PERVERSE CONSEQUENCES OF WELL-INTENTIONED REGULATION: EVIDENCE FROM INDIA'S CHILD LABOR BAN

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ABSTRACT. While bans against child labor are a common policy tool, there is very little empirical evidence validating their effectiveness. Most of the existing literature evaluating the impact of child labor bans has been theoretical. In this paper we examine empirically the consequences of India's landmark legislation against child labor, the Child Labor (Prohibition and Regulation) Act of 1986. Using data from employment surveys conducted before and after the ban, and using age restrictions that determined whom the ban applied to, we show that child wages decrease and child labor *increases* after the ban. The increase appears to come mainly from families where the head is less educated, suggesting poverty as a key determinant of why families use child labor. These results are consistent with a theoretical model building on the seminal work of Basu and Van (1998) and Basu (2005), where families use child labor to reach subsistence constraints and where child wages decrease in response to bans, leading poor families to utilize more child labor.

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1. Introduction

Legal interventions have been suggested as the predominant way by which societies have attempted to bring about equality and justice. Bans against discrimination in hiring decisions, child marriage, segregation of schools on the basis of race, etc. are prime examples of legal action taken in the hopes of improving overall welfare and bringing about equality of opportunity. While legal interventions have been undoubtedly useful in many situations, the idea that well intentioned laws can have perverse or self defeating consequences is a central concern in the economic analysis of laws and regulations (Sunstein 1994). This idea is crucial when evaluating the consequences of legal action taken against a controversial yet pervasive aspect of developing societies: child labor.

Despite a lengthy history of advocacy against it and bans designed to mitigate it, child labor continues to be a major issue. According to a recent report by the International Labor Organization, there are nearly 168 million child laborers, out of which 85 million are reported to work under hazardous conditions (International Labour Organization (2013)). As might be expected, almost all of these child laborers are in the developing world. Such evidence leads to increased media and academic scrutiny of bans against child labor, and advocates against child labor often demand stricter and stricter regulations.[1] The impact of such laws on child labor are theoretically ambiguous. Basu and Van (1998) suggest that child labor bans could decrease the incidence of child labor by a resulting rise in adult wages; however, in subsequent papers, Basu (1999, 2005) emphasizes that such bans could also have perverse consequences by *increasing* child labor if bans result in lowering child wages. In a single sector model, Basu (2005) illustrates that bans can decrease child wages if employers now face expensive fines for hiring child labor. Among poor families that already use child labor to fulfill consumption needs, a decrease in wages could result in a corresponding increase in child labor.

Given the sharp debates in the academic and policy spheres and the theoretical ambiguities, what do we know about the impacts of such bans?[2] In a comprehensive review, Edmonds (2007)

[1]Such laws are not uncommon. In a detailed report published by the US Department of Labor's Bureau of International Labor Affairs, such fines and regulations are found in countries like Egypt, Kenya, Nicaragua, Mexico and Thailand (US Department of Labor 1998).

[2]While there is a wealth of empirical and theoretical work examining the determinants (see excellent reviews by Basu (1999), Edmonds and Pavcnik (2005), Edmonds (2007)) and consequences (see for example Beegle, Dehejia and Gatti

concludes, "...despite all this policy discussion, there does not appear to be any study of the effectiveness of restrictions on work that would meet current standards of evidence." (pg. 66) This paper sets out to fill this critical gap in the literature by empirically examining the impacts of India's flagship legislation against child labor, the Child Labor (Prohibition and Regulation) Act of 1986. Most recent articles in the press cite this law as essentially the starting point for legal action against child labor in India.[3]

To tie the existing theory closely with the Act of 1986, we first illustrate in a simple model that a ban on child labor may lead to an increase in child labor. As in Basu (2005) the intuition is that an imperfectly enforced ban lowers child wages, which forces families reliant on child labor income to increase levels of child labor to reach subsistence. We then extend the intuition of this model to allow for the existence of multiple sectors where the ban is more strictly enforced in only one sector (manufacturing as opposed to agriculture, as was the case in the 1986 law). This extension gives rise to three possible cases, depending on the level of labor market frictions. The main conclusion from this extension is that as long as there are labor market frictions that prevent free movement of labor from one sector to another, a ban may increase in child labor in both sectors. If there are no labor market frictions across sectors, a child labor ban in one sector results in a reallocation of child and adult labor between sectors but has no effect on overall levels of child labor.

Using nationally representative data from India, we find that child labor increases after the ban. The empirical method in this paper is a difference in differences model using detailed data on employment from the 1983, 1987 and 1993 rounds of the National Sample Survey, which were integrated by IPUMS International. We classify the 1983 round as the "pre-ban" period and

(2009)) of child labor there is extremely little *empirical* evidence on the effectiveness of child labor bans. While other policies directly intending to affect child labor (such as cash transfers, see Skoufias et al. (2001) among many others), and policies not directly intending to but having spillovers into affecting child labor (such as trade liberalization, see Edmonds and Pavcnik (2005b)) have been evaluated, empirical work on the impacts of governmental bans on child labor are essentially non-existent. The set of papers that are perhaps most closely connected are the papers that examine the role of child labor laws in reducing child labor participation in the historical United States. In an important article, Moehling (1999) finds that child labor laws barely contributed to the decline in child labor between 1890 and 1910, though Lleras-Muney (2002) shows that compulsory schooling and minimum working age laws did increase educational attainment between 1915 and 1935. Manacorda (2006) finds that the legal working age eligibility affects both own employment status and siblings' employment and schooling in 1920 America.

[3]A detailed description of the law is presented in Section 2 of this paper and in an online Appendix.

the 1987 and 1993 rounds as the "post-ban" period.[4] To account for other factors that may have affected wages and employment over time, we compare the changes in employment and wages of children below 14 to the changes of those above 14, before and after the law came into effect in 1986 since the Child Labor Act applied only to those under age 14. To tie the empirical work closely to the theoretical model where the only increases in child labor come from the extensive margin, we examine how the employment of children (those below the age of 14) changes when their *sibling* is under or over the age of 14. The idea is that if the child labor ban applies to children under the age of 14, depressing their wages (and if adult labor is always supplied inelastically), the siblings of affected children are pushed into work.[5]

Our results are in line with the extension of the baseline model to a case with limited mobility across both sectors. We find that child wages decrease *more* than adult wages as a result of the ban, and that child labor increases in both sectors, although the increases are larger in the agricultural sector. We find that a child between the ages of 10-13 and with a sibling who is below the age of 14, increases labor force participation by 0.8 percentage points after the ban, which is approximately 5.6% over the pre-ban average participation for that age group. When we examine children between the ages of 10-13 who have an under 14 sibling working in manufacturing (the sector with the most stringent bans), we see an increase in child labor participation of 4.6 percentage points, which is nearly 32% over the pre-ban mean. These effects are statistically significant.

We see no increases in labor coming from older age groups (those aged 25 and over for example), in line with an important aspect of the model that adults likely supply labor inelastically. Examining the gender composition in the child labor response increase, we find that girls increase

[4]As section 2 will reveal, it is not as though before this ban in 1986 child labor was legal in India. However, we do not need this to be the case to employ our empirical methodology. The main point is that this legislation *tightened* the rules on child labor overall and brought uniformity and awareness to the code. A related concern stems from recent work by Edmonds and Shrestha (2012). They document that minimum age regulations are rarely enforced across a broad set of developing countries by showing a lack of discontinuity in labor force participation around the minimum age. Similarly, Boockmann (2010) finds little evidence that ILO minimum age conventions increase school attendance. Our paper's findings are not inconsistent with these in the sense that we show that such laws can lead to more labor force participation and less schooling among those below the minimum legal working age, which would obfuscate the finding of such a discontinuity.

[5]While it would be interesting to examine responses along the intensive margin, the data do not contain any information on hours worked. While we use the term "siblings", the data only allows us to identify children in the same household. We note this strategy is similar to the one in Manacorda (2006), but different in the sense that we use siblings to define treatment status rather than explicitly measure the impact of one sibling's labor supply on another. To be precise, our estimating strategy is similar to what Manacorda calls the "reduced form" in his 2006 paper.

their labor supply more than boys. This is likely if young boys are already working and if young girls are only brought into the workforce if wages decrease.[6] We also find that 10-13 year old children are less likely to be in school after the ban; hence, the increases in child labor likely come at an important cost of decreased investments in human capital. A key aspect of the theory is grounded in the idea that only the very poor supply child labor since in general, households would prefer to not make their children work. Empirically, we use education of the household head as a proxy for income and find that most of the child labor increases in our case appears to come from households where the head is less educated.[7]

Importantly, other labor laws that would be pertinent to our case did not have age specific restrictions and were passed before 1983.[8] We account for all other temporal and spatial differences in laws and other factors by using survey-year and state fixed effects, and our results are also robust to controlling for state by year fixed effects. Ultimately, any other factor that would be confounding in our case would have to be one that *differentially* affects those under 14 and was implemented between 1983-1994 at the national level as our identification is built on the difference in changes in employment and wages for those above and below age 14.

Our work highlights the importance of careful economic analysis of laws in a context where there could be multiple market failures. There exists a rich tradition of research at the intersection of law and economics in developed countries (Commons 1924, Stigler 1992); however, there is considerably less empirical work in the developing country setting. One of the major factors inhibiting this research is the weak institutional setting typical of developing countries, leading to partial implementation of laws. This paper's analysis is broadly applicable to other

[6]Bhalotra (2007) shows that boys are more likely to work in response to poverty than girls in rural Pakistan. Our evidence is not contradictory if we believe boys are already working due to poverty and this law only makes matters worse for families, pushing girls into work as well. A similar logic is applied to the results is found in Thirumurthy, Graff Zivin and Goldstein (2007).

[7]We unfortunately cannot observe land or asset holdings in our data, and hence resort to using education of the household head as a proxy. Using data from the National Family Health Survey in India (1992 round) we find that education and asset holdings are highly correlated. Those with secondary school education score nearly a standard deviation more on an “asset index" compared to people without secondary school education.

[8]The Bonded Labour System (abolition) Act was passed in 1976, the Contract Labour (regulation and abolition) Act in 1970 and the Inter-State Migrant Workmen's Act in 1979. Using Besley and Burgess's (2004) state level changes to the Industrial Disputes Act of 1947 that classify states as pro-worker, pro-employer or neutral, we find that only 3 out of 16 states in their sample changes classification between 1983-1994.

developing countries as our results do not hinge on complete enforcement, but rather, some of the perversity is highlighted as a result of incomplete enforcement.

Finally, our paper speaks to the broader debate on rights based activism in India (The Economist 2010). Current policy making in India involves legally guaranteeing certain “rights" such as the right to employment (NREGA) or the right to education. While such policies could have beneficial effects, understanding how such rights interact with the broader context of corruption, poverty or other behavioral responses of the household is crucial. Specific to our context, in February 2013, a bill was introduced to the Parliament of India, which called for complete abolishment of child labor of any form. Among other provisions, the proposed bill also calls for increased monitoring and punishment for violators of such laws. The results of this paper cautions against such policy making in the presence of broader institutional and market failures.

2. The Child Labor (Prohibition and Regulation) Act of 1986

The impetus for the 1986 law[9] came from various reports from Government committees that suggested weak implementation of prior laws against child labor (see descriptions of these committee reports, the Sanat Mehta Committee of 1986, and the Gurupadaswamy Committee on Child Labor of 1979, in Ramanathan (2009); more details are provided in an online Appendix as well). Hence, the major draw of the 1986 law was uniformity of the minimum age restriction (people up to age 14 were defined as children and therefore ineligible to work in certain industries and occupations). The law provides a list of occupations where children below the age of 14 are prohibited from working (subsequent additions to this list were made at various points between 1989-2008).

The main occupations which were banned from hiring child labor after 1986 and before 1993 (the periods of data we examine) were occupations that involved transport of passengers, catering establishments at railway stations, ports, foundries, handling of toxic or inflammable substances, handloom or power loom industry and mines among many others. The list of “processes” that are banned for children are perhaps more exhaustive, including beedi (hand rolled cigarette)

[9]The entire Act of 1986 is available easily online and also from the authors.

making, manufacturing of various kinds (matches, explosives, shellac, soap etc), construction, automobile repairs, production of garments etc.[10] The *major* caveat to these bans was that children were permitted to work in family run businesses and agriculture is not included as a sector that is banned from hiring child labor.

However, despite these two important caveats, the 1986 law places various regulations on how many hours and when children can work, regardless of industry/process (as long as they were not explicitly banned from working in such industries). For example, Section III of the law states that for every three hours of work, a child would get an hour of rest; no child shall work between 8pm and 7am; and no child shall be permitted or required to work over time. These laws apply to all industries and sectors as long as the operation is not family run.

Important for our purposes, the law states what the penalties would be if firms banned from hiring children were caught doing so: "(1) Whoever employs any child or permits any child to work in contravention of the provisions of section 3 [section detailing banned occupations] shall be punishable with imprisonment for a term which shall not be less than three months but which may extend to one year or with fine which shall not be less than ten thousand rupees but which may extend to twenty thousand rupees or with both. (2) Whoever, having been convicted of an offense under section 3, commits a like offense afterwards, he shall be punishable with imprisonment for a term which shall not be less than six months but which may extend to two years." Smaller fines are levied for failing to comply with some of the provisions that regulate the conditions under which children can work in approved occupations.

While enforcement of the 1986 law has been largely weak, it does appear that employers were aware of this law. According to a report by Human Rights Watch, it seems that employers found loopholes to work around the specifics of the law. For example, the report (Human Rights Watch, 2003) provides anecdotal evidence on factories contracting with adults to take their work

[10]While these provisions came into effect immediately after the law was passed, a section of the law allowed for state specific introduction of regulations for child labor in sectors that were not explicitly banned. In this paper we concern ourselves only with the impact of the centrally enacted law. Using state level variation in implementing the regulations component of the law is left for another time as this analysis will be complicated by which states select into adopting these regulations earlier etc. We think that if the state level component was the most critical component of the law, then our current estimates are biased downwards.

home and employ their children on it since work at home was allowed under the law (see the online Appendix for details). Similar anecdotal evidence can be found in a Times of India article from 1994: “Employers neutralise the statutory ban on child labour by not showing them on the pay-roll. . . . The local central excise staff of an inspection of home workers employed by a leading beedi company found that the output of a woman worker at Thatchanallur village was recorded in a passbook issued in the name of her husband. In the same village, Pitchammal and her daughter, Prema, had a passbook carrying the name of the Naina Moopanar, who had died years ago." (‘Appalling plight of TN beedi workers’, Times of India, August 6th, 1995; pg.7)

While hard data on prosecutions regarding child labor is difficult to come by, a Human Rights Watch report stated, “At the national level, from 1990 to 1993, 537 inspections were carried out under the Child Labour (Prohibition and Regulation) Act. These inspections turned up 1,203 violations. Inexplicably, only seven prosecutions were launched. At the state level, the years 1990 to 1993 produced 60,717 inspections in which 5,060 violations of the act were detected; 772 of these 5,060 violations resulted in convictions.” (Human Rights Watch, 1996) While overall enforcement might have been weak, it seems entirely plausible that employers were more aware of the possibility of inspections and the consequent fines after the passage of the 1986 Act. In January 1987 in Ferozabad, (an important town at the time for bangle manufacturing in the state of Uttar Pradesh) there were arrests of employers who were found to be in violation of the law which made national news. This incident was heralded as the "beginning that has to be made somewhere in ending child labour" (Times of India, January 17, 1987; pg.18). Hence, we believe that the Act raised the level of inspections and awareness of the law as the government put renewed effort into enforcing the Act.

3. MODEL

In this section, we describe a basic model to illustrate the potential effects of a ban on child labor in several settings. The main setup of the model builds on the general equilibrium framework established in Basu (2005) and Basu and Van (1998). We begin with the baseline case in which there is one sector and describe the main predictions of the model when a ban on child labor is enforced, namely that a ban could lead to lower wages and *increased* levels of child labor. We then extend the intuition of this model to a setting with multiple sectors and with labor market frictions across sectors. We encourage the reader familiar with Basu (2005) and Basu and Van (1998) to directly go to section 3.2. We use the same notation as in Basu (2005) and Basu and Van (1998) to preserve continuity with previous work.

3.1. **Effects of a child labor ban with one sector**

Consider a labor market in which there are two types of labor, adult and child. There is a representative firm with technology $Y = f(L)$ where L represents effective units of labor. Child and adult labor are perfect substitutes in production up to a constant, γ; each unit of adult labor is equal to 1 unit of effective labor ($L^A = L$) and each unit of child labor is worth γ units of effective labor ($L^C = \gamma L$), where $\gamma < 1$.[11] The price of output is normalized to 1. Firms take prices as given; wages are w^A and w^C for adults and children, respectively. Now introduce an imperfectly enforced ban on child labor; firms found employing child labor are fined an amount D and firms are audited with probability p.[12] The firm maximization problem is as follows[13]:

$$\max_{L} f\left(L\right) - C(L; w^A, w^C, p, D) \tag{1}$$

[11]Existing empirical evidence suggests that employers treat child and adult labor as substitutes. See for example Doran (2013).

[12](i) A more general specification of the ban allows the probability of detection to vary non-linearly with the level of child labor, i.e. where $p = p\left(L\right)$. Since firms are more likely to be detected the more children they hire, $p\left(L\right)$ is increasing in the amount of child labor employed. Here we assume a very simple linear form of $p\left(L\right)$, i.e. $p\left(L\right) = pL$, where p is a constant. When p is large, a linear function may not be a suitable approximation for $p\left(L\right)$ as $p\left(L\right)$ may exceed 1 when both p and L are large. However, as discussed in the previous section, enforcement of the ban was perceived to be quite weak and thus p was likely to be very low. In this case, a linear specification as an approximation of $p\left(L\right)$ is more justifiable, as there is less concern that $p\left(L\right) > 1$.
(ii) Note that this definition of imperfect enforcement is as in Basu (2005) and differs from that used in Basu and Van (1998), which specifies that the ban is perfectly enforced for a proportion of firms while the remainder of firms are unregulated. While most of the intuition is similar with this alternate definition of enforcement, the perfect enforcement assumption does change the predictions of the model. Most importantly, depending on size of labor demand from the perfectly enforced firms relative to the supply of adult labor, N, there are cases in which a ban on child labor could increase adult wages and possibly decrease child labor. However, we model the imperfect enforcement as in the Basu (2005) model because we believe that this is more applicable to the way in which the *actual* 1986 ban was enforced and therefore is the most relevant for our empirical work.

[13]To see the full firm maximization problem and the derivation of labor demand and supply curves as well as labor market equilibria, please see the Online Appendix.

where $L = L^A = L^C/\gamma$ and $C(\cdot)$ characterizes total cost of *effective* labor. Since adult and child labor are prefect substitutes (after accounting for γ), firms are indifferent between hiring adults and children as long as the marginal cost of labor is the same for both (i.e. when $w^C = \gamma w^A - pD$) and care only about satisfying total labor demand as implicitly defined by the first order condition $f'(L^*) = w^*$ where w represents the "total" marginal cost of labor (including any associated expected costs of the fine if the marginal unit of labor is child labor).

There are N families, each endowed with 1 (nondivisible) unit of adult labor which they supply inelastically and m children who are also endowed with 1 unit of labor each. Households value their children's leisure (and/or schooling) and therefore supply child labor only in the case in which adult wages is not sufficient to reach subsistence consumption, s. When they do supply child labor, they do so only to the extent necessary to bridge the gap between subsistence consumption and income from adult work.[14]

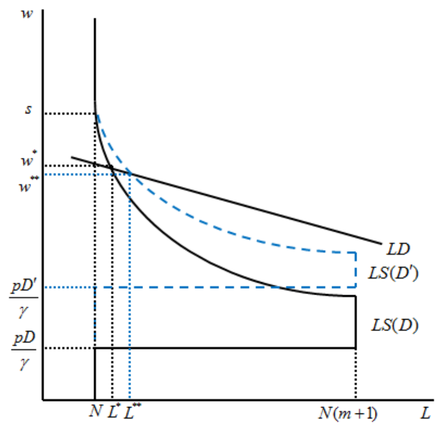
We will assume that the output market is always in equilibrium where the equilibrium price of output is normalized to 1. In this economy, we define equilibrium as a wage pair (w^{A*}, w^{C*}) such that (i) the adult labor market is in equilibrium; (ii) the child labor market is in equilibrium; and (iii) there are no arbitrage opportunities, i.e. the effective wages of each type of labor are equal (net of the expected fine) $w^{A*} = \frac{w^{C*}+pD}{\gamma} = w^*$.[15] We limit our attention to the case in which we start from a single equilibrium, i.e. one in which total labor demand is sufficiently elastic such that the demand curve crosses the supply curve only once, where total labor demand exceeds the supply of adult labor and therefore at least some children are hired.[16] Since the intention of this

[14]This framework is used to keep the analysis simple and tractable but the behavior described in the model can be deduced from more general models of optimizing households; the results are based only on two essential assumptions: (1) child and adult labor are to some extent substitutable and (2) all else equal, households prefer not to work their children if income is high enough. For example, see Basu and Van (1998) for a model in which children are less productive than adults but also consume less than adults, time is divisible so that children choose labor from a continuum of possibilities and household preferences are represented by a Stone-Geary utility function.

[15]In the Online Appendix, we consider additional equilibria that are ruled out with this definition. For example, we consider the effects of a ban on a potential class of equilibria where adult wages are lower than effective child wages ($w^C > \gamma w^A - pD$) but the demand for total labor exceeds the fixed supply of adult labor. However, since the results are qualitatively very similar, we do not present those results here.

[16]As in earlier work, this framework allows for multiple equilibria, where an economy can be in either a good equilibrium in which no children work (where $w^A < \frac{w^C+pD}{\gamma}$ and $N > L^*$ and aggregate firm demand is satisfied by aggregate adult labor supply) or a bad one in which children are forced to work (a possibility raised by many previous works such as Basu and Van (1998), Swinnerton and Rogers (1999), and Jafarey and Lahiri (2002)). It is worth noting

FIGURE 1. General equilibrium representation of the effect of an increase in fines on adult and child labor markets.



model is to study the effect of a ban on child labor, we are less concerned with equilibria in which there is no child labor to begin with.

Under this definition of equilibrium where wages are equated in effective terms $w^{A*} = \frac{w^{C*}+pD}{\gamma} = w^*$, we can represent general equilibrium in this model in a single graph as in Figure 1. Here, the vertical axis represents the wage/marginal cost of an additional unit of labor (net of any expected fines), w, which is the same for both children and adults in effective terms. The horizontal axis represents aggregate effective units of labor. Demand for total labor is smooth and is determined only by the firm's first order condition, $f'(L*) = w^*$, though the composition of employed labor can be any split of child and adult labor (the firm is indifferent between any mix). Total household labor supply includes an inelastic portion for adult labor (up to N) and then a downward-sloping portion when child labor is supplied ($L > N$). Here, individual household supply is given by

$$S^C(w) = \begin{cases} 0 & \text{if } w \geq s \text{ or } \gamma w - pD \leq 0 \\ \min\{m, \frac{s-w}{\gamma w - pD}\} & \text{otherwise} \end{cases} \quad (2)$$

where we have already restricted ourselves to the case where $w^{C*} = \frac{w^{A*}-pD}{\gamma} = \frac{w^*-pD}{\gamma}$. The intuition is that for any given wage $w < s$, as wages increase, fewer children need to work in

that when multiple equilibria exist and an economy is in the "bad" equilibrium, a *perfectly* enforced ban on child labor can jolt the economy to the "good" equilibrium, making households better off (see Basu and Van (1998) for details.)

order to reach subsistence consumption. The reverse is true as w falls; when wages are lower the household needs to supply more children to the labor market in order to be able to achieve subsistence. Since households have only a limited number of children, at the aggregate level labor supply reaches a maximum at $N(m+1)$. Note that it is possible for effective wages to be low enough that the child wage is 0 or negative. In this case, households do not supply any child labor and so the supply curve reverts to only the inelastic supply of adult labor, N, for any w that implies a negative child wage.

Consider the effects of increasing the fine levied on firms employing child labor from some initial level, D, to a new higher level D'; we can even think of the case in which D=0 and $D' > 0$; from this point forth, we often refer to the period when the fine is D as the pre-ban period and the period when the fine is D' as the post-ban period. What is the effect of increased fines? Initially the increase in fines has no effect on total labor demand because child wages fall to offset the increase in fines, leaving the marginal cost of an effective unit of labor unchanged. This is because of the perfect substitutability between child and adult labor; since firms are indifferent between hiring adults and children, children must bear the full burden of the increase in expected fines. However the household supply curve shifts outward to $LS(D')$ as shown in Figure 1 because at every given w children now earn less and household income is lower so more children must work to achieve subsistence.[17] As more children flow into the market, this puts downward pressure on wages and the effective wage falls from w^* to w^{**}. Though both adult and child wages fall in response to the ban, child wages fall by more proportionally than do adult wages because they must also adjust for the higher costs associated with hiring child labor about by the increased fines (the proof can be found in the Online Appendix). In the new general equilibrium (w^{**}, L^{**}), total labor employed has increased and this increase has come only from children as households were already supply all adult labor.

[17] In the Online Appendix, we outline how the shift in the child labor supply curve in the partial equilibrium analysis is due to the effect of the ban on household income through the channel of adult wages. General equilibrium labor supply responses of this nature are formally discussed in Basu et al. (1998).

3.2. **Two sectors, complete mobility**

Extending the baseline model to two sectors requires careful consideration of the underlying labor market conditions. In particular, the existence of labor market frictions that restrict the flow between sectors is very important for the implications of policies to restrict child labor. We assume that the labor supply and demand function in both sectors are as characterized above; namely that child labor supply is backward bending and the labor demand curve is flat enough for there to exist a single equilibrium before the change in fines and that within each sector, the labor markets for both adults and children must be in equilibrium and effective wages equalized. In the following equations and figures, superscripts refer to person (Child, Adult) while subscripts refer to sector (manufacturing, agriculture).[18]

In the absence of any labor market frictions, the predictions of the Basu (2005) model break down. As long as labor is able to freely flow between sectors, wages equalize across sectors (even before the ban) so that $w_a^* = w_m^* = w^*$. Now consider a fine for employing child labor that is imposed in the manufacturing sector but not the agricultural sector. Children are now paid less in manufacturing, as their wages drop to offset the increased fine. Since there is no ban imposed in the agricultural sector, all child labor flows from manufacturing into agriculture, where the child wages are unchanged. However, the influx of children into agriculture drives down all wages in that sector (though the opposite is true for manufacturing; the exit of children from manufacturing relieves pressure in that sector and thus increasing effective wages there). As a result, adult labor flows out of agriculture and into manufacturing (raising wages for those who stay in agriculture). Because labor moves freely, these labor flows continue until wages are once again equated between sectors. Aggregate effective demand for labor is unchanged; the cost of a marginal unit of effective labor has remained the same in each sector though firms reallocate the type of labor they use.

In essence, the end result of a ban in one sector when labor moves freely is a labor reallocation between sectors; the decrease in child labor in the manufacturing sector is exactly offset

[18]With the exception of the case in which there is no labor mobility, we assume that both manufacturing and agricultural jobs are available to at least some households. While one might expect that agricultural jobs are only available in rural areas and manufacturing jobs in urban areas, we find that children engage in both types of work regardless of location. Nearly 18% of working children (age 17 or less) in rural areas are engaged in "manufacturing" (defined in more detail in the next section), while 16.5% of working children in urban areas are employed in agricultural jobs.

by the increase in child labor in the agricultural sector (in effective labor units). Only adult labor is employed in the sector where the fine has been levied as long as there is no shortage of adult labor supply.[19] In that sector, adults continue to earn w^{A*}. Some mix of adult and child labor is employed in agriculture, earning w^{A*} and γw^{A*}, respectively. Because the shifts in adult labor exactly offset the opposite shifts in child labor caused by the fine, overall levels of child labor are unchanged although the composition of labor across sectors has changed. From a policymaking point of view, while the fine does not achieve its goal of reducing child labor, it does not lead to the perverse outcome of increased child labor as in Basu (2005).

3.3. **Two sectors, no mobility**

The second case we consider is one in which there is no mobility between sectors. As labor is not mobile between sectors, wages are not necessarily equalized across sectors before the ban; for example, it is likely that barriers to entry into manufacturing jobs may yield an equilibrium in which the agricultural wage is below that of the manufacturing wage. A ban on child labor is then imposed in the manufacturing sector. This lowers all wages in manufacturing. The decrease in household income brought about by the ban will lead families to send more children to work, but it is not clear to which sector families will send their additional children. Their decision depends on which sector offers the higher child wages after the ban and the nature of labor market barriers to entry. In what seems the most likely case, some families have access to manufacturing jobs that pay higher wages to children even after the ban, and so they will send additional children to work in that sector. Yet other families do not have access to those manufacturing jobs and will send their children to agriculture, which shifts the labor supply curve out in that sector. Note that whenever there is an increase in child labor supply, this puts downward pressure on adult wages in that sector and household income decreases. The drop in household income forces these households to send more children to the labor market, shifting out the labor supply curve in the affected sector. Thus one possible post-ban equilibrium is one characterized by lower wages for both adults and children and where more children are working in both sectors. Child wages fall by more (relative to adult

[19] We can think of the case where there is not enough adult labor supply to completely satisfy demand in the manufacturing sector as a special case of asymmetric labor market frictions that limit movement from agriculture to manufacturing but not vice versa. This case is discussed in detail in the following section.

wages) in manufacturing; in agriculture, while both adult and child wages decrease in response to the ban, relative wages remain constant. This is because in agriculture, child wages do not need to decrease to compensate for any fines; child wages will be a constant fraction of adult wages both before and after the ban.

3.4. **Two sectors, partial mobility**

Now consider a setting in which labor markets are imperfect such that there is limited mobility between sectors. In particular, we can think that while labor flows freely into agriculture, there are some barriers to entry into employment in the manufacturing sector. For example, manufacturing jobs may require a different set of skills than agriculture that are not easily acquired, or access to manufacturing jobs may be limited to urban or semi-urban areas. Again, one reasonable effect of such labor market frictions may be higher pre-ban wages in the manufacturing sector relative to wages in the agriculture sector.

In this partial mobility setting, an increased fine for hiring child labor in the manufacturing sector has virtually the same effects on child labor as in the no mobility case if the increased fines leave child wages higher in manufacturing post-ban than in agriculture. Children now earn less in manufacturing than before the ban but since they still earn more in manufacturing relative to agriculture, those who hold manufacturing jobs are not induced to move to agriculture. Since the ban lowers manufacturing wages, working families in that sector are now poorer and will send more children to work in order to achieve subsistence consumption. Since the barriers to entry in manufacturing would divert at least some of the additional labor into the agricultural sector, the labor supply curve would likely shift out agriculture. Lower wages agriculture (in response to the increase in child labor) leads to further increases in child labor supply, possibly both in manufacturing and agriculture, depending on which sector children enter. The new equilibrium would be characterized by lower wages in each sector and more child labor overall, as in the no mobility case. As discussed before, child wages fall by more proportionately than adult wages in the manufacturing sector, while child and adult wages fall by the same proportion (and thus relative wages are unchanged) in the agricultural sector.

If the higher fines lower child wages in manufacturing enough to induce an outflow of child labor in manufacturing into agriculture, then there may be additional effects on the child labor markets. Child labor will flow out of manufacturing and into agriculture until child wages are equated between sectors. Adult wages will not equate, as there are barriers to entry into manufacturing so even though increased child labor supply in agriculture puts downward pressure on adult agricultural wages, adults will not be able to leave. Now if the ban in manufacturing leads to a large enough outflow of child labor from manufacturing, it is possible that adult wages will *rise* in that sector and children in households earning adult manufacturing wages will be pulled from the labor market. Depending on the sector in which these children work before the ban, supply of child labor could shift inward in both manufacturing and agriculture. If this is the case, then it is not clear whether child employment rises or falls in aggregate and within each sector. However if the effect of the ban is to lower adult wages in manufacturing, then child labor should unambiguously increase in both sectors and overall.

Therefore labor market frictions, whether partial or full, can lead to perverse effects of a ban on child labor even when there are two sectors. Notice that even in the full mobility case, child labor does not decrease but remains at pre-ban levels (although all child labor flows to agriculture). Thus independent of the state of the labor market, it is possible that an incomplete ban on child labor will *not* decrease the number of working children.[20]

3.5. **Summary of Theoretical Model**

In summary, the model illustrates that in a simple model, child labor may rise and child wages fall in response to an imperfectly enforced ban on child labor. The predictions of the model for individual sectors and for wage and employment responses vary depending on the state of the labor market. Perfect labor mobility allows for coplete labor reallocation following a ban; while child labor falls to zero in manufacturing, compensating increases in child employment will be found in the agricultural sector leaving overall levels of child labor (and wages) unchanged. In contrast, when there is imperfect or no labor mobility, child labor rises in response to a ban. Note

[20]If the child labor ban were in agriculture rather than in manufacturing, child labor would unambiguously increase. This is because labor is restricted from flowing into manufacturing and therefore the agricultural households face the full burden of the child labor ban in terms of decreased household income and increased child labor.

however that this model only illustrates the effects of *imperfectly* enforced bans on child labor. As pointed out in Basu and Van (1998), it possible that a perfectly enforced ban will lead to higher adult wages and successfully eradicate child labor. Nonetheless, in a two-sector model where the ban is only enforced in manufacturing, there may still be scope for an overall increase in child labor if adult wages do not rise by enough to eliminate households' need for child labor income. If the increase in adult wages is not sufficient to obtain subsistence then the effect of a perfectly enforced ban in manufacturing may serve only to shift child workers into agriculture, where child wages may be even lower, resulting in more child labor.

4. Data

The data we use to test the predictions of this model are from the Integrated Public Use Microdata Series International (IPUMS) database for India. The database is built from the employment and unemployment surveys collected by the National Sample Survey Organization (NSS) of the Government of India. We make use of the 1983, 1987 and 1993 rounds of the survey as these most closely correspond to periods before and after the 1986 Child Labor Act. We examine labor supply responses of nearly 515,000 children between the ages of 6 and 17 (roughly a third from each survey round). Tables 1a and 1b give the summary statistics for this sample. Employment information is only collected for children over the age of 5 and therefore sample statistics are displayed only only for children 6 years and older. Child labor is generally decreasing over time; before the ban is in place, 14.8% of individuals age 6-17 are formally employed while after the ban the proportion falls to 11.7%, although there is significant variation by age and gender.

Note that this measure of employment captures children who report working as their primary activity, in both paid and unpaid work and for employers, self-employment and family enterprises (including farms); the measure does *not* include children whose primary activity is domestic chores or housework as this is observed separately. We define "agricultural" employment as any employment in agriculture, fishing or forestry. We use the term "manufacturing" to refer to employment in any other industry (i.e. the converse of agricultural employment); these industries include (but are not limited to) mining, manufacturing, construction, wholesale and resale trade, and various services (e.g. in hotels, restaurants, and private households). Only about half of children are in school in 1983 but this proportion rises to nearly 65% in the later rounds of data. The children come from over 208,000 families, where the average family size is falling over time and education and non-agricultural employment is rising.

One major concern with the data is the scope for measurement error in employment status, particularly with strategic underreporting of child labor. While we believe this measurement error does not present a serious problem for our estimating strategy described in the next section, we discuss this issue in great detail in Section 6, where we also address other potential threats to validity.

5. Empirical Strategy and Results

5.1. Effect of the ban on wages

The model in the previous section indicates that one effect of the the ban on child labor is to reduce child wages by more relative to adult wages. To test this implication empirically, we use data on child wages to run the following difference-in-difference specification:[21]

$$\begin{aligned} \log(wage)_{it} &= \gamma_0 + \gamma_1 Under14_i + \gamma_2 Post1986_t \\ &\quad + \gamma_3 \left(Under14_i \times Post1986_t\right) + \gamma_X X_{it} + \delta_t + \nu_{it} \end{aligned} \tag{3}$$

X_{it} is a vector of household- and child-level covariates such as family size, household head characteristics, gender, and state-region fixed effects and δ_t captures survey year fixed effects (this is important to control for national level changes such as trade policies that changed in 1992).

If the theory holds, we should expect that wages for children (legally defined as those under 14) fall relative to adult wages after the ban, i.e. $\gamma_3 < 0$. The identifying assumption in (3) is that in the absence of the ban, the difference in wages for those just above age 14 and those just below age 14 should be stable over time; the effect of the ban is to decrease wages more for those

[21]Note that we are unable to implement this within the framework of a regression discontinuity since we have very few data points under the age of 14. An RD approach would require us to fit a line through essentially 6-7 points on either side of the cut off, which is too few. Similarly, lack of multiple rounds of data before the ban also does not allow us to control for pre-trends in age.

just under 14 than for those just over 14 after controlling for other observable characteristics and year fixed effects.

In the data, wages are only reported for those engaged in regular or casual labor. Notably, this excludes those working in home enterprises and farms. This does not affect our employment regressions, since we observe children's employment regardless of whether the work takes place within or outside the home. However, one major issue with the data is that wages are reported as zero (rather than missing) for those working in home enterprises and farms as well as those in all of the following groups: the unemployed, those working but unpaid, the self-employed, and beggars and prostitutes. Therefore reliable wage data is available for only a select subsample of workers and observing zero wages does not provide clear information. Because of the lack of clarity in the wage data when wages are recorded as zero, we only use positive wages for the specifications. Whether this leads to an under or overestimate of the drop in wages depends on the shadow wages of children pushed into or out of home production as a result of the ban. If shadow wages are equal to the wage for work outside the home, our estimates should be unaffected. If shadow wages are below market wages, we will underestimate the fall in child wages due to the ban as our sample will leave out the lowest earners. The opposite is true if the shadow wages are higher because in that case we are mechanically removing all children who actually move from outside work to home production and see an increase in their wages. While it is important to recognize the limitations of the wage data, our results will still be informative about the wages of those engaged in work outside the household, which is the focus of the model in Section 3.

The results of estimating equation (3) are displayed in Table 2. As predicted by the theory, child (under 14) wages fall by more than adult (above 14) wages. Each column gives the results of running (3) on various age samples. As expected, those under 14 always earn less than those over 14; the pre-ban wage gap is approximately 10 to 20 percent, depending on the sample (these coefficients are suppressed in the tables but are available upon request). Wages also rise over time for all ages. The difference in difference coefficient in Table 2 shows consistent drops in child wages relative to adult wages and this effect is largely concentrated from the manufacturing industries (column 3). Consistent with the model from the previous section, we find no *relative* decrease in wages for children in agriculture. For narrower age ranges around 14, the wage effect is smaller than for broader age ranges. When restricting the sample to those between the ages of 6-20, we find that wages for those under 14 drop by nearly 4%, whereas for the sample between ages 6-30 we find the wage drop to be nearly 10%.[22]

5.2. **Effect of the ban on child labor**

Our analysis for examining employment effects of the ban is different from the analysis above for wages. While the above section showed that wages for those under the age of 14 decreased after the ban, in this section we attempt to show that this decrease in wage results in more child labor being supplied from families affected by the ban. However the restricted way in which we observe employment necessitates a different approach for evaluating the effect of the ban on child labor. In particular, our measure of employment is largely an *extensive* measure of employment: whether or not a child is employed. Given that we only observe employment status rather than intensity, the straightforward difference-in-difference approach of the previous section is not appropriate for evaluating the effect of the ban on employment. To see why this is the case, imagine a child who is working before the ban is in place. After the ban is enforced, her wages drop and she must work more hours in order to afford subsistence consumption; the effect of the ban on her employment is to increase hours of work. However, since we observe only employment status (employed or not), it would appear as if the ban had no effect on her employment, as her employment *status* did not change due to the ban even as her hours increased. Additionally note that there is no possible wage change for a child who was not working before the ban was in place and so the ban will have no direct effect on her employment status either. Put another way, a wage change can have an income effect on labor only if work hours are positive.

To be able to examine the effects of a ban on the extensive margin of child labor, we draw our empirical strategy from the model in the previous section. In fact, the model itself specifies changes in child labor on the extensive margin; as the wages of children fall due to the ban, *new* children are drawn into the labor force. In other words, the *siblings* of working children are forced to enter into the labor market as a consequence of the ban. Thus our empirical strategy is built

[22]See the robustness section for the discussion of alternate explanations for these findings.

around the notion that a child's employment status will respond to the ban through its effect on *sibling* wages.

Ideally, we would observe the same families both before and after a ban has been implemented and study the effect of the ban on families with working children before the ban was in place. Unfortunately, the data are cross-sectional in nature so while we observe families before (1983) and after the ban (1987 and 1993), we are unable to link families across time. Therefore we do not know whether families in the post-ban period had children working before the ban was enacted. In the absence of longitudinal data, we use the families in the pre-ban period as counterfactuals for families in the post-ban period. The heart of our identification lies in the fact that the Child Labor Act of 1986 banned employment of children *less* than 14 years of age. We use this rule as the building block of our difference-in-difference strategy. In the absence of the ban, families with a 12-13 year old working child should be very similar to those with a 14-15 year old working child. However, once the ban is in place, the 12-13 year old will earn lower wages and his/her family may need to send another child to work. The 14-15 year old may also suffer a drop in wages due to the general equilibrium effects of the ban but as illustrated in the previous section, the fall in wages should be relatively larger for children under 14 and thus the labor responses should be concentrated in families with children under 14. Therefore the identification in our empirical strategy comes essentially from comparing the employment outcomes of children with siblings who are below or above age 14, both before and after the ban is in place.

Our baseline specification is

$$\begin{aligned} Y_{it} &= \beta_1 Treatment_i + \beta_2 * Post1986_t \\ &\quad + \beta_3 \left(Treatment_i \times Post1986_t\right) + \beta_X X_{it} + \delta_t + \varepsilon_{it} \end{aligned} \tag{4}$$

where Y_{it} is a child-level outcome, most often a dummy variable for whether the child i in survey round t is employed although we also examine other child outcomes such as schooling status to study how the ban impacts child time allocation and human capital formation.[23] $Post1986_t$ is an

[23]Ideally, we would observe child labor on both the intensive (hours worked) and extensive margin (labor force engagement) since families may respond to the ban by increasing both the hours each child works and the number of

indicator variable for survey rounds occurring after the Child Labor Act in 1986; X_{it} is a vector of household- and child-level covariates such as family size, household head characteristics, gender, and state-region fixed effects[24]; δ_t is a survey-round fixed effect.

$Treatment_i$ is a dummy variable taking the value of 1 when the child has at least one sibling who is both underage in the eyes of the law and working age, which we define to be a sibling of age 10-13; the treatment dummy is 0 when a child has a sibling in the age range 14-25 *or* below the age of 9. We choose this age range because the treatment is intended to isolate families affected by the ban - specifically those with children below the age of 13 and likely to be working before the ban. Since only 2% of children under the age of 10 are working before the ban as compared to 14% in the age range 10-13 (see Table 1b), we believe that a child with a sibling in this age range is much more likely to be affected by the ban than a child with any sibling under the age of 13.[25]

Our coefficient of interest is β_3, which captures the differential increase in the likelihood a child is employed after the ban is in place, for children with underage siblings versus children with siblings just above the 14 year old cutoff. The standard errors are clustered at the family-level, as the "treatment" of having a sibling in the right age range to be affected by the ban is at the household- rather than child-level.[26] In a sense, β_3 captures an "Intent-to-treat" effect of the ban because we define "Treatment" in (4) as having a sibling who is both underage relative to the law and *likely* to be working. In our baseline sample, we include all children with siblings in the 10-25 age range, but in Section 6 we show that our results are robust to restricting the sample to those with siblings just above and below the age of 14. We estimate equation 4 separately for children in the age range 10-13 and for children in the age range 14-17 and so on. An analogous way of

working children. However, the survey collects information only on the extensive margin so we can only consider the effects of the ban on whether a child is employed.

[24]Although the control set varies by specification, most regressions include controls for gender, family size, age of household head, age fixed effects, gender of household head, a dummy variable for urban status, survey year fixed effects, state-region fixed effects, hh type fixed effects, religion fixed effects, household head's education level fixed effects, household head's industry fixed effects. See notes below each table for a full list of included covariates.

[25]We show that our results are robust to alternate "treatment" age ranges in a later section.

[26]Our results are also robust to clustering at the region-year, state-year, region and state levels. Results not shown but are available upon request.

estimating this would be to include children of all ages and include interactions of own age with the $Post$ dummy.

Table 3 displays the results for estimating our baseline specification in (4) on three age groups: the very young (age 6-9), the young (10-13) and older children (14-17). The estimated effect of the ban is to increase employment among children under the age of 14. Having an underage sibling leads to a 0.003 percentage point increase in the likelihood of engaging in work for the very young. While this point estimate it is small, it is both statistically and economically significant; the pre-ban proportion of children employed in that age range is only 2 percent so the effect of the ban is to increase employment by 15% over the mean for this group. The ban increases the probability of employment by 0.008 percentage points (5.6% over the mean) for young children ages 10-13. However, older children ages 14-17 overall are unaffected by the ban. The effect for this group is both small relative to the mean and statistically insignificant.

In reality, since only 14% of children ages 10-13 are working before the ban, the effect of the ban on truly "treated" families (those with working children ages 10-13) could be much larger. By altering our definition of "Treatment" to include only those children who have siblings under 14 *working in manufacturing* (the sector affected by the Act of 1986), we can identify a more focused "Intent-to-Treat" effect. This "narrow" definition isolates families who we believe are the most likely to be affected by the ban, though as discussed below, we can not distinguish between families who had children working in manufacturing before the ban from those whose children began working as a consequence of the ban. For this reason, we do not regard this approach as yielding a true "Treatment-on-the-Treated" effect but we do believe it may give estimates that are closer to the true impact of the ban on affected families. The results of estimating (4) when defining treatment in this more narrow way are displayed in Table 4. Indeed, these effects are much larger suggesting that the ban increased employment in the 10-13 age group by 4.6 percentage points, which is nearly 33% over the mean pre-ban employment for this age group. However, we also find an increase in this case for the age group 14-17, although, given higher pre-ban means for this age group, the overall effect is smaller. One of the assumptions in the model is that adults supply labor inelastically. As long as children in the age range 14-17 are not completely considered as adults, our results are consistent.

However, we do not consider these as our baseline results for two important reasons. First, when we define treatment in this way, we must restrict the regression sample to only those children with siblings working in manufacturing. We do this in order to keep the “control” group as comparable to the “treatment group” as possible; comparing families with underage siblings working in manufacturing to all other families would be problematic as there are likely to be many unobserved differences between these two groups. Even with the relatively large samples of the IPUMS-India, our restricted sample of children with siblings working in manufacturing is less than 12% of our main regression sample (some samples drop to only 11,500 observations). Second and more importantly, this definition of treatment is partially based on a potential outcome of the ban; whether or not an underage sibling is working. Since we do not observe pre-ban employment, we are unable to distinguish between the employment of a sibling under 13 as a consequence of the ban and as a measure of whether the family is affected by the ban. As detailed in the previous section, the general equilibrium effect of a child labor ban is often to reduce adult wages; this drop in family income may be enough in and of itself to trigger entry of underage children into manufacturing. In this case, the “treatment” of having an underage sibling working in manufacturing may in fact be a response to the ban. Despite these two caveats, we believe that the results from defining treatment in this way are still informative as they give us a sense of how large the impacts of the ban may be for affected families.

Finally, while our results in Tables 3 and 4 allow us to identify the effect of the ban on the extensive margin of child labor, they are unable to capture the effects on the intensive margin. Given that households may respond to the ban by increasing the work hours of children employed pre-ban in addition to (or rather than) forcing other children to enter the workforce, our estimates may substantially understate the overall impact of the ban. However, the way in which our only employment variable is defined may help us shed some light on the intensive margin effects of the ban. Although we do not observe work hours, the survey defines a child as “employed” only if employment is her *primary* activity. In other words, it is possible that children classified as currently in school, engaged in household chores, or participating in other activities may actually be employed but not working enough hours to for parents to consider work as the primary activity. Thus if a child's work status moves from "not employed" to "employed", this may actually be the result of the child increasing work hours by enough for parents to consider the primary activity as switching from other activities to "employed." If this is the case, *changes in* our the employment status measure may reflect changes in employment on the intensive margin if, for example, children are upgraded from part-time to full-time work. Thus we also present the results of running the simpler version of our difference-in-difference strategy, where "Treatment" is defined by own work-age eligibility rather than sibling work-age eligibility. This strategy is identical to that used for evaluating the effect of the ban on child wages.

The results of this estimation are displayed in Table 5. We refer to these results as "reduced form" results because unlike the results presented in Tables 3 and 4 they are not directly derived the model in the previous section. The results show that to the extent that changes in employment status capture changes in the intensive margin of child labor, we also find evidence that the ban on child labor actually increases levels of child labor. In other words, not only do children enter the labor force when their family suffers a reduction in income due to the ban, working children also increase their hours. The estimated effects are quite large; if we restrict the sample to individuals between ages 6 and 20 we find that children are nearly 2 percentage points more likely to report employment as their primary activity - an increase of 25% over the pre-ban mean for children under the age of 14 (column 2). The estimated effect is robust to further restricting the sample to a narrower range of ages to the cutoff of 14, though the effect does disappear when considering much larger age ranges.

5.3. **Results by Gender, Sector and Poverty**

We can further break down the effects of the ban identified in (4) by age and gender. We believe this is important because baseline levels of employment are very different across these groups. For example, the proportion of boys who are employed is always larger than that of girls (for equivalent age groups) so we may expect a bigger labor supply response from girls, whose labor supply was less used before the ban. Table 6a finds this to be the case. Girls in the age range

10-13 years increase employment as a result of the ban by nearly 8.3% over the pre-ban mean. Boys in the 6-9 age range also increase employment, although boys in the 14-17 age range seem to *decrease* employment. This is somewhat surprising, although the magnitudes are quite small (the decrease is only 3% over the pre-ban mean). Although, as Table 6b shows, the effects when using the more narrow definition of treatment are mainly coming from girls increasing labor supply in response to the ban.

We can also examine employment responses by sector (agriculture and manufacturing) to test whether there are spillover effects in agriculture as predicted by the imperfect labor market cases of the two-sector model in the previous section. In Table 7 we find that almost all of the responses are coming from agriculture rather than manufacturing. This is reasonable under the idea of labor market frictions preventing entry into manufacturing but not agriculture.

Another source of important heterogeneity in treatment comes from the economic status of the household. As outlined in the previous section, the driving force behind families' decision to employ their children is the need to reach subsistence levels of consumption. Those who are most likely to resort to child labor before the ban and thus be affected by the ban are those with low incomes. Unfortunately we do not observe household income in the data. We therefore rely on education of the household head as an indicator of household economic status and measure the effect of the ban on children in households with an educated head (completed secondary school) versus those in households with an uneducated head (primary school or less). Table 8 shows that almost all the labor response is coming from households with less educated household heads. The response from households with educated heads is small and statistically insignificant.

5.4. **Substitution from schooling**

Table 9 shows that the increases in employment for children is offset by decreases in schooling enrollment in the 10-13 age range. In terms of the gender breakdown of the schooling response, the biggest effects are for older boys (14-17) who see a 2.1 pp increase in schooling attendance and girls 10-13 who see a 1.3pp decrease in schooling (pre-ban means are .483 and .431, respectively). The remaining point estimates are negative but not as large or significant. The results by gender are available upon request.

It is important to note that schooling and work are not the only activities that children may engage in. There is a substantial literature on “idle” children, i.e. those who report neither being in school nor in economic activities (see for example Edmonds and Pavcnik (2005a), Biggeri et al. (2003) and Bacolod and Ranjan (2008)). Fortunately, the IPUMS data are quite detailed and provide a fairly exhaustive list of primary activities for children above 5 years of age (respondents are forced to choose a primary activity for each child). As discussed in Edmonds et al. (2010), policies affecting child labor may also impact idleness of children. The results in Tables 3 and 9 suggest a complete switching of activities for children ages 10-13, as the increase in employment is offset by the decrease in schooling. When we further examine the effect of the ban on other outcomes (results not shown but available upon request), we find that children in this age range are not more likely to be engaged in housework, be unemployed, or be reported as “too young to work”. Children are *slightly* less likely to be ill or disabled (coefficient=-0.0006; significant at the 10% level) and also less likely to be inactive for other reasons (coefficient=-0.0046; also significant at the 10% level). This may suggest that children spend less time in leisure, on homework or in activities other than those listed above as a consequence of the ban. However, the aggregate effect on these other categories is small relative to the effects on schooling and employment.

5.5. **Effects of the ban for other ages**

One of the main assumptions in our model is that adults supply labor inelastically. Hence, in response to lower child wages, we should not expect to see a response from adults. In Table 9, we show that this is precisely the case. Men and women in the age range 18-55 do not show any increases or decreases in labor supply in response to the ban. Note that “treatment" here is defined as having a child under the age of 14 in the household, and note it is slightly different in interpretation from the previous tables where we refer to these children and their “siblings".

6. Robustness

As outlined above, the identifying assumption of our strategy is that children with siblings just above the age cutoff of 14 are otherwise similar to children with siblings just below 14. In this section we describe in detail threats to this identifying assumption and various robustness checks we implement to address the validity of our strategy.

6.1. **Measurement error and misreporting**

As described in Section 4, there is scope for measurement error in the reporting of child activities, especially with respect to child labor. In particular parents may underreport the labor of their children due to social or other types of pressure. Moreover it is possible that this under-reporting increases differentially for children under the legal working age of 14 after the ban on child labor. To rule out this possibility, we examine two alternate specifications. First, we are able to directly observe whether parents report that children are "too young" to participate in work. As discussed in the previous section, we do not observe any significant change in the likelihood that a child is reported being "too young to work" for children likely to be affected by the ban (results not shown but are available upon request). Second, our identification of the causal effect of the ban is based on the age of the *siblings*, rather than the age of the child. That is, we classify a child as being affected by the ban if he or she has a sibling under the legal working age in the post-ban period. If there is measurement error in that child's work status, it should not be correlated with our notion of "treatment" and thus this measurement error should be regarded as classical and affecting only the precision (not the consistency) of our estimates. To the extent that there exists a correlation between sibling and own age, we repeat our baseline specification while adding in age-by-time interactions. The results are displayed in Table A.1. The age and "Post-1986" interactions (with either quadratic specifications of age or fixed effects by age) should capture any differential change in child labor reporting for children just above and below the legal work cutoff, before and after the ban. The effect of the ban is very robust to the inclusion of age-by-time interactions, indicating that this type of strategic reporting is not an issue for identification.

Finally, families may circumvent the law by misreporting age rather than work status. This too could introduce some bias into our estimates if parents report siblings to be older than they truly are in order to get them to work "legally." Although we believe this would cause a downward bias in our estimated effects (as "treated" children are classified as "untreated"), we address this concern in Figures 2 and 3 of the Online Appendix. If parents strategically report their children as being older in order to justify their employment we should see distinct jumps in reported age of children, particularly from age 13 to 14. Moreover, such systematic jumps should be apparent only after the ban is in place. However, we do not observe a larger jump in age reporting at 14 versus 13 after the ban is in place (neither in overall nor for children employed in manufacturing), thus it appears that the ban does not impact age reporting by parents. Taken together, the evidence on both age and employment reporting indicates that measurement error of the sort discussed above does not impede our identification of the causal effect of the ban.

6.2. **Alternate definitions of treatment**

Our preferred measure of treatment includes children with siblings in the underage but working age group (10-13), but in reality even children with siblings outside of this age range may be affected by the ban. For example, the Child Labor Act of 1986 defines a child as "a person who has not yet completed his fourteenth year of age." This language may have generated some confusion as to whether persons who are 14 years old are classified as children by this definition and employers may be reluctant to employ 14 year olds as a result. It is also possible that there is some uncertainty in a child's age and a risk-averse employer may be hesitant to employ children who *appear* under the age of 14. To allow for these possibilities, we expand the definition of "underage" sibling to include siblings aged 14. The results are displayed in columns 1-3 of Table A.2. The results are very similar to the baseline results, although slightly higher for ages 10-13 when treatment is defined in this way; children in this group see an increase in the probability of employment of 1.4 percentage points (about 10 percent over the mean) due to the ban.

In columns 4-5 of Table A.2 we further relax the definition of treatment to include children with siblings as young as 7 as a small but nonzero proportion of children ages 7-9 work and could therefore potentially suffer wage drops as a consequence of the ban. Defining treatment in this way leaves our point estimates almost identical to our baseline results.

Finally, our identification strategy essentially compares children with siblings just above and below the age cutoff, though our baseline specification includes siblings up to age 25. In Table A.3, we narrow the sample to only those children with a sibling in the age range 7-17. Both the broad and narrow definition of treatment yield effects that are almost identical to our baseline results; children in the 10-13 age group are 0.007 percentage points (about 5% over the mean) and .041 percentage points (29 percent over the mean) more likely to work after the ban, according to the estimates using the broad and the narrow definitions of treatment respectively. However, the narrower age range reduces the sample size and thus the estimates are not quite as precise in our baseline sample, although all are statistically significant at or below the 10% level.

6.3. **Family-level specifications**

The original model of Basu (2005) and the model in Section 3 are constructed at the level of the household. Although we define treatment at the household level in the empirical specification, we conduct the primary analysis at the child level to pick up important heterogeneity in the effect of the ban by child age and gender. That said, we also perform family-level regressions to more closely match the theoretical model. Our regressions are of the form

$$
\begin{aligned}
Prop_{jt} &= \alpha_1 Treatment_j + \alpha_2 * Post1986_t \\
&+ \alpha_3 \left(Treatment_j \times Post1986_t \right) + \alpha_X X_{jt} + \delta_t + u_{jt}
\end{aligned} \tag{5}
$$

where $Prop_{jt}$ is the proportion of working children in a given age range for family j in year t. $Treatment_j$ is a dummy variable for whether the family has at least one child who is both underage in the eyes of the law and working age, which we define to be a child of age 10-13 when we consider the proportion of working children ages 6-9 or 14-17. When we look at the effects of the ban on the proportion of children working in the 10-13 age range, we define $Treatment_j$ as 1 if a family has at least *two* children who are age 10-13. This is because for this age range, each family automatically has at least 1 child 10-13 (otherwise the outcome variable, proportion of working children, is undefined). In order for children ages 10-13 to be eligible to be affected by the ban, they must have another sibling who is also in this age range, i.e. there must be two children in that age range.

Table A.4 reports the results for specification (5) for both the broad and the narrow definitions of treatment. Both sets of results are consistent with the child-level results. This is largely because the chosen age ranges are fairly narrow; since there are few families with multiple children in each of the 6-9, 10-13 and 14-17 age ranges, the variation in the proportion of working children at the family level is very similar to the variation in child-level employment for each family.

6.4. **Falsification exercises**

The underlying assumption for our identification strategy is that the difference between children with siblings just above and below the legal working age should be steady across time in the absence of the ban. One way to test whether the changes in child employment were due to ban and not some other change occurring at the same time is to impose "false" age restrictions on our untreated sample. In Appendix Table A.5 we see that when we define treatment as having a very young sibling (ages 0-4), we find no such effect of the ban (column 2); the same can be said when we define treatment as having sibling of ages 15-19 or 20-25. The results of this "placebo" test leads us to believe that our estimated effect of the ban is not picking up the effect of having an older or younger sibling.

If the variation in treatment and post-ban interaction is truly exogenous, we should not find that it has any effect on other child and household characteristics. In Online Appendix Table 2 we test whether the ban had any impact on the gender of children and 9 household demographic variables. Among these ten variables, we find that the ban significantly impacted only 3 variables: family size, age of household head and mother's age. While these effects are statistically significant, relative to the pre-ban means they are very small. For example, the change in the age of the household head is 0.259 years, or less than one percent of the pre-ban mean. This stands in contrast to the effects on child employment which are much larger. To be conservative we include all of the covariates listed in Online Appendix Table 2 to account for the possible effect of the ban on demographics. When we add each set of covariates in turn (child-level, household-level, mother-level and region and time fixed effects) in Online Appendix Table 3, the estimated impact of the ban is remarkably stable. Thus while we recognize that the variation we are using does not constitute a perfect "experiment" we believe that the weight of the evidence is in favor of our baseline specification yielding causal estimates of the impact of the ban.

6.5. Sampling weights, economic growth and other state policies

Our baseline specification does not include sample weights, despite the fact that the surveys are collected using stratification.[27] We do not weight our regressions for two reasons. First, the documentation for the earlier rounds (1983 and 1987) does not include a description of the sampling frame. Second, we believe that the true effect of the ban is heterogeneous on many levels, including ones that are likely to be used for sampling (urban location, economic status of the household) as well as child-specific ones as well (gender and age). For this reason, we follow Solon et al. (2013) and directly model the heterogeneity rather than include sample weights, as including sample weights could lead to inconsistent estimates. Nonetheless, we do re-run our baseline specification with sampling weights as a robustness check and find that doing so leaves the estimated effects virtually unchanged, though it does reduce precision for some of the results. These results are reported in Online Appendix Table 4.

One potential explanation for our finding that child wages drop relative to adult wages is the rapid economic growth India experienced during the period under study. In section 3 we assume that all children are equally productive and all households share the same disutility of working their children. However, in reality, households may differ in their valuation of children's leisure or schooling time, resulting in variation children's in reservation wages. Similarly, different outside opportunities and skills could also lead to different reservation wages for children even within the same family. If this were the case, we would expect the rise in household income during this period to lead to a reduction in child wages because those children with the highest reservation wages would be the first to exit employment as the household becomes more able to afford child leisure. The nonrandom exit of the highest earning children would lead to lower average child wages (of those still employed) relative to adult wages. However, while economic development and heterogeneity in reservation wages can explain the decline in average child wages relative to adult wages, it cannot simultaneously explain the relative rise in child employment during this time (see reduced form effects presented in Table 5). If rising incomes were the only reason behind the

[27] All of our descriptive sample statistics such as pre-ban means of depend variables and summary statistics are all calculated using weights.

drop in child wages relative to adult wages, we would expect to see declines in child labor as well; however we observe just the opposite. Thus we do not believe our results are driven by economic growth alone.

Another alternate explanation for our wage results could be that the results capture an average decline in wages for children due to skill-biased technical change. If skills are positively correlated with age, we one might expect that skill-biased technical change may reduce wages more for younger individuals (under 14) than older individuals (over 14), leading to decreases in wages and increases in child labor independent of the ban. To rule out this possibility we refer to our results in Appendix Table A.5. Here we can see that child labor only increases for children who have siblings under the official age limit of 14; increased work is not the consequence of having a younger sibling more generally. If skill-biased technical change lowered wages of all young workers, then we should find positive effects of having a younger sibling (of any working age) on employment; however we find such an effect only age divisions corresponding to the actual legal definition of working age. Thus we do not believe that skill-biased technical change is driving our results.

Finally, we account for state level policy changes by including state by year fixed effects. Unless these state level policies directly affect those under 14 differentially, we should not expect large changes in the estimates. Table A.6 shows that including state-year interactions does little to change our estimates.

7. Conclusion

This paper is the first empirical investigation of the impact of India's most important legal action against child labor. While the Child Labor (Prohibition and Regulation) Act of 1986 prevented employers from employing children in certain sectors and increased regulation of child labor in non-family run businesses, the net result of this ban appears to be an *increase* in child labor in some families. We find that child wages decrease in response to such laws and poor families send out more children into the workforce. Due to increased employment, affected children (primarily girls) are less likely to be in school. These results are consistent with a two sector model with some frictions on mobility across sectors where the ban is more stringently enforced in one sector than the other.

This paper does not intend to suggest that child labor bans are useless. In fact, well formulated and implemented bans could absolutely help in eliminating child labor;[28] but whether this decreases or increases child *welfare* is a separate question (Baland and Robinson (2000); Beegle, Dehejia and Gatti (2009)). To echo the reasoning in Basu (2004): “Legal interventions, on the other hand, even when they are properly enforced so that they do diminish child *labor*, may or may not increase child *welfare*. This is one of the most important lessons that modern economics has taught us and is something that often eludes the policy maker." While we certainly want to exercise caution while extrapolating our results to make statements about child welfare, it is difficult to imagine a case for child welfare *improving* (relative to the status quo) under the circumstances that we examine in this paper. When poor families send their children to work out of necessity and employers can cut wages in response to such laws as a way of insuring themselves against the cost of penalties, then such bans are ineffective and likely hurt the poor even further. There are many options available to policy makers who wish to reduce the incidence of child labor (like cash transfers, increasing investments in and returns to education, etc). If anything, we think a discussion in policy circles about these alternatives should be heightened since it appears from our study that child labor bans of the type instituted under the Child Labor (Prohibition and Regulation) Act can be ineffective.

[28]One way of achieving this in our context might be to increase fines and penalties to a point where employers no longer hire child labor.

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TABLES

TABLE 1A. Summary statistics: Means of household variables

	1983	1987, 1993
Family Size	6.202	5.968
HH Head is Male	0.915	0.918
HH Head Age	43.613	43.064
HH Head Completed Secondary School	0.098	0.135
HH Head is in Agriculture	0.509	0.499
HH Head is in Manufacturing	0.361	0.392
Hindu	0.835	0.831
Nuclear family household (no extended family)	0.562	0.608
Urban	0.233	0.236
Number of Observations	72,276	136,510

TABLE 1B. Summary statistics: Means of child variables

	1983				1987, 1993			
	All ages	Ages 6-9	Ages 10-13	Ages 14-17	All Ages	Ages 6-9	Ages 10-13	Ages 14-17
Age	10.934				10.985			
Male	0.528	0.518	0.533	0.535	0.535	0.525	0.537	0.548
Mother's Age*	37.161	33.815	37.824	41.715	36.575	33.02	37.106	41.233
Mother Completed Secondary School*	0.032	0.035	0.029	0.029	0.057	0.057	0.056	0.060
Number of Siblings Ages 7-17	1.932	1.829	2.016	1.964	1.787	1.685	1.923	1.751
Employed (1=Yes, 0=No)	0.148	0.020	0.142	0.336	0.117	0.012	0.096	0.288
In School (1=Yes, 0=No)	0.504	0.545	0.557	0.375	0.605	0.649	0.666	0.465
Weekly Real Wages (1982 Rupees)	31.34	17.74	23.29	35.47	46.34	26.33	30.97	52.34
Number of Observations	177,965	65,685	63,761	48,519	336,724	121,440	118,243	97,041

Mother's age and educational attainment are not available for all child observations. Real wage statistics are conditional on paid employment.

TABLE 2. Effect of Ban on Child Wages

Dependent Variable: Log(Real Wages)				
	All Sectors Ages 6-30 (1)	All Sectors Ages 6-20 (2)	Manufacturing Ages 6-30 (3)	Agriculture Ages 6-30 (4)
Under14*Post1986	-0.100***	-0.038**	-0.048**	-0.007
	(0.036)	(0.018)	(0.023)	(0.014)
Observations	100,394	33,038	60,284	40,154
R-squared	0.546	0.375	0.493	0.366

*** p<0.01, ** p<0.05, * p<0.1 All regressions include a dummy for Post-1986, a dummy for ”Under 14” as well as controls for gender, family size, age of household head, sector dummy (agricultural or manufacturing), age fixed effects, gender of household head, urban status, survey year fixed effects, state-region fixed effects, hh type fixed effects, religion fixed effects, household head’s education level fixed effects, household head’s industry fixed effects. “Under 14” is a dummy variable that takes the value of 1 if the child is under 14 years of age. Sample consists of all individuals who are currently employed in paid jobs and are related to the household head. Standard errors are clustered by age-year. Real wages are nominal wages deflated by the average wholesale price index reported by the Government of India for the respective year. Sample only contains respondents with non-zero wages, trimmed at the 1% and 99% percentiles.

TABLE 3. Effect of Ban on Child Employment: Intent-to-treat effects

Dependent Variable: Employed (1=Yes, 0=No)			
	Ages 6-9 (1)	Ages 10-13 (2)	Ages 14-17 (3)
Treatment*Post1986	0.003***	0.008***	-0.002
	(0.001)	(0.003)	(0.005)
Pre-Ban Mean of Dep. Var.	0.020	0.142	0.336
Observations	187,126	182,005	145,562
R-squared	0.025	0.098	0.180

*** p<0.01, ** p<0.05, * p<0.1 All regressions include a dummy for Post-1986, a dummy for “Treatment” as well as controls for gender, family size, age of household head, age fixed effects, gender of household head, urban status, survey year fixed effects, state-region fixed effects, hh type fixed effects, religion fixed effects, household head’s education level fixed effects, household head’s industry fixed effects. Sample of children consists of all who are related to the household head, excluding any who are the household head or the spouse of the household head. “Treatment” is a dummy variable that takes the value of 1 if the child has a sibling who is between the ages of 10 and 13 (inclusive) and takes on a value of 0 if sibling is between ages of 14-25 (inclusive) or below the age of 9. Standard errors are clustered by household.

TABLE 4. Effect of Ban on Child Employment: Intent-to-treat effects (Narrow Definition)

Dependent Variable: Employed (1=Yes, 0=No)			
	Ages 6-9	Ages 10-13	Ages 14-17
	(1)	(2)	(3)
Treatment*Post1986	0.017**	0.046**	0.044**
	(0.009)	(0.023)	(0.019)
Pre-Ban Mean of Dep. Var.	0.022	0.147	0.327
Observations	22,164	26,977	29,290
R-squared	0.050	0.132	0.208

*** p<0.01, ** p<0.05, * p<0.1 All regressions include a dummy for Post-1986, a dummy for "Treatment" as well as controls for gender, family size, age of household head, age fixed effects, gender of household head, urban status, survey year fixed effects, state-region fixed effects, hh type fixed effects, religion fixed effects, household head's education level fixed effects, household head's industry fixed effects. Sample consists of all children with at least one sibling under 25 years old working in manufacturing who are related to the household head, excluding any who are the household head or the spouse of the household head. "Treatment" is a dummy variable that takes the value of 1 if the child has a sibling who is under age 14 and working in manufacturing. Standard errors are clustered by household.

TABLE 5. Reduced Form Effect of Child Labor Ban on Employment

	Ages 6-30	Ages 6-20	Ages 10-17
	(1)	(2)	(3)
Under14*Post1986	-0.002	0.019**	0.017***
	(0.007)	(0.008)	(0.005)
Mean of Dep. Var. (for children under 14)	0.080	0.080	0.141
Observations	966,941	644,893	332,282
R-squared	0.387	0.256	0.172

*** p<0.01, ** p<0.05, * p<0.1 All regressions include a dummy for Post-1986, a dummy for "Under 14" as well as controls for gender, family size, age of household head, age fixed effects, gender of household head, urban status, survey year fixed effects, state-region fixed effects, hh type fixed effects, religion fixed effects, household head's education level fixed effects, household head's industry fixed effects. "Under 14" is a dummy variable that takes the value of 1 if the child is under 14 years of age. Sample consists of all individuals age 6-40 who are currently employed in paid jobs and are related to the household head. Standard errors are clustered by age-year.

TABLE 6A. Effect of Ban on Child Employment: Intent-to-treat effects by Gender

	Boys			Girls		
	Ages 6-9	Ages 10-13	Ages 14-17	Ages 6-9	Ages 10-13	Ages 14-17
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment*Post1986	0.005***	0.004	-0.014**	0.001	0.011***	0.009
	(0.002)	(0.004)	(0.007)	(0.002)	(0.004)	(0.006)
Pre-Ban Mean of Dep. Var.	0.021	0.159	0.425	0.019	0.122	0.233
Observations	97,639	96,932	77,946	89,487	85,073	67,616
R-squared	0.026	0.100	0.191	0.029	0.112	0.168

TABLE 6B. Effect of Ban on Child Employment: Intent-to-treat effects (Narrow Definition)

	Boys			Girls		
	Ages 6-9	Ages 10-13	Ages 14-17	Ages 6-9	Ages 10-13	Ages 14-17
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment*Post1986	0.019	0.009	0.022	0.014	0.088***	0.078***
	(0.012)	(0.031)	(0.024)	(0.012)	(0.031)	(0.029)
Pre-Ban Mean of Dep. Var.	0.025	0.175	0.445	0.019	0.117	0.207
Observations	11,427	14,163	14,798	10,737	12,814	14,492
R-squared	0.052	0.133	0.204	0.059	0.156	0.185

*** p<0.01, ** p<0.05, * p<0.1 Panel A: See notes under Table 3. Panel B: See notes under Table 4.

TABLE 7. Effect of Ban on Employment by Sector

	Treatment = At least 1 child age 10-13		Treatment = At least 1 child under under 14 working in manufacturing	
	Ages 10-13		Ages 10-13	
	Agriculture	Manufacturing	Agriculture	Manufacturing
	(1)	(2)	(3)	(4)
Treatment*Post1986	0.006**	0.002	0.016*	0.031
	(0.003)	(0.002)	(0.008)	(0.023)
Pre-Ban Mean of Dep. Var.	0.115	0.027	0.050	0.097
Observations	181,712	181,712	26,945	26,945
R-squared	0.104	0.045	0.076	0.122

*** p<0.01, ** p<0.05, * p<0.1 See notes under Table 3.

TABLE 8. Heterogeneous Effects of Child Labor Ban on Child Employment by Education

	Household Head Has Less Than Secondary Education			Household Head Has At Least Secondary Education		
	Ages 6-9	Ages 10-13	Ages 14-17	Ages 6-9	Ages 10-13	Ages 14-17
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment*Post1986	0.004**	0.008**	-0.003	0.001	-0.002	0.005
	(0.001)	(0.003)	(0.006)	(0.001)	(0.003)	(0.007)
Pre-Ban Mean of Dep. Var.	0.022	0.155	0.366	0.002	0.017	0.061
Observations	158,827	153,145	120,369	28,299	28,860	25,193
R-squared	0.027	0.096	0.156	0.005	0.016	0.061

*** p<0.01, ** p<0.05, * p<0.1 See notes under Table 3.

TABLE 9. Effect of Ban on Schooling Status

Dependent Variable: Currently in School (1=Yes, 0=No)			
	Ages 6-9	Ages 10-13	Ages 14-17
	(1)	(2)	(3)
Treatment*Post1986	-0.004	-0.008*	0.010*
	(0.005)	(0.005)	(0.005)
Pre-Ban Mean of Dep. Var.	0.545	0.557	0.375
Observations	186,722	181,667	145,327
R-squared	0.255	0.248	0.284

*** p<0.01, ** p<0.05, * p<0.1 See notes under Table 3

TABLE 10. Effect of Child Labor Ban on Employment of Other Age Groups

	Men & Women		
	Ages 18-25	Ages 26-55	Ages 55+
Treatment*Post1986	-0.003	-0.001	-0.001
	(0.004)	(0.002)	(0.005)
Pre-Ban Mean of Dep. Var.	0.530	0.650	0.380
Observations	216,922	611,785	141,331
R-squared	0.307	0.448	0.356
	Men		
	Ages 18-25	Ages 26-55	Ages 55+
Treatment*Post1986	-0.006	-0.001	0.002
	(0.006)	(0.002)	(0.007)
Pre-Ban Mean of Dep. Var.	0.746	0.933	0.607
Observations	121,939	311,861	72,293
R-squared	0.193	0.021	0.239
	Women		
	Ages 18-25	Ages 26-55	Ages 55+
Treatment*Post1986	0.000	-0.003	0.006
	(0.006)	(0.003)	(0.005)
Pre-Ban Mean of Dep. Var.	0.248	0.360	0.153
Observations	94,983	299,924	69,038
R-squared	0.173	0.188	0.147

*** p<0.01, ** p<0.05, * p<0.1 See notes under Table 3.

Appendix A. Appendix Tables

Table A.1. Adding age by time interactions

	Baseline	Age*Post, Age^2*Post	Age*Post FE	Age*Year FE, Age^2*Year FE	Age*Year FE
	(1)	(2)	(3)	(4)	(5)
Treatment*Post1986	0.008***	0.008***	0.008***	0.008***	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	182,005	182,005	182,005	182,005	182,005
R-squared	0.098	0.099	0.099	0.099	0.099

*** p<0.01, ** p<0.05, * p<0.1 All regressions include a dummy for Post-1986, a dummy for ”Treatment” as well as controls for gender, family size, age of household head, age fixed effects, gender of household head, urban status, survey year fixed effects, state-region fixed effects, hh type fixed effects, religion fixed effects, household head’s education level fixed effects, household head’s industry fixed effects. Sample of children consists of all ages 10-13 who are related to the household head, excluding any who are the household head or the spouse of the household head. ”Treatment” is a dummy variable that takes the value of 1 if the child has a sibling who is between the ages of 10 and 13 (inclusive). Standard errors are clustered by household.

TABLE A.2. Alternate definitions of Treatment

	Treatment = At least 1 sibling ages 10-14			Treatment = At least 1 sibling ages 8-13		
	Ages 6-9	Ages 10-13	Ages 14-17	Ages 6-9	Ages 10-13	Ages 14-17
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment*Post1986	0.003**	0.014***	-0.002	0.006***	0.004	0.001
Pre-Ban Mean of Dep. Var.	0.020	0.142	0.336	0.020	0.142	0.336
Observations	187,126	182,005	145,562	187,126	182,005	145,562
R-squared	0.025	0.099	0.180	0.025	0.098	0.180

TABLE A.3. Restricted Samples: Only children with siblings ages 6-17

	Treatment = At least 1 sibling ages 10-13			Treatment = At least 1 child under 14 working in manufacturing		
	Ages 6-9	Ages 10-13	Ages 14-17	Ages 6-9	Ages 10-13	Ages 14-17
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment*Post1986	0.005***	0.007**	-0.004	0.017**	0.041*	0.036*
	(0.002)	(0.003)	(0.006)	(0.009)	(0.023)	(0.019)
Pre-Ban Mean of Dep. Var.	0.020	0.138	0.333	0.023	0.145	0.330
Observations	153,452	164,708	120,271	19,571	24,802	23,637
R-squared	0.025	0.098	0.179	0.053	0.136	0.216

*** p<0.01, ** p<0.05, * p<0.1 All regressions include a dummy for Post-1986, a dummy for ”Treatment” as well as controls for gender, family size, age of household head, age fixed effects, gender of household head, a dummy variable for urban status, survey year fixed effects, state-region fixed effects, hh type fixed effects, religion fixed effects, household head’s education level fixed effects, household head’s industry fixed effects. Sample of children consists of all ages 10-13 who are related to the household head, excluding any who are the household head or the spouse of the household head.

TABLE A.4. Family-level regressions

	Treatment = At least 1 sibling ages 10-13			Treatment = At least 1 child under 14 working in manufacturing		
	Ages 6-9	Ages 10-13	Ages 14-17	Ages 6-9	Ages 10-13	Ages 14-17
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment*Post1986	0.004***	0.008***	-0.010**	0.029**	0.013	0.047**
	(0.001)	(0.003)	(0.005)	(0.012)	(0.009)	(0.021)
Pre-Ban Mean of Dep. Var.	0.020	0.150	0.351	0.031	0.240	0.482
Observations	140,725	139,301	117,700	16,903	21,953	27,513
R-squared	0.021	0.098	0.149	0.125	0.730	0.172

TABLE A.5. Falsification exercise 1: Imposing False Age Restrictions

	Baseline	Underage Sibling = Under 4	Underage Sibling = Aged 15-19	Underage Sibling = Aged 20-25
	(1)	(2)	(3)	(4)
Treatment*Post1986	0.008***	-0.003	0.005	0.002
	(0.003)	(0.004)	(0.004)	(0.004)
Observations	182,005	96,325	96,325	96,325
R-squared	0.098	0.101	0.100	0.100

*** p<0.01, ** p<0.05, * p<0.1 All regressions include a dummy for Post-1986, a dummy for "Treatment" as well as age controls for gender, family size, age of HH head, age fixed effects, gender of HH head, urban status, survey year fixed effects, state-region fixed effects, hh type fixed effects, religion fixed effects, HH head's education level fixed effects, HH head's industry fixed effects. Standard errors are clustered by household. Columns (2)-(4) include the sample of control households only (no sibling between 10-14). Column (4) includes data from the 1993, 1999, and 2004 rounds only.

TABLE A.6. Adding state-by-year fixed effects

	Treatment = At least 1 sibling ages 10-14			Treatment = At least 1 sibling ages 7-13		
	Ages 6-9	Ages 10-13	Ages 14-17	Ages 6-9	Ages 10-13	Ages 14-17
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment*Post1986	0.004***	0.007**	-0.003	0.014	0.046**	0.052***
	(0.001)	(0.003)	(0.005)	(0.009)	(0.023)	(0.020)
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	187,126	182,005	145,562	22,164	26,977	29,290
R-squared	0.024	0.092	0.175	0.062	0.128	0.205

*** p<0.01, ** p<0.05, * p<0.1 All regressions include a dummy for Post-1986, a dummy for ”Treatment” as well as controls for gender, family size, age of HH head, age fixed effects, gender of HH head, urban status, survey year fixed effects, state fixed effects, state-year fixed effects, hh type fixed effects, religion fixed effects, HH head's education level fixed effects, HH head's industry fixed effects. Table A.4: Columns (1)-(3): Sample consists of all households with at least 1 child in the given age range. Treatment = 1 if household has at least 1 child ages 10-13 (for column 1 and 3) or at least 2 children ages 10-13 (column 2). Columns (4)-(6): Sample consists of all households with at least 1 child in the given age range and who have at least 1 child working in manufacturing the 0-25 age range. Robust standard errors reported. Table A.6: Sample of children consists of all who are related to the household head, excluding any who are the household head or the spouse of the household head. Standard errors are clustered at the household level.