NBER WORKING PAPER SERIES

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Working Paper 30908
http://www.nber.org/papers/w30908

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2023

We are grateful to discussion from Bob Rijkers and participants in the 2022 World Bank Deep Trade Agreements Conference and to Erhan Artuç for crucial advice. This paper has benefited from support from the World Bank's Umbrella Facility for Trade trust fund financed by the governments of the Netherlands, Norway, Sweden, Switzerland and the United Kingdom. All errors are our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 30908
February 2023
JEL No. F66,J13,O15

ABSTRACT

We study the impact of child labor standards in Regional Trade Agreements (RTAs) on a variety of child labor market outcomes, including employment, education, and household inequality. We develop a stylized general equilibrium model of child labor in an economy open to international trade and consider the impact of RTAs with and without child labor bans. We empirically investigate the effects of these clauses in trade agreements in a broad international panel of 101 developing countries using harmonized survey microdata. Exploiting quasi-experimental methods to obtain plausibly causal estimates, we find that RTAs without child-labor bans lead to reductions in child employment and increases in school enrollment, particularly for older children aged 14--17. Child labor bans in RTAs perversely increase child employment among 14--17 year olds and decrease school enrollment for both young and older children. These effects appear to decrease inter-household income inequality through increased child earnings. Our findings are consistent with theoretical predictions from our model and the literature on child labor bans.

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1 Introduction

Over the last 30 years, Regional Trade Agreements (RTAs) have become the de facto policy tool in trade liberalization. The bilateral nature of RTA negotiations has introduced opportunities to broaden the scope of trade policy well-beyond liberalization in tariff rates and non-tariff trade barriers, with contemporary RTAs frequently including binding commitments in areas such as investment, intellectual property, labor and environmental policy, among others (Mattoo *et al.*, 2020). Such policy interdependencies are referred to as "issue linkages" in the literature (e.g. Maggi, 2016; Limão, 2005, 2007, 2016). In particular, concerns about undesirable labor practices such as the exploitation of child labor have increasingly dominated trade debates and motivated the inclusion of provisions banning child labor in trade agreements. Figure 1 displays the evolution of child labor content in RTAs over time.[1] Though sparse prior to 2000, child labor provisions became increasingly common through the 2000s and 2010s. Despite their increasing use, there is no systematic evidence on their impacts on labor outcomes, both intended and unintended.

In this paper we investigate the effect of child labor provisions in regional trade agreements on a variety of child labor market outcomes in developing countries. To structure our analysis, we develop a general-equilibrium model of an open economy in which both children and adults can supply labor as part of a household-level joint labor-supply decision. We show that a trade agreement is likely to decrease the incidence of child labor in developing countries through income effects. We find that trade agreements that include provisions banning household supply of child labor will lead to reductions in employment of the youngest children (i.e. those under 14 years of age that are directly affected by the ban), but increases in employment of older children (i.e. those above 14 years of age that are not included in the ban) via intra-household substitution effects. If instead a trade agreement includes prohibitions on child labor that are implemented as a fine-based restriction on hiring children below 14 years of age, employment of the youngest children is ambiguous—if young children contribute a non-negligible share of household earnings, their labor supply is likely to increase when exposed to a fine-based restriction. We find that employment of

[1] See Table A.1 for the list of RTAs that have included such provisions.

older children is again likely to increase in developing countries via intra-household substitution effects, even with demand-side implementations.

To empirically study this question we bring to bear harmonized income survey microdata from 170 countries covering response years between 1960 and 2020. The data include over 1,700 unique nationally representative survey years covering over 180 million respondents. We combine these surveys with detailed data on trade agreements reported to the WTO that include non-trade characteristics such as whether an agreement includes provisions banning the use of child labor and how said provisions are enforced. Because child labor is less of a concern in developed countries, we focus our analysis on a sub-sample of 101 developing countries. We estimate a variety of difference-in-differences (DiD) and triple DiD models on this developing country sample to assess within-country changes in child labor market outcomes before and after a given trade agreement enters into force, and whether these effects differ for trade agreements with the child labor provisions compared to those without.

We find that, consistent with our model predictions, child labor—especially employment of 14 to 17 year olds—declines following entry into force of RTAs that do not include child labor protections with corresponding increases in school enrollment rates. However, in trade agreements that include provisions banning child labor, those provisions perversely *increase* child employment for both young (under 14 years of age) and, especially, 14 to 17 year olds. Consistently with this finding, we also observe a decrease in school enrollment rates for both young and older children. Finally, we find evidence that these provisions decrease inter-household income inequality for households with children, presumably through increases in child labor income.

This is the first paper to establish systematic evidence on the impacts of child labor protections in RTAs on child labor and related outcomes. In doing so, we also make significant contributions to the literature on the impacts of free trade on child labor more broadly. Our empirical setting offers credibly exogenous variation in trade openness applied to a broad panel of developing countries and improves upon existing identification strategies in the literature that leverage cross-country differences in trade and child labor. Our study also provides new causal evidence on the impacts of child labor bans on child labor, a literature which has largely focused on domestic labor bans examined through single-country analysis. Ours is the first study, to our knowledge, that investigates the impact of child labor bans across a broad sample of developing countries. This paper also contributes to the nascent literature on the non-trade effects of provisions in trade agreements that have non-trade objectives, such as labor and environmental protection.

Our findings contribute to a rich literature on child labor in economics. On the theoretical side of the literature, Basu and Van (1998) propose a simple equilibrium model in which households choose to send their children into market work only when necessary to meet a basic standard of consumption. Child workers and adult workers are perfect substitutes, so their wages move together; when the adult wage is above a subsistence threshold, no children are sent to work. This can create two equilibria: One in which wages are low, and there is child labor; and another in which wages are high, and there is no child labor. Basu (2005) shows how child-labor bans in such a model can also paradoxically increase child labor if they are implemented as fines on employers. Baland and Robinson (2000) and Ranjan (2001) both study models in which child labor is inefficient, either because children cannot credibly commit to repaying parents in the future for investments in schooling or because of credit constraints more generally. Basu (1999) surveys the economics literature on the subject, including a discussion (Section 8) of the scope for international coordination to deal with the problem.

The empirical relationship between trade and child labor is examined by Edmonds and Pavcnik (2005), who show that when Vietnam lifted export restrictions on rice, rural locations where the rice price rose the most had the most rapid reductions in child labor. This appears to suggest that the rising income effect of the trade liberalization facilitated a drop in the need for child labor. This conclusion is supported by cross-country evidence in Edmonds and Pavcnik (2006). As a matter of theory, Ranjan (2001) shows that trade sanctions against a country with child labor can be counterproductive, by increasing income inequality and thus the credit constraint problem. We contribute to this literature by looking specifically at the role of trade *agreements*, and labor clauses within those agreements, in addressing child labor.

We also draw on studies of explicit child-labor bans. Bharadwaj *et al.* (2020) study a child-labor ban in India in 1986, which imposed a fine on employers found in violation, and find that it paradoxically *increased* child labor, as predicted in Basu (2005). In a similar vein, Lakdawala *et al.* (2022) study a policy change that legalized certain forms of child labor in Bolivia, with the surprising result that use of child labor declined. These are consistent with our own results, as we will point out. Finally, Doran (2013) studies the effects of the PROGRESA program in Mexico, which paid parents to keep their children in school, and finds that the program resulted in less use of child labor and more employment of adult labor. The contrast between the Indian policy, which worked on the labor-demand side, and the Mexican policy, which worked on the labor-supply side, will be important in our interpretations.

Child labor is also closely linked to education, since the most important opportunity cost of children's time is schooling. Several authors have explored the effect of trade liberalization on educational attainment. Leight and Pan (2021) show that areas in China most affected by the boom in low-skill-intensive manufactured exports saw relative reductions in educational attainment. Greenland and Lopresti (2016) show that locations in the US most affected by low-skilled-manufacturing import competition from China saw relative *increases* in high-school graduation. Atkin (2016) shows that access to low-skill export manufacturing jobs provided by multinational firms in Mexico is correlated with reduced high-school attainment. Edmonds *et al.* (2010) show that in Indian trade liberalization, import competition shocks hitting industries important to local employment were correlated with reduced school enrollment. Blanchard and Olney (2017) show in a panel of countries that increased low-skill-labor-intensive exports are correlated at the national level with reduced high-school enrollment, with the opposite effect for high-skill-intensive exports. These results together suggest that a trade agreement on its own, even without a labor clause, could have an effect on child labor by raising or lowering the returns to schooling. This is a question that we explore empirically.

Our work also contributes to the large literature studying the effects of trade agreements. This literature has consistently found that trade agreements significantly increase trade flows between member countries.[2] The more recent strand of this work has emphasized the importance of the varying content of RTAs in assessing their trade effects. Baier *et al.* (2014) find evidence of dif-

[2]Seminal works include Baier and Bergstrand (2007); Egger *et al.* (2011); Bergstrand *et al.* (2015). See Limão (2016) for a survey.

ferential trade effects from different types of agreements (e.g. partial scope agreements, free trade agreements or custom unions), while Mattoo *et al.* (2017) show that "deep" agreements (i.e. RTAs that cover a larger number of policy areas beyond tariff reduction) are associated with a stronger trade impact.[3] The literature on trade agreements that focuses on non-trade outcomes such as labor and the environment is mostly theoretical and focuses on the conditions under which issue linkage in trade agreements is beneficial (e.g. Ederington, 2002; Limão, 2007; Maggi, 2016). Notable exceptions include Abman *et al.* (2021) who find that environmental provisions in RTAs effectively mitigate deforestation that otherwise arises from trade liberalization, Baghdadi *et al.* (2013) that study the impact of RTAs with environmental clauses on emissions, Brandi *et al.* (2020) that investigate whether environmental provisions in RTAs make exports from developing countries greener, and Lundberg *et al.* (2022) that find linking RTAs to international environmental agreements can improve participation and strengthen enforcement.

The remainder of the paper is structured as follows: in Section 2 we develop our theoretical model. In Section 3 we elaborate on our data an empirical strategy. We discuss our empirical findings in Section 4 and offer some concluding remarks in Section 5.

2 Theory

We present here a simple, stylized general-equilibrium model of an open economy in which both children and adults supply labor as part of a household-level joint labor-supply decision.[4] Suppose that the economy produces two goods, X, which is an export, and Y, which is import-competing. Good Y is the numeraire. Denote the domestic price of X as p. Each good j is produced with low-skilled labor L^j, high-skilled labor H^j, and a specific factor K^j, by a constant-returns-to-scale production function $f^j(L^j, H^j, K^j) = \left(\beta^{jL}(L^j)^{\frac{\sigma^j-1}{\sigma^j}} + \beta^{jH}(H^j)^{\frac{\sigma^j-1}{\sigma^j}} + \beta^{jK}(K^j)^{\frac{\sigma^j-1}{\sigma^j}}\right)^{\frac{\sigma^j}{\sigma^j-1}}$,

[3] Carrère *et al.* (2022) study how labor provisions in RTAs affect bilateral trade flows and find that on average these clauses do not have a statistically significant impact. Robertson (2021) finds that specific labor clauses in trade agreements, such as child labor standards, are associated with an increase in trade, most likely as they boost demand from reputation-conscious buyers (Maskus, 1997).

[4] The model presented here is static, close to the spirit of Basu and Van (1998) and the theory model of Edmonds and Pavcnik (2005). The important intertemporal considerations raised in approaches such as Baland and Robinson (2000), Ranjan (2001), and Section 6.4 of Basu (1999) are beyond the scope of this paper.

where $\sigma^j > 0$ is the elasticity of substitution between the three factors. The specific factors K^j are inelastically supplied. Labor is supplied by households, indexed by $i \in [0, N^L]$ for low-skilled workers and $i \in [0, N^H]$ for high-skilled.

Each household is either exogenously low skilled, in which case all of its members are low-skilled workers, or high skilled, in which case all of its members are high-skilled workers. Household i has $\bar{k}(i)$ members, each of whom can work for wages or do something else such as household work or education. A worker of age $a \in [1, 2, \ldots \infty)$ in household i produces $A(i)e(a) \in [0, \infty)$ units of effective labor if employed in the labor market, where $A(i)$ is a productivity factor that measures household-wide ability and $e(a)$ captures the portion of ability that is an increasing function of age. Household member k in household i has age $a(k, i)$. Without loss of generality, number household members so that $a(k, i)$ is increasing in k.

For a worker of age a, there is a cost $v(L, a)$ of working L hours in the labor market. This can be the opportunity cost of the time that would otherwise be spent in school, with family, at play, in household production, and so on, as well as drudgery and direct disutility of work. It is a convex, differentiable function with $v_1 > 0, v_{11} > 0$ over its whole domain, and $v_1(L, a) > v_1(L, a')$ if $a < a'$ so work is costly at the margin—with an increasing marginal cost and a marginal cost that is greater for younger workers.

Each household has a head who acts as a benevolent dictator, choosing labor supply and allocation of consumption to maximize household welfare. The household head chooses the market labor supply $L_k \in [0, \bar{L}_k]$ for each member k. The wage per unit of effective labor for a worker of type t is denoted w^t, where $t = L, H$ indicates low-skilled or high-skilled, and so the total household income is equal to $I(i) = w^t \Sigma_{k=1,\ldots,\bar{k}(i)} A(i)e(a(k, i))L_k$.

The benefit of market work to the household depends on per-capita labor income. Specifically, the benefit to household i of labor income I is given by $u(c(i))$, where $c(i)$ is per-capita consumption and $u(\cdot)$ is strictly increasing, concave, and differentiable. Total consumption is an aggregate of X and Y consumption, with $c = \frac{h(X,Y)}{\bar{k}}$, where $h(\cdot, \cdot)$ is a linear-homogeneous function. The consumer price index P is the unit cost of consumption derived from $h(\cdot, \cdot)$, which can be written as a function $\phi(\cdot)$ of the price p of X given that Y is the numeraire: $P = \phi(p)$. Therefore, household consumption per capita can be written as $c(i) = \frac{I(i)}{P\bar{k}}$.

Given this structure, the household head will choose the labor supply L_k of each household member k to maximize:

$$W(\tilde{L}(i)) \equiv u\left(\frac{w^t \Sigma_{k=1,\ldots,\bar{k}(i)} A(i)e(a(k,i))L_k}{P\bar{k}(i)}\right) - \Sigma_{k=1,\ldots,\bar{k}(i)} v(L_k, a(k)). \tag{1}$$

The first-order condition for the choice of L_k in the event of an interior solution is:

$$u'\left(\frac{w^t \Sigma_{k=1,\ldots,\bar{k}(i)} A(i)e(a(k,i))L_k}{P\bar{k}(i)}\right)\left(\frac{w^t A(i)e(a(k,i))}{P\bar{k}(i)}\right) = v_1(L_k, a(k,i)). \tag{2}$$

Clearly, within a household, younger household members will be assigned less market work, since $e(a)$ is lower and $v_1(L,a)$ is higher for younger members.

We shall assume that in any equilibrium w^H is high enough, and the opportunity cost of labor for children in high-skilled households is high enough, that no high-skilled household uses child labor.

2.1 Labor supply behavior

To analyze the effects of trade agreements and their labor clauses on child labor, it is important to understand how labor supply responds to changes in the environment. We will analyze two interpretations of a child-labor ban. The first is a ban that prohibits households from sending their children under the age of 14 to work in the labor market. This type of ban is a labor-supply shock. Alternatively, the ban could take the form of a fine imposed on employers that are caught with under-age workers. This type of ban is a labor-demand shock. We will see that these two types of ban have very different implications, for welfare and for empirical predictions.

To simplify the analysis, consider the following special case. Suppose that each household i has adults whose marginal opportunity costs are always low enough that they always work the maximum $\bar{L}_k$, and whose effective labor sums to E_i. In addition, each household has one child below the age of 14, whose index is $k = 1$, and one child above the age of 14, whose index is $k = 2$.

There are two important effects on labor supply to identify: The effect of a change in the real low-skilled wage and the effect of a labor clause in the trade agreement. For the former, it will be important to distinguish between cases in which:

$$u'(x) + xu''(x) < 0 \; \forall \; x \tag{3}$$

holds and those in which it does not. Condition (3) states that the utility function is sufficiently concave that $cu'(c)$ is a decreasing function of c. This ensures that the income effect of a wage change dominates the substitution effect for labor supply, so that child labor is less likely to be used when low-skilled wages are high, as would be consistent with the empirical findings of Edmonds and Pavcnik (2005, 2006). The main result is:

Proposition 1. *An increase in w^L will lower the labor supply in the labor market of all children in the household if (3) is satisfied, and will increase their labor supply otherwise.*

Proof: See appendix.

The general equilibrium impacts of a trade agreement can be determined as follows. The endogenous variables are low- and high-skilled wages w^L and w^H, and the rental prices r^X and r^Y of the specific factors. These can be determined by the zero-profit conditions for the two sectors, plus the requirement that the total demand for each of the four factors equals its supply. Since good X is the export good, the effect of the trade agreement can be modelled as an increase in p, as foreign import barriers are lowered, making it easier to export X (and as domestic import barriers are lowered, lowering the domestic relative price of imported good Y).

An important technical issue needs to be raised. Basu and Van (1998) showed that a model with endogenous choice of child labor by households can create multiple equilibria if it results in a downward-sloped supply of low-skilled labor that is sufficiently elastic. The main results below will depend on the elasticity of labor supply either being positive or being negative with an elasticity that is not too large in magnitude, which avoids those complications. Even in the Basu and Van

(1998) model, labor supply is inelastic *locally* at the equilibrium, so the sort of marginal analysis we use here would apply.

The first result is that the trade agreement itself, apart from any labor clause, has effects on the labor market, income distribution, and child labor which depend on the nature of the export sector that the agreement promotes relative to the import-competing sector:

Proposition 2. *If the trade agreement is sufficiently low-skilled-labor biased, meaning if the production-function parameters β^{XH} and β^{YL} are sufficiently small, then the trade agreement, by raising p, will increase real low-skilled wages $\frac{w^L}{P}$ and lower high-skilled real wages $\frac{w^H}{P}$, as long as the supply elasticity of low-skilled labor is not too negative. If (3) holds, this change in $\frac{w^L}{P}$ will lower the use of child labor.*

If the trade agreement is sufficiently high-skilled-labor biased, meaning β^{XL} and β^{YH} are sufficiently small, then the trade agreement will have the opposite effect on all counts.

Proof: See appendix.

In most cases of a developing economy signing a trade agreement with a high-income country, the export sector that will be promoted will be low-skill-labor-intensive manufactures, meaning that the first of these two cases will be more relevant.

2.2 Supply-side Implementation of a Child Labor Ban

We now consider the effects of a child labor ban implemented as a supply restriction on households. This typically affects younger children directly, but it can have an indirect effect on older children whose labor is not explicitly banned:

Proposition 3. *Holding w^L constant, an enforced ban on children under the age of 14 working in the labor market will increase the market labor supply of children above the age of 14 but lower the total supply of low skilled labor.*

Proof: See appendix.

Banning the market labor of younger children induces households to increase the labor supply of older children as a *partial* compensation for the lost income. An enforced child-labor clause will also have effects on the labor-market equilibrium:

Proposition 4. *A ban on the labor of children under the age of 14 will result in an increase in the low-skilled wage either if the supply of low-skilled labor is increasing in the low-skilled real wage, or if it is decreasing in the low-skilled real wage with an elasticity that is not too large.*

Proof: See appendix.

Banning child labor reduces the supply of low-skilled labor, raising its price. Note that putting together Propositions 1, 3 and 4, we see two separate effects of a child labor supply ban on the employment of 14-17-year-olds: Holding w^L constant, the ban will increase the labor supply of 14-17-year-olds, but if (3) holds, the resulting increase in w^L will tend to *reduce* the labor supply of 14-17-year olds. The net effect is ambiguous.

2.3 Demand-side Implementation of a Child Labor Ban

Now, consider the possibility that the child-labor ban acts on the employers rather than the households, creating a labor-demand shock rather than a labor-supply shock. Suppose that in a country that has a child-labor ban in effect as specified in a trade agreement, if an employer is caught employing children under the age of 14, it will need to pay a fine. There is some probability of getting caught, and the expected fine taking into account this probability is t times the wages paid to the children workers employed.[5]

In this situation, an employer will be willing to employ those under-age workers only if their wage is sufficiently lower than the wages of other workers to compensate for the anticipated fine, so if the wage per effective unit of low-skilled labor is w^L, the wage paid to under-age labor in the presence of a child-labor ban will be $(1-t)w^L$. Henceforth, we will use w^L to denote the full wage

[5] This specification of an *ad valorem* fine is certainly not the way such fines would likely be implemented in practice. We use it for convenience, to provide an example that is easy to understand, and we conjecture that the result of a per-child fine would be similar.

as viewed by the employer, inclusive of the expected fine, and $(1-t)w^L$ to denote the wage received by the under-age worker.

To analyze the effects of such a policy, consider the special case of Proposition 1, where each household has adults with inelastic labor supply plus one child ($k=1$) under the age of 14 and one child ($k=2$) in the 14-17 range. There is no child labor among high-skilled households. In this case, we can summarize the equilibrium by a supply curve for low-skill labor, the aggregate of all low-skill household decisions for any given value of w^L, and a demand curve for low-skill labor as derived in the proof of Proposition 4.[6] If (3) holds, the labor-supply curve will be downward sloping (in which case we will assume the stability condition that it is steeper than the labor-demand curve), and otherwise it will be upward sloping.

First, we need to see how a household responds to the imposition of a fine, for a given value of w^L:

Proposition 5. *Holding w^L fixed, an increase in the fine t will increase the labor of the older child in each household. The labor supply of the younger child will fall if $u'(c)+\chi cu''(c)>0$, where $\chi \equiv \frac{w^L A(i)e(a(1,i)L_1}{w^L \Sigma_{k=1,\ldots,\bar{k}(i)} A(i)e(a(k,i))L_k}$ is the fraction of the household's income derived from the younger child's labor.*

Proof: See appendix.

Holding the low-skill wage constant, an increase in t lowers the wage only for under-age child workers. Therefore, it does not change the opportunity cost of leisure time for any household member other than the youngest worker, and so the usual tension between income and substitution effects is absent. For workers other than under-age children, there is only the income effect: Those workers work more in order to recoup some of the lost income. For the under-age worker, however, the reduction in wage does create both an income and substitution effect, and therefore an ambiguity in the effect on labor supply. Note that the income effect is larger, the larger is under-age child labor in the family's income, and so the condition given in the proposition is a statement that either the utility function is not too concave, or that χ, the share of under-age labor in total income, is

[6] In both cases, it should be underlined that these are not partial-equilibrium labor-supply and labor-demand curves. In particular, as we move along the labor-demand curve by exogenously increasing the effective supply of low-skill labor, the other factor prices are adjusting to maintain zero profits.

not too large.

Proposition 6. *Starting from $t = 0$, a small increase in t will shift the low-skill labor-supply curve to the right if*

$$u'(c(i)) + \chi(i)c(i)u''(c(i)) < 0 \tag{4}$$

for each low-skilled household i.

Proof: See appendix.

In this case, the introduction of the fine will lower the low-skill wage; lower the wage more for under-age workers; and increase the total labor supply of low-skill household members other than under-age workers (with the effect on the latter ambiguous).

The condition (4) is a very strong sufficient condition, and the rightward shift in the labor-supply curve will likely occur under much weaker conditions as well. Note that (4) is strictly stronger than (3) because $\chi(i) < 1$. It amounts to a requirement that $RRA * \chi(i) > 1$, where RRA is the coefficient of relative risk aversion. Estimates of RRA in the developing world vary widely. Cardenas and Carpenter (2008) survey numerous studies and find values from 0.05 to 2.57. Yesuf and Bluffstone (2009) provide estimates from experiments in Ethiopia that range from close to 0 up to over 15, with an average around 4. Whether (4) would be satisfied or not in practice is not clear. On the other hand, Basu (2005) and Bharadwaj *et al.* (2020) show that, regardless of RRA, imposing a fine on child labor can result in both an increase in child labor and in total unskilled labor in a model with a household subsistence consumption constraint, so the possibility of such an effect is much broader than (4) would suggest.[7]

The contrast between the implications of the supply-side implementation and the demand-side implementation of the child-labor ban is striking. The demand-side implementation implies much more adverse effects on low-skilled households. From Proposition 4, a supply-side ban, by withdrawing low-skilled labor from the market, tends to raise unskilled wages, but by Proposition 6, a demand-side ban tends to lower them, and lower them especially for the under-age child workers

[7]Indeed, the assumption that there is a minimum necessary consumption requirement, and that this constraint binds in households that use child labor, is equivalent to assuming a locally infinite RRA. Under that interpretation, the estimates of average values of RRA mentioned above are not relevant; only the value for child-labor-using households would matter, and that value may be much larger than the average.

themselves, who may still be employed in the market despite the ban. This latter effect is both through the direct effect on labor demand of penalizing employers for hiring a certain type of low-skilled worker, but it can also work through the indirect effect of increasing low-skilled labor supply through income effects.[8] Finally, under the supply-side model, the ban can only lower the incidence of child labor, while in the demand-side interpretation it is ironically possible that labor supply by the under-age child workers themselves may rise because of the ban. This is due to income effects, and is made the more likely by the possibility that unskilled wages will fall due to the ban, further lowering the incomes of low-skilled households.

2.4 Testable Hypotheses

Our theory yields a number of testable hypotheses that we summarize below:

1. A trade agreement itself can either increase or decrease child labor even without a labor clause, depending on the nature of trade that it promotes. In the likely case that it promotes low-skilled-labor-intensive manufactures from a low- or middle-income country, and (3) holds, it is likely to reduce child labor. In our regressions, we aim to measure the effect of an agreement with no labor clause on the incidence of child labor.

2. A child labor clause that prohibits households from sending children below the age of 14 into the labor market (a supply-side ban) could either raise or lower the employment of 14-and-over children. For a given real low-skilled wage, the removal of under-14 children from the labor market will tend to increase the use of 14-and-over children to replace the lost income, but if (3) holds, the increase in low-skilled wages will work in the opposite direction. In our regressions, we look both for effects of the labor clauses on the labor supply of children under 14, who are typically explicitly covered by such clauses, and also for the indirect effect on children aged 14 and up. If the latter is positive, the interpretation will be that the replacement-of-lost-income effect dominates any induced wage effect, and *vice versa* if it is

[8] It may be surprising that modeling the ban as a labor-demand-side policy works through shifting the labor-supply curve. This arises because, as noted above, these are not partial-equilibrium supply and demand curves. In particular, the labor-supply curve here shifts because of a change in household income.

negative.

3. In contrast, a child-labor ban that takes the form of a fine imposed on employers caught with under-age workers (a demand-side ban) will tend to *lower* unskilled wages, lowering them more for underage workers than for other low-skill workers, and unambiguously increase the labor supply of the rest of the household, including children aged 14-17. In addition, it will not stop under-age child labor and can even increase it.

4. A labor clause that prohibits households from sending children below the age of 14 into the labor market is likely to raise low-skilled wages, lowering wage inequality. However, the direct effect of the lost labor on income of low-income households may tend to push inequality upward. For example, if high-skilled households—that never uses child labor—are in the upper household income quantiles and low-skilled household—that would use child labor when it is allowed—are in the lower quantiles, income inequality will rise if the drop in $\frac{w^H}{w^L}$ dominates the direct loss of labor income to low-skill households from loss of child labor earnings.

3 Data and Empirical Strategy

Our labor market data comes from the World Bank's International Income Distribution Database (I2D2) which harmonizes income survey microdata from 170 countries covering response years (non-uniformly) between 1960 and 2019. The data include 1,777 unique nationally representative survey years covering 180,027,049 respondents. The underlying survey data comes from a variety of survey modules that cover income-related questions, including respondent age, employment status, wages, household income, etc. Because the incidence of and concerns over child labor are concentrated on developing countries, we likewise restrict our analysis to developing countries in the I2D2 data, resulting in a sample of 101 countries.

The temporal resolution varies by country and surveys are repeated cross-sections—respondent identification is unique only within survey-country-year, which precludes linking any repeatedly sampled individuals across time. To circumvent this limitation, we aggregate the individual microdata to the country-year. Because the surveys are nationally representative, this aggregation yields an unbalanced pseudo-panel at our unit of analysis: country years. Notably, the pseudo-panel imbalance is not systematically related to country trade policy or labor market outcomes and is instead a function of pre-determined survey schedules. As surveys are nationally representative, we use expansion weights in our calculation of country-level aggregates.

Several of our aggregated variables warrant more detailed explanation. We calculate employment rates for children under 14 and children 14–17 as the share of the surveyed age group reporting employment. We separately calculate under 14 and 14–17 employment for several interrelated reasons. First, we are concerned that survey respondents might misreport employment by young children due to social, religious, cultural, or even legal prohibitions, since 14 is frequently the legislated minimum age for employment and hence misreporting or mismeasurement of employment rates for 14–17 year olds is far less likely.[9] Second, our theoretical model explicitly predicts different responses in younger child labor markets and older child labor markets. By leveraging our rich survey microdata we are able to separately calculate child employment by age ranges and test our theoretical hypotheses. Finally, 14 years is commonly the age that differentiates primary and secondary schooling. Our differentiated child employment rates will roughly correspond to primary- and secondary-school-aged children.

For each country at each date, we calculate the relative income of households with children as the median household per capita income weighted by the share of the household members that are children, divided by the median household per capita income for the whole sample. We compute two related versions of this measure. In the first, we consider households with *any* children (i.e. under 18 years of age) and weight household observations by the share of the household members under 18. In the second measure we only consider households that have children in both of our age ranges (younger than 14 and 14–17) to better capture intra-household child labor substitution effects predicted by our theoretical model. Both measures will approximate income inequality impacts on households that contain children accounting for how exposed a household is to potential child labor bans.[10]

[9]For example, the United States Fair Labor Standards Act establishes the minimum age for non-agricultural employment at 14.

[10]Survey weights are for individual respondents, not households. Consequently we do not use expansion weights when computing these aggregate household income equality measures.

We also consider primary and secondary school enrollment rates using data from the United Nations Educational, Scientific and Cultural Organization (UNESCO) Institute for Statistics. Because education is one of the most important opportunity costs of child employment, child labor market changes are likely to be reflected in school enrollment rates. Furthermore, primary school enrollment rates in particular should reflect changes in under 14 employment without potential misreporting due to cultural stigma or legal prohibitions on child labor. School enrollment rates should also capture child labor market changes at both the intensive and extensive margin. Our child employment rates only capture changes in child labor at the *extensive* margin—entry and exit from employment—while changes in the intensity of child employment might also be reflected in school enrollment rates.

We also include in the Appendix estimates on what we refer to as wage "premia" for these age groups. We calculate these child wage "premia" as the median wage for the age group divided by the median wage over the entire population.[11] As with our relative household equality measures discussed above, these relative wage measures will avoid confounded results driven by cross-sectional income or exchange rate differences. In addition to these child wage premia, we also calculate a skill premium as the median wage for survey respondents who completed secondary education divided by the median wage for respondents who did not complete secondary schooling.

From this psuedo-panel, we construct as our analysis dataset a "stacked" country panel with a ± 3 year event window around entry into force of any RTAs. To do so we consider every year an RTA enters into force for a country and include three years before and three years after. If a country has multiple RTAs enter into force in adjacent years, each of ± 3 year event windows enter into the analysis dataset separately and are "stacked" or pooled.[12] We report summary statistics from this analysis dataset in Table 1.

Our data on RTAs and agreement content have been collected as part of the broader World Bank

[11] Here we condition on strictly positive wages. Numerous respondents report a wage of zero despite reporting employment. The majority of these respondents are employed in the agricultural sector or the "commerce" sector; we speculate these individuals are employed by family-owned businesses in which household members do not draw individual wage compensation. Including such zero wages will severely bias aggregated wages to zero in our psuedo-panel.

[12] Given our focus on child employment, we filter our data on country-year observations for which aggregated child employment rates (both under 14 and 14–17) are not missing.

project on the content of trade agreements. These data are described in Mattoo *et al.* (2020) with labor market provisions in particular being discussed in detail in Raess and Sari (2020). We focus in particular on trade agreements that include reference to the "abolition of child labor." We also consider whether such provisions are legally binding. The trade agreement content coded in Raess and Sari (2020) does not differentiate between non-derogation child labor provisions—provisions that reinforce existing child labor bans under national laws—from child labor bans introduced via issue linkage in the trade agreement. Regardless of whether such provisions reinforce existing national laws or introduce new legal commitments via the trade agreement, issue linkage under the RTA exposes non-compliant countries to potential trade retaliation under the RTA, regardless of national legislation. Additionally, our trade agreement data does not record how child labor provisions are implemented at the national level. As a result, we cannot differentiate child labor supply shocks (labor supply bans) from child labor demand shocks (employer-based fines) and, hence, we cannot directly test for heterogeneous impacts of different implementation schemes as predicted in our theoretical model. Our estimates of the effects of child labor provisions will therefore reflect average effects across different unobserved implementation schemes. Table A.1 provides the list of RTAs notified to the WTO and currently in force that have included child labor provisions and the legal enforceability of such clauses.

3.1 Methodology

We identify both the effects of trade liberalization on our target labor market outcomes as well as the impact of child labor provisions in RTAs with the following triple-difference model on our stacked country panel:

$$y_{igt} = \beta_1 RTA_{igt} + \beta_2 RTA_{igt} * Child_{ig} + \alpha_{ig} + \delta_t + \varepsilon_{igt} \tag{5}$$

where i indexes countries, t indexes time, and g indexes RTAs with y_{igt} as the outcome of interest. RTA_{igt} is an indicator equal to 1 if year t is later than the year that RTA g enters into force and zero prior, and $Child_{ig}$ is in indicator equal to 1 if RTA g includes the child labor provision. We include country-RTA fixed effects with α_{ig}. By including α_{ig} we normalize the pre-RTA level of output to 0 for each country-RTA "stack" in our dataset—estimated triple difference coefficients describe outcome changes at the country level relative to the pre-period within country and within RTA. Country characteristics such as political, legal and religious institutions, persistent demographic profiles, and time-invariant comparative advantages (e.g. driven by population size or geography) are accounted for via the inclusion of α_{ig}. Critically, α_{ig} also controls for all RTA-level factors that might lead to endogenous inclusion of child labor provisions, including relative characteristics across signatory countries (e.g. legal, religious, demographic, comparative advantages, etc.). We also include time fixed effects with δ_t. We consider two approaches to controlling for common changes across time: calendar-year and event-time fixed effects. Calendar-year fixed effects control for common, cross-country shocks that affect the global economy while event-time fixed fixed effects account for common dynamics around trade liberalization. The inclusion of event-time fixed effects does not allow us to separately identify β_1 in equation (5) from the event-time fixed effects. Hence in models that include event-time fixed effects we only identify the differential effects of child labor provisions *relative to trade agreements that do not include them.*[13] We also note that since our event window is fixed in time for each RTA and country, our country-RTA fixed effects in conjunction with event time fixed effects control for a large share of variation in country-year outcomes.

We report two-way, cluster-robust standard errors clustered at the country-RTA level—to account for temporal autocorrelation within treatment units—and at the country-year level—to account for correlation across RTA "stacks" that might arise from overlapping, adjacent RTA treatment at the country level.

As argued in Abman and Lundberg (2020) and Abman *et al.* (2021), entry into force of RTAs requires separate, independent ratification from multiple signatory countries— countries cannot unilaterally enact the RTA which creates plausibly exogenous variation in the timing of RTA policy exposure.[14] Hence, our triple difference parameters recover plausibly causal impacts of trade

[13] Our event stacking approach yields an analog to two-way FE triple difference models with common treatment timing. In such models, only the triple-difference coefficient is identifiable

[14] Some RTAs enter into force upon independent ratification by *all* signatory countries while other RTAs only require ratification by a threshold share of signatories. Regardless, the multilateral ratification process creates plausible exogeneity in the timing an RTA enters into force.

liberalization. In Model (5), β_1 captures the changes in country outcomes after the enactment of an RTA without labor protections while β_2 captures the differential effect from RTAs with relevant child labor provisions. Thus, entry into force of RTAs that include child labor provisions will lead to an estimated $\beta_1 + \beta_2$ increase in the outcome variable.

In addition to these main triple-difference models, we also estimate a modified two-way fixed effect synthetic difference-in-difference (DiD) model from Arkhangelsky *et al.* (2021). The synthetic DiD model combines elements of the traditional DiD framework with elements from the synthetic control framework. Observations are weighted to align pre-trends in outcomes between treated and untreated units—effectively "parallelizing" pre-trends. Observations are also weighted to balance pre-treatment time periods with post-treatment time periods. The weighted observations are then used in a two-way fixed effect model to estimate treatment effects. Our particular application of this model requires some explanation and additional data processing. Arkhangelsky *et al.* (2021) develops the model for a balanced panel with homogeneous treatment timing. To facilitate identification in our setting, we balance our stacked treatment window panel by backward interpolation at the country-RTA level. Our interpolation scheme is constant—if observation y_{igt} is missing, we interpolate it to $y_{igt'}$ where $t' > t$ is the first non-missing observation for country-RTA ig. While treatment years are not homogeneous, our windowed treatment approach yields homogeneous treatment timing in *event time*, which we index by $s = -3, -2, -1, \ldots, 3$. The cross-sectional observational unit in our application is the country-RTA level (ig) while the temporal observational unit in our synthetic DiD application is event-time units (s). Hence, our synthetic DiD model estimates the following:

$$\underset{\beta_2,\alpha,\delta}{\operatorname{argmin}} \left\{ \sum_{ig} \sum_{gt} \left(y_{igs} - \beta_2 RTA_{igs} * Child_{ig} - \alpha_{ig} - \delta_s \right)^2 \hat{\omega}_{ig}^{sdid} \hat{\lambda}_{gs}^{sdid} \right\} \tag{6}$$

where $\hat{\omega}_{ig}^{sdid}$ are the unit weights that parallelize pre-trends and $\hat{\lambda}_{gs}^{sdid}$ are the time weights that balance pre/post treatment (Arkhangelsky *et al.*, 2021). α_{ig} and δ_s are unit and time fixed effects respectively and our estimate $\hat{\beta}_2$ from Equation (6) is the synthetic DiD treatment effect of child labor provisions in RTAs relative to RTAs without child labor provisions. As in the model with event-time fixed effects described above, we cannot separately identify the effect of an RTA without a provision on our outcomes of interest from the event-time fixed effects.

4 Results

We present estimates from triple difference-in-differences and synthetic difference-in-differences models on youth employment rates in Table 2. In Panel A we report employment rates among children under 14 years of age and in Panel B examine employment rates among children aged 14 to 17. In both panels, column (1) reports estimates from triple DiD models country-RTA fixed effects. Column (2) reports estimates from a DiD-analog with country-RTA fixed effects and event time FE. Because we cannot separately identify event time FE and the post-RTA coefficient (β_1 from equation (5)), Column (2) only reports the marginal impact of child labor provisions (β_2 from equation (5)). Column (3) reports estimates from a triple-DiD model with country-RTA fixed effects and calendar year FE. Columns (4)–(6) report estimates from models equivalent to those in Columns (1)–(3) but using our balanced, backward interpolated panel. Finally, Column (7) reports estimates from our synthetic DiD model.

Our estimates on the under-14 employment rate in Panel A are generally imprecise. We find no statistically significant response to trade liberalization or child labor provisions in under 14 employment. We find some suggestive support for the type of income effect documented in the literature (e.g. Edmonds and Pavcnik, 2005, 2006) with the under-14 employment rate in developing countries falling by .02–.2 percentage points following entry into force of an RTA. We find robust but imprecise evidence suggesting that child labor provisions in RTAs lead to small *increases* in under 14 employment of 0.1–0.4 percentage points. Although statistically imprecise, these estimates are economically significant and large in magnitude, corresponding to increases of 5%–20% relative to the mean under-14 employment rate of 2%.

In contrast, our estimates on the 14–17 employment rate in Panel B are highly significant and generally consistent across specifications. We find additional evidence of an income effect from the literature, with reductions in the 14–17 employment rate of between 0.3 and 1.3 percentage points which correspond to approximately 2%–9% declines relative to a mean of 15%, but these estimates become insignificant upon inclusion of time controls. We find that child labor provisions significantly increase the 14–17 employment rate by 0.9–1.3 percentage points. This findings are statistically significant at the 5% level or smaller across all model specifications and correspond to an increase in 14–17 employment by between 6%–9% relative to the mean.

Overall, we find empirical support for income effects of trade liberalization driving down child employment, consistent with both the literature (e.g. Edmonds and Pavcnik, 2005, 2006) as well as our theoretical model. In contrast, we find that provisions in RTAs prohibiting child labor *increase* employed children, especially for older children. This paradoxical finding is consistent with empirical and theoretical findings in the literature (e.g. Bharadwaj *et al.*, 2020; Basu, 2005), in which the ban reduces incomes of low-income households and induces them to increase child labor supply.[15] These results are also consistent with our theoretical model which suggests that a ban on the youngest children working leads to intra-household substitution towards older children. We identify imprecisely estimated but positive effects of child labor bans on under-14 employment which is consistent with the fine-based, demand-side implementation of the ban but not with a household supply-side implementation. We note, however, that there are reasons to believe effects on under-14 employment might be difficult to identify in the presence of mismeasured outcomes. For the arguably better measured older-child employment (14–17) rates, our findings are robust across model specifications, statistically significant, and large in economic magnitude.

We turn our attention to educational enrollment rates in Table 3. We report primary school enrollment rates in Panel A. As above, Columns (1)–(3) report models with country-RTA fixed effects and no time fixed effects, event time fixed effects, and calendar year fixed effects, respectively. Columns (4)–(6) report analogous models using our balanced, backward interpolated panel and Column (7) reports synthetic DiD estimates. We find small imprecisely estimated positive impacts on primary school enrollment rates following trade liberalization that are statistically and economically insignificant. While we cannot differentiate these estimates from zero, this weak evidence is

[15]The results in Lakdawala *et al.* (2022) are somewhat more ambiguous. The policy change studied there is a law in Bolivia that made certain types of child labor legal, but subject to rules that were enforceable by fine. Overall it was a relaxation of rules imposed on employers, which resulted in a reduction in the use of child labor.

at least consistent with the reductions in primary-school-age child employment (under 14) that we observe in Table 2. We find highly significant reductions in primary school enrollment rates with exposure to child labor provisions. Our estimates range from 0.3–1.2 percentage point drops which correspond reductions in primary school enrollment by 0.3%–1.3% of the mean. These findings are consistent with our weak, statistically insignificant findings from Table 2 which suggests that under 14 employment increases following exposure to child labor provisions in RTAs.

Two points on these primary school results warrant further discussion. First, primary school enrollment rates are unlikely to suffer the same misreporting issues that direct enumeration of under-14 workers in our survey microdata might suffer from. Consequently, these more precisely estimated drops in primary school enrollment rates might reflect stronger employment effects than we are able to directly identify with under-14 employment data. Second, primary school enrollment results might reflect under-14 labor market effects at both the intensive and extensive margin. Our child employment rates capture changes in child labor only at the *extensive* margin—entry and exit from employment. But there may also be intensive margin effects of child labor provisions in RTAs, in which under-14 children who are employed end up working more hours on average. Such intensive margin effects would not be reflected in changes in country-level under-14 employment rates but may be reflected in lower primary school enrollment rates as work and school together become unfeasible.

We find similar patterns with secondary school enrollment rates in Panel B of Table 3, however the income effects from trade liberalization are now statistically and economically significant in some specifications. We estimate increases in secondary school enrollment rates of up to 3% of the mean following entry into force of RTAs without child labor provisions. As above, Columns (1)–(3) report models with country-RTA fixed effects and no time fixed effects, event time fixed effects, and calendar year fixed effects, respectively. Columns (4)–(6) report analogous models using our balanced, backward interpolated panel and Column (7) reports synthetic DiD estimates. Inclusion of child labor provisions reduces secondary school enrollment rates by 0.2–0.9 percentage points, up to 1.2% of the mean. These findings are significant across a number of specifications, notably at the 5% level in our synthetic DiD specification. In general, these findings lend credence to our findings on 14–17 employment rates in Panel B of Table 2—increases in 14–17 employment correspond to decreases in secondary school enrollment rates.

Finally, we consider impacts of trade liberalization on household (in)equality in Table 4. Consistent with our other tables, Columns (1)–(3) report models with country-RTA fixed effects and no time fixed effects, event time fixed effects, and calendar year fixed effects, respectively. Columns (4)–(6) report analogous models using our balanced, backward interpolated panel and Column (7) reports synthetic DiD estimates. In Panel A we consider per capita incomes of households with children relative to all households in a country, year. We weight household incomes in the numerator by the share of the household that is under 18 since households with a larger share of children will be more strongly exposed to child labor provisions. For the most part, our findings are qualitatively consistent with our findings on under 14 employment. We do not find consistent (or even suggestive) evidence of an impact from RTAs without child labor provisions—in some specifications we find small, imprecise negative impacts and in some specifications small, imprecise positive impacts. The inclusion of child labor provisions in general tends to lead to increases in the income of households with children relative to the household population. Notably, this positive effect is significant at the 1% level in our synthetic DiD specification which corresponds to an increase in relative incomes of households with children of approximately 1.6% at the mean. These findings are again consistent with patterns and findings on child employment and school enrollment rates—the inclusion of child labor provisions tends to increase child employment, decrease school enrollment and, as a result, raise incomes in households with children relative to the population of households. We are careful to note that this last finding does not follow trivially from our findings on child employment. Our theoretical model makes clear that these policies can push down wages for child workers and increase their labor supply at the same time, creating competing forces on total household income.

In Panel B of Table 4 we focus on the role of substitutability of under-14 and 14–17 labor in these household income effects. As above, we consider per capita incomes of households with children relative to all households in a country, year but here limit our attention in the numerator to households that have *both* under 14 and 14–17 year old children. We continue to weight household incomes in the numerator by the share of the household that is under 18. Columns (1)–(3) report models with country-RTA fixed effects and no time fixed effects, event time fixed effects, and calendar year fixed effects, respectively. Columns (4)–(6) report analogous models using our balanced, backward interpolated panel and Column (7) reports synthetic DiD estimates. We find similar patterns to those documented in Panel A but with much stronger and more precisely estimated effects. We find significant reductions in relative incomes of households with both types of children. We note that these are *relative* income effects and, as such, are still entirely consistent with income effects driving child labor market response to RTAs without child labor prohibitions. It is likely the case that household incomes across the population rise in response to trade liberalization but that for households with children, this facilitates pulling those children out of the labor market which reduces per capita earnings relative to the population. These relative income effects are significant at the 1% level in some specifications and correspond to a more than 3% reduction in household equality relative to the mean.

We find robust evidence of increases in relative household incomes with the inclusion of child labor provisions with significantly positive increases in relative incomes across most specifications of up to 3.3%. These positive impacts are entirely consistent with increases in child employment and decreases in school enrollments that we document in Tables 2 and 3. Our theoretical model suggests that a demand-side restriction on child labor implemented as a fine on employers of underage children will drive down wages earned by the youngest children and, ceteris paribus, lower household income. This pushes 14-17 year old children into employment with potentially ambiguous results for under-14 children for which the provisions create both income and substitution effects. Hence, the ex ante impact of child labor provisions on per capita income for households with both younger and older children is ambiguous. Panel B of Table 4 suggests that the net effect on household incomes is positive—child labor provisions lead to income equality between households with children and the general population, but apparently through increases in child employment. We also note that our identification strategy cannot identify the longer-run impacts that will arise from changes in schooling behavior. Reductions in primary and secondary school enrollment rates attributable to child labor provisions are likely to have longer-term negative impacts on household inequality that we are unable to capture with our relatively narrow event window.

We present additional evidence supporting our identification strategy with fully-specified event study models in Figure 2. We extend our triple DiD model with country-RTA fixed effects and event time fixed effects (Columns (2) and (4) in Tables 2–4) by interacting our treatment variable $Child_{ig}$ with a full set of event-time dummy variables rather than a simple dichotomous post treatment variable as in Equation (5). We omit the dummy variable for the year before an RTA enters into force as a reference year. As in our triple DiD models with event time fixed effects, we cannot separately identify trade liberalization dynamics with this event study specification. Instead, our estimated event study coefficients will reflect the dynamics of outcomes in RTAs with child labor provisions relative to RTAs without, normalized to the year before the RTA with provisions enters into force. Violation of the parallel trends identifying assumption should be evident as pre-trends in these event studies. We present event study coefficient estimates and 95% confidence intervals in Figure 2.[16] We find no evidence that our results are spuriously driven by differential trends.[17]

We also consider the role that enforceability plays in child labor outcomes which we report in the Appendix. To do so we add an additional differencing dimension (i.e. a quadruple difference model) capturing whether provisions are legally binding and enforceable under the RTA. Coefficient estimates of the added interaction effect are generally imprecise as the majority of the RTAs with provisions in our sample have enforceable provisions (21/29). For some outcomes, it seems the dimension of enforceability may attenuate the impacts from the provisions themselves (child employment, primary education) however, for secondary school enrollment, the larger effect is driven by the agreements with enforceable child labor provisions. While we cannot reject the hypothesis that there is no difference between the two types of provisions, our estimates of the differential effect lack precision. We present additional results on child wage premia, the skill premium in the Appendix.

[16]Confidence intervals are derived from two-way cluster robust standard errors clustered by country-RTA and country-year.

[17]In addition to this event study evidence that supports our identifying assumptions, our synthetic DiD approach is uniquely designed to address failures in parallel trends by "parallelizing" pre-trends.

5 Concluding Remarks

In this paper, we study the impact of labor provisions in trade agreements aimed at eliminating the use of child labor. Such provisions have become more common over time as trade agreements have become more expansive in scope and increasingly cover non-trade areas. Using a large set of survey data from across 101 developing countries, we employ a variety of DiD and triple DiD regressions to identify the effect of RTAs without a child-labor provisions on child labor outcomes, as well as the differential effect of an agreement that does have such a provision.

We find evidence that a trade agreement with no child-labor provision reduces the fraction of 14-17-year-olds in the labor force by a statistically and economically significant amount with corresponding increases in secondary school enrollment. We find similar effects for children under the age of 14 but these estimates are imprecise and not statistically significant. The pattern is consistent with an interpretation that a developing economy joining a trade agreement opens up possibilities for low-skill export employment and is consistent with the empirical findings of Edmonds and Pavcnik (2005) and Edmonds and Pavcnik (2006) that opening trade can reduce child labor in developing economies.

On the other hand, we find that the inclusion of child labor prohibitions in RTAs perversely *increases* child labor—the opposite of their intended effect. We find statistically and economically significant increases in the employment of 14-17 year old children and statistically imprecise but economically significant increases in under-14 labor as well. We find corresponding drops in both primary and secondary school enrollment rates following entry into force of RTAs with child labor prohibitions.

Our results are strikingly similar to those predicted in Basu (2005) and found empirically in Bharadwaj *et al.* (2020)—a case study of the 1986 ban in India. As in these works and in our theoretical model, if the ban is enforced through a fine imposed on employers, it will tend to depress child wages, inducing the household to increase total labor supply in order to make up the lost income—including the household child labor supply. We find a strong, significant increase in employment for 14-17-year-olds but none for under-14-year-olds, which is consistent with the model prediction of an ambiguous effect on labor supply for the younger children who are the targets of the ban. But it is also possible that employment effects are not well measured for the younger children, since families with children working illegally may be reluctant to report that information to survey-takers. The strongly significant reduction in school enrollment rates for both age ranges, which is larger for elementary school enrollment, suggests that there may be an employment effect for the younger children that is missed in the data.

The policy implications of this work are subtle. On the one hand, the findings suggest that labor provisions in RTAs are effective, as they impact on child labor outcomes and other relevant non-trade outcomes such as schooling and household inequality. On the other hand, the evidence suggests that in a developing-country context, opening opportunities for low-skill intensive exports is more effective at reducing child labor than an outright ban. Child labor bans in RTAs tend to be counterproductive because they push down the income of households who use child labor as an income source, requiring them to double down on the use of child labor. While beyond the scope of this paper, the analysis seems to suggest that provisions in RTAs aiming at eliminating child labor should consider approaches other than bans, such as the introduction of direct payments to households for school attendance, as discussed in Doran (2013).

While we bring new information on these relationships, there are many areas outside the scope of this paper that present valuable avenues for future research. The narrow time window used in our analysis (+/- 3 years around entry into force) limits our ability to study medium- and long-run impacts of child labor provisions on these outcomes. The increases in school enrollment we observe following trade agreements without labor clauses might increase human capital accumulation and increase incomes later in affected children's households. We also do not address sector-specific trade shocks or heterogeneity by sectors that may employ more or less child/youth labor. There may be complementarities arising from child labor provisions included in RTAs that lower tariffs on exports from industries most likely to use child labor. Finally, our broad dataset limits our ability to explicitly test underlying mechanisms driving the results that we find. We believe all of these areas offer important avenues for future work.

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Table 1: Summary Statistics for Child Labor Market Outcomes

Panel A: Main Analysis Data ("Stacked" Panel of 7 Year Event Windows)

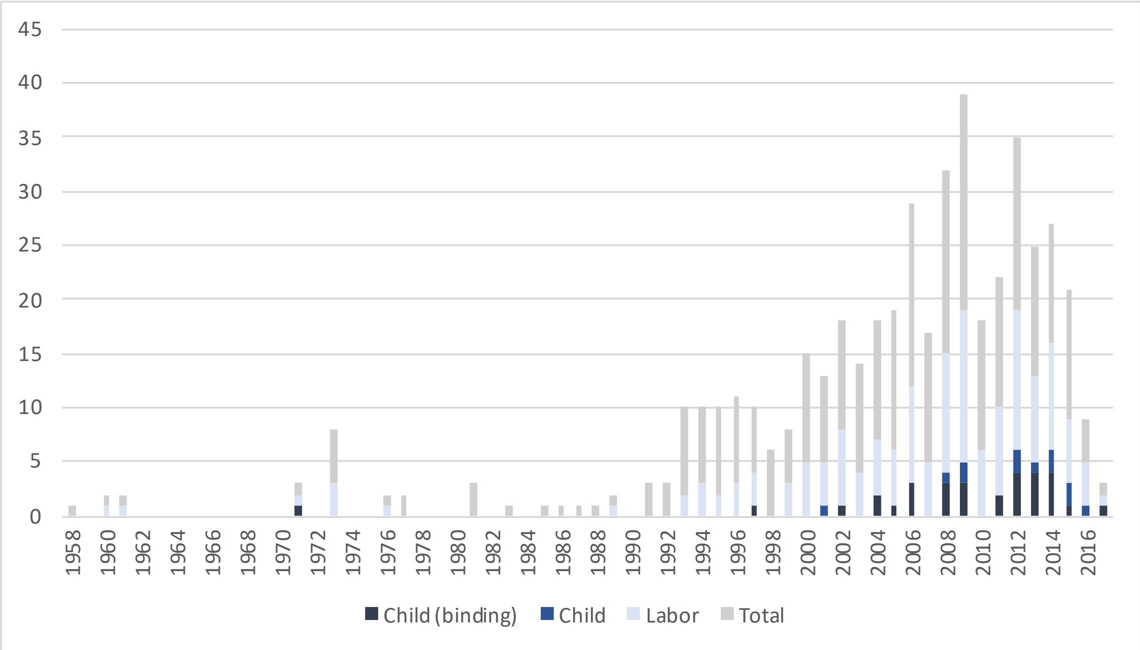
	N	Mean	St. Dev.	Min	Max
Under 14 Employment Rate	2,779	0.021	0.052	0.000	0.628
14–17 Employment Rate	2,779	0.154	0.169	0.000	0.838
Primary School Enrollment Rate	2,317	0.917	0.095	0.214	0.999
Secondary School Enrollment Rate	1,812	0.739	0.209	0.044	0.998
Household Equality (any child)	1,777	0.812	0.130	0.466	1.623
Household Eq (both under 14 and 14–17)	1,755	0.700	0.179	0.348	1.977

Panel B: Unique Country, Year Observations from 7 Year Event Windows

	N	Mean	St. Dev.	Min	Max
Under 14 Employment Rate	1,148	0.032	0.065	0.000	0.628
14–17 Employment Rate	1,148	0.190	0.180	0.000	0.838
Primary School Enrollment Rate	870	0.888	0.129	0.214	0.999
Secondary School Enrollment Rate	626	0.632	0.235	0.044	0.998
Household Equality (any child)	619	0.809	0.148	0.466	1.623
Household Eq (both under 14 and 14–17)	612	0.715	0.203	0.348	1.977

This table reports summary statistics for the main outcomes in our analysis. In Panel A we report statistics from our main analysis dataset that "stacks" or pools overlapping event windows three years before through three years after entry into force of an RTA. In Panel B we report statistics from unique country, year observations from these event windows (i.e. we omit duplicated observations from adjacent event windows).

Figure 1: RTAs enacted with different labor provisions over time



This figure presents all RTAs that enter into force in a given year. We plot all RTAs, RTAs with labor provisions, RTAs with child labor provisions and RTAs with binding child labor provisions over time.

Table 2: Child Labor Provisions and Child Employment Rates in Developing Countries

Panel A: Under 14 Employment Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post RTA	−0.0002		−0.002	−0.0002		−0.002	
	(0.003)		(0.002)	(0.003)		(0.002)	
Post × Child	0.001	0.001	0.004	0.001	0.001	0.004	0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	2,182	2,182	2,182	2,186	2,186	2,186	2,186
R^2	0.805	0.805	0.817	0.805	0.805	0.817	0.817
Mean	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Panel	unbal	unbal	unbal	interp	interp	interp	interp
Country×RTA FE	✓	✓	✓	✓	✓	✓	✓
Event time FE		✓			✓		✓
Year FE			✓			✓	
Synth DiD							✓

Panel B: 14–17 Employment Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post RTA	−0.013***		−0.003	−0.013***		−0.003	
	(0.003)		(0.002)	(0.003)		(0.002)	
Post × Child	0.013***	0.013***	0.012**	0.013***	0.013***	0.012**	0.009**
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)
Observations	2,779	2,779	2,779	2,783	2,783	2,783	2,783
R^2	0.971	0.971	0.972	0.971	0.971	0.972	0.972
Mean	0.15	0.15	0.15	0.15	0.15	0.15	0.15
Panel	unbal	unbal	unbal	interp	interp	interp	interp
Country×RTA FE	✓	✓	✓	✓	✓	✓	✓
Event time FE		✓			✓		✓
Year FE			✓			✓	
Synth DiD							✓

FE regressions on a stacked country-level panel with a ±3 year event window around RTA entry into force. Child is an indicator for provisions prohibiting child labor. All models include country-RTA fixed effects. Regressions with event time FE are equivalent to two-way FE difference-in-difference models while regressions with only country-RTA FE and with country-RTA and year FE are equivalent to one-way and two-way triple-difference models, respectively. Unbalanced panels are reported in columns (1)–(3) while columns (4)–(7) use backwards constant interpolation to balance the panel. Column (7) estimates a synthetic two-way FE difference-in-difference model (Arkhangelsky *et al.*, 2021) on the balanced data. Robust standard errors are two-way clustered at the country-RTA and country-year levels. Statistical significance from two-sided t tests are denoted by *p<0.1; **p<0.05; ***p<0.01.

Table 3: Child Labor Provisions and School Enrollment Rates in Developing Countries

Panel A: Primary School Enrollment Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post RTA	0.001		0.001	0.003		0.002	
	(0.002)		(0.002)	(0.002)		(0.002)	
Post × Child	−0.011***	−0.011***	−0.004	−0.012***	−0.012***	−0.003	−0.009***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.002)
Observations	2,317	2,317	2,317	2,512	2,512	2,512	2,512
R^2	0.964	0.965	0.968	0.963	0.964	0.967	0.967
Mean	0.92	0.92	0.92	0.92	0.92	0.92	0.92
Panel	unbal	unbal	unbal	interp	interp	interp	interp
Country×RTA FE	✓	✓	✓	✓	✓	✓	✓
Event time FE		✓			✓		✓
Year FE			✓			✓	
Synth DiD							✓

Panel B: Secondary School Enrollment Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post RTA	0.023***		0.001	0.021***		0.001	
	(0.003)		(0.002)	(0.002)		(0.002)	
Post × Child	−0.008*	−0.009**	−0.005	−0.006	−0.006	−0.002	−0.009**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	1,812	1,812	1,812	2,080	2,080	2,080	2,080
R^2	0.992	0.993	0.993	0.992	0.992	0.993	0.993
Mean	0.74	0.74	0.74	0.74	0.74	0.74	0.74
Panel	unbal	unbal	unbal	interp	interp	interp	interp
Country×RTA FE	✓	✓	✓	✓	✓	✓	✓
Event time FE		✓			✓		✓
Year FE			✓			✓	
Synth DiD							✓

FE regressions on a stacked country-level panel with a ±3 year event window around RTA entry into force. Child is an indicator for provisions prohibiting child labor. All models include country-RTA fixed effects. Regressions with event time FE are equivalent to two-way FE difference-in-difference models while regressions with only country-RTA FE and with country-RTA and year FE are equivalent to one-way and two-way triple-difference models, respectively. Unbalanced panels are reported in columns (1)–(3) while columns (4)–(7) use backwards constant interpolation to balance the panel. Column (7) estimates a synthetic two-way FE difference-in-difference model (Arkhangelsky *et al.*, 2021) on the balanced data. Robust standard errors are two-way clustered at the country-RTA and country-year levels. Statistical significance from two-sided t tests are denoted by *p<0.1; **p<0.05; ***p<0.01.

Table 4: Child Labor Provisions and Relative Incomes of Households with Children

Panel A: Relative Income of Households with *any* Children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post RTA	−0.008		0.006	−0.008		0.002	
	(0.007)		(0.005)	(0.007)		(0.004)	
Post × Child	0.009	0.008	−0.001	0.008	0.007	0.001	0.013***
	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)	(0.007)	(0.005)
Observations	1,506	1,506	1,506	1,693	1,693	1,693	1,693
R^2	0.869	0.870	0.900	0.884	0.885	0.909	0.909
Mean	0.81	0.81	0.81	0.81	0.81	0.81	0.81
Panel	unbal	unbal	unbal	interp	interp	interp	interp
Country×RTA FE	✓	✓	✓	✓	✓	✓	✓
Event time FE		✓			✓		✓
Year FE			✓			✓	
Synth DiD							✓

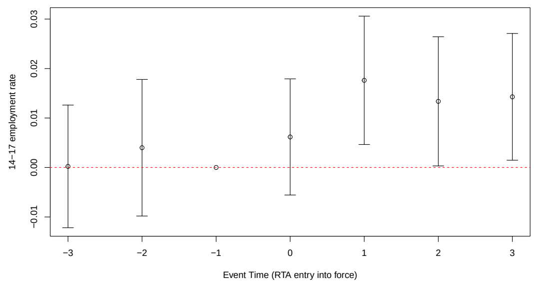
Panel B: Relative Income of Households with both <14 and 14–17 Children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post RTA	−0.023***		0.005	−0.023***		0.002	
	(0.006)		(0.006)	(0.007)		(0.005)	
Post × Child	0.016**	0.015**	0.004	0.015*	0.014*	0.006	0.023***
	(0.008)	(0.007)	(0.006)	(0.008)	(0.008)	(0.007)	(0.007)
Observations	1,486	1,486	1,486	1,673	1,673	1,673	1,673
R^2	0.905	0.908	0.932	0.916	0.918	0.937	0.937
Mean	0.70	0.70	0.70	0.70	0.70	0.70	0.70
Panel	unbal	unbal	unbal	interp	interp	interp	interp
Country×RTA FE	✓	✓	✓	✓	✓	✓	✓
Event time FE		✓			✓		✓
Year FE			✓			✓	
Synth DiD							✓

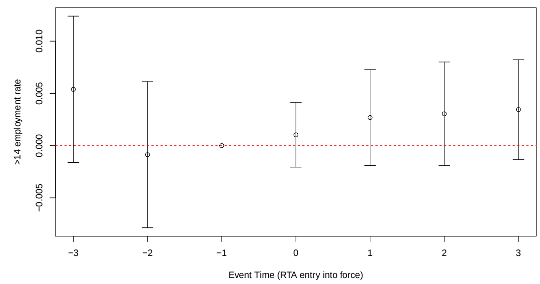
FE regressions on a stacked country-level panel with a ±3 year event window around RTA entry into force. The dependent variable is the weighted median of household per capita income from from households with under 18 children—weighted by the share of the household that is under 18—divided by the median household per capita income of the population. In the top panel, we consider households with any under 18 children. In the bottom panel we focus on household that have both young children (<14) and older children (14–17). Child is an indicator for provisions prohibiting child labor. All models include country-RTA fixed effects. Regressions with event time FE are equivalent to two-way FE difference-in-difference models while regressions with only country-RTA FE and with country-RTA and year FE are equivalent to one-way and two-way triple-difference models, respectively. Unbalanced panels are reported in columns (1)–(3) while columns (4)–(7) use backwards constant interpolation to balance the panel. Column (7) estimates a synthetic two-way FE difference-in-difference model (Arkhangelsky *et al.*, 2021) on the balanced data. Robust standard errors are two-way clustered at the country-RTA and country-year levels. Statistical significance from two-sided t tests are denoted by *p<0.1; **p<0.05; ***p<0.01.

Figure 2: Event Studies

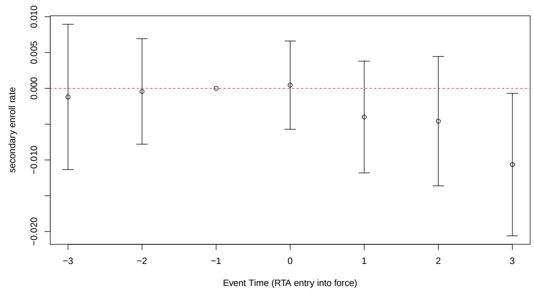
(a) 14–17 Employment Rate



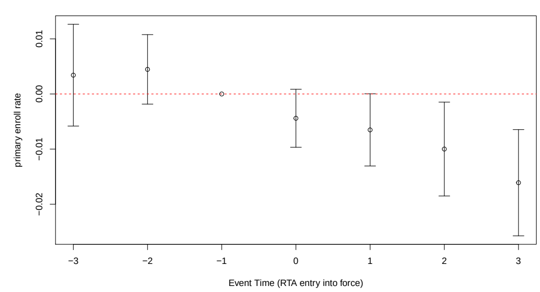
(b) <14 Employment Rate



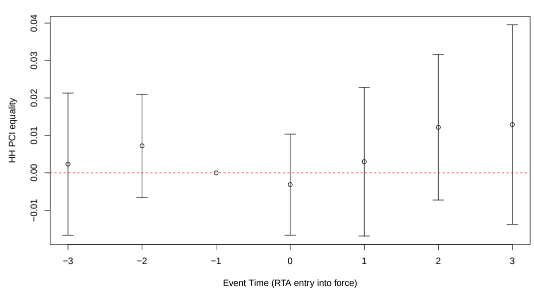
(c) Secondary School Enrollment Rate



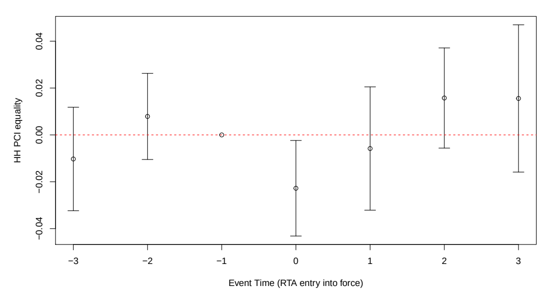
(d) Primary School Enrollment Rate



(e) <18 Household Income Equality



(f) <14, 14–17 Household Income Equality



This figure presents coefficient estimates with 95% confidence intervals from fully specified two-way fixed effect event studies for main child labor outcomes. All models include country-RTA fixed effects and event time fixed effects. Coefficients correspond to dynamic effects of entry into force of RTAs that include child labor provisions relative to RTAs that do not, normalized to the year before entry into force. All models estimated on backward interpolated balanced panels and include two-way cluster-robust standard errors clustered by country-RTA and country-year.

A Appendix—Theory Proofs

Proposition 1. *An increase in w^L will lower the labor supply of children (of either type) in the labor market if (3) is satisfied, and will increase the labor supply otherwise.*

Proof. We will analyze the problem for a household i, and in what follows we will omit the i argument. The total derivative of (2) for a low-skilled household with respect to w^L is:

$$u''(c)\left[\frac{\Sigma_{k'}L_{k'}Ae_{k'}+E}{P\bar{k}}\right]\frac{w^L e_k A}{P\bar{k}}+u''(c)w^L\left[\Sigma_{k'}\frac{dL_{k'}}{dw^L}\frac{Ae_{k'}}{P\bar{k}}\right]\frac{w^L e_k A}{P\bar{k}}+u'(c)\frac{e_k A}{P\bar{k}}$$
$$=V_{11}(L_k,a_k)\frac{dL_k}{dw^L}$$

for $k=1,2$, where c is household per-capita consumption, E is the fixed adult labor supply, and e_k and a_k denote $e(a(k,i))$ and $a(k,i)$ respectively.

These conditions can be written as:

$$\mathbf{M}\frac{d\mathbf{L}}{dw^L}=\mathbf{b}, \tag{7}$$

where $\mathbf{L}\equiv(L_1,L_2)'$,

$$\mathbf{M}=u''(c)\left(\frac{w^LA}{P\bar{k}}\right)^2\begin{pmatrix} e_1^2 & e_1e_2 \\ e_1e_2 & e_2^2\end{pmatrix}-\begin{pmatrix} V_{11}(L_1,a_1) & 0 \\ 0 & V_{11}(L_2,a_2)\end{pmatrix},$$

and

$$\mathbf{b}=-(u''(c)c+u'(c))\frac{A}{P\bar{k}}\begin{pmatrix} e_1 \\ e_2\end{pmatrix}.$$

The determinant of $\mathbf{M}$ is:

$$D=-u''(c)\left(\frac{w^LA}{P\bar{k}}\right)^2[V_{11}(L_2,a_2)e_1^2+V_{11}(L_1,a_1)e_2^2]+V_{11}(L_1,a_1)V_{11}(L_2,a_2)>0.$$

The inverse of $\mathbf{M}$ is given by:

$$D\mathbf{M}^{-1} = u''(c)\left(\frac{w^L A}{P\bar{k}}\right)^2 \begin{pmatrix} e_2^2 & -e_1 e_2 \\ -e_1 e_2 & e_1^2 \end{pmatrix} - \begin{pmatrix} V_{11}(L_2, a_2) & 0 \\ 0 & V_{11}(L_1, a_1) \end{pmatrix},$$

and so the labor-supply derivatives are:

$$\frac{dL_k}{dw^L} = \frac{1}{D}\frac{Ae_k}{P\bar{k}} V_{11}(L_{k',a'})(u''(c)c + u'(c)),$$

where $k = 1$ and $k' = 2$ (so a' denotes $a(2,i)$), or $k = 2$ and $k' = 1$ (so a' denotes $a(1,i)$). Clearly, these labor-supply derivatives have the same sign as $(u''(c)c + u'(c))$, so both labor supplies are decreasing in the wage if (3) is satisfied, and increasing otherwise. **Q.E.D.**

Proposition 2. *Holding w^L constant, an enforced ban on children under the age of 14 working in the labor market will increase the market labor supply of children above the age of 14 but lower the total supply of low skilled labor.*

Proof. Denote the labor supply for the two children before the ban by L_1' and L_2', and the household per-capita consumption by c', respectively. After the ban denote these variables by $L_1'' = 0$, and the L_2'', and c'' respectively. If $L_2'' \leq L_2'$, then total household income would be lower after the ban, so $c'' < c'$. Consequently, the left-hand side of the first-order condition (2) for $k = 2$ would be higher than before the ban, but the right-hand side would be unchanged or lower. This is a contradiction, so we must have $L_2'' > L_2'$.

Now, suppose that the household total labor supply is greater following the ban. That would mean that household income and therefore consumption are also greater, so $c'' > c'$ while $L_2'' > L_2'$. But this would mean that the left-hand side of the first-order condition (2) is lower than before the ban, while the right-hand side is higher. This is a contradiction, so total household labor supply must be lower than before the ban. **Q.E.D.**

Proposition 3. *If the trade agreement is sufficiently low-skilled-labor biassed, meaning if the production-function parameters β^{HX} and β^{LY} are sufficiently small, then the trade agreement, by* *raising p, will increase real low-skilled wages $\frac{w^L}{P}$ and lower high-skilled real wages $\frac{w^H}{P}$, as long as the supply elasticity of low-skilled labor is not too negative. If (3) holds, this will lower the use of child labor.*

If the trade agreement is sufficiently high-skilled-labor biassed, meaning β^{LX} and β^{HY} are sufficiently small, then the trade agreement will have the opposite effect on all counts.

Proof. Consider the extreme case, with $\beta^{HX} = \beta^{LY} = 0$. In that case, low-skilled labor will be employed only in X and high-skilled labor only in Y, and we can write the zero-profit condition for X as:

$$p = c(w^L, r^X), \tag{8}$$

where $c(\cdot,\cdot)$ is the unit-cost function derived from the production function $f^X(\cdot,\cdot,\cdot)$. Taking the total derivative of the zero-profit condition, we obtain:

$$\hat{p} = \theta_{XL}\hat{w}^L + (1-\theta_{XL})\hat{r}^X, \tag{9}$$

where the notation $\hat{z}$ denotes $\frac{1}{z}\frac{dz}{dp}$, and θ_{XL} is the share of low-skilled labor in the cost of production for X. Consequently,

$$\hat{r}^X = \frac{\hat{p} - \theta_{XL}\hat{w}^L}{(1-\theta_{XL})} \tag{10}$$

Given the production function and the inelastic supply of the specific factor, the proportional change in the demand for low-skilled labor is:

$$\sigma^X(\hat{r}^X - \hat{w}^L) = \sigma^X\left(\frac{\hat{p} - \theta_{XL}\hat{w}^L}{(1-\theta_{XL})} - \hat{w}^L\right) = \sigma^X\left(\frac{\hat{p} - \hat{w}^L}{(1-\theta_{XL})}\right) \tag{11}$$

This must be equal to the proportional change in the supply of low-skilled labor. This supply response would be the result of low-skilled households altering their children's labor supply in response to a change in the real wage $\frac{w^L}{P}$, as analyzed in Proposition 1. Let the aggregate elasticity of low-skilled labor supply with respect to the real wage be denoted ϵ. As Proposition 1 indicated, we will have $\epsilon < 0$ if (3) holds and $\epsilon > 0$ otherwise.

Note as well that from Shepard's Lemma, the derivative of the unit expenditure function $\phi(p)$ that determines the consumer price index is equal to the unit consumption demand for X. Multiplying and dividing by p and P shows that the elasticity of $\phi(p)$ is equal to the share of X in consumer expenditure. Denote this share by α. Then the proportional change in the consumer price index is given by $\alpha\hat{p}$, and the proportional change in the low skilled real wage is $\hat{w}^L - \alpha\hat{p}$. Consequently, the equilibrium condition is:

$$\sigma^X \left(\frac{\hat{p} - \hat{w}^L}{(1 - \theta_{XL})} \right) = \epsilon(\hat{w}^L - \alpha\hat{p}). \tag{12}$$

The effect on the low-skill wage is:

$$\hat{w}^L = \left(\frac{\epsilon\alpha + \frac{\sigma^X}{1-\theta_{XL}}}{\epsilon + \frac{\sigma^X}{1-\theta_{XL}}} \right) \hat{p} \tag{13}$$

As long as the denominator of this expression is positive, the coefficient on $\hat{p}$ on the right-hand side can readily be seen to be greater than α. This implies that as along as $\epsilon > -\frac{\sigma^X}{1-\theta_{XL}}$, the change $\hat{w}^L - \alpha\hat{p}$ in the low-skilled real wage is positive. For the high-skilled real wage, the case is simpler: The high-skilled wage in terms of the numeraire, w^H, is not affected by p, so it will remain unchanged as the consumer price index rises. Consequently, its real value falls.

The case with a skill-biassed agreement is analogous. **Q.E.D.**

Proposition 4. *A ban on the labor of children under the age of 14 will result in an increase in the low-skilled wage either if the supply of low-skilled labor is increasing the the real low-skilled wage, or if it is decreasing in the low-skilled wage with an elasticity that is not too large.*

Proof. First, note that the model satisfies all of the conditions necessary for equilibrium to be represented by a revenue function as described in Dixit and Norman (1980, Ch. 2–4). That is to say that for any p, for any given supply of low-skilled labor L, high-skilled labor H, and the fixed factors K^X and K^Y, the equilibrium allocation of labor generates a level of national income that is the given by

$$R(p; L, H, K^X, K^Y) \equiv max_{\{L^X, H^X\}} \left[pf^X(L^X, H^X, K^X) + f^y(L - L^X, H - H^X, K^Y) \right]. \tag{14}$$

Per standard arguments, $w^L = R_1(p; L, H, K^X, K^Y)$, where again subscripts indicate partial derivatives, and R is concave in its last four arguments (Dixit and Norman, 1980, pp. 33–34). Therefore, $R_{11} < 0$, so holding all else constant, adding low-skilled labor to the economy will lower the low-skilled wage. This relationship defines a downward-sloping curve in (L, w^L) space. (One might think of this as defining a demand curve for low-skilled labor, but one must be careful to remember that this is a general-equilibrium relationship: Adding L will change *all* factor prices, not merely the low-skill wage, so it is not a demand curve in the usual partial-equilibrium sense.)

At the same time, as analyzed in Proposition 1, a change in the real low-skilled wage will affect the low-skilled labor supply. This defines a different curve in (L, w^L) space. It will be downward-sloping if (3) holds and upward-sloping otherwise.

An equilibrium must feature a value of L and w^L that are on both of these curves. Now, Proposition 2 shows that banning child labor will lower the supply of low-skilled labor for any given real wage, shifting the supply curve to the left. This will result in an increase in w^L if either the supply curve is upward-sloping (i.e., (3)) or it is downward-sloping (i.e., (3)) and less elastic than the 'demand curve.'

Q.E.D.

Proposition 5. Holding w^L fixed, an increase in the fine t will increase the labor of the older child in each household. The labor supply of the younger child will fall if $u'(c) + \chi c u''(c) > 0$, where $\chi \equiv \frac{w^L A(i) e(a(1,i) L_1}{w^L \Sigma_{k=1,\ldots,\bar{k}(i)} A(i) e(a(k,i)) L_k}$ is the fraction of the household's income derived from the younger child's labor.

Proof. We will analyze the problem for a household i, and in what follows we will omit the i argument. For the younger child, $k = 1$, the total derivative of (2) for a low-skilled household with respect to t is:

$$\begin{aligned} -u''(c)\left(\frac{w^L e_1 A}{P\bar{k}}\right)^2 (1-t)L_1 + u''(c)\left(\frac{w^L e_1 (1-t) A}{P\bar{k}}\right)^2 \frac{dL_1}{dt} \\ + u''(c)\left(\frac{w^L A}{P\bar{k}}\right)^2 e_1 e_2 (1-t)\frac{dL_2}{dt} - u'(c)\frac{w^L e_1 A}{P\bar{k}} \\ = V_{11}(L_1, a_1)\frac{dL_1}{dt}, \end{aligned}$$

where c is household per-capita consumption and e_k and a_k denote $e(a(k,i))$ and $a(k,i)$ respectively. For the older child, $k=2$, the total derivative is:

$$
\begin{aligned}
-u''(c)\left(\frac{w^L A}{P\bar{k}}\right)^2 e_1 e_2 L_1 + u''(c)\left(\frac{w^L A}{P\bar{k}}\right)^2 e_1 e_2 (1-t)\frac{dL_1}{dt} \\
+u''(c)\left(\frac{w^L A}{P\bar{k}}\right)^2 e_2^2 \frac{dL_2}{dt} = V_{11}(L_2, a_2)\frac{dL_2}{dt},
\end{aligned}
$$

These conditions can be written as:

$$
\tilde{\mathbf{M}}\frac{d\mathbf{L}}{dw^L} = \tilde{\mathbf{b}}, \tag{15}
$$

where $\mathbf{L} \equiv (L_1, L_2)'$,

$$
\tilde{\mathbf{M}} = u''(c)\left(\frac{w^L A}{P\bar{k}}\right)^2 \begin{pmatrix} (1-t)^2 e_1^2 & (1-t)e_1 e_2 \\ (1-t)e_1 e_2 & e_2^2 \end{pmatrix} - \begin{pmatrix} V_{11}(L_1, a_1) & 0 \\ 0 & V_{11}(L_2, a_2) \end{pmatrix},
$$

and

$$
\tilde{\mathbf{b}} = \left(\frac{w^L A}{P\bar{k}}\right) \begin{pmatrix} (u''(c)c\chi + u'(c))e_1 \\ (u''(c)\frac{c\chi}{(1-t)})e_2 \end{pmatrix}.
$$

The determinant of $\tilde{\mathbf{M}}$, $\tilde{D}$, is easily checked to be strictly positive. The inverse of $\tilde{\mathbf{M}}$ is:

$$
\tilde{\mathbf{M}}^{-1} = \frac{u''(c)}{\tilde{D}}\left(\frac{w^L A}{P\bar{k}}\right)^2 \begin{pmatrix} e_2^2 & -(1-t)e_1 e_2 \\ -(1-t)e_1 e_2 & (1-t)^2 e_1^2 \end{pmatrix} - \begin{pmatrix} V_{11}(L_2, a_2) & 0 \\ 0 & V_{11}(L_1, a_1) \end{pmatrix},
$$

and so the labor-supply derivatives are:

$$
\begin{aligned}
\tilde{D}\frac{dL_1}{dt} &= u''(c)\left(\frac{w^L A}{P\bar{k}}\right)^3 u'(c) e_1 e_2^2 - \left(\frac{w^L A}{P\bar{k}}\right)[u''(c)c\chi + u'(c)]e_1 V_{11}(L_2, a_2) \\
\tilde{D}\frac{dL_2}{dt} &= -u''(c)\left(\frac{w^L A}{P\bar{k}}\right)^3 u'(c) e_1^2 e_2 (1-t) - \left(\frac{w^L A}{P\bar{k}}\right) u''(c)\frac{c\chi}{(1-t)} e_2 V_{11}(L_1, a_1).
\end{aligned} \tag{16}
$$

Clearly $\frac{dL_2}{dt} > 0$, and a sufficient condition for $\frac{dL_1}{dt} < 0$ is that the expression in square brackets is positive, which is the indicated condition. **Q.E.D.**

Proposition 6. Starting from $t = 0$, a small increase in t will shift the low-skill labor-supply curve to the right if

$$u'(c(i)) + \chi(i)c(i)u''(c(i)) < 0$$

for each low-skilled household i. In this case, the introduction of the fine will lower the low-skill wage; lower the wage more for under-age workers; and increase the total labor supply of low-skill household members other than under-age workers (with the effect on the latter ambiguous).

Proof: The labor-supply curve shifts to the right iff the sum of $e_1 \frac{dL_1}{dt} + e_2 \frac{dL_2}{dt}$ across low-skill households is positive. Applying (16), we obtain:

$$\begin{aligned} e_1 \frac{dL_1}{dt} + e_2 \frac{dL_2}{dt} &= tu''(c) \left(\frac{w^L A}{P\bar{k}} \right)^2 u'(c) e_1^2 e_2^2 \\ -\left(\frac{w^L A}{P\bar{k}} \right) [u''(c)c\chi + u'(c)] e_1^2 V_{11}(L_2, a_2) &- \left(\frac{w^L A}{P\bar{k}} \right) u''(c) \frac{c\chi}{(1-t)} e_2^2 V_{11}(L_1, a_1). \end{aligned}$$

With $t = 0$, the indicated condition ensures that this is positive. If it is positive for each household, the sum across households will be positive. **Q.E.D.**

B Appendix—Additional Tables and Figures

Table A.1: List of Regional Trade Agreements with Child Labor Provisions

Agreement	Entry into force	Binding?
EU – Overseas Countries and Territories (OCT)	1971	Yes
Canada - Chile	1997	Yes
United States - Jordan	2001	No
Canada - Costa Rica	2002	Yes
United States - Chile	2004	Yes
United States - Singapore	2004	Yes
United States - Australia	2005	Yes
Dom Rep - Central America - US Free Trade Agreement (CAFTA-DR)	2006	Yes
United States - Bahrain	2006	Yes
United States - Morocco	2006	Yes
EU - Montenegro	2008	Yes
Nicaragua - Chinese Taipei	2008	Yes
EU - CARIFORUM States EPA	2008	Yes
Japan - Philippines	2008	No
Peru - Chile	2009	No
Canada - Peru	2009	Yes
United States - Oman	2009	Yes
Chile - Colombia	2009	No
United States - Peru	2009	Yes
Canada - Colombia	2011	Yes
EU - Korea, Republic of	2011	Yes
United States - Colombia	2012	Yes
EFTA - Hong Kong, China	2012	No
EFTA - Montenegro	2012	No
Canada - Jordan	2012	Yes
United States - Panama	2012	Yes
Korea, Republic of - United States	2012	Yes
EU - Central America	2013	No
Canada - Panama	2013	Yes
EU - Colombia and Peru	2013	Yes
New Zealand - Chinese Taipei	2013	Yes
Korea, Republic of - Turkey	2013	Yes
EU - Ukraine	2014	Yes
Canada - Honduras	2014	Yes
EFTA - Central America (Costa Rica and Panama)	2014	No
Korea, Republic of - Australia	2014	No
EU - Georgia	2014	Yes
EU - Moldova, Republic of	2014	Yes
Korea, Republic of - New Zealand	2015	No
EFTA - Bosnia and Herzegovina	2015	No
Canada - Korea, Republic of	2015	Yes
Korea, Republic of - Colombia	2016	No
Comp and Prog Agreement for Trans-Pacific Partnership (CPTPP)	2017	Yes

Table A.2: Child Labor Provisions and Child Wage Premia in Developing Countries

	Dependent variable:						
	<14 Wage Premium						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post RTA	0.025		0.023*	0.021		0.015	
	(0.016)		(0.012)	(0.015)		(0.011)	
Post × Child	0.006	0.004	−0.004	0.008	0.007	−0.001	0.021
	(0.027)	(0.027)	(0.028)	(0.024)	(0.025)	(0.025)	(0.023)
Observations	1,193	1,193	1,193	1,288	1,288	1,288	1,288
R^2	0.838	0.839	0.851	0.844	0.844	0.855	0.855
Mean	0.43	0.43	0.43	0.43	0.43	0.43	0.43
	Dependent variable:						
	14–17 Wage Premium						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post RTA	0.010		−0.014	0.010		−0.025	
	(0.011)		(0.017)	(0.011)		(0.018)	
Post × Child	0.057	0.056	0.055	0.086	0.085	0.071	0.064*
	(0.048)	(0.047)	(0.037)	(0.054)	(0.054)	(0.045)	(0.038)
Observations	2,167	2,167	2,167	2,439	2,439	2,439	2,439
R^2	0.752	0.754	0.760	0.751	0.752	0.759	0.759
Mean	0.55	0.55	0.55	0.55	0.55	0.55	0.55
Panel	unbal	unbal	unbal	interp	interp	interp	interp
Country × RTA FE	✓	✓	✓	✓	✓	✓	✓
Event time FE		✓			✓		✓
Year FE			✓			✓	
Synth DiD							✓

FE regressions on a stacked country-level panel with a ±3 year event window around RTA entry into force. Child is an indicator for provisions prohibiting child labor. All models include country-RTA fixed effects. Regressions with event time FE are equivalent to two-way FE difference-in-difference models while regressions with only country-RTA FE and with country-RTA and year FE are equivalent to one-way and two-way triple-difference models, respectively. Unbalanced panels are reported in columns (1)–(3) while columns (4)–(7) use backwards constant interpolation to balance the panel. Column (7) estimates a synthetic two-way FE difference-in-difference model (Arkhangelsky *et al.*, 2021) on the balanced data. Robust standard errors are two-way clustered at the country-RTA and country-year levels. Statistical significance from two-sided t tests are denoted by *p<0.1; **p<0.05; ***p<0.01.

Table A.3: Child Labor Provisions and Skill Premium in Developing Countries

	Dependent variable:						
	High/low Skill Wages						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post RTA	−0.121		0.143	−10.481		0.243	
	(0.314)		(0.451)	(8.075)		(4.462)	
Post × Child	0.011	0.008	−0.344	−4.149	−3.701	7.154	0.157
	(0.316)	(0.321)	(0.386)	(14.803)	(14.693)	(16.163)	(11.839)
Observations	1,153	1,153	1,153	1,759	1,759	1,759	1,759
R^2	0.585	0.587	0.699	0.395	0.396	0.429	0.429
Mean	3.03	3.03	3.03	3.03	3.03	3.03	3.03
Panel	unbal	unbal	unbal	interp	interp	interp	interp
Country × RTA FE	✓	✓	✓	✓	✓	✓	✓
Event time FE		✓			✓		✓
Year FE			✓			✓	
Synth DiD							✓

FE regressions on a stacked country-level panel with a ±3 year event window around RTA entry into force. Child is an indicator for provisions prohibiting child labor. All models include country-RTA fixed effects. Regressions with event time FE are equivalent to two-way FE difference-in-difference models while regressions with only country-RTA FE and with country-RTA and year FE are equivalent to one-way and two-way triple-difference models, respectively. Unbalanced panels are reported in columns (1)–(3) while columns (4)–(7) use backwards constant interpolation to balance the panel. Column (7) estimates a synthetic two-way FE difference-in-difference model (Arkhangelsky *et al.*, 2021) on the balanced data. Robust standard errors are two-way clustered at the country-RTA and country-year levels. Statistical significance from two-sided t tests are denoted by *p<0.1; **p<0.05; ***p<0.01.

Table A.4: Enforceable of Child Labor Provisions and Child Employment Rates in Developing Countries

	Dependent variable:					
	<14 Employment Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Post RTA	−0.0002		−0.002	−0.0002		−0.002
	(0.003)		(0.002)	(0.003)		(0.002)
Post × Child	0.006	0.006	0.010*	0.006	0.006	0.010*
	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.005)
Post × Child× Enforce	−0.007	−0.007	−0.008	−0.007	−0.007	−0.008
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	2,182	2,182	2,182	2,186	2,186	2,186
R^2	0.794	0.794	0.806	0.794	0.794	0.806
Mean	0.02	0.02	0.02	0.02	0.02	0.02
	Dependent variable:					
	14–17 Employment Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Post RTA	−0.013***		−0.003	−0.013***		−0.003
	(0.003)		(0.002)	(0.003)		(0.002)
Post × Child	0.017***	0.017***	0.018***	0.017***	0.017***	0.018***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Post × Child × Enforce	−0.006	−0.006	−0.007	−0.006	−0.006	−0.007
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	2,779	2,779	2,779	2,783	2,783	2,783
R^2	0.971	0.971	0.972	0.971	0.971	0.972
Mean	0.15	0.15	0.15	0.15	0.15	0.15
Panel	unbal	unbal	unbal	interp	interp	interp
Country × RTA FE	✓	✓	✓	✓	✓	✓
Event time FE		✓			✓	
Year FE			✓			✓

FE regressions on a stacked country-level panel with a ±3 year event window around RTA entry into force. Child is an indicator for provisions prohibiting child labor. All models include country-RTA fixed effects. Regressions with event time FE are equivalent to two-way FE difference-in-difference models while regressions with only country-RTA FE and with country-RTA and year FE are equivalent to one-way and two-way triple-difference models, respectively. Unbalanced panels are reported in columns (1)–(3) while columns (4)–(7) use backwards constant interpolation to balance the panel. Column (7) estimates a synthetic two-way FE difference-in-difference model (Arkhangelsky *et al.*, 2021) on the balanced data. Robust standard errors are two-way clustered at the country-RTA and country-year levels. Statistical significance from two-sided t tests are denoted by *p<0.1; **p<0.05; ***p<0.01.

Table A.5: Enforceable Child Labor Provisions and School Enrollment Rates in Developing Countries

	Dependent variable:					
	Primary School Enrollment Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Post RTA	0.001		0.001	0.003		0.002
	(0.002)		(0.002)	(0.002)		(0.002)
Post × Child	−0.017**	−0.017**	−0.009	−0.015*	−0.014*	−0.005
	(0.008)	(0.008)	(0.006)	(0.008)	(0.008)	(0.007)
Post × Child × Enforce	0.007	0.007	0.006	0.003	0.003	0.002
	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)
Observations	2,317	2,317	2,317	2,512	2,512	2,512
R^2	0.965	0.965	0.968	0.963	0.964	0.967
Mean	0.92	0.92	0.92	0.92	0.92	0.92

	Dependent variable:					
	Secondary School Enrollment Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Post RTA	0.023***		0.001	0.021***		0.001
	(0.003)		(0.002)	(0.002)		(0.002)
Post × Child	−0.001	−0.002	0.001	0.001	0.001	0.004
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)
Post × Child × Enforce	−0.009	−0.009	−0.008	−0.009	−0.009	−0.008
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	1,812	1,812	1,812	2,080	2,080	2,080
R^2	0.992	0.993	0.993	0.992	0.993	0.993
Mean	0.74	0.74	0.74	0.74	0.74	0.74
Panel	unbal	unbal	unbal	interp	interp	interp
Country × RTA FE	✓	✓	✓	✓	✓	✓
Event time FE		✓			✓	
Year FE			✓			✓

FE regressions on a stacked country-level panel with a ±3 year event window around RTA entry into force. Child is an indicator for provisions prohibiting child labor. All models include country-RTA fixed effects. Regressions with event time FE are equivalent to two-way FE difference-in-difference models while regressions with only country-RTA FE and with country-RTA and year FE are equivalent to one-way and two-way triple-difference models, respectively. Unbalanced panels are reported in columns (1)–(3) while columns (4)–(7) use backwards constant interpolation to balance the panel. Column (7) estimates a synthetic two-way FE difference-in-difference model (Arkhangelsky *et al.*, 2021) on the balanced data. Robust standard errors are two-way clustered at the country-RTA and country-year levels. Statistical significance from two-sided t tests are denoted by *p<0.1; **p<0.05; ***p<0.01.

Table A.6: Enforceable Child Labor Provisions and Household Equality in Developing Countries

	Dependent variable:					
	Income Equality of Households with *any* Children					
	(1)	(2)	(3)	(4)	(5)	(6)
Post RTA	−0.006		0.004	−0.006		0.002
	(0.006)		(0.005)	(0.006)		(0.004)
Post × Child	0.014	0.014	0.009	0.015	0.015	0.011
	(0.010)	(0.010)	(0.008)	(0.009)	(0.009)	(0.008)
Post× Child × Enforce	−0.007	−0.007	−0.012	−0.008	−0.008	−0.012
	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Observations	1,777	1,777	1,777	1,964	1,964	1,964
R^2	0.882	0.882	0.903	0.893	0.893	0.910
Mean	0.81	0.81	0.81	0.81	0.81	0.81

	Dependent variable:					
	Income Equality of Households with both <14 and 14–17 Children					
	(1)	(2)	(3)	(4)	(5)	(6)
Post RTA	−0.018***		0.004	−0.019***		0.002
	(0.005)		(0.005)	(0.006)		(0.004)
Post × Child	0.010	0.011	0.007	0.011	0.011	0.009
	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)	(0.011)
Post × Child × Enforce	0.006	0.006	−0.003	0.005	0.004	−0.004
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)
Observations	1,755	1,755	1,755	1,942	1,942	1,942
R^2	0.914	0.916	0.932	0.922	0.923	0.936
Mean	0.70	0.70	0.70	0.70	0.70	0.70
Panel	unbal	unbal	unbal	interp	interp	interp
Country × RTA FE	✓	✓	✓	✓	✓	✓
Event time FE		✓			✓	
Year FE			✓			✓

FE regressions on a stacked country-level panel with a ±3 year event window around RTA entry into force. The dependent variable is the weighted median of household per capita income from from households with under 18 children—weighted by the share of the household that is under 18—divided by the median household per capita income of the population. In the top panel, we consider households with any under 18 children. In the bottom panel we focus on household that have both young children (<14) and older children (14–17). Child is an indicator for provisions prohibiting child labor. All models include country-RTA fixed effects. Regressions with event time FE are equivalent to two-way FE difference-in-difference models while regressions with only country-RTA FE and with country-RTA and year FE are equivalent to one-way and two-way triple-difference models, respectively. Unbalanced panels are reported in columns (1)–(3) while columns (4)–(7) use backwards constant interpolation to balance the panel. Column (7) estimates a synthetic two-way FE difference-in-difference model (Arkhangelsky *et al.*, 2021) on the balanced data. Robust standard errors are two-way clustered at the country-RTA and country-year levels. Statistical significance from two-sided t tests are denoted by *p<0.1; **p<0.05; ***p<0.01.