	t-value	p-value
Compliance Severity	0.060	1.438	-0.04	0.967
_cons	6.588	4.616	1.43	0.168

Table 4 Linear meta-regression secondary

Mechanism	Mean (%)	Std. Dev.	Freq.
LCT	2.875	3.753	7
Information	7.325	4.419	4
Lenient	5.677	11.186	8
Strict	4.687	5.786	8
Total	4.901	7.100	27

Table 5 Primary effect size descriptive statistics by compliance mechanism

Mechanism	Mean (%)	Std. Dev.	Freq.
LCT	6.592	5.487	4
Information	5.166	5.152	3
Lenient	9.743	11.722	7
Strict	6.416	7.853	10
Total	7.259	8.310	24

Table 6 Secondary effect size descriptive statistics by compliance mechanism

4.2 Primary attendance meta-regression analysis

Columns (1-4) of table 7 display the meta-regression results for primary attendance, when running a variety of specifications. Regression (1) shows the same linear regression as seen in table 3 in the previous section**,** regression (2) shows the full specification with continental dummies and the additional presence of start-up constraints (dummy = 1 if age≤1), regression (3) omits the Africa dummy, while regression (4) omits both continental dummies and the start-up dummy. Omission of these dummies brings compliance severity, baseline enrolment and supply component into the 1% confidence range, and the national dummy into the 10% confidence range, with model (4) explaining 80.4% of between study heterogeneity and a residual heterogeneity of 47%, versus 73% and 50%, and 67.6% and 53% respectively for regressions 2 and 3. The omission of these variables is justified on the grounds that the continental dummies do not represent program or transfer characteristics that could have a potential impact on effect sizes, but simply indicate that a program is likely to have certain features - there is no unobservable quality of being an 'African' or 'LAC' CCT program that isn't already captured by the other covariates. While the start-up dummy is insignificant and close to 0, so likely has no relevance. A Monte Carlo permutation test (table 8) adjusts the p-values to a more accurate distribution, and brings compliance severity and baseline enrolment to the 5% confidence level, while rendering the national dummy insignificant, and having no effect on the supply variable's confidence range.

Primary meta-regression Independent variable = primary attendance effect size

VARIABLES	(1)	(2)	(3)	(4)
Compliance Severity	0.280	1.578**	1.661**	1.804***
	(0.709)	(0.670)	(0.574)	(0.510)
LAC dummy		0.794	1.193	
		(2.691)	(2.007)	
Africa dummy		-0.790		
		(3.675)		
Meets evidence standards		-2.061	-2.219	-1.925
		(3.625)	(3.307)	(2.596)
Baseline enrolment		-34.305***	-33.674***	-33.039***
		(10.509)	(9.845)	(8.554)
Years of exposure		-0.148	-0.138	-0.104
		(0.468)	(0.442)	(0.296)
Mother dummy		-0.166	-0.152	0.490
		(2.472)	(2.318)	(1.757)
National dummy		-3.320	-3.385*	-3.539*
		(2.008)	(1.903)	(1.734)
Start-up dummy		-0.240	-0.058	
		(2.067)	(1.825)	
Payment frequency		0.797	0.356	1.339
		(3.231)	(2.482)	(1.790)
Average transfer		0.017	0.018	0.023

		(0.022)	(0.021)	(0.016)
Achievement conditionality		0.093	0.051	-0.066
		(2.266)	(2.116)	(1.880)
Supply component		6.020**	6.105**	7.015***
		(2.309)	(2.194)	(1.676)
Constant	2.755	32.019***	31.206***	29.395***
	(2.089)	(10.029)	(9.314)	(8.695)
Observations	27	27	27	27

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7 Primary multiple meta-regression

Primary attendance Monte Carlo permutation test

P-values unadjusted and adjusted for multiple testing

Number of observations = 27
Permutations = 10,000

	P	
Primary Attendance ES	Unadjusted	Adjusted
Compliance Severity	0.002***	0.024**
Meets evidence standards	0.498	0.995
Baseline enrolment	0.003***	0.026**
Years of exposure	0.761	1.000
Mother dummy	0.721	1.000
National dummy	0.058*	0.297
Payment frequency	0.442	0.986
Average transfer	0.129	0.630
Achievement conditionality	0.933	1.000
Supply component	0.001***	0.008***

Largest Monte Carlo SE(P) = 0.0050

Table 8 primary permutation test

4.3 Secondary attendance meta-regression analysis

Columns (1-4) of table 9 overleaf display the meta-regression results for secondary attendance with a variety of specifications. Regression (1) shows the same linear regression as seen in table 4 in section 4.1**,** regression (2) shows the full specification with the Africa and start-up dummies, regression (3) omits the Africa dummy, and regression (4) omits the Africa and start-up dummies. Again the start-up dummy is omitted due to insignificance, while the Africa dummy is dropped due to multicollinearity. Within regression (4), baseline enrolment is significant at the 1% level, supply component at the 5% level, and the LAC dummy at the 10% level, while the model explains 21.7% of between-study heterogeneity, and a residual heterogeneity of 86.2%. A Monte Carlo permutation test (table 10), however, renders every variable in the model insignificant, likely due to small sample size and a large number of covariates.

Secondary meta-regression	Independent variable = secondary attendance effect size			
VARIABLES	(1)	(2)	(3)	(4)
Compliance Severity	-0.060	-0.558	-0.558	-0.150
	(1.438)	(2.736)	(2.736)	(2.506)
LAC dummy		-3.098	10.982	13.533*
		(5.933)	(9.204)	(6.425)
Africa dummy		-14.080*		
		(6.681)		
Meets evidence standards		-2.532	-2.532	-3.416
		(6.543)	(6.543)	(5.870)
Baseline enrolment		-45.822**	-45.822**	-48.620***
		(17.511)	(17.511)	(15.485)
Years of exposure		0.732	0.732	0.499
		(1.106)	(1.106)	(0.894)
Mother dummy		-6.102	-6.102	-6.916
		(5.397)	(5.397)	(4.924)
National dummy		4.697	4.697	4.669
		(4.868)	(4.868)	(4.696)
Start-up dummy		2.741	2.741	
		(6.340)	(6.340)	
Payment frequency		0.081	0.081	0.072
		(0.055)	(0.055)	(0.051)
Average transfer		1.961	1.961	2.183
		(4.674)	(4.674)	(4.470)
Achievement conditionality		-16.034	-16.034	-13.666
		(10.038)	(10.038)	(8.188)
Supply component			-14.080*	-15.008**
			(6.681)	(5.967)
Constant	6.588	33.733**	33.733**	36.168***
	-0.060	-0.558	-0.558	-0.150
Observations	24	23	23	23

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9 Secondary multiple meta-regression

Secondary attendance Monte Carlo permutation test

P-values unadjusted and adjusted for multiple testing

Number of observations = 24
Permutations = 10,000

	P	
Secondary Attendance ES	Unadjusted	Adjusted
Compliance Severity	0.991	1.000
LAC	0.059	0.558
Meets evidence standards	0.802	1.000
Baseline enrolment	0.009	0.156
Years of exposure	0.389	0.980
Mother dummy	0.167	0.722
National dummy	0.344	0.946
Payment frequency	0.050	0.350
Average transfer	0.233	0.858
Achievement conditionality	0.613	0.999
Supply component	0.141	0.643

Table 10 Secondary permutation test

Largest Monte Carlo SE(P) = 0.0050

4.4 Hypothesis tests

The compliance category hypothesis test (h_1), combined with the hypothesis tests (h_{2-7}) conducted in Saavedra and Garcia (2017) are set out formally below, before being tested against the meta-regression results in tables 7 and 9 to determine how their coefficients respond with the addition of the h_1 prediction to the household decision making model. The impact of other covariates outside the household model are also discussed.

h_1 - Stricter compliance mechanisms will positively correlate with increased attendance
h_2 - Greater transfer size will positively correlate with increased attendance
h_3 - National programs will positively correlate with increased attendance
h_4 - Programs with achievement conditions will positively correlate with increased attendance
h_5 - Low baseline enrolment and supply-side investments will positively correlate with increased attendance
h_6 - Mother as recipient will positively correlate with increased attendance
h_7 - More frequent transfers will positively correlate with increased attendance

The results for compliance mechanism (when ordered by severity) are highly statistically significant for primary attendance effect sizes ($p<0.01$); representing a 1.8% increase in attendance rates per additional category. A permutation test brings the significance up to the 5% level. This is strong evidence that, all else constant, households do indeed respond to the threat of stricter penalties by investing more in their children's education. The result for secondary attendance is negative and insignificant, made even more so by a permutation test. H_1 is therefore accepted for primary attendance and rejected for secondary. This is consistent with results from Baird et al (2013), although they don't distinguish between primary and secondary schooling, and find an 6.7% increase in odds ratio per category (six in total), converting to an effect size of around 3.7% when using the method recommended in Chinn (2000). Running the regressions with individual dummy variables for compliance mechanism, as in appendix tables 15, show that Information, Lenient and Strict programs perform 3.31%, 3.05% and 5.12% respectively better than LCT programs for primary attendance, and -.75%, 2.6% and -5.2% relative to LCT programs respectively for secondary attendance, although these secondary results are highly insignificant.

Transfer size is modestly negative but insignificant for both primary and secondary attendance, and made even more so by permutation tests. H_2 is therefore rejected when tested within the parameters of the model. This is consistent with the results from Saavedra

and Garcia (2017), who predict that larger transfers will reduce the opportunity costs of education investments and therefore increase attendance rates, but find negative and positive coefficients for primary and secondary effect sizes respectively. This is again consistent with results from Baird et al (2013).

Saavedra and Garcia make the h_3 prediction on the basis that pilot programs represent a temporary, rather than permanent, increase to household income, and that parents will make less education investments when anticipating an end to payments. Contrary to this prediction, with the inclusion of h_1 and all else constant, national (versus pilot) programs experience a 3.5% reduction in primary attendance rates significant at the 10% level, although this is made insignificant by a permutation test. Secondary attendance rates experience an increase of 4.7%, but this is insignificant. These results are consistent with the coefficient signs and significance levels in Saavedra and Garcia, although the coefficients are larger with the inclusion of compliance categories.

H_4 predicts that programs with achievement conditionalities will incentivise households to invest more in education and lead to greater attendance rates. The results are negative and insignificant for both primary and secondary attendance, although the latter is very large with a 13.7% reduction. These results are inconsistent with those from Saavedra and Garcia, whose coefficient signs are both positive, and significant for primary attendance at the 10% level, at an 8% increase. H_4 is therefore rejected, in contrast to accepted for primary attendance by the other authors.

H_5 predicts that attendance effect sizes will be larger in areas with lower baseline enrolment and where programs undertake supply-side investments. The model results show large negative and statistically significant ($p<0.01$) coefficients for baseline enrolment, equalling reductions of 0.33% and 0.49% in attendance rates per 1% increase in baseline enrolment respectively for primary and secondary. P-values are reduced by permutation tests to the 5% level and insignificant respectively for primary and secondary attendance. These results are consistent with those in Saavedra and Garcia, although the model in this paper exhibits larger coefficients and better confidence levels, pre-permutation, in addition to larger adjusted r squared figures. The results for the supply dummy display a 7% increase in primary attendance rates, significant at the 1% level, and a 15% reduction in secondary attendance rates, significant at the 5% level. These results are reduced to 5% significance and insignificant respectively by permutation tests. As Saavedra and Garcia note however, there are only three programs in the data-set that make supply side investments, so these results

should be interpreted cautiously. The model results accept h_5 for primary attendance but reject it for secondary.

Based on insights from wider literature, h_6 predicts that, all else constant, programs that transfer to mothers will exhibit larger attendance effect sizes as mothers tend to make more human capital investments than fathers. Results from the model are small and positive and large and negative for primary and secondary attendance respectively, although both results are insignificant. They are consistent with the results from Saavedra and Garcia, as well as Baird et al. (2013), and the Tayssir RCT conducted by Benhassine et al. (2015), all of whom point to a rejection of h_6, and point to a unitary model of household decision making.

H_7 predicts that programs with more frequent transfers will exhibit higher attendance effects as households will face less liquidity and savings constraints. The model displays small positive but insignificant coefficients for both primary and secondary attendance. This is consistent with the primary results from Saavedra and Garcia, but differs from their secondary results, which find a 14% reduction in attendance rates for more frequent payments, significant at the 10% level, although the authors seem to discount this and don't make any inferences. The model rejects h_7.

Lastly, there are some possible confounders outside of the household model that could explain variation in effect sizes, these are: study evidence standards, length of exposure, presence of start-up costs, and the LAC dummy. The only of these to produce a significant coefficient is the LAC dummy for secondary attendance effects, and this becomes highly insignificant when subject to a permutation test. These results are consistent with Saavedra and Garcia, although they find a coefficient of 5.1% for the LAC dummy for primary attendance ($p<0.1$), while the author's model displays a coefficient of 13.5% for secondary attendance ($p<0.1$).

5. Implications, Limitations and Theoretical Discussion

Discussion takes place in four parts. First and second, the two research questions presented in chapter 1 - regarding compliance severity and African program design - shall be revisited in light of the meta-regression results, and discussed in relation to the literature outlined in chapter 2. Third, limitations in sampling and data analysis will be debated, before implications for future research are discussed in closing.

5.1 Attendance and compliance severity

1. *Do households in receipt of conditional cash transfers respond with more stringent attendance rates when subject to stricter compliance mechanisms?*

Meta-regression results from table 7 show an increase of 1.8% per category of severity, while table 15 show that relative to the LCT category, programs that employ information, lenient, and strict compliance mechanisms display 3.31%, 3.05% and 5.12% primary attendance rate increases respectively: lenient programs outperform LCTs, information programs outperform lenient ones, and strict programs outperform all three. Given these results, it's clear that both h_1 and research question 1 are answered affirmatively with regard to primary attendance, but negatively for secondary attendance due to insignificance. The following paragraphs will discuss these findings within the context of the literature review, with discussion of secondary attendance results left for section 5.3.

The results have a number of important implications for the debate presented in chapter 2 around internalities and nudging versus correcting misinformation. Firstly, the fact that transfer size is highly insignificant in both meta-regression models, suggests that household income has little bearing on education investment choices, and indicates that the internalities of misinformation and incomplete altruism mentioned in section 2.1 are likely at play. Secondly, the fact that programs employing workshops to target misinformation as a compliance mechanism experienced on average 3.31% higher attendance rates compared to LCT programs, is consistent with Fiszbein and Schady's (2009) prediction that passively received information from, for instance, simply 'labelling' to increase the salience of the value of education, is not sufficient to correct household internalities, because they are too subtle and implicit, and lack additional learning and spillover effects arising from social interaction present in the workshop category. All else constant, information compliance mechanisms are

preferable to LCTs in correcting internalities and increasing attendance by educating, rather than nudging households. In the Moroccan context, Benhassine et al. (2015) found their LCT to increase attendance by 82%, with only modest gains arising from the addition of conditionalities. Two possible conclusions arise from this; either their LCT was better designed than the average in our meta-dataset, or more likely, there are some covariates at play - like low baseline enrolment and a supply component - that ensured large attendance effects regardless of nuances in treatment. Either way, more research is needed in optimal LCT design to maximise outcomes and minimise costs, before we rule it out entirely.

The results also have a number of important implications for the household model presented in Saavedra and Garcia (2017). Again from the coefficients in tables 7 and 15, it is obvious that households do indeed respond to stricter compliance mechanisms with increased primary attendance, with this best interpreted as parents taking action to retain transfer access in situations where it is more readily terminated. Not only do they omit this aspect from the model, they also treat households as rational economic agents that make decisions according to three constraints, when in fact, as discussed in the previous paragraph and section 2.1, it's likely that they suffer from misinformation and incomplete altruism, which aren't considered within their unitary framework. Household models within CCT research should in future make predictions according to the inclusion of compliance severity, as in Baird et al. 2013, and test for the presence of internalities, as Fiszbein and Schady discuss, and as evidenced by the negligible effect of transfer size on primary attendance rates in table 7.

The fact that programs with strict enforcement, all else constant, performed on average 2.07% better than those in the lenient category, is consistent with the findings in Brollo et al. (2017), who find evidence of more stringent attendance rate responses to increasing severity of penalties as households under the *Bolsa Familia* cycle through the sanctions described in section 2.3.3. They also find evidence of learning and spillover effects from peers and siblings within and between households at later stages of penalties, which is again consistent with the findings of this paper, as these additional effects are plausibly captured within the additional 2.07% figure, where households within the strict category observe and learn faster about enforcement from the more immediate experiences of those around them, than for instance a lenient program where penalties are initially taken more lightly. The results are also consistent with Baird et al. (2013), who find a (converted) 3.7% increase in effect size when increasing severity between the equivalent categories, although our dataset contains more observations and covariates, so is likely more accurate. With regard to cost-effectiveness, data from Grosh (2008) shows no real pattern in monitoring and enforcement costs between lenient and strict;

Bolsa Familia, Familias en Accion, and *Juntos* had figures of 12.3%, 10.5% and 11.6% respectively, while *PATH* and *Progresa* spent 13% and 6%. Sample sizes are small however, and as noted by Caldes et al. (2013), there are other covariates at play like presence of start-up costs and scale economies that bias these figures, so it is difficult to comment on financial trade-offs of increased compliance between the two categories.

Lastly, the inconclusive results for secondary attendance present a puzzle for hypothesis testing and conclusions, which are discussed in the limitations section.

5.2 African program design

2. What are the implications for African program design?

The results in chapter 4 have some important implications and recommendations for CCT design in the African context. All else constant, programs employing LCT mechanisms exhibit a statistically significant 1.8% reduction in primary attendance rates than those using workshops and education, which increases by 1.8% per category of compliance severity through lenient and strict varieties. But, given the significance in both regressions of baseline enrolment, African CCTs have large gains to be made, whatever compliance mechanism in use. Linear regressions between the Africa dummy and baseline enrolment produce figures of -9.5% and -23.8%, significant at the 5% and 1% level respectively, meaning that African countries in the sample had, on average, 9.5% and 23.8% lower baseline enrolment than their Latin American and Asian counterparts. Running these figures through the models in tables 7 and 9 predicts within a 1% confidence interval, all else constant, average gains of 3.1% and 11.7% for primary and secondary attendance rates, simply because African countries are much more likely to have lower baseline enrolment (although permutation tests make the p-values of these coefficients 5% and insignificant respectively). This is strong evidence that in the African context, large attendance gains are to be had from the introduction of CCT programs, even where complex and expensive compliance systems aren't feasible.

The second implication is that CCT design in Africa should mimic the workshop style systems being implemented in Burkina Faso and Zimbabwe. Participation and workshop style compliance mechanisms are demonstrably better at raising primary attendance rates than LCTs, with the plausible explanation being that passively received information is insufficient to correct internalities, as evidenced by the insignificant results for transfer size. While these schemes will certainly be more expensive than LCTs due to the costs associated with running

information sessions, they represent a cheaper alternative to monitoring and enforcement, while experiencing only small reductions in attendance.

5.3 Limitations

Firstly, hypothesis testing and discussion are limited by sampling and data constraints. Due to the relatively new phenomenon of CCT programs, combined with the lack of randomisation on roll out, the notoriously bad statistical record keeping of many developing country governments (Jerven, 2013), and below-evidence-standards reporting, meta-datasets on CCT attendance effects are significantly limited, in our case to just 27 and 24 observations respectively for primary and secondary schooling. Furthermore, the fact that within the secondary attendance data, only 4 and 3 observations are within the LCT and Information classes respectively, creates very large standard errors and residual heterogeneity within the model, with 78.3% of the between-study heterogeneity left unexplained. The conclusion for h_1 is that, within the data, there is no relationship between secondary school attendance and compliance mechanism. This isn't a definitive conclusion, but rather, simply a reflection of the sampling limitations. It could be that compliance severity matters less for secondary attendance schooling than primary, for any number of reasons around opportunity costs, supply constraints and household decision making, but the extent to which this can be explored with the available data is vastly limited, even with the permutation tests and large number of covariates attached. Research question 2 is also limited by the problems in cost data sampling outlined in 2.4, and while Saavedra and Garcia (2017) do attempt to construct a model based on a ratio of transfer size to attendance effect size, we would really like to compare administrative costs (as a % of total cost) against attendance in order to properly examine trade-offs between enforcement and outcome.

Secondly, the statistical exercises conducted in this paper themselves limit discussion and conclusions. As noted in Thompson and Higgins (2002), meta-regression is limited in its ability to derive causal relationships as the results display associations that are observational in nature, are affected by within-study heterogeneity, and are much weaker evidence than analysis of randomised data, which can be relied on to produce more robust conclusions, as discussed in the following section. Consequently, sampling and data analysis constraints place limitations on hypothesis testing and discussion, especially for secondary attendance. Implications for further research are presented next to mitigate these issues.

5.4 Implications for future research

The implications for future research are numerous. Firstly, as discussed in 5.3, future meta-regression of CCT program-level covariates would ideally have larger sample sizes to reduce the likelihood of both false positive and false negative conclusions, and test for interaction between covariates that are left unexplored in this paper, Saavedra and Garcia (2017) and Baird et al. (2013). Ideally speaking, the ultimate way to answer both research questions would be by carrying out an RCT. It would have four treatment arms (the four compliance mechanisms) and a control group, it would be carried out in a typical African country with the accompanying constraints this faces, and it would be fully costed and audited. A cost-attendance outcome ratio could then be constructed from the results, and more definitive conclusions drawn around the merits and costs of each treatment arm, and their suitability in the African context. Failing funding and support for an RCT, more archival and auditing work should be carried out on CCTs and statistical offices to overcome the problems listed in Caldes et al. (2006) around lack of financial data with which to conduct proper meta-analyses, in order to better inform African policy design regarding the costs associated with monitoring and enforcement. Fourth, CCTs also aim to increase health investments through their conditionalities, which may carry with them different opportunity-cost and internality considerations, so more meta-analysis is needed of compliance mechanisms and health outcomes. Lastly, more work is needed on the quite vague concept of the LCT - different labelling designs need to be trialled, and in contexts where low baseline enrolment won't upwardly bias the attendance effects, before they can be fully rejected in favour of workshop style mechanisms.

6. Conclusions

This paper examines the role of conditionality compliance mechanisms in improving attendance rates under CCT programs in developing countries. It opened with a discussion of internalities and household human capital investments, in the context of a principal agent problem, before outlining the four strategies employed by CCT programs to overcome this. Meta-regression techniques were used on samples of 27 and 24 programs in investigation of this relationship, and primary attendance rates were found, all else constant, to increase by a highly statistically significant 1.8% per additional category of severity, while secondary attendance rates were found to have no relation to compliance mechanisms. Programs with supply components and low baseline enrolment were also found to positively correlate with attendance effects, for both primary and secondary schooling.

The results imply that primary attendance rates benefit from more severe compliance mechanisms, while secondary attendance rates exhibit no relationship. They also indicate that African nations, all else constant, will display reduced attendance rates to CCTs on other continents by nature of compliance mechanism choice. African nations, however, do stand to experience substantial attendance gains from the introduction of CCT programs due to low baseline enrolment rates. The paper is limited in drawing other conclusions due to constraints in sampling and data-analysis, but has a number of important implications for future research, ideally in the form of an RCT. Although more research is needed in some areas, the main implications of this paper are that the workshop style model is the most appropriate for policy design in Africa, and that African nations ought to introduce CCT programs regardless of compliance mechanism constraints, as they have large latent attendance gains to be made, which is especially true for secondary schooling.

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8. Appendix

Country	Program Name	First Author and Year (of study)	Conditional on …. Attendance	Minimum Attendance Rate	Conditions Verification	Avg. Primary Transfer (2015 $)	Avg. Secondary Transfer (2015 $)	Transfer Frequency	Recipient	Supply side intervent-ons?	Pilot or RCT?
Argentina	AUHPC	Salvia, A. (2014)	Primary & Secondary	0	Yes	175.43	175.43	Monthly	Father	No	No
Bangladesh	Shombhob Project	Ferro, C. (2014)	Primary	.80	No	9.37	13.04	Bimonthly	Father	No	Yes
Brazil	Programa Erradicacao do Trabalho Infantil	Cardoso, E. (2004)	Primary & Secondary	.8	Yes	7.88	7.88	Monthly	Father	No	No
Burkina Faso	OVC	Akresh, R. (2013)	Primary & Secondary	.90	Yes	7.70	3.27	Quarterly	Mother	No	Yes
Cambodia	Education Sector Support Scholarship Program	Filmer, D. (2011)	Secondary	.95	Yes	22.86	4.55	Quarterly	Father	No	No
Cambodia	JFPRS	Filmer, D. (2008)	Secondary	.95	Yes	8.83	4.55	Quarterly	Student	No	Yes
Cambodia	Scholarship Pilot	Barrera, F. (2012)	Primary	.85	Yes	8.72	10.42	Annually	Mother	No	Yes
Colombia	Familias en Accion	Departament o Nacional de Planeacion (2006)	Primary & Secondary	.80	Yes	1.45	31.45	Bimonthly	Father	No	No
Colombia	Subsidios Condicionados Bogota (Basic)	Barrera, F. (2011)	Secondary	.80	Yes	5.16	14.86	Bimonthly	Student	No	Yes
Colombia	Subsidios Condicionados Bogota (Savings)	Barrera, F. (2011)	Secondary	.80	Yes	175.43	175.43	Bimonthly	Student	No	Yes
Colombia	Subsidios Condicionados Bogota (Tertiary)	Barrera, F. (2011)	Secondary	.80	Yes	22.86	4.55	Bimonthly	Father	No	Yes

Dominican Republic	Programa Solidaridad	DR. Gov (2008)	Primary & Secondary	.85	No	15.50	34.19	Bimonthly	Father	No	No
Ghana	LEAP	Handa, S. (2013)	Primary & Secondary	.80	No	4.46	N/A	Bimonthly	Father	No	No
Honduras	PRAF II	De Souza, P. (2005)	Primary	.85	No	8.34	N/A	Quarterly	Mother	Yes	No
Indonesia	JPS	Sparrow, R. (2007)	Primary & Secondary	.0	No	1.22	3.26	Quarterly	Father	Yes	No
Indonesia	Keluarga Harapan	Alatas, V. (2011)	Primary & Secondary	.85	Yes	48.87	48.87	Quarterly	Mother	No	No
Jamaica	PATH	Levy, D. (2010)	Primary & Secondary	.85	Yes	13.99	11.76	Bimonthly	Mother	No	No
Kenya	CT-OVC	Ward, P. (2010)	Primary & Secondary	.80	No	11.43	22.86	Bimonthly	Father	No	No
Macedonia	Macedonia CCT Project	Armand, A. (2013)	Secondary	.85	Yes	N/A	23.18	Quarterly	Father	No	No
Malawi	CCT for Schooling	Baird, S. (2011)	Primary & Secondary	.75	Yes	13.04	11.43	Monthly	Guardian & Student	No	Yes
Mexico	Oportunidades	Parker, S. (2006)	Primary & Secondary	.85	Yes	7.70	58.08	Bimonthly	Mother	No	No
Mexico	Progresa	Skoufias, E. (2001)	Primary & Secondary	.85	Yes	11.76	18.20	Bimonthly	Father	No	No
Morocco	Tayssir	Benhassine, N. (2013)	Primary	.80	No	11.43	N/A	Monthly	Mother	No	Yes
Nicaragua	Red de Proteccion Social	Dammert, A. (2009)	Primary	.85	Yes	11.56	N/A	Bimonthly	Father	Yes	No
Nicaragua	Sistema de Atencion en Crisis	Macours, K. (2009)	Primary & Secondary	.85	Yes	14.86	33.13	Bimonthly	Student	No	Yes
Paraguay	Tekopor	Perez, R. (2011)	Primary & Secondary	.85	No	13.04	11.43	Bimonthly	Mother	No	Yes
Peru	Juntos	Gagate, G. (2013)	Primary	.85	Yes	40.05	N/A	Monthly	Mother	No	No
Philippines	Pantawid	Chaudhury, N. (2013)	Primary & Secondary	.85	Yes	1.84	7.70	Quarterly	Mother	No	No

Philippines	Pantawid	Chaudhury, N. (2013)	Primary & Secondary	.85	Yes	6.25	7.70	Quarterly	Father	No	No
Tanzania	Community Based CCT	Evans, D. (2014)	Primary & Secondary	.80	Yes	9.37	13.0	Bimonthly	Father	No	Yes
Zimbabwe	Manicaland HIV/STD Prevention Project	Robertson, L. (2013)	Primary & Secondary	.90	Yes	18.81	8.83	Bimonthly	Father	No	Yes

Table 11 Study information

Africa dummy	African CCTs are likely to be younger, less well funded, and lack monitoring and enforcement technologies	Akinola, 2016
Average size of transfer in 2015 dollars	More generous transfers create upward bias on attendance as they better compensate for opportunity cost of child labour	Saavedra and Garcia, 2012, p. 4
Baseline enrolment	Attendance effect sizes will be upwardly biased in programs with lower baseline enrolment	Saavedra and Garcia, 2017
Conditional on achievement	Creates upward bias on attendance rates through incentives for performance	Kremer, Miguel and Thornton, 2009
Frequency of transfer	More frequent transfers are associated with lower attendance rates as limited attention and self-control can constrain ability to save	Saavedra and Garcia, 2012, p. 4
Latin America or Caribbean dummy	Programs in this region are better established, more complex, better funded and better managed	Fiszbein and Schady, 2009, p. 38
Length of treatment	Longer exposure to treatment may display larger effect sizes through a number of channels; including savings and compliance mechanisms	Behrman, Cheng and Todd, 2004
Meets evidence standards	High attrition rates and low study quality can create biased effect sizes with large errors	Institute of Educational Sciences, 2011
Mother dummy	Evidence shows mothers allocate more resources to children's education and nutrition than fathers	Duflo, 2003
Pilot dummy	Households may view pilot studies as a temporary rise in current income rather than a permanent one, and behaviour will be different	Fiszbein and Schady, 2009, p. 310
Start-up dummy	Start-up costs in first-year programs may reduce funds available for other program features and create a downward bias on effect sizes	Caldes, Coady and Maluccio, 2006
Supply side interventions	If a program has supply-side interventions on education quality and infrastructure, this could upwardly bias attendance rates	Handa and Davis, 2006, pp. 516-517

Table 12 Moderator variables

Variable N (Studies)	Value	Frequency 27
Compliance Mechanism	LCT	7 (26%)
	Information	4 (15%)
	Lenient	8 (30%)
	Strict	8 (30%)
Years of Exposure	<1	1 (4%)
	1	9 (33%)
	2	13 (48%)
	3	2 (7%)
	5	1 (4%)
	10	1 (4%)
Start-Up Constraints (age=1 year)	0 No	17 (63%)
	1 Yes	10 (37%)
Mother Receives Payment	0 No	16 (59%)
	1 Yes	11 (41%)
National vs Pilot Program	0 Pilot	10 (37%)
	1 National	17 (63%)
Payment Frequency – 1= monthly or bimonthly (0=less frequent)	0	7 (26%)
	1	20 (74%)
Latin America or Caribbean Program	0 No	13 (48%)
	1 Yes	14 (52%)
Africa	0 No	20 (74%)
	1 Yes	7 (26%)
Achievement Conditions Present	0 No	22 (81%)
	1 Yes	5 (19%)
Program Makes Supply-Side Investments	0 No	23 (85%)
	1 Yes	4 (15%)
Meets Evidence Standards (W or W/o Reservations)	0 No	4 (15%)
	1 Yes	23 (85%)
Primary Baseline Enrolment (Mean) (Std.Dev.)		.87817407 (.10487499)
Average Monthly Transfer per Person in 2015 Dollars (Mean) (Std.Dev.)		18.60841 (33.12762)

Table 13 Primary Attendance Summary Statistics

Variable N (Studies)	Value	Frequency 24
Compliance Mechanism	LCT	4 (17%)
	Information	3 (13%)
	Lenient	7 (29%)
	Strict	10 (42%)
Years of Exposure	1	11 (46%)
	2	9 (38%)
	3	2 (8%)
	5	1 (4%)
	10	1 (4%)
Start-Up Constraints (age=1 year)	0 No	13 (54%)
	1 Yes	11 (46%)
Mother Receives Payment	0 No	17 (71%)
	1 Yes	7 (29%)
National vs Pilot Program	0 Pilot	9 (38%)
	1 National	15 (63%)
Payment Frequency – 1= monthly or bimonthly (0=less frequent)	0	6 (25%)
	1	18 (75%)
Latin America or Caribbean Program	0 No	12 (50%)
	1 Yes	12 (50%)
Africa	0 No	18 (75%)
	1 Yes	6 (25%)
Achievement Conditions Present	0 No	16 (67%)
	1 Yes	8 (33%)
Program Makes Supply-Side Investments	0 No	23 (96%)
	1 Yes	1 (4%)
Meets Evidence Standards (W or W/o Reservations)	0 No	3 (12.5%)
	1 Yes	21 (82.5%)
Secondary Baseline Enrolment, (Mean) (Std.Dev.)		.51890417 (.18144166)
Average Monthly Transfer per Person in 2015 Dollars, (Mean) (Std.Dev.)		26.20373 (35.63518)

Table 14 Secondary Attendance Summary Statistics

Information	3.310
	(2.681)
Lenient	3.050
	(2.532)
Strict	5.125***
	(1.679)
Meets evidence standards	-1.808
	(2.793)
Baseline enrolment	-31.320***
	(9.928)
Exposure	-0.074
	(0.332)
Mother	0.510
	(2.043)
National	-2.511
	(2.292)
Transfer frequency	1.608
	(2.114)
Average transfer	0.022
	(0.020)
Achievement conditionality	-0.300
	(2.457)
Supply	6.920***
	(1.813)
Constant	28.639**
	(10.591)
Observations	27

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Information	-0.753
	(10.865)
Lenient	2.598
	(11.478)
Strict	-5.203
	(10.543)
LAC	-2.097
	(4.831)
Africa	-14.716*
	(6.890)
Meets evidence standards	1.342
	(12.751)
Baseline enrolment	-34.495
	(29.681)
Exposure	0.123
	(1.098)
Mother	-5.561
	(5.606)
National	6.044
	(7.505)
Average transfer	0.020
	(0.113)
Achievement conditionality	4.806
	(7.525)
Supply	-15.977
	(10.143)
Constant	26.194
	(30.130)
Observations	23

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 15 Primary and secondary meta-regression with individual compliance dummies

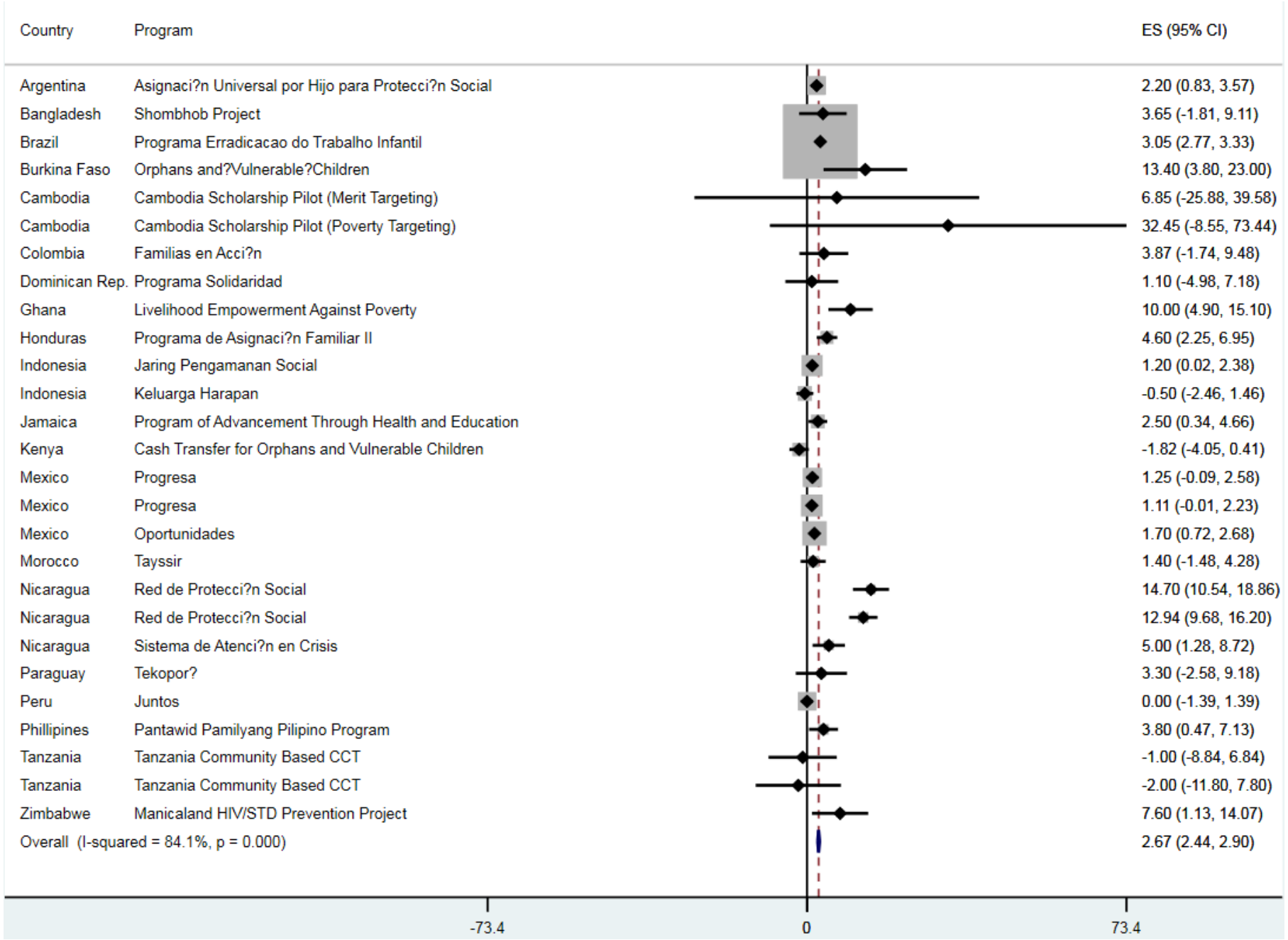

Figure 3 Primary Effect Size (%) Forest Plot

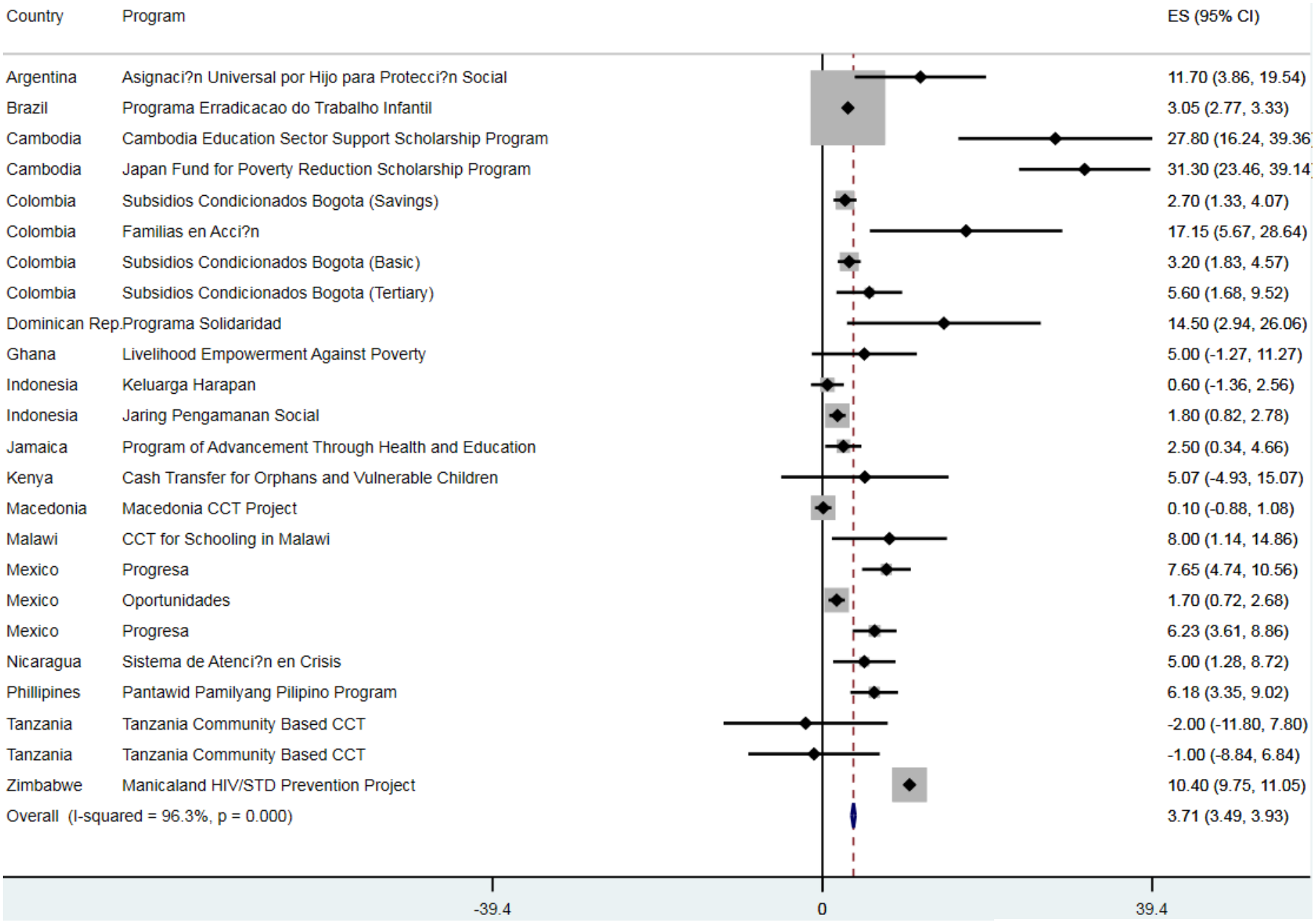

Figure 4 Secondary Effect Size (%) Forest Plot

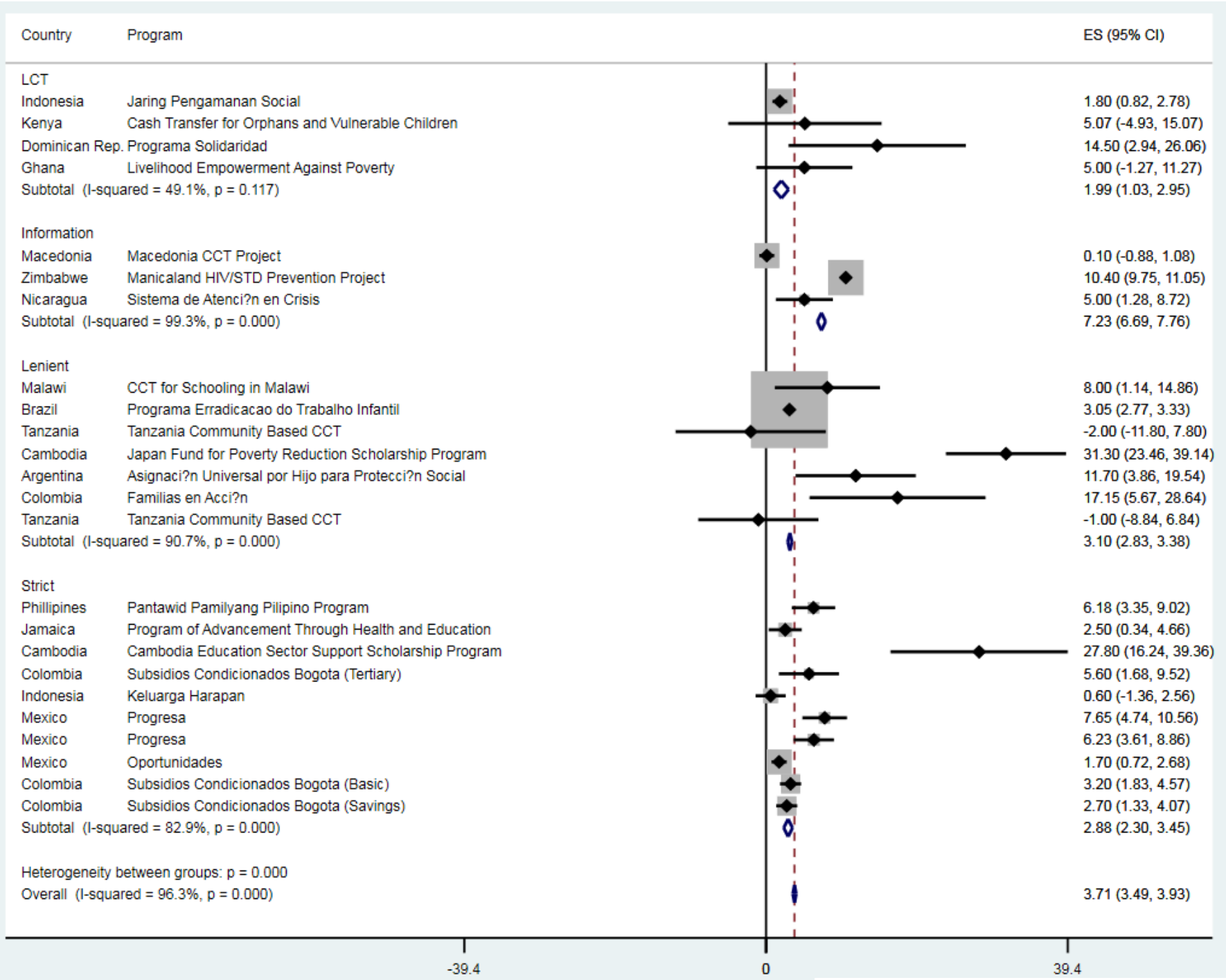

Figure 5 Primary forest plot sorted by compliance mechanism

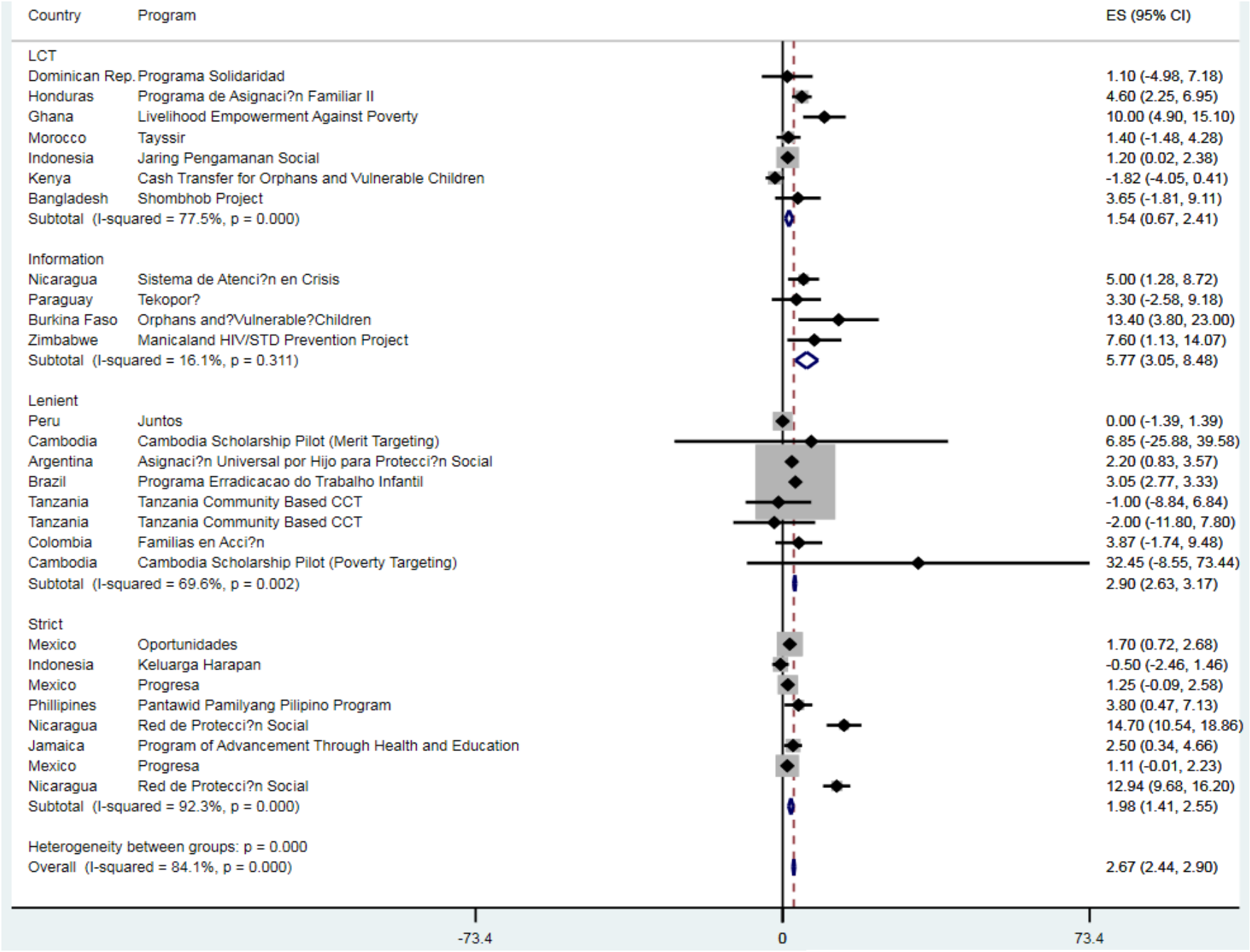

Figure 6 Secondary forest plot sorted by compliance mechanism