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The complex effects of public policy on child labour

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As part of broader efforts towards durable solutions to child labor, the International Labour Organization (ILO), the United Nations Children's Fund (UNICEF), and the World Bank initiated the interagency Understanding Children's Work (UCW) Programme in December 2000. The Programme is guided by the Oslo Agenda for Action, which laid out the priorities for the international community in the fight against child labor. Through a variety of data collection, research, and assessment activities, the UCW Programme is broadly directed toward improving understanding of child labor, its causes and effects, how it can be measured, and effective policies for addressing it. For further information, see the project website at www.ucw-project.org.

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MAPPING THE IMPACT OF PUBLIC POLICY ON CHILD LABOUR

In 2011 the Understanding Children's Work program launched an extensive effort to map the evidence on the impact of public policy on child labour. On the basis of the wide-ranging evidence we drafted two working papers, each with a distinct aim. The current paper is the overarching result of the mapping exercise. It reviews the impact of interventions falling in six broad intervention clusters: (i) social protection, (ii) education, (iii) labour markets, (iv) human settlement, (v) microfinance, and (vi) health and family planning. A second paper, entitled "Cash Transfers and Child Labour", takes an in-depth look at the effects of cash transfer programs focusing on issues such as heterogeneity, spillover effects, long-run effects, and protection from shocks. It also provides background on the prevalence of child labour around the world.

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ABSTRACT

This paper reviews how public policy and programs affect child labour. The paper discusses the evidence generated by rigorous, although not necessarily randomized, impact evaluations of (i) social protection programs, (ii) education interventions, (iii) labour market oriented interventions, (iv) human settlement programs, (v) microfinance programs, and (vi) health and demographic interventions. Although interventions in all six policy clusters can reduce child labour, reductions in child labour are not guaranteed. In fact, some interventions (such as those that affect intra-household specialization in productive activities) can increase child labour. We build on the evidence to provide directions for a systematic research agenda.

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CONTENTS

1. INTRODUCTION

1. A wide variety of policies and programs implemented by governments, international organizations, and NGOs either explicitly aims to tackle child labour or may influence it even if the policy was designed to achieve other primary objectives. Over the past decade, rigorous evaluations have examined the impact of a substantial number of these programs on child labour. This paper is the first to provide a systematic review of the evidence on interventions that can broadly be grouped into 6 policy clusters relevant for child labour: (i) social protection, (ii) education, (iii) the labour market, (iv) human settlement, (v) access to finance, and (vi) health and family planning.

2. Children's labour supply is the outcome of a complex intra-household decision making process.[1] Public interventions can alter the intra-household productive structure and incentives in complex ways, making it hard to predict their net effect. The evidence we review helps to overcome the theoretical ambiguity and provides an indication of the net effect of the different the interventions. Our review suggests that interventions based on transfers of resources (whether unconditional or conditional, in cash or in kind) tend to reduce child labour, although reductions in child labour are *not* guaranteed.[2] The impact of cash transfers appears to depend partly on their integration with other interventions. Combining (conditional) cash transfers with supply side interventions such as provision of health and education facilities and/or after school education possibly increases impact on child work. Interventions that aim to improve income-generating activities may reduce the impact of conditional cash transfer on child labour, by generating increased demand for children time within the household.

3. Programs affecting household productive activities do not necessarily reduce child labor. A public works scheme in Ethiopia, for example, reduced child labour. However, when the public works program was combined with food security interventions it resulted in increased child labour. Similarly, the impact of microcredit interventions is mixed. In Bosnia, a microcredit scheme significantly increased work participation of older children and in Malawi a microcredit program significantly reduced child labour while another significantly increased it.

4. Education and health interventions do not uniformly lower child labour either. An extensive education intervention in Burkina Faso, combining multiple interventions to increase school participation, did not reduce child labour although it effectively increased school participation.

[1] We refer the reader to Cigno and Rosati (2005) and Edmonds (2007) for theoretical discussion of this process.

[2] For in-depth discussion of the impact of cash transfers on child labour, focusing on issues such as heterogeneity, spillover effects, long-run effects, and protection from shocks, see De Hoop and Rosati (2013a & 2013b). Those papers also provide more background on the prevalence of child labour around the world.

The effect of health and demographic interventions is not uniform either. A family planning program in Bangladesh increased boys' participation in work by roughly 10 percentage points. Provision of anti-retroviral treatment to HIV/AIDS positive adults in Kenya, on the other hand, resulted in a very pronounced reduction in work of boys residing in the same household.

5. Importantly however, the evidence we have for non-transfer interventions is limited, this makes it challenging to draw general conclusions. Moreover, the evaluation of the impact of public policy on child labour does not appear to be driven by a coherent and systematic research agenda. Gender dimensions of child labour are often ignored, there is virtually no evidence on changes in the worst forms of child labour, and for some important intervention categories evidence is lacking altogether. Towards the end of this paper we discuss these challenges and shortcomings in more detail, hopefully providing guidance for the direction of future research.

2. METHODOLOGY

2.1 Search Strategy

6. In 2011 the Understanding Children's Work project (UCW) launched an extensive effort to construct a comprehensive online database of all rigorous impact evaluations looking at child labour. As part of this effort a literature search was carried out, covering Google Scholar, the World Bank Development Impact Evaluation Initiative (DIME), the Poverty Action Lab, the Social Science Research Network (SSRN), the Network of Networks for Impact Evaluation (NONIE), and the International Initiative for Impact evaluation (3IE). The research team also drew heavily on the extensive network of the UCW project (in international partner organizations and academic institutions) to obtain information on further relevant impact evaluations.

7. We drew on the UCW inventory to obtain an overview of the relevant literature. We decided to include all of the peer-reviewed papers in this review. We included non-reviewed studies only if they apply a plausible strategy to deal with endogenous program placement and self-selection into the program.3 We decided not to limit our overview to randomized trials, but also to consider other methodologies such as regression discontinuity designs, natural experiments, and propensity score matching studies.4 We discussed all papers in detail to avoid bias and to ensure that they provide sufficient detail and rigor (in terms of

[3] Sometimes doubts arise regarding the strategy used to deal with endogenous program placement and self-selection in both peer reviewed and non-reviewed papers. In those cases we discuss these doubts in the text.

[4] For readers requiring more background, we recommend the following references: Duflo, Glennerster, and Kremer (2008), Gertler et al. (2011) and Khandker, Koolwal, and Samad (2010).

identification strategy, estimation procedure, and data collection) for inclusion in the review. To further ensure comprehensiveness of the review we conducted citation searches for the included papers.5For the purpose of this paper we included all impact evaluations identified by the summer of 2012 (there is no restriction on the start date of the search). No further selection criteria were applied.

2.2 Concepts

8. Child labour is a legal rather than statistical concept. The three principal international conventions on child labour – ILO Convention No. 138 (Minimum Age), United Nations Convention on the Rights of the Child, and ILO Convention No. 182 (Worst Forms) together set the legal boundaries for child labour, and provide the legal basis for national and international actions against it. The translation of these broad legal norms into statistical terms for measurement purposes is not straightforward. The international legal standards contain a number of flexibility clauses left to the discretion of the competent national authority in consultation (where relevant) with worker and employer organizations (e.g., minimum ages, scope of application). Therefore, there is no single legal definition of child labour across countries and concomitantly there is no single standard statistical measure of child labour.

9. Consequently, the terminology and concepts used to categorize children's work and child labour (and to distinguish between the two) are at times inconsistent in published statistics and research reports. Similarly, there is substantial variation in the productive activities covered by the impact evaluations discussed in this review. Some studies focus on specific activities (such as work in agriculture) whereas others use a more general definition of work (such as work for pay). There is also variation in the reference period. Some studies look at work in the 7 days prior to the household survey, some studies look at work in the past 2 days etc. Moreover, where some studies focus on the extensive margin of child work other studies examine indicators of the intensity of work (e.g. hours worked). Finally, some studies present results for different categories of activities.

10. As a consequence, at times we necessarily compare somewhat different outcomes in this review. To achieve a minimum degree of consistency, our discussion primarily focuses on impact estimates for children's participation in economic activities conducted for pay and/or for the household (i.e. excluding household chores). However, in some cases it is relevant to discuss program impacts for different categories of economic activities or household chores. We mainly focus on the extensive margin of child labour (as this is the outcome that most studies examine), although at

[5] Moreover, the paper was presented on multiple occasions to diverse audiences, allowing for further feedback on relevant papers to be included.

times we present additional evidence on the intensive margin of child labour. The exact definition of child labour used in each individual study is summarized in appendix 1.

2.3 Presentation of the Results

11. We summarize the impact of each group of interventions in Tables 1 to 10. For each program the tables provide the following information: (i) the reference for the impact evaluation study, (ii) the method used to identify the impact of the program, (iii) the outcome variables considered, (iv) the stratum covered by the impact estimate, (v) the impact estimate, standard error, and significance level, and (vi) the average prevalence of child work in a comparison group. The displayed impact estimate is the authors' preferred estimate of the change in the extensive margin of child labour that can be attributed to the program. The information about the comparison group helps to understand the relative magnitude of the impact of the project. Our preferred reference group is the control group at follow-up. If the prevalence of child labour for this group is not available, we resort to other control groups, such as the intervention group at follow-up or the control group at baseline.

3. SOCIAL PROTECTION PROGRAMS

12. This section presents the results for three types of transfers (unconditional cash transfers, conditional cash transfers, and conditional in-kind transfers) and for public works schemes.[6]

3.1 Unconditional Cash Transfers

13. We identified 6 studies that examine the impact of unconditional cash transfers on child labour (5 of which are summarized in Table 1).[7] Broadly, the evidence suggests that unconditional cash transfers tend to reduce child labour. Two studies of pension schemes suggest that the mitigation of credit constraints is one of the primary channels by which (unconditional) cash transfer programs lower child labour.

14. We start by discussing the Bono de Desarollo Humano program in Ecuador. This program provides means tested income transfers equivalent to roughly 7% of monthly household expenditures to households in the poorest two quintiles of the Ecuadorian population. Two studies of this program rely on a household-level randomized trial, conducted as part of the rollout of the program. Schady and Araujo (2006) find that, roughly one

[6] We did not find rigorous evaluations of unconditional in-kind transfers, which is why this intervention category is not discussed in this review.

[7] One paper, DSD, SASSA and UNICEF (2012), is not included in Table 1 because, as we explain, its dose response methodology differs markedly from the methodology used in the other papers.

year after the start of the program, 6-17 year old children (at baseline) in a treated household were 6.2 percentage points less likely to participate in economic activities than children in the control group (mean participation rate in control group is 54%). Edmonds and Schady (2011) find that 11 to 16 year old children (also at baseline) in treated households were 8 percentage points less likely to participate in economic activities (mean in control is 71%). The latter paper also shows that participation in household chores[8] was not significantly reduced.

15. As Edmonds and Schady (2011) explain, there was substantial non-compliance with the treatment status assigned in the randomized trial: 38 percent of children aged 11-16 in the control group received transfers while only 69 percent of children 11-16 in treatment group received the intervention. Both papers therefore also use eligibility status as an instrument for participating in the program to estimate the effect of the lottery-induced take-up of the unconditional cash transfer scheme (i.e. the effect of treatment on those who comply with the original treatment assignment). The IV estimates substantially exceed the intent-to-treat estimates. Schady and Araujo (2006) find a 17.2 percentage point reduction in economic activities of 6-17 year old children, while Edmonds and Schady (2011) find a 24.5 percentage point decrease for 11-16 year olds. Edmonds and Schady (2011) show that reductions in economic activities for pay were pronounced and represent about a third of the overall reduction in economic activities.

16. The second study we look at is by Covarrubias, Davis, and Winters (2012), who examine the impact of a cash transfer program in Malawi, called the Malawi Social Cash Transfer Scheme (SCT). The SCT provides cash transfers ranging in value from US$4 to US$13 per household per month depending on the number of household members (average monthly per-capita income in the initial target district was US$7.80). In addition, the program offers a bonus ranging from US$1.30 per month for primary school age children to US$2.60 per month for secondary school age children.[9] The program is expected to serve 300,000 households with 910,000 children by 2015. The study by Covarrubias Davis, and Winters (2012) is based on a one-year pilot in which 8 groups of villages (containing 23 villages in total) were randomly divided into an equally sized treatment group and control group (4 groups of villages each). The authors compare children from participating households to children from similar households in control areas using difference in differences regressions controlling for a vector of baseline controls and difference in differences propensity score matching.[10]

[8] results not displayed in Table 1

[9] It is not explicitly discussed whether this bonus is conditional on school participation.

[10] Given that the number of clusters is limited (and standard errors cannot be clustered) this study cannot be interpreted as a randomized trial.

17. The authors find that "there is evidence of reductions in child labour outside the household", but "the time freed seems to be replaced with greater involvement in within-household tasks." To give some examples, participation in household chores increased significantly by 8 to 14 percentage points (48% in control group at baseline). Depending on the estimation procedure work in the family farm or family business did not change significantly or increased by 7 percentage points (22% in the control group at baseline). And participation in paid domestic work outside the household decreased significantly by 7 to 8 percentage points (6% in the control group at baseline). Other estimates, not displayed in Table 1, indicate that the program did not affect children's participation in care for children or for adults in the household. Work outside the household for income was also not affected by the intervention, while depending on the specification, work for a family farm or non-farm business may have increased significantly or did not change.

18. Next, we turn to two large-scale unconditional cash transfer schemes in South Africa. The first scheme is the Child Support Grant (CSG), which provides means tested transfers to the (caregivers of) children growing up in South Africa's poorest households. Over the past decade and a half, the CSG has expanded rapidly. The program raised the age limit for participating children from 7 to 17, loosened the means test on the basis of which it is allocated (maximum income of caregiver and spouse equal to 10 times the value of the grant), and removed conditions such as caregiver's participation in "development programs". As a result, the number of children receiving the grant has increased rapidly and by 2012 the CSG covered nearly 10 million beneficiaries.[11]

19. DSD, SASSA and UNICEF (2012) use dose-response analysis to identify the impact of the program. Dose-response analysis does not identify the impact of receiving a program versus not receiving the program. Instead, it shows whether receiving a program for a longer period of time results in a stronger impact (in this case reduction in child labour) than receiving the program for a shorter period of time. To ensure that the treatment and control group are comparable, the authors rely on a propensity score methodology for continuous treatments developed by Hirano and Imbens (2004).

20. Among 10 year old children, the probability of being involved in household chores hardly changed with the age at which the child enrolled in the CSG. The authors find that the CSG did affect the probability that an adolescent aged 15-17 works outside the home. 21% of individuals who started receiving the grant at the age of 6 works outside the home compared

[11] Despite the wide coverage of the CSG, a substantial share of eligible children (households) does not participate in the cash transfer scheme. The reasons for exclusion of eligible households, such as misperceptions regarding eligibility criteria and difficulty in acquiring the necessary documentation, are described in detail in DSD, SASSA and UNICEF (2012). The organizations indicate that exclusion of poor, vulnerable children in the South African population motivates serious consideration of universal provision of the Child Support Grant."

to 14% of individuals who started receiving the grant at the age of 0. The dose response curve is considerably steeper for girls (drop from 20% when started receiving the program at the age of 16 to 7% when started receiving the program at the age of 0) than for boys (drop from 28% to 23%). However, the authors do not show whether differences between individuals treated from the age of 0 and those treated from the age of 16 are significantly different.[12]

21. The second South African unconditional cash transfer scheme we discuss is a nationwide old-age pension scheme. The pensions are means tested and, as a result, primarily cover the comparatively deprived black population of South Africa. Conditional on passing the means test, women become eligible for receiving pensions at the age of 60 and men at the age of 65. The benefits provided by the pension scheme are large; in 1999 they represented roughly 125% of median per capita income of South Africa's black population. To identify the impact of these pension benefits on child labour, Edmonds (2006) compares children in rural households with a nearly eligible elderly person (i.e. a person who is poor and nearly old enough to start receiving the transfers) to children in rural households with a person who just became eligible to participate in the program (and are thus similar in all respects including the present value of the pension stream they expect to receive) in a regression discontinuity framework.

22. The old-age pensions in South Africa provide substantial benefits and are highly institutionalized. These old-age pensions thus represent an *anticipated* future income stream. In the presence of perfect capital markets, the behavior of households should be similar above and below the pension age and we should not expect to find a discontinuity in households child labour supply when a poor elderly person reaches the eligibility age. Interestingly, however, Edmonds (2006) finds that school participation of 13 to 17 year old children in the household increased substantially when an eligible elderly person reached the pension age, particularly for male pensioners. Children's participation in economic activities did not decline significantly when an elderly person in the household became eligible for the old-age pensions. However, Edmonds (2006) finds substantial and significant declines in hours worked when a pensioner reaches the eligibility age (-0.6 hours for male pensioners and -0.5 hours for female pensioners). Further results not displayed in Table 1 suggest that boys experience stronger reductions in time spent in economic activities than girls. Girls appear to experience stronger reductions in time spent in household chores. The fact that these effects are observed when individuals reach retirement age is an indication of the important role of credit constraints.[13]

[12] These results are not displayed in Table 1 since the nature of the dose response estimates differs from the usual treatment / control estimates displayed in this and the remaining tables.

[13] Alternative explanations for the observed impact, which Edmonds (2006) deems less plausible, include time-inconsistent preferences not captured in the standard theories of human capital investment.

23. A study of Brazil's social pension scheme by de Carvalho Filho (2012) broadly confirms the findings of Edmonds (2006). To identify the impact of this program, the author exploits a 1991 social security reform that (i) increased the minimum benefit paid to rural old-age beneficiaries from 50 percent to 100 percent of the minimum wage, (ii) reduced the minimum eligibility age for rural old-age benefits for men from 65 to 60, (iii) extended old-age benefits to female rural workers who were not head of their households, and (iv) reduced the age at which women qualified for benefits from 65 to 55. The author compares rural households that became eligible to receive old-age benefits as a result of the reform (such as households with men aged 60-64) to rural households that were nearly eligible (such as households with men aged 55-59) after the reform. To correct for age-specific trends not related to the reform, the author compares difference-in-differences estimates for rural households to difference-in-differences estimates for urban households (which were not affected by the reform) in a triple difference framework.

24. Reduced form estimates (not displayed in Table 1) indicate that for the age group 10 to 14 the reform significantly increased in particular girls' school participation and reduced their participation in economic activities for pay. IV estimates that disentangle this effect show that an increase in benefits of 100 Reais increases girls' school enrollment by nearly 10 percentage points and reduces the probability that girls work for pay by 3.6 percentage points. The gender of the benefit receiver appears to matter for girls' participation in economic activities for pay: only benefits provided to women result in a significant reduction in this outcome variable. Hours worked (not displayed in Table 1) also decrease significantly only when the benefits are received by women. No changes can be observed for households with individuals just below the post-reform eligibility ages. The latter finding suggests that credit constraints, similar to those observed for South Africa by Edmonds (2006), are at play.

3.2 Conditional Cash Transfers

25. Conditional cash transfers have been evaluated extensively.[14] In this section we review conditional cash transfer schemes that include human capital accumulation requirements either as the only condition for disbursement of the cash transfer or as part of a broader set of conditionalities. We only discuss evidence from evaluation studies that include child work as an outcome. Table 2 synthesizes the average impact of all conditional cash transfer programs for which rigorous estimates of their impact on the prevalence of child labour are available. Some of the evaluations provide only disaggregated estimates by gender and/or age.

[14] We refer the reader to Fiszbein and Schady (2009) for a comprehensive review of the recent proliferation of conditional cash transfer schemes in developing countries including a detailed discussion of the (political economy) arguments in favour and against these schemes.

Because we want to provide a comparison of average program effects, we impute the average program effect for these studies by taking the unweighted mean of the impact estimates given for different age and gender groups (these imputed estimates are marked with an ǂ in the table).[15] To the extent possible, we separately show impact estimates for urban and rural areas.

3.2.1 Latin America's Flagship Programs: Oportunidades and Bolsa Escola

26. We begin with Mexico's conditional cash transfer program called Oportunidades.[16] Oportunidades provides poor Mexican households with monthly cash transfers equivalent to approximately 20% of average recipient household income on the condition that children in the households attend school and that all household members obtain preventive medical care and are present at health education talks.[17] The program is among the largest conditional cash transfer programs in the world: by 2010 it reached approximately 5.5 million households (more than 20% of all households in Mexico) living in nearly 100,000 marginalized localities (14% of which were located in urban and semi-urban areas).

27. Evidence on the impact of this program on child labour is mixed. Estimates based on a cluster-randomized trial suggest that the impact of the cash transfer scheme on child labour in *rural* areas is modest. The estimate displayed in Table 2 averages over estimates for multiple age and gender subgroups from a study by Skoufias and Parker (2001). According to this aggregated estimate, the program had no significant effect on economic activities conducted by children aged 8 to 17 (prevalence of child labour is 15% among baseline beneficiaries). However, Skoufias and Parker (2001)'s disaggregated estimates show that Oportunidades did result in a statistically significant reduction in work for older children (aged 12-17).[18] In accordance with the latter finding, Schultz (2004) finds only a limited effect of the program among primary school pupils, but a markedly stronger effect among secondary school pupils.

28. Because the conditional cash transfer scheme was only randomly phased-in in rural areas, experimental analysis is not possible in urban areas. Berhman et al. (2010) therefore rely on a non-experimental matching procedure to identify the impact of the cash transfer scheme in urban areas. Our aggregated estimate based on their non-experimental estimates suggest

[15] To calculate standard errors associated with these estimates, we assume that the covariance between the individual estimates is zero.

[16] Oportunidades was initially known under the name PROGRESA.

[17] The program also provides various supply side interventions, such as extra resources to primary schools that enroll students in disadvantaged rural communities. These supply-interventions appear to play a limited role in comparison with the conditional cash transfer interventions (Coady and Parker, 2002). We therefore treat Oportunidades as a pure conditional cash transfer program in this paper.

[18] Non-significant estimates of a similar order of magnitude were found by Diaz and Handa (2006) for the one-year impact of the program on work of pay of 12 to 16 year old children..

that in this setting Oportunidades reduced 12-14 year old children's participation in economic activities for pay by 7 percentage points (6% in the control group at baseline). The disaggregated estimates on which this average is based suggest that this impact is almost entirely due to a reduction in work participation of boys.

29. Brazil's Bolsa Família is a similarly large-scale conditional cash transfer scheme covering a total of 13 million families nationwide.[19] Like Oportunidades in Mexico, it provides conditional cash transfers designed to strengthen access to basic social rights in the areas of education, health, and social care. A government evaluation, Secretaria de Avaliação e Gestão da Informação (2012), using difference-in-differences propensity score matching over the period 2005 – 2009, indicated that the program reduced employment among 5-17 year-olds by almost two percentage points and reduced the number of weekly hours dedicated to domestic work by 4.5 hours for the same age group. The program was also responsible for a delay of approximately 10 months in entry into the labour market for children and adolescent males.

30. Ferro, Kassouf, and Levison (2010) use data from 2003 to evaluate the impact of Bolsa Escola, an earlier national conditional cash transfer program which terminated in 2003. At the time, Bolsa Escola provided poor households with a monthly cash transfer of 15 Reais for each 6 to 15 year old child attending school at least 85 percent of school days. For comparison, Ferro, Kassouf, and Levison (2010) indicate that children who worked for pay earned on average 100 Reais a month. The authors use a propensity score matching method in which eligible households who signed up for the program but did not yet receive any benefits are compared to program beneficiaries. They argue that these two groups are similar, although they note that "because program approval is on a first-come-first-serve basis […] it is possible that persons who are already receiving the benefits have a stronger desire to participate or are more motivated than people who signed up later". Their findings suggest that Bolsa Escola resulted in a substantial reduction in economic activities. The estimated reduction in rural areas is 8.7 percentage points (34.8% in the control group at follow-up). The 2.5 percentage point reduction in urban areas is weaker but still statistically significant (9% in the control group at follow-up).

3.2.2 Other Programs in Latin America

31. Maluccio and Flores (2005) register a strong impact for the Red de Protección Social cash transfer program in Nicaragua. This program provides poor rural households with two bi-monthly transfers. One of these is provided contingent on attending educational workshops on topics such as sanitation, hygiene, etc. and on bringing young children (under the age of 5) to scheduled preventative healthcare appointments. The other transfer is

[19] Program overage according to http://www.mds.gov.br/bolsafamilia (accessed April 2013)

provided on the condition that older children (7-13 years) attend school regularly up to the fourth grade of primary school. The income transfer is equivalent to approximately 18% of total recipient annual household expenditures. Estimates based on a cluster-randomized trial (in which 42 geographical areas were randomized into a treatment and control group of 21 areas each) show that this program reduced children's participation in economic activities by 5.6 percentage points (12% in control group at follow-up).

32. Four other conditional cash transfer programs implemented in the Latin American and Caribbean region do *not* appear to have strongly affected children's participation in economic activities. Familias en Acción, is a nationwide program providing poor households in Colombia with cash transfers conditional on school participation of older children (7-17) as well as an unconditional nutrition subsidy for younger children (0-6). According to Fiszbein and Schady (2009) these transfers are equal to approximately 17% of pre-transfer consumption among all beneficiaries. This program was evaluated by Attanasio et al. (2010) by comparing poor households in program municipalities to poor households in purposefully chosen similar non-program municipalities using difference-in-differences propensity score matching and multivariate probit analysis. On average, the program did not result in a statistically significant reduction in the participation in economic activities for pay by 10 to 17-year-old children in either urban or rural areas.[20] However, an impact on adolescent (14-17 years of age) participation in economic activities for pays was observed. Participation in household chores was also reduced significantly for children aged 10-17 in urban areas.

33. Jamaica's Program of Advancement Through Health and Education (Path) is a nationwide program providing health grants for young children conditional on visits to health clinics, education grants to older children conditional on regular school participation, and waivers for health and education fees.[21] The transfers cover approximately 8% of recipients' pre-transfer consumption (Fiszbein and Schady, 2009). Poor households eligible for participation in Path are identified on the basis of a vulnerability index. Levy and Ohls (2007) show that households just above and below the threshold score in the vulnerability index were similar at baseline and provide regression discontinuity estimates suggesting that the program had no statistically significant effect on children's participation in economic activities or household chores.

34. The third program that does not appear to have affected child labour is the cash transfer component of Uruguay's Plan Nacional de Atención a

[20] A preliminary analysis of the impact of the program on income generating work by children (conducted only a few months after the start of the program) also found no impact (Institute for Fiscal Studies, 2004).

[21] The program also provides unconditional benefits to poor pregnant and lactating women and the elderly poor. Initially the benefits provided to the elderly were also conditional on visits to health clinics, but these conditionalities were soon dropped.

Crisis (PANES). PANES was a temporary anti-poverty program implemented in 2005 to provide direct assistance to households that had experienced a rapid deterioration in living standards because of the economic downturn of 2001-2002. The cash transfer scheme provided income transfers equal to roughly 50% of the average self-reported pre-program income. The transfers were conditional on school attendance and health checkups, although enforcement of these conditions appears to have been limited. The program targeted the bottom quintile of households below the national poverty line. In total, approximately 10% of Uruguayan households became program beneficiaries. Regression discontinuity estimates, which rely on the vulnerability index used to establish program eligibility, provide no evidence that the cash transfer program affected child labour (Amarante, Ferrando, and Vigorito, 2011).[22]

35. Finally, Glewwe and Olinto (2004) and Galiani and McEwan (2011) investigate the impact of the Honduran conditional cash transfer scheme called Programa de Asignación Social II (PRAF-II).[23] This program was implemented as a small scale prototype upon which the country's larger scale PRAF-I cash transfer scheme, which covered more than 600,000 Hondurans in 2005 (Moore, 2008), would later be remodeled. The cash transfer scheme provides relatively modest cash transfers conditional on school and health clinic attendance equal to approximately 7% of pre-transfer consumption of beneficiary households. Both Glewwe and Olinto (2004) and Galiani and McEwan (2011) exploit a cluster-randomized trial that randomly assigned the program to 40 out of 70 poor municipalities. Using household survey data collected as part of the pilot program, Glewwe and Olinto (2004) find no evidence that the program had a statistically significant effect on children's participation in economic activities (11.6% in control group). Galiani and McEwan, who use census data, also find no evidence of a statistically significant reduction in the prevalence of economic activities (9.9% in control group) in the entire study population. Both studies do find an effect on child labour among children from poor socioeconomic backgrounds.

3.2.3 Programs Outside Latin America

36. Interestingly, the strongest registered percentage point impact was generated by the CESSP scholarship program in Cambodia, one of three rigorously evaluated conditional cash transfer schemes *outside* the Latin America and Caribbean region. When the study of this cash transfer scheme was conducted, the program was active in 100 of Cambodia's 800 middle

[22] The paper does not define what type of activities the outcome indicator represents.

[23] The estimates of Galiani and McEwan (2011) differ from those of an earlier study by Glewwe and Olinto (2004). Galiani and McEwan (2011) discuss in detail some concerns regarding this study and their methods to circumvent these concerns.

schools.[24] Its benefits were allocated to a subgroup of the pupils in the final grade of the primary schools that feed to these middle schools. This subgroup was selected on the basis of an index that predicted the likelihood of pupils dropping out of school. Exploiting this assignment procedure in a regression discontinuity framework, Ferreira, Filmer, and Schady (2009) find that the program reduced children's participation in economic activities for pay by 10.4 percentage points (33.8% in control group at follow-up) among the first cohort of CESSP students. Given that the program provided only a modest transfer (equivalent to between 2 and 3 percent of the total expenditures of the average recipient household) it induced a remarkable reduction in child labour.

37. The impact of female school stipends provided in Pakistan also lies in the upper half of the impact distribution. This program too provided households with only a very limited cash transfer equal to 3.4% of median household expenditures (just sufficient to cover the cost of attending school) conditional on sufficient school attendance. In 2007 it covered approximately a 250,000 girls in middle schools in the province of Punjab. Alam, Baez, and Del Carpio (2011) identify the impact of this program using a difference in differences regression discontinuity methodology, which exploits the fact that district eligibility was determined on the basis of the average district literacy levels. Their estimates suggest that girls' participation in economic activities decreased by 4.9 percentage points in response to the program (no comparison group is given in the paper).

38. The Indonesian conditional cash transfer program called Jaringan Pengaman Social, a temporary program, was implemented in 1998 to preserve education access during a sharp economic crisis. Under the program conditional cash transfers were provided to almost 4 million primary and secondary school pupils. The amount transferred increased with grade attended and varied from 7 to 18% of average per capita household consumption. Participation in the program was conditional on enrollment and on passing the grade at the end of the school year. The cash transfers were targeted primarily at the poorest districts of Indonesia and within those districts they were targeted specifically at disadvantaged students. Sparrow (2004) uses exogenous variation in treatment status due to unintended mis-targeting to identify the impact of this program. Instrumental variables estimates show that the program reduced participation in economic activities for pay or for the household for 10 to 18 year old children (conditional on enrollment) by 1.4 percentage points in urban areas and 5.1% in rural areas (average participation in work at follow-up combined in urban and rural areas is nearly 16%).

[24] The authors explain that these "schools were selected on the basis of administrative data which indicated that poverty rates in the areas served by these schools were high and, by implication, secondary school enrollment rates low."

3.2.4 Variations on the Basic Conditional Cash Transfer Scheme

3.2.4.1 Conditional Cash Transfers with a Twist

39. Some studies examine how the impact of cash transfer programs changes under deviations from the pure conditional cash transfer scheme (see Table 3). Here, we present the results of studies that examine what happens when conditional cash transfer programs are combined with saving or investment services. In the next subsection we discuss studies that look at combining conditional cash transfer programs with supply side health and education interventions.

40. Del Carpio and Loayza (2012) rely on a randomized experiment in Nicaragua to compare the impact of simple conditional cash transfers to that of conditional cash transfers combined with a "grant for productive investments" to start a new income-generating non-agricultural activity. The grant was provided to households conditional on the development of a business plan and was accompanied with technical assistance and training in basic business skills. The authors find that the simple conditional cash transfer scheme and the cash transfer plus grant for productive investments do not have the same impact on child participation in economic activities or household chores: the conditional cash transfers in combination with the productive investment grant reduced child labour by 0.94 hours a week whereas the basic conditional cash transfer reduced child labour by 1.76 hours a week. The difference between these two point estimates is statistically significant at the 5% level. The fact that the transfers combined with a grant for investment have a weaker impact on child labour is consistent with the hypothesis that promoting household business might increase demand for child labour.

41. Similarly, Barrera-Osorio et al. (2008) rely on a randomized experiment to explicitly compare the impact of simple conditional cash transfers to the impact of a variation on the traditional conditional cash transfer that includes a built-in savings component. The latter intervention provided regular cash transfers that were equal to two-thirds of the basic conditional cash transfer. The remaining third was automatically held in a savings account and made available to households in the period in which students enroll for the following school year. The savings component should help households cope with long-run savings constraints that keep children from proceeding from one grade to the next. The authors provide results for two subgroups: pupils in grades 6-10 and pupils in grade 11. Among pupils in grade 6-10, the intervention with the built in savings component resulted in a smaller reduction in hours worked last week (0.26 hours) than the basic conditional cash transfer program (0.38 hours).[25] Among pupils in grade 11 neither the basic transfers nor the transfers with built in saving scheme resulted in a statistically significant change in labour.

[25] No further definition of work is provided.

42. The authors also provide evidence on the impact of a second variation of the basic conditional cash transfer scheme, which changes the conditionality requirements and the amount transferred to the household. In this variation, the monthly subsidy was reduced by a third, but students earned the right to receive a graduation transfer (worth twice what the other pupils received in transfers over the period of one year). In this case, pupils who were in grades 6-8 were randomly assigned to a control group and a group receiving basic transfers. Pupils who were in grades 9-11 were randomly selected into a control group and a group receiving the conditional cash transfers with graduation grant.

43. Because these two types of treatment were administered to pupils in different grades and in different districts, we cannot directly compare the impact of the basic conditional cash transfers and conditional cash transfers with graduation grant.. However, if we are willing to make the assumption that there are no structural differences between districts, we can compare the impact of the basic conditional cash transfers in one district to the impact of conditional cash transfers with graduation grant in the other among pupils in grade 11. We observe that the latter variant of the conditional cash transfer scheme significantly reduced hours worked by 7 hours a week. It thus had a much stronger impact than the basic conditional cash transfers in San Cristobal, which did not significantly affect child work.

3.2.4.2 Conditional Cash Transfers Combined with Supply Side Interventions

44. Several studies investigate the impact of administering conditional cash transfers in combination with supply side interventions. Glewe and Olinto (2004) and Galiani and McEwan (2011) compare the impact of receiving Honduras' PRAF-II conditional cash transfers only (discussed in more detail above) to the impact of receiving the conditional cash transfer in combination with direct investments in the communities' health and education facilities. Glewe and Olinto (2004) find no statistically significant change in child participation in economic activities as a result both of the conditional cash transfers and of the cash transfers combined with supply side interventions. Galiani and McEwan (2011) also do not find evidence of a statistically significant reduction in economic activities outside the household as a result of the conditional cash transfers alone. However, they find that the conditional cash transfers in combination with the investments in health and education facilities did significantly decrease child work by 4.3 percentage points.[26]

45. The Programa de Erradicação do Trabalho Infantil (PETI) or Child Labour Elimination Programme in Brazil is another example of a

[26] The point estimates for conditional cash transfers in combination with investments in health and education facilities exceed those of conditional cash transfers only, but the estimated coefficients are not significantly different from each other

conditional cash transfer program combined with a supply-side intervention. PETI, which merged with Bolsa Familia in 2006, was targeted at poor households in rural areas of the country where the prevalence rates of child labour are high and included a mandatory after-school program that essentially doubled the length of the school day for participating children. The content of the after school education program differed per community and could contain academic and physical education components. The non-experimental identification strategy employed by Yap et al. (2002) to examine the impact of PETI suffers from significant drawbacks.[27] Nonetheless, given the obvious relevance of the setup of the intervention for child labour outcomes, we feel that it is worth briefly mentioning that the authors find that PETI lowered child labour by 5 to 25 percentage points in different regions. Taken at face value, these results suggest that the program resulted in strong reductions in child labour. The extended school day of the PETI programme is likely to have been instrumental in keeping children out of work.

3.2.5 Summing Up

46. While conditional cash transfer programs do often have an impact on child labour, a reduction in child labour does not appear to be guaranteed. The effect of conditional cash transfers on child labour differs considerably across contexts and programs. Impact estimates range from no statistically significant change in child labour to a reduction in child labour of 10 percentage points for Cambodia's CESSP scholarship program (Ferreira et al., 2010).

47. Impact on child labour tends to be higher when the prevalence of child labour is higher and the margin for improvement is larger (final column of Table 2). However, we also note that the relationship between the prevalence of child labour and the impact of conditional cash transfers is not mechanical. Some programs operating in high child labour environments have only a limited impact on child labour (e.g. Familias en Acción in Colombia and Oportunidades in rural Mexico) and vice versa some programs operating in low child labour environments do have a substantial impact on child labour (e.g. Bolsa Escola in Brazil).

48. Interestingly, there is little evidence that observed reductions in child labour are linked to the amount disbursed by the cash transfer program. The CESSP scholarship program in Cambodia, for instance, resulted in the second strongest decrease in child labour of all evaluated conditional cash transfer programs even though it provided only very

[27] To identify the effect of this program, Yap et al. (2002) compare children in 9 treatment municipalities to children in 9 control municipalities, which had expressed an interest in participating but were scheduled for later incorporation in the program. The authors do not discuss how municipalities were selected for early treatment by the program, so it is not clear whether their cross section regression estimates could be biased due to endogenous program placement. The authors also do not discuss how they calculate standard errors and whether these are clustered at the level of control municipalities.

modest transfers. Uruguay's PANES, on the other hand, provided income transfers equal to approximately 50% of average self-reported pre-program income and yet does not appear to have lowered child labour.

49. We also note that there is no clear evidence that the impact of conditional cash transfers tends to be stronger either in urban or rural areas. Ferro et al. (2010) and Sparrow (2004) respectively find stronger effects of Bolsa Escola in Brazil and Indonesia's Jaringan Penganam on child work in rural areas than in urban areas. On the other hand, Attanasio et al. (2010) show that Familias en Acción in Colombia reduced participation in economic activities for pay among 14-17 year old children living in urban areas. Among children living in urban areas it also resulted in a substantial reduction in participation in household chores. No significant changes were registered for children living in rural areas. Neither Behrman et al. (2010) nor Skoufias and Parker (2001) find an overall statistically significant effect of Mexico's Oportunidades on child labour in either urban or rural areas.

50. The impact of conditional cash transfer programs appears to depend partly on their integration with other interventions. When they are combined with supply side education and health interventions the impact on child labour appears to increase. However, when conditional cash transfers are combined with auxiliary services encouraging investment in productive assets and activities, they risk increasing children's participation in work.

3.3 Conditional In-kind Transfers

51. We examine the impact of two types of in-kind transfers on child labour: school vouchers and food for education programs (results are displayed in Table 4). School vouchers cover (part of) the cost of education at a public or private school. Given that school vouchers are of value only if the pupil enrolls, they are essentially conditioned on school participation. The same holds for the two main types of food for education programs: school feeding programs and programs providing take-home rations.

52. Although, conditional in-kind transfer programs are closely related to conditional cash transfer schemes, their impact on outcomes such as schooling and child labour is not necessarily equivalent. To the extent that the goods and services provided by conditional in-kind transfer programs are not fungible, they result in a more limited expansion of the consumption sets of the households than conditional cash transfers. The impact of conditional in-kind transfers might also differ from the impact of conditional cash transfers, because members of the household are forced to consume goods that potentially are complements to (or inputs for) the outcome of interest. For example, by improving the nutrition status of the child, school meals might have a stronger effect on education and child labour outcomes than conditional cash transfers of equal monetary value.[28]

[28] For a review of (the rationale behind) in-kind transfers we refer the reader to Currie and Gahvari (2007). For in-depth discussion of food for education programs see Adelman et al. (2008) and Bundy et al. (2006).

3.3.1 School Vouchers

53. Angrist et al. (2002) investigate the impact of Colombia's Programa de Ampliación de Cobertura de la Educación Secundaria (PACES), a program that provided vouchers to children from families in the lowest two of six income strata. The vouchers covered slightly more than half the cost of private secondary school fees. The vouchers were renewable conditional on satisfactory academic performance. Cities and towns used lotteries to allocate vouchers when demand exceeded supply and Angrist et al. (2002) rely on these lotteries to identify the impact of the program.

54. The program had a substantial impact on education outcomes: school attainment and performance on achievement tests improved. It did not affect the extensive margin of child work (not clearly defined in the paper) of either boys or girls. Point estimates for the reduction in child work are of the expected sign but not statistically significant. There is evidence, however, that the number of hours worked by girls decreased significantly by about 1.5 hours a week as a result of the program (2.7 hours a week in control group at follow-up).

3.3.2 Food for Education Programs

55. Adelman et al. (2008) and Bundy et al. (2006) discuss (the rationale behind) food for education programs. Adelman et al. (2008) argue that most studies examining the impact of food for education programs on school participation (and other education outcomes) have considerable limitations. Taking these limitations into account, they claim that there is evidence of these programs having modest effects on school participation.

56. Two papers investigate the impact of providing take home rations on child labour. Both studies find a substantial impact.[29] Ravallion and Wodon (2000) examine take-home rations distributed in Bangladesh using (non-random) program placement as an instrument for receiving the program. They find that the take-home rations reduced child participation in economic activities or household chores by 4 percentage points for boys and 2 percentage points for girls (respectively 12 and 13% in the control group at follow-up). These reductions in child work, however, are markedly lower than the increases in education amounting to 19 and 18 percentage points respectively for boys and girls.

57. Kazianga, de Walque, and Alderman (2009) exploit a cluster randomized trial in which schools in rural Burkina Faso were randomly assigned to 1 of 3 groups: a group in which *female* pupils receive take-home rations, a group in which *all* pupils receive school meals, and a control group. Among girls in schools assigned to the take-home rations

[29] Below, we discuss the impact of BRIGHT, a program that combines food for education interventions with a range of supply side education interventions in Burkina Faso.

group both farm and non-farm economic activities decreased significantly by 9 percentage points (respectively 57 and 16% among all girls in the control group at baseline). School meals did not significantly affect either of these two activities for boys or girls. It is not clear whether the value of the food disbursed through the school meals and take-home rations programs was comparable. Hence, it is not possible to say whether the difference in the impact of the interventions is due to the difference in the value of the transfer or to a differential impact of school meals and take-home rations as such.

3.4 Public Works Schemes

58. Public works programs guarantee employment during periods of low labour demand by providing a basic salary in return for work during relatively short periods. Similar to the transfer schemes discussed above the effect of these programs is hard to predict theoretically. Public works programs induce changes in the time allocation of adult household members that can result in offsetting income and substitution effects. On the one hand, the income provided as part of the program will tend to increase children's school participation and lower their participation in work. On the other hand, as adults enter the labour force, child labour may substitute adult labour inside or outside the household.

59. A study by Hoddinott, Giligan, and Taffesse (2009) evaluates the impact of a large-scale public work program in Ethiopia: the Public Safety Net Program (see Table 5). This program provides food or cash for work on labour-intensive projects designed to build community assets to poor households in 262 food-insecure Ethiopian districts. A subgroup of these households also benefits from a package of food security interventions including access to credit, irrigation, and water schemes as well as advice on agricultural technology. While the public work program was intended to protect households from asset depletion as a result of economic shocks, the food security interventions aimed to facilitate asset accumulation and income growth. Community officials were responsible for the selection of households most in need of the program.

60. Hoddinott, Giligan, and Taffesse (2009) use a representative survey implemented among beneficiary and non-beneficiary households in the intervention districts to evaluate the impact of this program. To identify its impact the authors use a nearest-neighbor matching procedure. Boys aged 6 to 10 decreased their weekly work activities by 4.7 hours (27 hours a week in the control group at follow-up). Separate estimates for hours in agricultural activities and hours in household chores indicate that the decrease in hours worked by boys is the result of a reduction participation in household chores *and* a reduction in agricultural activities. For older boys or girls the estimates are not statistically significant.

61. Interestingly, when the public works program is combined with food security interventions the impact estimates turn largely positive. Girls aged 6 to 10 exhibit a statistically significant increase in weekly work activities of 4.5 hours (23 hours a week in the control group at follow-up).[30] The point estimate for boys aged 11-16 is similar but not statistically significant. It is not clear if the *difference* between the impact estimates for the public works program only and the public works program combined with food security interventions is statistically significant for girls and boys in either age group. However, these estimates suggest that the food security interventions introduced a substitution effect that increased both girls' and boys' participation in agricultural work and household chores.

4. EDUCATION INTERVENTIONS

62. Evidence on the impact of the potentially the most relevant supply-side interventions (like better access to schools or improved quality of education) on child labour is limited. This section discusses two types of supply side education interventions for which we have some evidence: (i) preschools, which prepare young children for entry into the regular school system, and (ii) integrated education interventions, which combine a range of supply and demand side interventions. The results on which the discussion is based are displayed in Table 6.

4.1 Preschools

63. Pre-schools prepare young children for primary school attendance. By increasing children's opportunities to thrive in school and by sensitizing parents to the importance of school participation, pre-schools may affect school attendance and child work in the long run.

64. Martinez, Naudeau, and Pereira (2012) evaluate the impact of a pre-school program implemented in Mozambique by Save the Children in 30 villages that were randomly selected from a larger group of 98 eligible villages.[31] The program consisted of a range of interventions. Communities received technical assistance and materials for the construction of up to three classrooms with capacity for 35 children each. In addition, each community received technical assistance and materials to build playgrounds, child-sized latrines, and a washing station. Each class was staffed with two volunteer teachers selected by the schoolmanagement committee. Finally, parents and caregivers of preschoolers in the community had the opportunity to participate in monthly parenting

[30] These are the estimates for intervention households that receive transfers worth at least 90 Birr. Estimates including households receiving lower transfers are similar in terms of magnitude, but not always significant.

[31] villages in 5 areas were deemed eligible if they committed to providing extensive support to the program

meetings focusing on thematic topics, including health, nutrition, and literacy.

65. The authors provide estimates of both the intent-to-treat effect and the effect of treatment on the treated (IV estimates). Both sets of estimates include baseline individual and household level controls and exploit the panel nature of the baseline and follow-up data in a difference in differences framework. Preschool participation increased substantially in the intervention villages (42% of the 3-9 years old children, i.e. those who could have participated in the program's preschools) vis-à-vis the control villages (11.7%) and pre-schools appear to have affected subsequent primary school participation. The program also affected child work. IV estimates indicate that hours worked at the family plot in the week prior to the interview decreased by 1.3 hours among 5-9 year old children attending preschool (2.9 hours on average in the control group). However, hours spent on household chores and caring for children, elderly, and sick did not change significantly.

4.2 Integrated Education Interventions

66. Interventions are often not implemented in isolation, but as part of a larger integrated package in order to achieve sustainably change in the way that a single intervention cannot.[32] This sub-section describes the impact of an integrated program called BRIGHT administered in 132 rural villages in rural Burkina Faso. The interventions consisted of the construction of a primary school, of the provision of school kits, and of school meals to all pupils. In addition, female pupils received take-home rations on the condition that they attended school regularly.

67. To identify the impact of this program, De Hoop and Rosati (2012) exploit the village selection criteria in a regression discontinuity framework. The authors find that, while BRIGHT strongly improved school participation (by roughly 13 percentage points) it did not reduce child labour. The authors examine child participation in a range of household chores and economic activities. Most of these individual activities were not significantly affected. The most relevant changes were a reduction in the probability of participating in farming activities and an increase in the probability of working in the family business or selling goods in the street.[33] Accordingly, an aggregate indicator of participation in any economic activities and / or any household chores shows little change in response to the program.

68. Because the content of BRIGHT differed for boys and girls (girls receive take-home rations conditional on sufficient school attendance, while

[32] The conditional cash transfer interventions combined with supply side interventions are closely related to the integrated education intervention discussed here.

[33] This finding contrasts with Kazianga et al. (2012), who suggest that BRIGHT lowered participation in individual work activities.

boys do not), the authors also assess whether the effects of BRIGHT were different for the following three groups of children: girls, boys without female siblings, and boys with female siblings. The latter subgroup may be affected by increased school participation of female siblings and potentially benefits from spillover effects of the take-home rations supplied to female siblings. Boys who did not have female siblings experienced both an increase in school participation *and* an increase in participation in economic activities or chores. Girls and boys with female siblings also experienced an increase in school participation but no substantial change in the same work outcome variable.

69. Interestingly, BRIGHT had a strong positive impact on pupil learning. Kazianga et al. (2012) show that children's scores on mathematics and French tests increased markedly as a result of the program. De Hoop and Rosati (2012) argue that this finding holds both for children who are in school only and for children who combine work with school attendance. Together, the presented results suggest that in a low-income country like Burkina Faso promoting school enrollment does not necessarily reduce children's involvement in work. On the contrary, it might raise participation in work for some groups of children. However, there is no evidence that the increase in child work reduces learning in school.

5. LABOUR MARKET ORIENTED PROGRAMS

70. Income transfers are only one strategy for poverty reduction. Other strategies include labour market oriented programs, which can increase household members access to the labour market and thus generate sustainable changes. In this section, we discuss the evidence on the impact of one example of a labour market oriented program: business training (the outcomes are summarized in Table 7).The effect of business training programs on child labour is hard to predict. Business training programs increase the (adult) human capital available to the household enterprise. Higher human capital may increase household income, but at the same time it may result in an intra-household substitution effect that depends on the degree of complementarity between adult human capital and child work. Although programs aiming to improve the skills of labourers are being implemented on a wide scale, evidence of their effect on child labour is limited.

71. Karlan and Valdivivia (2010) examine the impact of an entrepreneurial training program that was randomly offered to female participants in a microcredit scheme in Peru.[34] The training consisted of weekly business skills and strategy training sessions offered over a period of two years. The training aimed to improve basic business practices. The

[34]Karlan and Valdivia (2010) argue that in addition to the income and substitution effect, training may also increase the value parents place on education thus increasing schooling and possibly lowering participation in child labour.

authors use a randomized control trial to identify the impact of this training on a range of outcomes. There is little evidence that the intervention improved key outcomes such as revenue, profits, or employment. Moreover, there is no evidence that the program had a statistically significant effect either on the extensive margin of child labour (not clearly defined in their paper) or on the number of daily hours spent in work.

72. Banerjee et al. (2011), study the effects of a program targeting the poorest of the poor in India and aiming to lift them out of poverty by improving their income generating capacity.[35] The program consists of a package of interventions. It begins with asset transfers (such as livestock, inventory, fodder and sheds) determined according to the livelihood option most suitable to the household. Subsequently, project staff meet beneficiaries on a weekly basis over a period of 18 months to provide information and training related to the household's enterprise (as well as broader social and health issues). Beneficiary households are required to save Rs. 10 (approximately US$ 0.25) per week. At the end of the 18-month period the households are integrated into a microcredit program by means of a mandatory 3-day orientation course.

73. For the purpose of the study, a total of 512 out of 991 ultra poor households in the poorest hamlets in Murshidabad (a district north of Kolkata) were randomly selected (stratified by hamlet) to receive an offer to participate in the program. The analysis relies on a baseline survey administered before the start of the program and a follow-up survey administered *before* the beneficiary households were incorporated into the microcredit scheme. Although non-participation rates were high (12.5% of selected households turned out to be ineligible to participate and 35.6% of selected households refused to participate), intent-to-treat estimates indicate that the intervention improved indicators of household welfare, such as per capita household consumption, nutritional intake, and perceived health. Children (age range not specified) of potential beneficiaries spent an additional 38 minutes studying in the 24 hours prior to the follow-up interview compared to the control group. However, they did not differ from children in the control group in terms of time spent working[36] in the 24 hours prior to the follow-up interview.

6. HUMAN SETTLEMENT PROGRAMS

74. Human settlement programs aim to improve living standards of inhabitants of urban slums. Examples of human settlement programs are upgrading schemes that provide electrification, sanitation and roads,

[35] To be considered "ultra poor", households must meet three of the following five criteria: the primary source of income is informal labor or begging; land holdings are below 20 decimals (10 katthas, 0.2 acres); the household owns no productive assets other than land; there are no able bodied males in the household; school-aged children work instead of attending school. In addition, households must meet two further requirements.

[36] No clear definition of work is given in the paper

resettlement schemes, and land titling schemes that provide property rights. These schemes can have important effects on child work, yet the evidence on the impact of these types of interventions is thin. The only rigorous evidence we have comes from Field (2011), who examines the impact of a large-scale land-titling program implemented by the government of Peru from 1996 to 2003 (see Table 8).

75. Field (2011) argues that, by reducing the need to protect property, well-enforced property rights can free up time available to adults for productive activities. Tenure insecurity can result in fear of eviction by the government and fear of property theft by other residents. This insecurity forces households to spend significant resources and time protecting their property. Formal property rights can mitigate these security concerns. As a result they can increase adult labour supply and thus generate a positive income effect. Moreover, when adults have a comparative advantage in security provision, children may substitute for adults in the labour market when tenure security is low. If property rights improve, the adult household labour supply may rise and child labour might fall.

76. Field (2011) examines the impact of the Peruvian titling scheme using survey data collected halfway trough the implementation of the program. Field identifies the impact of titling by comparing households in neighborhoods that were targeted for the intervention but had not yet been reached by the project to households in neighborhoods that had already been incorporated in the program. The author argues that program timing across neighborhoods was virtually random as "there [was] no clear pattern of movement [of project teams] according to neighborhood socioeconomic status or centrality." Accordingly, program and non-program neighborhoods were similar in terms of observed characteristics. To further reduce concerns about non-random program assignment, the author relies on a difference-in-differences estimation procedure, in which she compares differences in the labor supply of households that already had a registered title at the time the program started to households that did not have a registered title across early and late neighborhoods.

77. Field (2011) finds that the overall effect of titling on children's participation in economic activities for pay was not significant. However, within the group of households with less than 4 working age members, children in titled households worked 4 hours less per week than children in households without a registered title (significant at the 10% level, 8.9% of children in the overall sample work regularly).

7. ACCESS TO FINANCE

78. Microfinance programs offer financial services such as credit, saving, and insurance to disadvantaged individuals who do not have access

to regular financial institutions. Despite their popularity[37], evidence from rigorous evaluations on the impact of microfinance programs on outcomes such as school participation and child labour is surprisingly limited. We identified four impact evaluations focusing on the impact of microcredit (displayed in Table 9), but no studies that examine the impact of access to other microfinance products.

79. The effect of microcredit schemes, which increase access to capital for the household enterprise, is hard to predict. Increased enterprise capitalization may not only improve the household's income generating capacity, but may also result in intra-household substitution effects that depend on the degree of complementarity between physical capital, adult, and child work.

80. Accordingly, the evaluations find mixed effects of microcredit on child labour. Augsburg et al. (2012) collaborated with a microfinance institution in Bosnia to offer comparatively poor loan applicants (who had initially been turned down) a 50% chance of obtaining a loan. The loan amounts varied depending on the business plan with a mean of US$ 1012 and a median of US$ 920. The loans increased levels of business activity (ownership of a business increased by 6 percentage points) and self-employment (also up by 6 percentage points). Increased business activity, however, did not translate into increased profits or household income. Moreover, the loans led to a decline in school participation and an increase in labor supply of adolescent children (aged 16 to 19). Overall hours worked in this age group did not change significantly. However, for children living in a household with a business at baseline and for children from a household where the microfinance client has a low level of education hours worked respectively increased by 20 and 29 hours a week (not displayed in Table 9). Moreover, work intensity in the household business increased significantly in the full sample by 21 hours a week.

81. Three other studies also find a mixed impact of microcredit on child labour. Wydick (1999) finds that microcredit in Guatemala reduced the probability that children participate in economic activities *and* are not in school by 3 percentage points (on average 31% of children in the treatment and control group work and are not in school).[38] Hazarika and Sarangi (2008) find that microcredit in Malawi increased the probably that children work in economic activities by a statistically significant 0.7 percentage points (51.7% of children in the treatment and control group). Finally, Shimamura and Lastarria-Cornhiel (2010) find no significant effect of microcredit in Malawi on children's participation in crop farming, but a reduction in children's participation in household chores of 23 percentage points.

[37] By 2007 microfinance institutions had more than 150 million clients (Banerjee et al., 2010)

[38] Note that this result does not exclude the possibility that the 3% difference is due to children entering school rather than children stopping work.

82. However, it is important to note that these three evaluations suffers from endogeneity concerns. The Wydick (1999) estimates may be biased as they are based on instrumental variables estimates that use non-random program placement as an instrument. Hazarika and Sarangi (2008) use credit limits faced by households rather than credit uptake as the independent variable. The authors argue that credit limits are less likely to be endogenous than credit uptake, but there is no evidence to substantiate this claim and one can think of reasons why credit limits might be endogenous. Finally, Shimamura and Lastarria-Cornhiel (2010) base their analysis on data that ex post compare intervention communities to similar communities where the program was not implemented. Due to the lack of baseline data it is not possible to determine whether intervention and control communities were similar at baseline and the reliability of these estimates hinges on the identifying assumption that there are no unobserved factors that affect both program participation and child labour.

8. HEALTH AND DEMOGRAPHIC INTERVENTIONS

83. The health status of children and of the other household members has potentially important implications for children's labour supply. Health status can affect children's time allocation through three different channels. First, child health has been shown to be an important determinant of school attendance (e.g. Miguel and Kremer, 2004). Second, health status of adults within the household can have an impact on children's labour supply through an income effect, because of the reduced earning ability of the main breadwinners, and by raising the demand for children's time both to substitute for adult labour in economic and non economic activities in the household and to assist sick relatives. Finally, health expenditures can generate substantial income shocks in the household and this is likely to affect children's labour supply as discussed in previous sections.

84. This section discusses the limited rigorous empirical evidence (two studies) on the impact of health and family planning interventions on child labour (summarized in Table 10).

8.1 HIV/AIDS treatment

85. Thirumurthy, Graff Zivin, and Goldstein (2008) examine how children's labour supply changes when HIV positive household members gain access to antiretroviral treatment. The authors rely on longitudinal data (2 measurements with an interval of 6 months) for the members of 266 households with one or more HIV patients. Using individual fixed effect regressions, the authors estimate how the labour supply of household members changes as patients become eligible for ARV treatment. Treatment and the resulting changes in health are likely to be exogenous, as

treatment is administered by the health clinic conditional on biological markers that are not easily influenced by the patients.[39]

86. Labour force participation of HIV patients increased substantially in the 6 months after starting HIV treatment. Boys living in a household where one member gains access to ARV treatment did not experience significant changes in participation in economic activities for pay. However, boys who lived in a household where two or more members became eligible for HIV treatment were nearly 80 percentage points less likely to participate in these activities. No significant effects are registered for girls in either group. These results suggest that where HIV severely limits the adult supply within the household, children are pulled into work. By improving the health of adults within the household and by restoring labour supply it is possible to substantially reduce child labour.

8.2 Family planning

87. Sinha (2005) investigates the impact of a family planning program experiment in Bangladesh on fertility, school participation, and child labour. In this experiment 70 villages randomly selected from a larger group of 142 villages were provided an intensive family planning program. As part of this program female outreach workers visited households in these villages once every two weeks to provide non clinical contraceptives (pills, condoms, foam tablets) and administered depomedroxyprogesterone acetate (DMPA) injections, which provide pregnancy protection for a prolonged period of time. The outreach workers also provided information about the use of these contraceptives and potential side effects.

88. Sinha (2005) exploits the cluster-randomized trial to identify the impact of the program. She finds that the program substantially reduced fertility: her preferred estimate suggests that fertility dropped by 13% in intervention villages. The program also affected child labour. Reduced form estimates show that the program resulted in an 11 percentage point increase in boys' participation in economic activities or household chores. Instrumental variables estimates in which the number of children in the household is instrumented with a dummy for living in a program village suggest that each additional birth in the household reduces the probability that a boy works in these activities by 13 percentage points. Both reduced form and instrumental variables estimates detect no significant impact on labour force participation of girls.

9. DISCUSSION AND CONCLUSION

[39] Data from randomly selected households without HIV patients' households living in the same region are used to correct for seasonal fluctuations in labour supply.

89. Child labour is a complex phenomenon, resulting from household decisions influenced by a large number of factors including income, uncertainty, and relative returns to work and education among others. The complexity of the phenomenon implies that a large set of policy instruments can be used to address child labour or can affect child labour, even if designed to achieve other objectives. It also implies that predicting the impact of different interventions on child labour is far from straightforward. Within the household, complex patterns of substitution exist in the time allocation of its members as a response to changing circumstances. Policy interventions, therefore, might have effects that are not easy to predict. For example, if part of a cash transfer is invested in productive assets, the return to children's participation in productive household activities might increase. Cash transfers, public works schemes, microcredit, business training, and health and demographic interventions may all affect the household's income generating strategy in such a way that changes in child work are virtually impossible to predict theoretically. Even education interventions may have adverse effects and, in the limit, increase child labour. Evidence of the type discussed in this paper is therefore of critical importance to understand how policy and programs are likely to affect child labour.

90. There are a number of important caveats that need to be taken into account when interpreting the evidence presented. One concern is that rigorous evidence is available only for a limited subset of the policies potentially relevant to address child labour. Obviously the fact that there is no evidence for some intervention categories does not imply that these interventions do not affect child labour (possibly even more so than the interventions discussed in this review). Moreover, impact evaluations in the area of child labour tend to suffer from two additional limitations: (i) seldom is child labour the main outcome of interest and (ii) the interventions for which they are developed are not necessarily selected according to a consistent knowledge generating strategy. What we know about what works in addressing child labour on the basis of impact evaluations is defined by these limitations.

91. Beside these more general concerns, there are some more specific issues. A key issue, as can be inferred from the results presented, is that most impact evaluations focus on economic activity without considering household chores. This potentially results in underreporting of program impact on activities carried out by girls. Also, as a result of the focus on the broad category of economic activities (or one of its subcomponents), we have little evidence on the extent to which the interventions prevent and reduce the worst forms of child labour, including hazardous work.

92. The impact evaluations currently available focus almost exclusively on short-run outcomes. Evidence on the long-run impact of programs aimed at addressing child labour is very limited. Child labour potentially has negative effects on long-run outcomes in the labour market. Moreover, mental and physical harm experienced as a result of child labour may

manifest, persist and severely affect children at later ages. Hence, information on long-run effects would help generate a better understanding of child labour in general.

93. Similarly, there is also little evidence on the persistency of intervention effects after programs end. It seems unlikely that interventions targeted at individual beneficiaries result in persistent community-wide change (see Kremer and Miguel, 2007, for an example). But do programs that explicitly aim to permanently change the dynamics in villages or industries through extensive "integrated" packages of interventions and information campaigns effectively achieve sustained change?[40]

94. Finally the cost-effectiveness of the interventions discussed in this paper is seldom, if at all, addressed in the impact evaluations. More information on the expenditure per child kept out of labour would make the comparison of the different interventions more meaningful for policy makers. Detailed cost-effectiveness estimates are available for interventions that aim to increase school participation.[41] Unfortunately, virtually none of the impact evaluations we discussed provide detailed information on the cost of implementing the project under consideration and it is not possible to conduct a similar exercise for child labour outcomes.

[40] The IPEC programme of the International Labour Organization, for example, currently implements a series of large-scale programs to permanently eradicate child labour in the shrimp industry in Thailand and the cocoa sector in Ghana and Ivory Coast.

[41] See the J-PAL website: http://www.povertyactionlab.org/policy-lessons/education/student-participation (accessed April 12, 2012).

Appendix Table: Definitions of child labour		
Program & Country	**Reference**	**Definition of labour outcomes**
Anti-retroviral treatment, Kenya	Thirumurthy, Graff Zivin, and Goldstein (2008)	Engaged in income generating activities (wage labor, farm labor, and non-farm business labor) (during past week)
Atencion a Crisis, Nicaragua	Del Carpio and Loayza (2012)	Number of hours worked per week in agricultural and livestock activities, in nonagricultural activities (such as food production and elaboration, manufacturing, commerce, services, and professional jobs), and household chores (such as manning the house, cleaning, cooking, water gathering, wood cutting and gathering, and caring for siblings) (during past week)
Atencion a Crisis, Nicaragua	Del Carpio and Macours (2010)	Number of hours worked per week in economic activities including labor in agricultural and livestock activities, as well as labor in nonagricultural activities (during past week)
Bolsa Escola, Brazil	Ferro, Kassouf, and Levison (2010)	Spent at least one hour in paid or unpaid work (during reference week)
Bolsa Familia	Secretaria… (2012)	Activities in paid or unpaid work outside the home (no reference period given)
Bono de Desarrollo Humano, Ecuador	Edmonds and Schady (2011)	Engaged in work for pay or in unpaid labor in the family farm or business (during past week)
Bono de Desarrollo Humano, Ecuador	Schady and Araujo (2006)	Engaged in work for pay or in unpaid labor in the family farm or business (during past week)
BRIGHT, Burkina Faso	de Hoop and Rosati (2012)	Engaged in collecting firewood, cleaning, fetching water, taking care of younger siblings, tending animals, farming, shopping, other work for the family (in a business or selling goods in the street), and work for someone who is not a member of the household.
Business Training, Peru	Karlan and Valdivia (2010)	No definition given.
CESSP Scholarship Program, Cambodia	Ferreira, Filmer, and Schady (2009)	Work for pay on a farm, public or private sector, or in a business belonging to someone else (during past week)
Child Support Grant, South Africa	DSD, SASSA and UNICEF (2012)	Household chores for 10 year old children and work outside the home for 15 to 17 year old adolescents

Appendix Table: Definitions of child labour (continued)

Program & Country	Reference	Definition of labour outcomes
Familias en Accion, Colombia	Attanasio et al. (2010)	Engaged in domestic work, or in income generating work in the labor market or the family business (no information on reference period)
Family planning, Bangladesh	Sinha (2005)	A variable indicating engagement in work activities (paid or unpaid) was constructed on basis of two questions. A first question asked for the child's occupation. If the response to this question was "student", a second question asked whether the child was engaged in any productive activity while enrolled in school.
Female School Stipends, Pakistan	Alam, Baez, and Del Carpio (2011)	Looking for a job or engaged in work for pay or unpaid work (unpaid family helper and unpaid work outside the home). (no information on reference period)
Food for Education, Bangladesh	Wodon and Ravallion (2000)	Employed, employed but not working, household work, or seeking work (during past week)
Food for Education, Burkina Faso	Kazianga, de Walque, and Alderman (2009)	Farm labour and non-farm productive labour (during past week)
Income transfers including Bolsa Escola, Brazil	Cardoso and Souza (2004)	Regularly occupied in the labor market or in domestic activities linked to the market (no information on reference period)
Ingreso Ciudadano, Uruguay	Borraz and Gonzalez (2009)	Engaged in paid or unpaid activities outside the household or spending more than 3 hours working for the household in activities that can affect the normal development of the child like bricklaying, street sale, farm work, and housekeeping (no information on reference period)
Land Entitlements, Peru	Field (2011)	Employment hours (labor force participation excluding unpaid domestic work), constructed from number of days and mean hours per day (during past week)
Microcredit, Bosnia	Augsburg et al. (2012)	Total hours worked (not further defined) and hours worked in household business (during past week)
Microcredit, Guatemala	Wydick (1999)	Not in school and working in household enterprise (no information on reference period)

Appendix Table: Definitions of child labour (continued)

Program & Country	Reference	Definition of labour outcomes
Microcredit, Malawi	Hazarika and Sarangi (2008)	Fetching of firewood/dung/straw, helping in field/with animals, working at somebody else's for wage/meal, the fetching of drinking water, and other domestic housework (2 days prior to interview)
Microcredit, Malawi	Shimamura and Lastarria-Cornhiel (2010)	Crop farming and household chores (cooking, washing, cleaning, fetching water, collecting firewood, buying food, and taking care of other family members such as young children, elderly, and sick persons) (no information on reference period)
Old age pensions, Brazil	de Carvalho Filho (2012)	Child reported positive income from work (It is not entirely whether this is in a reference week (as mentioned in the main text) or monthly (as the appendix on variable construction suggests))
Old age pensions, South Africa	Edmonds (2005)	Engaged wage work, self-employment, or work in the family farm or business (no information on reference period)
Oportunidades, Mexico	Behrman et al. (2010)	Engaged in work for pay (no information on reference period)
Oportunidades, Mexico	Behrman et al. (2011)	The authors provide no definition of work, but state that it excludes domestic work.
Oportunidades, Mexico	Buddelmeyer and Skoufias (2004)	All workers who report that they work (paid or unpaid) and all individuals who report to be engaged in selling a product, helping in family business, making products to sell, caring for animals, and washing, cooking, or ironing, for pay (during past week).
Oportunidades, Mexico	De Janvry et al. (2006)	Engagement in productive activities including wage work, unpaid work outside of home, and work in the family business or farm (during past week)
Oportunidades, Mexico	Schultz (2004)	All workers who report that they work (paid or unpaid) and all individuals who report to be engaged in selling a product, helping in family business, making products to sell, caring for animals, and washing, cooking, or ironing, for pay (during past week).
Oportunidades, Mexico	Skoufias and Parker (2001)	All workers who report that they work (paid or unpaid) and all individuals who report to be engaged in selling a product, helping in family business, making products to sell, caring for animals, and washing, cooking, or ironing, for pay (during past week).
PANES, Uruguay	Amarante, Ferrando, and Vigorito (2011)	No definition given.
PANES, Uruguay	Borraz and Gonzalez (2009)	Carrying out paid or no paid activities outside the household and /or spending more than 3 hours working for the household. The authors include "activities that can affect the normal development of the child like bricklaying, street sale, farm work, housekeeping etc."

Appendix Table: Definitions of child labour (continued)

Program & Country	Reference	Definition of labour outcomes
Path, Jamaica	Levy and Ohls (2007)	Engaged in any kind of work or other activities that contribute towards the maintenance of the household or towards himself / herself (no information on reference period)
PETI, Brazil	Pianto and Soares (2004)	No definition given
PETI, Brazil	Yap, Sedlacek, and Orazem (2002)	No definition given
PRAF, Honduras	Galiani and McEwan (2011)	Labour force participation, including paid or unpaid work in a business or farm outside the home (during past week)
PRAF, Honduras	Glewwe and Olinto (2004)	Work, including work for the household farm or business (during past week)
Pre-school, Mozambique	Martinez, Naudeau, and Pereira (2012)	Hours worked at the family's plot, hours in household chores, and hours spent caring for children, elders, and sick (during past week)
Public Works, Ethiopia	Hoddinott, Giligan, and Taffesse (2009)	Hours worked in agriculture or domestic chores (during past week)
Red de Proteccion Social, Nicaragua	Dammert (2009)	Engaged in market work, which includes wage employment, selfemployment, agriculture, unpaid work in a family business, and helping on the family farm. (no information on reference period)
Red de Proteccion Social, Nicaragua	Maluccio and Flores (2005)	Work is a primary or secondary activity and number of hours worked is positive (no information on reference period). Nearly all child workers were agricultural laborers or unskilled helpers and typically worked without pay.
School vouchers, Colombia	Angrist et al. (2002)	No definition given.
Social Cash Transfer Scheme, Malawi	Covarrubias, Davis, and Winters (2012)	Several indicators of participation in work, including participation in household chores, participation in the family farm or non-farm business, and participation in domestic work outside the household for pay
School vouchers, Indonesia	Sparrow (2004)	Activities that contribute to household income, for at least one hour in the last week. This may include wage labour, but also non wage labour such as own farm activities.
Subsidios Condicionados a la Asistencia Escolar, Colombia	Barrera-Osorio et al. (2008)	Number of hours worked (during past week)
Targeting the hard-core poor, India	Banerjee et al. (2011)	Minutes worked (during 24 hours prior to interview), no further definition given.

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TABLES AND FIGURES

TABLE 1. UNCONDITIONAL CASH TRANSFERS

Program & Country	Reference	Method	Outcome	Stratum	Impact	Comparison group
Bono de Desarrollo Humano, Ecuador	Schady and Araujo (2006)	RCT	Economic activities for pay or for household	All 6-17, OLS	-0.062 (0.020)***	0.54 (control follow-up)
				All 6-17, IV	-0.172 (0.058)***	0.54 (control follow-up)
Bono de Desarrollo Humano, Ecuador	Edmonds and Schady (2011)	RCT	Economic activities for pay or for household	All 11-16, OLS	-0.080 (0.024)**	0.53 (baseline control)
				All 11-16, IV	-0.245 (0.079)**	0.53 (baseline control)
			Economic activities for pay	All 11-16, OLS	-0.032 (0.017)*	0.014 (baseline control)
				All 11-16, IV	-0.099 (0.054)*	0.014 (baseline control)
Social Cash Transfer Scheme, Malawi	Covarrubias, Davis, and Winters (2012)	RCT	Household chores	All, 0-18, dif-in-dif	0.137 (0.000)***	0.575 (baseline control)
				All, 0-18, PSM dif-in-dif	0.077 (0.023)***	0.575 (baseline control)
			Work in family farm or family business	All, 0-18, dif-in-dif	0.067 (0.024)***	0.218 (baseline control)
				All, 0-18, PSM dif-in-dif	0.021 (0.578)	0.218 (baseline control)
			Paid domestic work outside household	All, 0-18, dif-in-dif	–0.072 (0.000)***	0.057 (baseline control)
				All, 0-18, PSM dif-in-dif	–0.077 (0.000)***	0.057 (baseline control)
Old age pensions, South Africa	Edmonds (2006)	RDD	Economic activities for pay or for household	All 13-17 (male pensioner)	-0.108 (0.097)	0.51 (household with nearly eligible elderly)
				All 13-17 (female pensioner)	-0.108 (0.078)	0.51 (see above)
			Hours worked per day	All 13-17 (male pensioner)	-0.630 (0.313)**	0.99 (see above)
				All 13-17 (female pensioner)	-0.494 (0.247)**	0.99 (see above)
Old age pensions, Brazil	de Carvalho Filho (2012)	Triple diff	Economic activities for pay	Boys 10-14 (male pensioner)	0.054 (0.059)	0.083 (all, follow-up)
				Boys 10-14 (female pensioner)	-0.056 (0.062)	0.083 (all, follow-up)
				Girls 10-14 (male pensioner)	0.047 (0.046)	0.045 (all, follow-up)
				Girls 10-14 (female pensioner)	-0.130 (0.055)	0.045 (all, follow-up)

Note: * p<0.1, ** p<0.05, *** p<0.01. Unless indicated otherwise, figures in parentheses are standard errors. And, unless indicated otherwise, the comparison group is taken from the same stratum as the impact estimate.

TABLE 2. CASH TRANSFERS AND THE EXTENSIVE MARGIN OF CHILD LABOUR

Program & Country	Reference	Method	Outcome	Stratum	Impact	Comparison group
Significant reduction in child labour:						
CESSP Scholarship Program, Cambodia	Ferreira, Filmer, and Schady (2009)	RDD	Economic activities for pay	All 7-18	-0.104 (0.028) *** ǂ	0.338 (control follow-up)
Bolsa Escola, rural Brazil	Ferro et al. (2010)	PSM	Economic activities for pay or for household	All 6-15	-0.087 (T=-4.42)***	0.348 (matched control follow-up)
Oportunidades, urban Mexico	Behrman et al. (2010)	PSM	Economic activities for pay	All 12-14	-0.067 (0.032)** ǂ	0.06 (baseline control)
Red de Proteccion Social, Nicaragua	Maluccio and Flores (2005)	RCT	Economic activities for pay or for household	All 7-13	-0,056 (0.027)**	0.120 (follow-up control)
Jaringan Pengaman Social, rural Indonesia	Sparrow (2004)	IV	Economic activities for pay or for household	All 10-18	-0.051 (0.013)***	0.159 (follow-up all)
Female School Stipends, Pakistan	Alam, Baez, and Del Carpio (2011)	RDD	Economic activities for pay or for household	Girls 12-19	-0.049 (0.020)**	N.A.
Bolsa Escola, urban Brazil	Ferro et al. (2010)	PSM	Economic activities for pay or for household	All 6-15	-0.025 (T=-3.84)***	0.090 (matched control follow-up)
Bolsa Familia	Secretaria… (2012)	PSM	Economic activities for pay or for household	All 5-17	-0.019**	4.51 (all follow-up)
Jaringan Pengaman Social, urban Indonesia	Sparrow (2004)	IV	Economic activities for pay or for household	All 10-18	-0.014 (0.006)**	0.159 (follow-up all)
No significant change in child labor:						
PRAF-II, Honduras	Galiani and McEwan (2011)	RCT	Economic activities for pay or for household	All 6-12	-0.024 (0.017)	0.099 (control follow-up)
	Glewwe and Olinto (2004)	RCT	Economic activities for pay or for household	All 6-12	-0.049 (0.077)	0.116 (control follow-up)
Familias en Accion, urban Colombia	Attanasio et al. (2010)	Probit	Economic activities for pay	All 10-17	-0.023 (0.015) ǂ	0.071 (control follow-up)
Oportunidades, rural Mexico	Skoufias and Parker (2001)	RCT	Economic activities for pay or for household	All 8-17	-0.020 (0.019) ǂ	0.152 (baseline all)
Familias en Accion, rural Colombia	Attanasio et al. (2010)	Probit	Economic activities for pay	All 10-17	-0.004 (0.020) ǂ	0.114 (control follow-up)
Path, Jamaica	Levy and Ohls (2007)	RDD	Economic activities for pay or for household or chores	All 6-17	-0.001 (0.007)	0.018 (control follow-up)
PANES, Uruguay	Amarante, Ferrando, and Vigorito (2011)	RDD	Working (no definition given)	All 6-17	0.014 (0.029)	0.08 (control follow-up)

Note: * p<0.1, ** p<0.05, *** p<0.01. ǂ indicates that the average and standard error were imputed (see main text for details). Unless indicated otherwise, figures in parentheses are standard errors. And, unless indicated otherwise, the comparison group is taken from the same stratum as the impact estimate. Amarante et al. (2011) provide no comparison group for enrollment in the "all 6-17" stratum, but they do provide comparison groups for 6-13 (0.98) and 14-17 (0.78).

TABLE 3. VARIATIONS ON THE BASIC CONDITIONAL CASH TRANSFER SCHEME

Program & Country	Reference	Method	Outcome	Stratum	Impact	Comparison group
Panel A: Conditional Cash Transfers with a Twist						
Atencion a Crisis, Nicaragua	Del Carpio and Loayza (2012)	RCT	Hours worked last week in economic activities for pay or for household, or chores	All 7-14 (basic CCT)	-1.757 (0.347)***	12.37 (control follow-up)
				All 7-14 (CCT+investment)	-0.941 (0.399)**	12.37 (control follow-up)
Subsidios Condicionados a la Asistencia Escolar, Colombia	Barrera-Osorio et al. (2008)	RCT	Hours worked last week, no description of activities included in definition of work available	Grades 6-10, San Cristobal (basic CCT)	-0.375 (0.152)**	0.920 (control follow-up)
				Grades 6-10, San Cristobal (CCT + saving scheme)	-0.263 (0.140)*	0.920 (control follow-up)
				Grade 11, San Cristobal (basic CCT)	1.008 (1.887)	6.932 (control follow-up)
				Grade 11, San Cristobal (CCT + saving scheme)	0.180 (1.676)	6.932 (control follow-up)
				Grade 11, Suba (CCT + graduation grant)	-7.045 (1.325)***	8.548 (control follow-up)
Panel B: Conditional Cash Transfers Combined with Supply Side Interventions						
PRAF, Honduras	Galiani and McEwan (2011)	RCT	Economic activities for pay or for household	All 6-12 (basic CCT)	-0.024 (0.017)	0.099 (control follow-up)
				All 6-12 (CCT+supply side interventions	-0.043 (0.013)**	0.099 (control follow-up)
	Glewwe and Olinto (2004)	RCT	Economic activities for pay or for household	All 6-12 (basic CCT)	-0.049 (0.077)	0.116 (control follow-up)
				All 6-12 (CCT+supply side interventions	0.038 (0.124)	0.116 (control follow-up)
PETI, Brazil	Yap, Sedlacek, and Orazem (2002)	See text	No definition of work given	All 7-14, Bahia	-0.247**	N.A.
				All 7-14, Sergipe	-0.103**	N.A.
				All 7-14, Pernambuco	-0.049**	N.A.

Note: * p<0.1, ** p<0.05, *** p<0.01. Unless indicated otherwise, figures in parentheses are standard errors. And, unless indicated otherwise, the comparison group is taken from the same stratum as the impact estimate.

TABLE 4. CONDITIONAL IN-KIND TRANSFERS

Program & Country	Reference	Method	Outcome	Stratum	Impact	Comparison group
Panel A: School Vouchers						
Colombia	Angrist et al. (2002)	RCT	Participation, no definition of work given	Boys, 15 on average	-0.028 (0.028)	0.225 (control follow-up)
				Girls, 15 on average	-0.032 (0.020)	0.101 (control follow-up)
			Hours worked last week, no definition of work given	Boys, 15 on average	-0.623 (0.886)	6.198 (control follow-up)
				Girls, 15 on average	-1.499 (0.524)***	2.704 (control follow-up)
Panel B: Food for Education						
Bangladesh	Wodon and Ravallion (2000)	IV	Economic activities for pay or for household, or chores	Boys, 5-16 (average effects at 100 kg of rice)	-0.040***	0.122 (control follow-up)
				Girls, 5-16 (average effects at 100 kg of rice)	-0.020**	0.125 (control follow-up)
Burkina Faso	Kazianga, de Walque, and Alderman (2009)	RCT	Farm work	Boys, 6-15 (school meals)	0.01 (0.032)	0.574 (Baseline control, all)
				Girls, 6-15 (school meals)	0.033 (0.037)	0.574 (Baseline control, all)
				Girls, 6-15 (take-home rations)	-0.089 (0.037)**	0.574 (Baseline control, all)
			Non-farm work	Boys, 6-15 (school meals)	-0.022 (0.033)	0.163 (Baseline control, all)
				Girls, 6-15 (school meals)	0.007 (0.033)	0.163 (Baseline control, all)
				Girls, 6-15 (take-home rations)	-0.090 (0.030)***	0.163 (Baseline control, all)

Note: * p<0.1, ** p<0.05, *** p<0.01. Unless indicated otherwise, figures in parentheses are standard errors.And, unless indicated otherwise, the comparison group is taken from the same stratum as the impact estimate. No standard errors are available for Ravallion and Wodon (2000) marginal effects of probit estimates.

TABLE 5. PUBLIC WORKS SCHEMES

Program & Country	Reference	Method	Outcome	Stratum	Impact	Comparison group
Public Works, Ethiopia	Hoddinott, Giligan, and Taffesse (2009)	Non-PS matching	Hours worked in agriculture or chores during past week	boys, 6-10	-4.70 (Z=1.73)*	27.44 (control follow-up)
				boys, 11-16	-2.26 (Z=0.85)	31.80 (control follow-up)
				girls, 6-10	1.28 (Z=0.52)	25.90 (control follow-up)
				girls, 11-16	-1.94 (Z=0.44)	32.50 (control follow-up)
Public Works + Food security interventions, Ethiopia	Hoddinott, Giligan, and Taffesse (2009)	Non-PS matching	Hours worked in agriculture or chores during past week	boys, 6-10	-0.11 (Z=0.04)	26.08 (control follow-up)
				boys, 11-16	4.25 (Z=1.50)	32.65 (control follow-up)
				girls, 6-10	4.48 (Z=1.95)*	22.90 (control follow-up)
				girls, 11-16	2.53 (Z=0.65)	31.35 (control follow-up)

Note: * p<0.1, ** p<0.05, *** p<0.01. Unless indicated otherwise, figures in parentheses are standard errors.And, unless indicated otherwise, the comparison group is taken from the same stratum as the impact estimate.

TABLE 6. EDUCATION INTERVENTIONS

Program & Country	Reference	Method	Outcome	Stratum	Impact	Comparison group
Preschools, Mozambique	Martinez, Naudeau, and Pereira (2012)	RCT	Hours worked on family plot last week	All 5-9	-1.316 (0.637)**	2.540 (follow-up control)
			Hours worked in household chores last week	All 5-9	-0.529 (0.407)	0.748 (follow-up control)
			Hours spent caring for other household members	All 5-9	0.056 (0.320)	0.569 (follow-up control)
BRIGHT, Burkina Faso	de Hoop and Rosati (2012)	RDD	Economic activities for pay or for household, or chores	All, 5-12	0.033 (0.035)	0.748 (all, follow-up)
				Girls, 5-12	0.017 (0.037)	0.778 (all, follow-up)
				Boys without female siblings, 5-12	0.097 (0.047)**	0.705 (all, follow-up)
				Boys with female siblings, 5-12	0.032 (0.039)	0.729 (all, follow-up)

Note: * p<0.1, ** p<0.05, *** p<0.01. Unless indicated otherwise, figures in parentheses are standard errors. And, unless indicated otherwise, the comparison group is taken from the same stratum as the impact estimate.

TABLE 7. LABOUR MARKET ORIENTED PROGRAMS

Program & Country	Reference	Method	Outcome	Stratum	Impact	Comparison group
Business training, Peru	Karlan and Valdivia (2010)	RCT	Working, no definition of work given	All, 6-15	-0.026 (0.039)	0.325 (control follow-up)
			Daily hours in work, no definition of work given	All, 6-15	-0.071 (0.085)	0.614 (control follow-up)
Targeting the Hard-core Poor, India	Banerjee et al., 2011	RCT	Minutes worked in past 24 hours, no definition of work given	All, no age range given	2.59 (7.12)	20.82 (control follow-up)

Note: * p<0.1, ** p<0.05, *** p<0.01. Unless indicated otherwise, figures in parentheses are standard errors.And, unless indicated otherwise, the comparison group is taken from the same stratum as the impact estimate. No standard errors are available for the Hazarika and Sarangi (2008) and Wydick (1999) marginal effects of probit estimates.

TABLE 8. HUMAN SETTLEMENTS PROGRAMS

Program & Country	Reference	Method	Outcome	Stratum	Impact	Comparison group
Peru	Field (2011)	Quasi-Random	Hours worked in economic activities for pay during past week	All, 5-16	0.66 (2.21)	8.9% (overall sample)
				All, 5-16 (Less than four 5-69 year old household members)	-4.08 (1.87)*	8.9% (overall sample)

Note: * p<0.1, ** p<0.05, *** p<0.01. Unless indicated otherwise, figures in parentheses are standard errors.And, unless indicated otherwise, the comparison group is taken from the same stratum as the impact estimate. No standard errors are available for the Hazarika and Sarangi (2008) and Wydick (1999) marginal effects of probit estimates.

TABLE 9. MICROCREDIT PROGRAMS

Program & Country	Reference	Method	Outcome	Stratum	Impact	Comparison group
Bosnia	Augsburg et al. (2012)	RCT	Hours worked (not further defined)	All, 16-19	13.60 (10.62)	4.93 (all, baseline)
			Hours worked in HH business	All, 16-19	20.55 (9.996)**	3.79 (all, baseline)
Malawi	Hazarika and Sarangi (2008)	See text	Economic activities for pay or for household, or chores	All, 7-11	0.007***	0.517 (all, follow-up)
Malawi	Shimamura and Lastarria-Cornhiel (2010)	See text	Crop farming	All, 6-14	0.017 (0.072)	0.178 (control follow-up)
			Household chores	All, 6-14	-0.233 (0.084)***	0.226 (control follow-up)
Guatemala	Wydick (1999)	IV	Not in school and in economic activities for pay or for household	All, 10-18	-0.30***	0.31 (all, follow-up)

Note: * p<0.1, ** p<0.05, *** p<0.01. Unless indicated otherwise, figures in parentheses are standard errors.And, unless indicated otherwise, the comparison group is taken from the same stratum as the impact estimate. No standard errors are available for the Hazarika and Sarangi (2008) and Wydick (1999) marginal effects of probit

TABLE 10. HEALTH AND DEMOGRAPHIC PROGRAMS

Program & Country	Reference	Method	Outcome	Stratum	Impact	Comparison group
Family planning, Bangladesh	Sinha (2005)	RCT	Economic activities for pay or for household, or chores	Boys, 10-16	0.108 (Z=2.69)***	0.35 (control group)
				Girls, 10-16	0.009 (Z=0.24)	0.32 (control group)
Anti-retroviral treatment, Kenya	Thirumurthy, Graff Zivin, and Goldstein (2008)	Changes over time	Economic activities for pay	Boys, 8-12 (HH with 1 ARV recipient)	-0.143 (1.54)	0.74 (boys 8-18, baseline ARV households)
				Girls, 8-12 (HH with 1 ARV recipient)	0.027 (0.21)	0.63 (girls 8-18, baseline ARV households)
				Boys, 8-12 (HH with >1 ARV recipient)	-0.792 (3.44)***	0.74 (boys 8-18, baseline ARV households)
				Girls, 8-12 (HH with >1 ARV recipient)	0.338 (0.72)	0.63 (girls 8-18, baseline ARV households)

Note: * p<0.1, ** p<0.05, *** p<0.01. Unless indicated otherwise, figures in parentheses are standard errors.And, unless indicated otherwise, the comparison group is taken from the same stratum as the impact estimate.