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 ubiquitous policy tool, there is very little empirical evidence on their effectiveness. In this paper, we examine 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 the relative probability of child employment *increases* and child wages (relative to adult wages) decrease after the ban. Our main specification relies on comparing changes in work probabilities over time for children *of the same age* but with siblings who are rendered either eligible or ineligible for legal work when the ban is implemented. The increases in the probability of economic activity are largest for children (i) in areas where the industries targeted by the ban play a larger role in local labor markets, (ii) in areas where the probability of employer inspections are higher, and (iii) in families that are poorer. 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. We also find decreases in child participation in schooling (for younger children only) and no economically meaningful change in household outcomes like assets or calorie intake.

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1. Introduction

Despite facing near universal opposition for decades, child labor is endemic. According to a recent report by the International Labor Organization, there are nearly 168 million child laborers, of whom 85 million work under hazardous conditions (International Labour Organization (2013)). While many policy options exist to address this, laws banning or regulating child labor remain the predominant response. The possibility 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). The effect of these laws on child labor and household welfare is theoretically ambiguous (see Basu and Van (1998) and Baland and Robinson (2000)). On the one hand, when properly enforced, bans increase the cost to employers of hiring children thereby deterring their use. On the other hand, if poor families use child work in order to reach subsistence and employers respond to increased fines by lowering child wages, families will need to supply *more* child labor (Basu (1999), Basu (2005)). The latter result especially rings true when states lack the capacity to enforce child labor regulation – a likely scenario in developing countries. Given the theoretical ambiguities in academic work, what does rigorous empirical work have to say about the impacts of such bans? In a comprehensive review, Edmonds (2007) 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 examining the impact of India's flagship legislation against child labor, the Child Labor (Prohibition and Regulation) Act of 1986, which banned employment of children under the age of 14 in various occupations and industries. Most recent articles in the press cite this law as the starting point for legal action against child labor in India. Our results are important for understanding the impacts of such bans in settings where people live at the margin of subsistence and where legal enforcement is weak. Given the dearth of rigorous empirical evaluations of child labor bans in such settings, our paper bridges a fundamental gap in this literature.[1]

[1]Overall, the literature on child labor is rich, including work examining its causes (see reviews by Basu (1999), Edmonds et al. (2010), Edmonds (2007)), its consequences (e.g., Beegle et al. (2009)), theoretical effects of bans (e.g., Baland and Robinson (2000), Basu and Van (1998), and Doepke and Zilibotti (2005)), empirical effects of bans in

In Basu's (2005) one sector model, an imperfectly enforced ban lowers child wages, which forces families reliant on child labor income for subsistence to further increase levels of child labor. When we broaden the model to allow for heterogeneity in sibling composition and age structure, the impacts of the ban on the extensive margin of labor are felt most strongly by children whose siblings are directly affected by the ban via lower wages. A two sector extension of this model with the ban applying to only one sector (as was the case with the 1986 law) illustrates that the state of the labor market is important for determining the effects of a ban in one sector. Specifically when there are no labor market frictions that prevent free movement of labor from one sector to another, the ban has no impact on overall levels of child labor but simply reallocates it across sectors as in Edmonds and Shrestha (2012a). However when movement between sectors is limited, the main insight of the Basu (2005) model is preserved. In this case, a ban in one sector may increase child labor in either or both sectors.

We test the predictions of this theory using several difference in differences models and detailed data on employment from the multiple rounds of the National Sample Survey in India. We classify data before 1986 as the "pre-ban" period and data gathered after 1986 as the "post-ban" period. To estimate the overall impact of the ban on child time allocation, we compare the changes in employment of children below the age of 14 to the changes of those over 14, since the 1986 Act applied only to those under age 14.[2] However, given that this basic strategy may be influenced by pre-existing differences in the pre-ban levels or trajectories of work for children over and under age 14, our main empirical approach relies on two alternate strategies. The first uses predictions from the theoretical model and *sibling* work eligibility to assess the impact of the ban. Specifically, the theoretical model predicts that the ban will have an income effect on families with working children; as wages fall for those targeted by the ban (under the age of 14), incomes of their

the U.S. (Moehling (1999), Lleras-Muney (2002), Manacorda (2006), Bugni (2012)), other policies intended to affect child labor directly, such as cash transfers (Skoufias et al. (2001), Edmonds and Schady (2012), and many others), policies affecting child labor indirectly, such as trade liberalization (Edmonds and Pavcnik (2005b)) and workfare programs (Shah and Steinberg (2015)), and the impact of bans on adult and schooling outcomes (Boockmann (2010), Piza (2014)). However, there is limited evidence on the effectiveness of bans on reducing child labor developing countries. Exceptions include new work (since our first draft in October 2013) evaluating an increase in minimum age restrictions for work in Brazil (Bargain and Boutin (2017), Piza and Portela Souza (2016), and Piza and Souza (2017)), though the evidence on the impacts of the legislation is mixed.

[2]From hereon, "under age 14" means *strictly* under age 14, and "above 14" means greater than or equal to age 14.

families fall and thus their younger siblings are forced into the labor market. Therefore our main estimating equation is a difference-in-difference specification that compares the change in labor supply for children of *the same age* but whose *siblings* are under or over the legal working age. This sibling-based strategy directly addresses many of the issues related the simple difference-in-difference approach based on own age – such as differences in the pre-ban levels or trajectories of child labor for older and younger children – as it restricts comparisons to children of the same age.

The second approach exploits geographical heterogeneity in the predicted effects of the ban. In particular, we use two measures of geographical heterogeneity in a triple-difference strategy: (i) the importance of industries targeted by the ban in local labor markets, calculated as the state-wide proportion of households who are principally engaged in work in banned industries during the pre-ban period and (ii) the probability of employer inspection using data on inspections for child labor infractions and the pre-ban incidence of child employment in banned industries at the state level. We also show that the effects of the ban are larger for families that appear poorer (and thus subsistence constraints more binding), as predicted by the theoretical model.

Our simple difference-in-difference estimates show that the probability a child under 14 is economically active relative to a child over 14 rose by 2.6 percentage points after the ban. It is worth noting that overall the period under study was characterized by rising incomes and a steady decline in the probability of working for all children under 18; thus our findings imply that employment of children under 14 did not decrease by as much as it would have had the 1986 Act not been implemented. Given the potential issues with the simple difference-in-difference strategy outlined above, we regard the results from this method as largely descriptive. Using the sibling-based difference-in-difference approach more closely tied to the theoretical model and less subject to the criticisms of the simple strategy, our results show that a child between the ages of 10-13 with a sibling below the age of 14 significantly increases her likelihood of economic activity by 0.8 percentage points compared to a child of the same age with a sibling over the age of 14. This represents an increase of approximately 7.0% over the pre-ban employment rate for that age group and suggests that the channels in the model are quantitatively important. Examining heterogeneity in the impact of the ban, we find that the effect of the ban is greater in areas where we expect the

ban to have greater bite, and also among households that are poorer. We also find decreases in child participation in schooling (for younger children only), relative decrease in child wages, and no economically meaningful change in household outcomes like assets or calorie intake.

Our work highlights the importance of careful economic analysis of laws in a context where there could be multiple market failures.[3] 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 developing countries. The effects of laws could be quite different in developing countries when they are not fully enforced due to weak institutions. The paper's analysis is broadly applicable to child labor bans in other developing countries where imperfect enforcement combined with a subsistence motive creates the potential for perverse effects.[4] Hence, our paper speaks to the idea that optimal policy making in developing countries should take into account an environment of weak enforcement (example, in the case of tax policy see Gordon and Li (2009)) and non-standard behavior at the margin of subsistence (Jayachandran (2006)).

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

The impetus for the 1986 law came from multiple reports from Government committees that suggested only partial implementation of prior laws against child labor.[5] The major innovation of the 1986 law was uniformity in the minimum age restriction – people up to age 14 were defined as children and therefore ineligible to work in certain industries and occupations. Subsequent additions to the list of industries banned from hiring children under 14 were made at various points between 1989-2008. The occupations subject to the ban after 1986 and before 1994 (the period 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 were banned for

[3]Credit market failures are a prime example, as noted in Baland and Robinson (2000).

[4]By imperfect or partial enforcement, we mean a broad set of conditions where the probability of being caught by inspectors when using child labor was sufficiently low such that employers regarded the ban as a tax on the cost of hiring child labor rather than a strict inability to hire children.

[5]For descriptions of these government committee reports, the Sanat Mehta Committee of 1986 and the Gurupadaswamy Committee on Child Labor of 1979, see Ramanathan (2009)).

children includes beedi (hand rolled cigarette) making, manufacturing of various kinds (matches, explosives, shellac, soap, etc.), construction, automobile repairs, production of garments, etc. The major caveat to these bans was that agriculture was largely exempted and family-run businesses were allowed to employ their own children without age restrictions.

Importantly for our purpose, the law clearly states the penalties for employers who contravene the ban, including "... 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." and for repeat offenders, "... imprisonment for a term which shall not be less than six months but which may extend to two years."[6]

Though enforcement of the 1986 law has been imperfect, it does appear that employers were aware of the law. Hard data on inspections is difficult to come by for the period we study (1987-1994). However, reporting of the law in national newspapers at the time suggests that the law was implemented immediately and with some visibility. In January 1987 a series of arrests in Ferozabad, Uttar Pradesh (an important center for bangle manufacturing) made the national news. This incident was heralded as the "beginning that has to be made somewhere in ending child labour" and social workers acknowledged that the arrests "under the child labor law would augur well for its implementation" (Times of India, January 17, 1987; pg.18). This sentiment was echoed in February 1987, as states were "told to strictly enforce the Child Labour Law" ('Implement child labor law strictly', Times of India, February 28, 1987; pg.18). Data on inspections become more widely available in later years; between 1997 and 2005, over 2.34 million inspections were carried out across India resulting in nearly 144,000 violations (IndiaStat).

In response to the law and subsequent risk of inspection, many employers found loopholes to work around the specifics of the law. For example, a 2003 Human Rights Watch report provides anecdotal evidence on factories contracting with adults to take work home for their children since work at home was allowed under the terms of the law. Similarly, employers may have been able

[6]The 1986 Act did not impose any penalties on working children or their families. "Rehabilitation" of working children - which involved forcing children into school and transfers - was not legally required until 1996, after our period of study. The National Child Labour Project (NCLP) Scheme (which also involved forced school attendance) was implemented in 1988, but was very small in scope until after our period of study, affecting only 4,205 children from 12 districts by 1995-6 (Indian Ministry of Labour and Employment).

to work around the law through bribes paid to inspectors (a 1996 Human Rights Watch report confirmed that child labor inspectors were "notoriously corrupt and susceptible to bribery") or other means of thwarting the age authentication process: "Fake age certificates are produced in courts claiming the child's age above 14 years. These certificates can be bought for 100 Rs." ('Children exploited in the Land of Glass', Times of India, November 19, 1994; pg. 7) This suggests that whether through official channels (such as the threat of fines and imprisonment) or unofficial channels (such as bribes paid), one effect of the law was to increase the cost of employing children. Moreover, even for the industries/processes where child labor was not explicitly banned (including agricultural work but excluding household enterprises), the 1986 law placed additional limits on child work.[7] For these reasons, the ban may have increased the cost of hiring children (and subsequently lowered child wages) for a broader set of occupations than those listed in the 1986 Act.

At the national level, while there were over 3 million inspections (turning up about 163,000 violations) between 2002 and 2008, only about 45,500 cases were prosecuted and about 8,700 ultimately ended in convictions (IndiaStat). While overall the ban was only partially enforced, the anecdotal evidence on the increased threat of inspections and employers' subsequent responses leads us to believe that the media coverage and publicity surrounding the 1986 Act generated awareness of the law as the government put renewed effort into enforcing it.

3. Theoretical framework

In this section, we briefly describe the intuition of a basic model that illustrates the potential effects of a ban on child labor in the case where there are multiple sectors and market frictions that limit movement of labor between sectors. For a full discussion of the model see the Section 1 in the Appendix. The model setup builds on the one-sector general equilibrium framework established in Basu (2005) and Basu and Van (1998) and the multiple-sector frictionless model established in Edmonds and Shrestha (2012b).

[7]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 should work between 8pm and 7am; and no child should be permitted or required to work overtime.

3.1. **Baseline model: Single sector**

In a one sector model where households make decisions over child labor supply, a standard assumption in the literature is what Basu and Van (1998) call the "luxury axiom." This assumes that households send their children to work only if household income in the absence of child wages is below a subsistence constraint, and then supply only enough child workers to reach the subsistence level. Existing empirical evidence provides strong support for this assumption (Edmonds (2005)). Formally, we can model this through a household utility maximization problem with adult (A) and child (C) labor and a subsistence constraint:

$$\max_{L^A, L^C} u(w^A L^A + w_C L^C) - e(L^A) - Me(L^C) \; s.t. \; w^A L^A + w_C L^C \geq C_0$$

Here $u(.)$ is utility from consumption, C_0 is subsistence consumption, $e(.)$ is the disutility of labor, and M is the additional utility cost that child work imposes, where we assume that M is sufficiently large that in the unconstrained optimum the child would not work at all. Wages are denoted as w and labor supply as L. If adult labor is supplied perfectly inelastically ($L^A = \bar{L^A}$), and adult wages are below subsistence level, then children must also work, and child labor supply will be:

$$L^C = \begin{cases} \frac{C_0 - w^A \bar{L^A}}{w^C} & \text{if } w^A \bar{L^A} < C_0 \\ 0 & \text{else} \end{cases}$$

Child labor supply in this framework can be interpreted as an effect on the intensive margin (L^C is the number of hours worked by the child) or on the extensive margin (where L^C represents the *number* of children who work, the approach taken in Basu and Van (1998)). Provided some child labor is supplied initially, the household's child labor supply function is downward sloping in the child wage.

Whether through official channels (such as the threat of fines and imprisonment) or unofficial channels (such as bribes paid), one effect of an imperfectly enforced ban is to increase the cost of employing children beyond their wage. In other words, the ban acts like a tax on child

wages, driving a wedge between the cost to employers and the wage received by the child. The effect on equilibrium child wage and child labor quantity depends on the elasticity of child labor demand to the child wage given the downward sloping child labor supply. Basu and Van (1998) make a second assumption about labor demand - the "substitution axiom" - that states that adult and child labor are perfect substitutes up to a constant multiplier. In partial equilibrium (holding adult wages fixed), this assumption implies a perfectly elastic demand for child labor, implying that children bear the full incidence of the child labor tax. The tax is fully passed-through to children in the form of lower wages.[8] In general equilibrium, this decrease in child wages will induce more children to work to meet household subsistence needs and this increase in aggregate labor supply puts downward pressure on all (child and adult) wages, further increases the need for child work and ultimately leading to a decline in child wages that is larger than the tax alone.

The "substitution" axiom is stronger than what is needed for a ban/tax to increase child labor, as the necessary condition is that the child labor supply function intersects the child labor demand function from above. This is more likely when adult and child labor are closer substitutes in production such that child labor demand is more elastic with respect to the cost of hiring children. If this condition does not hold, the ban/tax could have the effect that its originators intended, which is to raise the relative cost of hiring children, decrease child labor, and (potentially) increase adult labor demand and wages.

Anker et al. (1998) review five studies on child labor in India and find that children are typically paid by piece rate and that under this payment scheme, children and adults often receive the same rate, suggesting that child and adults are indeed substitutable. Additionally, two recent studies (Doran (2013), Venkateswarlu and Ramakrishna (2010)) provide evidence that policies that successfully reduce child labor can increase adult wages and employment, implying that policies that increase child labor have the potential to lower adult labor demand and hence wages. If adult wages fall in response to the ban because child labor increases, this provides another channel

[8]Under perfect substitutability between child and adult labor, the relative cost of employing children compared to adults remains unchanged even though the wage received by children falls. If we relax the assumption of perfect substitutability between adult and child labor, then effects of a child labor ban become even stronger. In this case, the child wage falls by more than the change in the child labor tax and children actually become cheaper to hire (Kotlikoff and Summers (1987)). This depresses demand for adult labor and hence adult wages, and further increases the need for households to work their children to meet subsistence.

through which the ban may increase child labor besides lowering the wage of children relative to adults. Even households with no children working prior to the ban could theoretically respond to the ban by increasing their supply of child labor if adult wages decrease. These general equilibrium effects are not necessary for the main theoretical prediction that a ban/tax could increase child labor, and we would expect them to be second order empirically. However, accounting for the effects of the ban on the wages of other family members is potentially important as we show next.

3.2. **Heterogeneity in sibling composition and age**

The literature on child labor stresses that sibling composition and age are important predictors of child labor (Manacorda (2006)). We can easily extend the labor supply model above to a two-sibling family, which allows us to make additional points about heterogeneity in the impact of the ban and potential extensive margin and general equilibrium effects. This will also motivate our empirical strategy later.

Suppose the children who were the primary target of the ban (age 10-13) can either have an older (age 14-17), younger (age 6-9), or same age (10-13) sibling. The labor supply function for these children is:

$$L^C = \begin{cases} \frac{C_0 - w^A \bar{L^A} - w^S L^S}{w^C} & \text{if } w^A \bar{L^A} + w^S L^S < C_0 \\ & \text{and sibling is older} \\ \frac{C_0 - w^A \bar{L^A}}{w^C} & \text{if } w^A \bar{L^A} < C_0 \\ & \text{and sibling is younger} \\ 0 & \text{else} \end{cases}$$

where the S superscript now indicates the sibling. This functional form reflects the notion that older children are more likely to work than younger children, so that children only work once their older sibling (if they have one) has entered the labor market.

The ban may increase the labor supply of children who were not previously working (i.e. on the extensive margin) even in the absence of general equilibrium wage effects. If a child's

sibling was previously working and is directly affected by the ban (resulting in lower sibling wages and lower household income), this may induce that child to enter the workforce. This is most likely for children aged 10-13 with same-age siblings (for whom $w^S = w^C$), as those with younger siblings are unlikely to have a working sibling and the wages of older siblings are not directly affected by the ban. Children aged 10-13 who were already working could still increase labor supply on the intensive margin if their own wage falls, but without general equilibrium wage effects, this would be similar regardless of sibling age. Thus the effect of the ban on child labor of targeted children varies with family composition even when their own wage is similarly affected.

In the presence of general equilibrium effects on the wages of adults and older/unaffected children, we could observe extensive margin effects on targeted children in all types of households. If the ban lowers adult wages (and thus household income), those with younger or same-age siblings could start supplying labor. Those with older siblings could also start supplying labor; however, the effect on these children is more ambiguous. On the one hand, if the labor supply of older siblings is inelastic (e.g. if they have already exhausted their intensive margin, as we assume for adults), then children will need to start working to help the household reach subsistence. On the other hand, if older siblings have a more elastic labor supply than adults, they may be able to increase their labor supply by enough to offset income losses without forcing their younger sibling into the labor force.[9]

Summing up, without general equilibrium effects on wages, we expect children aged 10-13 with same age siblings to increase labor on the extensive margin by more than those with younger or older siblings. This remains true when there are general equilibrium effects on wages, and it is possible that children with older siblings even decrease their labor supply in response to the ban if the ban increases the wages of older children and the labor supply of older children is sufficiently elastic. Table 1 summarizes these effects.[10]

[9]If children and adults are not perfect substitutes but older children are closer substitutes to younger children, we would expect the wages of children to fall most and put downward pressure on the demand for older children to a lesser extent than adults. This means that the wages of older children could fall by more than adult wages.

[10]We further discuss the role of sibling age structure and general equilibrium effects and motivate the choice of control groups for our sibling-based empirical strategy in Section 1.1 in the Appendix.

TABLE 1. Sibling-based Effects of an Imperfectly Enforced Ban

Age of sibling	Effect of ban on sibling's wage	Extensive margin effect of ban on own labor supply
6-9 (younger)	None (sibling doesn't work)	Zero (if no GE effects) Positive (if adult wages fall)
10-13 (same-age)	Decrease	Strongly Positive
14-17 (older)	Uncertain	Zero (if no GE effects) Positive or negative depending on w^A and w^{14-17}

3.3. **Two sectors**

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 reduce child labor. Edmonds and Shrestha (2012b) show that in the absence of any labor market frictions, a ban on child labor in one sector has no impact on the overall level of child labor but simply reallocates child labor across sectors. The key intuition behind this result is that households are able to freely adjust on the margin of adult labor. Now consider a setting in which labor markets are imperfect such that there is limited mobility between sectors. In particular, while labor flows freely into one sector, there are some barriers to entering the other. These barriers to entry imply that when the banned sector reduces the wages of children, households are not able to adjust labor allocation across sectors freely to make up for lost family income. As a result, households respond by sending more children into the workforce, and these children may find jobs in banned or non-banned sectors. In summary, when we extend the canonical model of Basu and Van (1998) and Basu (2005) to two sectors with frictions, an imperfectly enforced ban on child labor decreases child wages (particularly in the banned sector). In order to offset the resulting loss of household income, the number of working children may rise. A full theoretical consideration of the two sector case is presented in Section 1 in the Appendix.

4. DATA

The data we use in the paper are from several rounds of the National Sample Survey (NSS) of India. We focus on the employment surveys (Schedule 10) of the NSS data that span the period before and after the ban. These employment rounds include the 38th (January - December 1983), 43rd (July 1987 - June 1988) and 50th (July 1993 - June 1994) rounds. The employment schedule collects information on employment activities and wages in addition to household and individual level demographic data.[11]

In our main analysis, we examine labor supply responses of over 317,000 children between the ages of 6 and 13 who are related to the head of the household. Table 2a presents the household level summary statistics in the rounds before (1983) and after the ban (1987-8, 1993-4); columns (1) and (4) display this information for the entire sample of households, while the remaining columns break down the main sample into "treated" and "control" households. The definition of "treated" and "control" groups is discussed in great detail in Section 5.2 and 5.5. It is worth noting that along most of the dimensions that we are able to measure, "treated" and "control" households appear similar in the pre-ban period and even when they are slightly different, the gap between treated and control households is stable from the pre- to the post-ban period.

The main measures of child time allocation we examine are based on the child's reported "principal usual activity" in the 365 days previous to the survey. These activities are reported as mutually exclusive categories. We define "Any Economic Activity" as any form of child work in any occupation, both within and outside the household, with or without pay, but excluding unpaid household chores performed for one's own family (reported separately as "Unpaid Household Services"). Children "Attending School" are those whose primary activity is attending school. A detailed breakdown of these categories are presented in Appendix Table A2. Columns (1) and (4) of Table 2b give the child level summary statistics for the overall sample of 10-13 year old children separately for the pre-ban and the post-ban periods. Child schooling is rising over time and participation in economic activities is falling. We plot work probabilities by age (for children 10-17, the age range used for our simple difference-in-difference estimates) and sample period in Figure

[11] Additional data from the NSS used in robustness checks is discussed in Section 2.1 of the Appendix.

1. The simple difference-in-difference estimate is apparent even in the raw work probabilities, as those under the age of 14 (i.e. those targeted by the ban) decrease their participation in economic activity by less than those just over the legal age limit.

The remaining columns in Table 2b show the child-level statistics by our sibling-based treatment status where treated children are expected to display a larger response to the ban. Here and in Figure 2, we can observe the foundation for our main result; the economic activity of treated children falls by less than economic activity of control children, after the ban is in place.

Table 2b also shows that the majority of economically active children are engaged in some form of work within the household rather than for an outside employer. About 8-10% of working children are employed in occupations banned under the 1986 Act, depending on the survey period. We classify children as working in banned versus non-banned occupations based on the 3-digit NIC codes reported for each employed child. These are matched to the list of processes and occupations listed as banned in the 1986 Act.[12] The definition of banned occupations that we use explicitly excludes any work in family enterprises or other home-based production, as any work within a family business was considered exempt under the 1986 Act regardless of the NIC code. Conditional on being principally engaged in economic activities, children report working a high number of work days on average (between 6.1 and 6.4 days in the past week) and subsequently very little time spent in other activities such as schooling and unpaid household duties.

5. Empirical Strategy and Results

5.1. Simple DID estimates of the effects of the ban on child time allocation

To assess the overall effects of the 1986 Act on child time allocation, we begin by running the following basic difference-in-difference specification:

$$(1) \qquad \begin{aligned} Y_{it} &= \gamma_0 + \gamma_1 Under14_i + \gamma_2 Post1986_t \\ &\quad + \gamma_3 \left(Under14_i \times Post1986_t\right) + \gamma_X X_{it} + \delta_t + \theta_q + \nu_{it} \end{aligned}$$

[12]Over time, additional occupations and processes have been added to the banned list though very few additions occur between 1986 and 1993. The majority (and more substantive) of the changes occur after 1993, including the prohibition of child employment in domestic work and dhabas (eateries) which were added in October 2006.

Y_{it} represents a measure of child time allocation such as work or schooling for child i in survey round t. $Under14_i$ is a dummy variable for under 14 (legally barred from working after the enactment of the 1986 Act). $Post1986_t$ is a dummy variable that is 1 for all periods after December 1986. In order to ensure that our above- and below-14 age groups are as comparable as possible (while maintaining enough age groups for credible estimation of standard errors) we restrict our estimation sample to those children between the ages of 10 and 17. X_{it} is a vector of household- and child-level covariates, such as characteristics of the household head and fixed effects for gender, state, household size, etc.; a full list of covariates appears in the notes below each table. In practice, we also include age fixed effects, so γ_1 is not separately identified. δ_t represents survey round fixed effects (and thus γ_2 is also not separately identified) and θ_q captures quarterly seasonality through quarterly dummies. In our main results we cluster our standard errors by age-survey round though our results are robust a number of alternate clustering approaches; see Appendix Table A3.

Our coefficient of interest is γ_3, which captures the differential change in child time allocation after the ban is in place, for children under the legal working age versus children of legal working age. The identifying assumption in (1) is that in the absence of the ban, the difference in outcomes for those above age 14 and those below age 14 should be stable over time. Under this assumption, the pre-ban to post-ban shift in relative child time allocation for those under 14 relative to those over 14 – controlling for other observable characteristics, general time trends, and seasonality – is then attributed to the ban. As with any difference-in-difference strategy, the validity of this assumption is essential to interpreting our estimates as capturing the causal effects of the ban.[13] Given that the period under study (1983-1994) was one of substantial change in India, there are reasons to believe that younger and older children may have been on been on differential trajectories, even prior to the 1986 Act. With only one pre-period survey round, we are unable to

[13]An alternative strategy would be to implement a regression discontinuity (RD) design in which we compare children just below the cutoff of age 14 to children just above the cutoff. However, we do not pursue an RD approach for our baseline estimates for several reasons: (i) low number of age bins (8); (ii) steep age gradient in work probabilities, leading to large differences in pre-ban work probabilities "close" to the cutoff (iii) significant heaping in reported ages (not systematically related to the ban).

test for such pre-existing differences. However national-level data from 1977-78 suggest that pre-ban trends in labor force participation were parallel for broad age groups that roughly correspond to the ban's target (Appendix Figure A8); on the other hand, the trends clearly diverge beginning in 1987-8 (the two years immediately after the passage of the Act), when child labor stagnates and remains steady while young adult labor declines. Note that these national data are published in pre-defined age groups (which we are not able to choose ourselves); these age groups are very wide and do not strictly conform to the age restriction of the 1986 Act, but data containing more narrowly defined age groups are not available.[14] Those caveats aside, to the extent that we are able to examine pre-ban trends by age group, we do not see any evidence of non-parallel trajectories.

It is also important to clarify that while the model in Section 3 provides a clear "first stage" prediction for the wage response to the ban (namely that child wages fall relative to adult wages), there is no such prediction for employment. According to the model, the ban increases the costs of hiring children and these costs are passed on to child workers in the form of lower wages. In order to offset the decrease in household income coming from child labor, households may increase the number of working children even if the only available jobs are in the (now lower-paying) banned sector. Thus levels of child labor may rise or fall after an imperfectly enforced ban, *even in the banned sector*.

The results of estimating (1) on the the main sample are displayed in Table 3. The first two columns report the results for the main outcome of interest, "Any Economic Activity", excluding and including additional controls, respectively. While the addition of controls greatly increases precision, the point estimate is very stable across specifications with and without covariates. The coefficient reported in column 2 indicates that the ban was associated with a 2.6 percentage points increased in the probability of participation in economic activity for children *under* the legal working age relative to those aged 14-17 (a 22% increase over the pre-ban mean for children ages 10-13). When we decompose this rise in employment into various categories of work, we find that while the majority of the increase is in non-banned occupations (2.3 percentage point increase, column 4), there is also a small but statistically significant increase in work in banned occupations

[14]Household- and individual-level NSS data are only available starting with the 1983 round. However, national-level data on labor force participation by age group is available for the 1977-78 round of the NSS.

(0.4 percentage point increase, column 3). In other words, though the ban was introduced to lower child labor in a specific set of occupations, these results indicate that the employment of children under the age of 14 (relative to those of legal age) *rose* in exactly those occupations. Columns 5 and 6 illustrate that the majority of the increase in employment is in paid work, consistent with the idea that households are in greater need for income generated by child work as a result of the ban. There is a small and statistically insignificant association for school attendance (column 7), and the ban seems to have decreased the incidence of unpaid household services (column 8).[15]

While strong assumptions are needed to interpret the coefficients reported in Table 3 as causal effects of the ban, the simple estimates are robust to some important checks. First, we estimate (1) using samples of children with increasingly narrow age ranges. The idea is that the narrower the age band, the more likely the data are to satisfy the parallel trends assumption and the less concerned we are with potential "floors" in child work, i.e. the possibility that child employment for those under 14 is mechanically less likely to fall because it starts from a lower pre-ban level. Appendix Table A4 shows that the point estimate remains positive and significant for all but the narrowest age range, the sample that contains only 13 and 14 year olds. When we use the narrowest sample, the point estimate remains positive but is considerably smaller and no longer statistically significant.

Next, we estimate (1) using a narrower time window around the implementation of the ban. The simple estimates are robust to shortening the period of study to 1983-1988 (rather than 1983-1994) by using only one of the post-ban rounds (Appendix Table A5). Another way to narrow the sample period is to use the 42nd round of the NSS which spans the six months immediately before and after the ban. We do not use the 42nd round in our main analysis because the definition of economic activity in the 42nd NSS round differs substantively from the definition in the other rounds; specifically it does not contain any information on economic activity for any child currently enrolled in school regardless of principal activity (see the Appendix Section 2.1 for more details).

[15]Schooling and work are not the only activities that children may engage in (though in our data, all children report a principal usual activity). There is a substantial literature on "idle" children, i.e. those who are neither in school nor in economic activities (see for example Edmonds and Pavcnik (2005a), Biggeri et al. (2003) and Bacolod and Ranjan (2008)). As discussed in Edmonds et al. (2010), policies affecting child labor may also impact idleness of children.

The results of estimating (1) on the 42nd round are displayed in Appendix Table A6.[16] The effect of the ban on child economic activity is positive but much smaller than in the main sample (0.3 percentage points, Column 2) and not statistically significant. We do find a small, statistically significant increase in paid work (Column 5) but not for any other measure of economic activity.

5.2. **Sibling-based estimates of the effects of the ban on child time allocation**

To strengthen our empirical approach and tie our empirical results more strongly to the theoretical predictions outlined in Section 3, we now turn to our main estimating strategy which uses sibling ages to identify the impact of the ban. According to the model, a ban reduces wages for children under the legal working age. For households reliant on child labor income, this decrease results in a pure income effect on the labor supply of *siblings* of working children. Recall that in the Basu (2005) and Basu and Van (1998) frameworks, child labor supply is modeled on the extensive margin so the most direct interpretation of these theories is that the ban reduces wages paid to working children, and in response, their siblings (who were previously not working) must enter the workforce to make up for the decrease in household income. Thus we expect that the effects of the ban on child employment through this channel should be largest in households that depend on child labor, i.e. households with working children under the legal age.[17]

To isolate the income channel empirically, the basic design would be to compare the employment status of children of the same age with working *siblings* between ages 10-13 to those with working *siblings* over the age of 14. However, we do not pursue this naive approach because the work status of siblings is endogenous to the ban. Instead, we rely on the age of siblings as a proxy for endogenous "treatment" of having a working sibling who becomes age ineligible for work as the result of the ban. Specifically the regressions we use to capture the sibling-based

[16]Narrowing the analysis to a short period of time also limits the influence of potential "floors" in child work, as we do not observe large changes in child labor for either age group in this period.

[17]Note that in the Basu (2005) and Basu and Van (1998) models, adult wages fall in equilibrium in response to the ban. This means that household income falls in all households, regardless of whether there is a working child in the household. The strategy laid out in this section identifies the differential effect of the ban on children with siblings likely to be working.

effects of the ban are of the form

$$(2)\quad Y_{it} = \beta_1 SibAge10-13_i + \beta_2 Post1986_t + \beta_3\left(SibAge10-13_i \times Post1986_t\right) + \beta_X X_{it} + \sum_{a=0}^{25}(\mu_a \times Number\ of\ HH\ Members\ of Age\ a) + \delta_t + \theta_q + \varepsilon_{it}$$

where Y_{it}, $Post1986_t$, X_{it}, δ_t, and θ_q are as defined in equation 1. Recall that X_{it} includes (own) age fixed effects; in order to flexibly control for the direct effects of sibling age composition, we also include in equation 2 a separate variable for each age (0-25) that counts the number of related household members of that age, and the total number of household members. Our standard set of controls also includes the number of adult females. We estimate this regression for all children with siblings aged 6-17.

$SibAge10-13_i$ 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 (i.e., strictly under 14) *and* likely to be working, which we define to be a sibling at least 10. In 1983 only 1.6% of children under the age of 10 are working as compared to 19.5% children ages 10-17; consequently children whose siblings are all (strictly) under age 10 are unlikely to be affected by the ban simply because their siblings are not likely to be working. Defining our "treatment" variable in this way ensures that our "control" children are those that could have older *or* younger siblings (i.e. any children without siblings in the "treatment" age range of 10-13). Moreover, Section 3.2 explains that when there are no general equilibrium effects of the ban and the main effects only operate through the income earned by targeted children, the composition of the control group (i.e. using children with younger or older siblings) is irrelevant. In Section 1.1 in the Appendix we show that if general equilibrium effects are important, using a comparison group children with both older and younger siblings helps isolate the direct effects of the ban that operate through a loss in child wage income. Thus our main specification in Table 4 includes a comparison group of 10-13 year olds with both older and younger siblings. However for robustness we also consider samples restricting the sibling age range to 10-17 (such that the comparison group only has older siblings aged 14-17) or 6-13 (such that the comparison group only has younger siblings aged 6-9) in Table 5 and Table 6 respectively.

To allow for differential effects of the ban for very young children, we estimate equation (2) separately for 10-13 year olds and 6-9 year olds. The standard errors for the sibling-based estimates 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 correlated across children within the same household. However, the results discussed in this section are very robust to allowing for spatially concentrated unobserved shocks (state-level clustering).

It is important to note that the identifying assumptions needed for the sibling-based regressions are potentially much weaker than those needed for estimating the overall impact of the ban (equation 1). The assumption in the sibling-based regressions is that in the absence of the ban, the difference in outcomes for those with and without siblings aged 10-13 will be stable over time, conditional on own age. In other words, our sibling-based regressions are comparing the changes in time allocation for children of the *same age* who happen to have siblings of slightly different ages (either older or younger). This strategy allows us to construct a much more similar "control" set of children who are less likely to be on a differential trajectory in the absence of the ban or to be affected by the ban through channels other than the income effect predicted by the model.[18] Moreover, because of the way we define "treated" children (having a sibling ages 10-13) and the symmetric way in which we choose to define siblings, our "control" set includes children with both younger *and* older siblings, making it unlikely that our results are simply capturing changes over time that affect older and younger families differently; nonetheless, we examine and rule out other potential differences due to sibling age and spacing in Section 6.1. Finally, since our sibling regressions compare children of the same age and thus the same pre-ban likelihood of work, we are not concerned with any potential "floor" effects, i.e. the possibility that child employment for those under 14 is mechanically less likely to fall because it starts from a lower pre-ban level.

Table 4 displays the results for estimating our main specification in (2) for both the very young (ages 6-9; Panel A) and the young (ages 10-13; Panel B) samples of children. We find that through the sibling channel, the ban increases the likelihood of a child engaging in any form of work by 0.6 percentage points for the very young and by 0.8 percentage points for young children

[18]See Bugni (2012) for a discussion of the issues encountered while estimating difference in difference models when the "control" group is also affected.

(column 1 in both panels). These represent 37.5% and 7% increases over the pre-ban mean, respectively. As with the overall estimates, the sibling-based estimates are larger for work in non-banned occupations than in banned occupations (columns 2 and 3). In fact the effect on work in banned occupations is a precisely estimated zero. Most of the increase in employment comes from work in household production (column 4). We observe negative effects of the ban on attending school for both age groups, though the effect is larger and statistically significant for only the very young (column 6). Time spent in unpaid household services increases for the very young but not for the young (column 7).

Tables 5 and 6 repeat the same analysis but vary the sibling age groups used for control children (using only children with older and younger siblings, respectively). The results are remarkably similar for all of our economic activity measures. Only the effects on school attendance and unpaid household service for 6-9 year old children differ significantly when we vary the sibling age-composition of comparison group, in a manner consistent with lower marginality of 6-9 year olds when they have a treated sibling (10-13) and have older sibling composition (10-17) rather than younger (6-13). Altogether these results suggest that the main effect of the ban operates through its effect on the income of families with working targeted children, rather than through general equilibrium effects that interact in subtle ways with sibling age-composition.

While the effects of the ban may initially seem large the magnitudes are plausible for several reasons. First, as discussed in Section 2, though the age restriction officially applied to only a certain set of industries, the 1986 Act also imposed a number of regulations on child labor that were likely to raise the cost of hiring children even in industries where they were legally allowed to work. Therefore, the Act may have affected a wider set of working children than just those working in industries subject to the age restriction for employment.

Second, it is possible that these estimates capture both intensive and extensive margin changes in economic activity. This is because child time allocations are based on *principal* activity; some of the increases in economic activity reflected in Tables 3 and 4 could be due to children who were primarily focused on non-economic activities (unpaid household services, school, etc.) before the ban but shifted on the intensive margin in a way such that economic activities became

the principal activity in the post-ban period. Appendix Figure A7 gives supportive evidence for this interpretation. It shows that among children aged 10-13 in the pre-ban period whose principal usual activity is *not* working, there are a number of children who work between 1-6 days per week; if the ban were to induce these children to work even a few more days, their principal activity may switch to work and thus be captured in our main results. To investigate this further, we show the results of estimating equation 2 using days spent in each activity in the previous week as outcomes in Appendix Table A7 (though we interpret these results with caution, given the heaping in the distributions of the outcome variables seen in Appendix Figures A5 and A6). The qualitative patterns in the results mirror those of Table 4, but the magnitudes suggest small average changes in days worked in response to the ban. For children ages 10-13 (ages 6-9), the ban increases the days spent working in the previous week by only 0.06 days (0.025 days) on average but if the effect were concentrated among those "marginal" children who spent some time working but whose principal status in the pre-ban period was *not* working, it is plausible that it would be enough to induce a change in principal status.

5.3. **Heterogeneity**

Though the 1986 Act was applied nationally, there is reason to expect that the ban had heterogeneous effects across geographical areas and household types. In particular, we expect the impacts of the ban to be larger in areas where the banned industries were more crucial to local labor markets. The theoretical models from Section 3 also indicate that the effects of a ban should be stronger the higher the probability of detection by inspectors and for households that are closer to the margin of subsistence. In order to tie our empirical results to the theoretical model and to rule out potential confounding factors (such as differential time trends for younger and older children, other national events or policies that may have occurred around the time of the ban, as well as "floors" in child work discussed in Section 5.1), we now turn to methods that exploit geographical and household variation specific to the ban.

Our first measure of geographical heterogeneity calculates the importance of the ban as the proportion of households in each region that are principally engaged in an industry which is

listed in the 1986 Act. In particular, we use the *pre-ban* data to calculate the proportion of households within each NSS region that derive income mostly from banned industries to capture the importance of banned industries to local labor markets. We use regions because they are the most disaggregated geographical units we can link consistently over time – regions are more disaggregated than states (there are 77 regions but only 31 states), and more disaggregated district-level identifiers are missing for 1983 and for rural areas in 1993-1994. As can be seen in Table 2a, about 11.4% of households nationally are primarily engaged in activities listed under the 1986 Act but at the region level this ranges from 1.5% to 31.5%, indicating considerable geographical heterogeneity. We classify regions into high and low importance regions based on the pre-ban median of the importance measure across all regions. Columns 1 and 2 of Table 7 display the results of estimating equations (1) and (2) in a triple difference approach, with additional interaction terms to capture the differential effect of the ban in regions with high (pre-ban) importance of banned industries.[19] As we are now using region-level variation to identify the effect of the ban, standard errors are clustered at the region-level. In regions where banned industries are likely to be more important to the local labor market, the overall effect of the ban is 0.3 percentage points larger but this difference is not statistically significant at conventional levels; in those same regions, the sibling-based effect is 1.3 percentage points larger (significant at the 5% level). In fact there is no statistically significant sibling effect in regions with low importance of banned industries.

The second measure of geographical heterogeneity we employ is based on the probability of detection faced by employers who hire child labor. To calculate this, we make use of state-level data on the (total) number of inspections for illegal child employment during the period 1997-2005. We scale the number of inspections by dividing by the the number of children working as of 1983 in occupations that would be banned under the 1986 Act. We then separate states into high (above median) and low (below median) enforcement states based on this measure. One potential issue with this measure of enforcement is that the data on inspections is collected well after the employment data we use for this analysis. Thus the main caveat involved with this measure is that we need to assume that the *ranking* of states along this measure of importance does not change

[19] All regressions include interactions between the measure of heterogeneity and all controls.

between 1986 and 2005 (in particular, the classification of states as above- or below-median). Columns 3 and 4 of Table 7 show that the ban significantly increases the probability of economic activity for those under the legal working age (relative to those of legal working age), and this effect is 1.5 percentage points higher in states where the probability of detection is high (significant at the 10% level).[20] Similarly, when the probability of inspection is high, the sibling-based effect is large (1.4 percentage points; significant at the 5% level), and there is no effect in low-inspection states.

A third measure of geographical heterogeneity we employ is based on the proportion of the region that is urban. The idea here is that the types of activities that were banned are more common in urban areas, which we can verify in our data.[21] It is also possible that enforcement of child labor laws is higher in urban areas compared to rural areas. We first calculate the proportion of each region classified as rural or urban in 1983 using the NSS. We do not directly use rural and urban classifications because these can change over time for the exact same location between censuses due to urban sprawl and re-classification[22], and urban areas within a region could be easily accessible from rural regions. Our region-based classification consistently measures the cross-sectional level of urbanization for the same set of locations and potential access to employment in banned industries in urban areas. We split regions into those with above and below median urbanization in 1983. The results of the triple-difference regression, displayed in Columns 5 and 6 of Table 7, are consistent with a larger effect of the ban in regions that are more urbanized for both overall or sibling based specifications, but the triple-interaction terms are not statistically significant.

In the canonical Basu (2005) and Basu and Van (1998) models, 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

[20]The correlation between the importance and probability of inspection indicators is 0.531. Pre-ban proportions of child workers at the state level are *not* highly correlated with the importance or detection measures (correlations are 0.310 and 0.046, respectively); the same is true for child work specifically in banned industries.

[21]Note however that child labor is higher in absolute and proportional levels in rural areas and employment in banned industries is higher in absolute levels in rural areas as well.

[22]See Hnatkovska and Lahiri (2016) for more indirect evidence that a large part of urban growth in India as measured in the NSS can be explained by re-classification.

are those with low incomes. In Table 8, we show that for various proxies of household income (education of the household head, non-staple share of foods consumed, and scheduled caste status) the effects of the ban are considerably larger in magnitude for households that appear to be poorer (though the level statistical significance varies across measures used). Admittedly the evidence of heterogeneous impacts of the ban is only suggestive of household income as a channel for the effect of the ban, as these proxies for income could be correlated with other attributes of the household. Nonetheless we believe that the weight of the evidence in this section favors the interpretation that, as predicted by the theoretical model, those households closer to the margin of subsistence are affected by the ban to a greater degree than those well above the subsistence threshold.

5.4. **Effect of the ban on wages**

Though the model in Section 3 does not predict a "first stage" or effect of the ban on employment, it does imply that the immediate impact of the ban is to increase the costs of hiring children and therefore decrease child wages relative to adult wages. In the NSS, wages are only reported for those engaged in regular or casual labor. Notably, this excludes children working in home enterprises and farms. Therefore the wage regressions are subject to the usual caveat – particularly important in developing countries and when examining wages of children – that they only apply to a select subsample of workers. 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 theoretical models in Basu (2005) and Basu and Van (1998). In Table 9 we see a substantial drop in child wages relative to adult wages after the ban is in place; once we control for observable individual characteristics the ban reduces child wages by 7.8% on average (column 2). Due to the very low number of wage observations, we widen our age band for these wage specifications only; the size of our main estimation sample (ages 10-17) drops by 96% when we restrict it to observations with reported wages. Columns 3-6 of Table 9 illustrate that these wage results are robust to narrower age ranges, though with some loss in precision for the narrowest band (p-value = 0.109).

One interpretation of these results that is consistent with the model in Section 3 is that the ban raised the costs of hiring children (for example, because employers paid bribes to inspectors)

and that these costs were passed on to child workers in the form of lower wages. Why do child wages fall by so much, given that so few children work in banned industries and thus so few employers are likely to face higher costs under the ban? First, it is important to recognize that while overall child employment in banned industries is low (8% of economically active children aged 10-13 and only 0.9% of children overall; Table 2b) as a proportion of *paid* child workers, children in banned industries represent a sizable group (23.8% of paid child workers aged 10-13). Thus when we restrict our attention to wage earners, we expect the ban to have a considerable impact even if overall child employment in banned activities is low. Second, the 1986 Act also imposed costs on employers of *legal* child workers (see Section 2) and therefore we may see average child wages fall relative to adult wages even in non-banned sectors.

Another potential explanation for our wage findings is a wage-earning workforce composition effect due to the rapid economic growth India experienced during the period under study. We might expect households to withdraw the least economically productive children as their incomes rise. If this type of positive selection on skills into paid work outside the household becomes stronger over time then any composition effect should work in the opposite direction of the ban; in other words, as the composition of children in the paid workforce favors more skilled children, the smaller the difference between child and adult wages we should observe. On the other hand, if we believe that the selection into paid work outside the household is negative (i.e. the lesser skilled children are more likely to engage in the paid workforce) *and* this negative selection becomes stronger over time, our estimated impact of the ban on wages could be confounded with this compositional change. However, to the best extent that we are able to measure skill (using education) we find no evidence of that changes over time in selection along this dimension drives our results (Appendix Table A8). These results also help us rule out the possibility that our wage results capture an average decline in wages for children due to skill-biased technical change. If skills are positively correlated with age, we 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. However, the results in Appendix Table A8 are not consistent with this explanation.[23]

5.5. **Effect of the ban on household outcomes**

We next turn to the impact of the child labor ban on various indicators of household welfare. The net effect of the ban – lower child wages and subsequent increase in supply of child work – on household income and consumption in our empirical context is unclear for several reasons. First, households that are unable to increase child labor supply could experience a decline in consumption to below "subsistence" levels. Second, if one of the responses to the ban was a shift from banned wage labor to household enterprise labor, we cannot observe the implicit wage and whether it declines like the market wage following the ban. Third, if households have other mechanisms for dealing with a drop in income due to the child labor ban, such as selling assets or reallocating expenditures across different types of goods, we may observe the effect of the child labor ban along some dimensions (declining assets, declining expenditures, or declining food quality) but not along others (such as the per capita calorie intake of the household).

Our approach is thus to look at changes along multiple components of household welfare for households that are more or less affected by the ban. We construct five household-level welfare measures as described in the data section (and described in greater detail in the Section 2.3 in the Appendix). Given that our welfare measures are at the household level, we define "treated" households as households that have at least one child between the ages of 10-13, which is similar to our sibling-based definition of treatment in equation 2. We use the sample of all households with children ages 6-17, as this represents the closest analogue to our sibling-based strategy (where households with only younger or older children form the control group).[24]

In Table 10, we find a negative point estimate of the ban's effect on all households outcomes, with the exception of caloric intakes. The effects of the ban are statistically significant

[23]These results are consistent with earlier findings that the return to education in India did not rise during this period (Dutta (2006) and Bargain et al. (2009)).

[24]For an alternative sample, we consider only families of children who appear in the sibling-based DID samples, i.e. children who are aged 6-13 with at least one sibling age 6-17. We present the results for this smaller sample in Appendix Table A9.

for both our indicator of the quality of calories (1 - Staple Share of Calories) and the asset index (columns 4 and 5) though they are small in magnitude. After the ban, non-staple foods make up about a 0.3 percentage point smaller share of affected households' diets (about a 1% change over the pre-ban mean). The asset index falls by 0.032 for affected households; this represents about a 0.016 standard deviation change. While these household-level impacts may initially seem small, it is important to keep in mind that, similar to the design of the sibling-based regressions, the household regressions capture an intent-to-treat effect of the ban as we are using the age of children in the household as a proxy for being directly impacted by the ban. In the pre-ban period, about 10% of households have at least one working child under the age of 14, suggesting that the implied treatment-on-the-treated could be up to a magnitude of order larger. Nonetheless we see these (precisely estimated) household results as primarily allowing us to rule out the possibility of welfare-improving effects of the 1986 ban. The lack of large changes in household consumption is in line with the model, which suggests that even when households send another child into the market, it is only to reach target subsistence.

6. Robustness checks

6.1. **Accounting for differential effects of sibling age structure and birth spacing over time**

Our sibling-based specification (2) compares children of the same age and therefore will not capture pre-existing secular trends in (own) age over time. To rule out the possibility that our results are contaminated by pre-existing trends in *sibling* age structure over time, we conduct several additional checks. First, in Appendix Table A11 we see that when we allow for the effect of sibling age to vary by treatment status and survey round, the main sibling-based results very robust.[25] In Appendix Table A12, we falsely define sibling-based treatment as having a sibling aged 1-4, 5-9, or 14-17; the results of this placebo test indicate no significant false "effect" of the ban. Finally, in Appendix Table A13 we show that our results are not simply due to differential trends by birth spacing over time. Column 1 restricts the sample to children whose nearest sibling

[25]We control for sibling age linearly in two ways. First, we use the age of the "treatment-generating sibling", i.e. the closest to the 13/14 cutoff (using the age of younger siblings in the case of ties). Second we use the age of the nearest sibling in terms of the absolute difference in years between sibling age and own age.

is within 3 years of age to account for the possibility that treated children are more closely spaced than control children. Columns 2-4 adds controls for age gaps (on average between all children and between each child and her oldest and youngest sibling) and their interactions with the post-ban dummy to allow the effects of spacing to vary over time. The estimated impact of the ban generally remains similar in magnitude and statistical significance, though in column 2, we can see that when we allow the effect of the average age gap among all children in the household to vary over time, the estimated effect of the ban decreases in magnitude to 0.005 and is no longer statistically significant at conventional levels (p-value=0.145).

6.2. Accounting for differential effects of economic growth and state-level policies by age group

To address the possible confounding effects of economic growth and/or state-level policies that may have affected younger children differently than older children, we perform several additional checks. First, we include interactions between time-varying state GDP measures and an indicator for having a sibling under 14. As columns 1 and 2 of Appendix Table A16 indicate, allowing for these differential effects of economic growth on treated and control groups does not change our estimates of the impact of the ban. This provides further reassurance that economic growth in India over the 1983-1994 period – even to the extent it led to to differential trends in child employment for treated and control children – does not drive all of our estimated effects of the ban.

Next, we consider two specific policy changes that may be of particular importance when studying child employment. The first is changes to other labor laws. Importantly, other national labor laws that would be pertinent to our case did *not* have age specific restrictions and were passed before 1983; for example, 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. In terms of state level labor policies, we examine changes to classifications as defined in Besley and Burgess (2004), which categorizes states as pro-worker, pro-employer or neutral. We find that only 3 out of 16 states in Besley-Burgess sample change classification between 1983-1994. When we restrict our sample to only those states without changes in these classifications,

the estimated effects of the ban are very similar to the baseline estimates in Table 4; see columns 3 and 4 Appendix Table A16.

Another policy that deserves further attention is the National Policy on Education that was amended and implemented in 1986. This policy sought to improve educational achievement and enrollment for all ages, with a particular focus on primary education via "Operation Blackboard" (see Chin (2005) for more details). We show that our results are unchanged by restricting our sample to states less affected by "Operation Blackboard" in columns 5 and 6 of Appendix Table A16.

6.3. **Measurement error and misreporting**

With survey data, there is scope for measurement error in the reporting of child activities, especially with respect to child labor. We investigate this concern in Figures A9 and A10 of the 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. 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 banned occupations), thus it appears that the ban does not impact misreporting by parents. This does not rule out conventional (non-systematic) measurement error in reported age, which would serve to attenuate our estimates.

To further illustrate that our results are not affected by strategic reporting of ages, we also re-run our main specifications on the restricted sample of children omitting those who are exactly age 13 or 14 (or who have siblings aged 13 or 14), i.e. those whose ages are most likely to be reported strategically in response to the ban. Both the simple and sibling-based estimates are very similar in this sample (see Appendix Tables A17 and A18).

7. Conclusion

This paper does not intend to suggest that *all* child labor bans are ineffective. In fact, well formulated and implemented bans could help in eliminating child labor;[26] but the key question

[26]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 or to increase enforcement.

in this area is how a decrease in child labor affects child and household welfare (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 this paper addresses some of the short run consequences of the 1986 ban, we only scratch the surface of the much larger question related to household welfare. Future research on child labor bans could focus on long run effects such as human capital accumulation (as in Piza (2014)), and other measures related to household and child welfare. 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 imperfectly implemented child labor bans *alone* can be ineffective. Our results highlight the importance of taking into account imperfect enforcement and behavior at the margin of subsistence when formulating important policies in developing countries. An approach that combines bans with other poverty alleviation strategies might be more effective in tackling the issue of child work.

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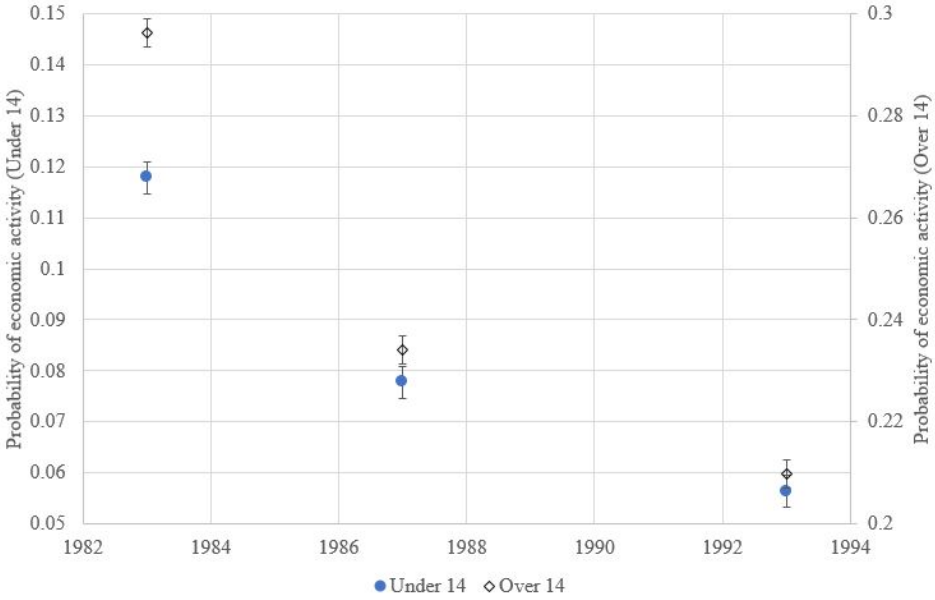
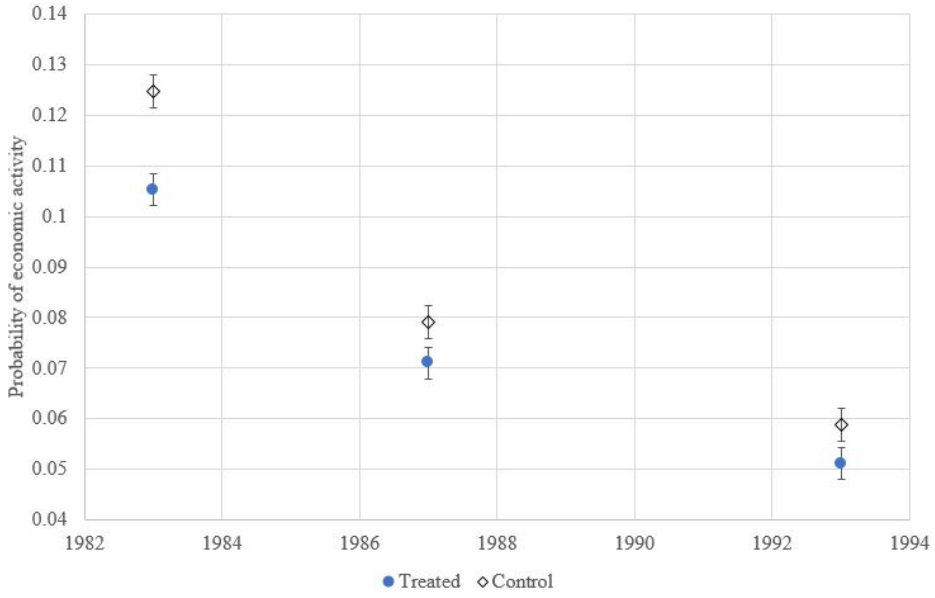
FIGURES AND TABLES

FIGURE 1. Probability of economic activity by age, pre- and post-1986.



Sample includes all children of the household head ages 10-17.

FIGURE 2. Probability of economic activity by sibling-based treatment status, pre- and post-1986.



Sample includes all children of the household head ages 10-13 with at least one sibling age 6-17. Treated children are those with at least 1 sibling age 10-13.

TABLE 2A. Summary statistics: Means of child variables by sibling treatment status and period

	1983			1987-8, 1993-4		
	All	Control	Treated	All	Control	Treated
	(1)	(2)	(3)	(4)	(5)	(6)
Family Size	6.94	6.64	7.00	6.74	6.57	6.77
Head Age	44.8	43.4	45.0	44.6	43.4	44.9
Head Is Male	0.924	0.932	0.923	0.924	0.928	0.923
Head Has No Education	0.511	0.502	0.512	0.428	0.442	0.425
Head Has at least Some Primary Eduction	0.264	0.253	0.267	0.271	0.264	0.273
Head Has Middle Eduction	0.103	0.107	0.102	0.120	0.114	0.121
Head Has Secondary Education or More	0.122	0.138	0.119	0.181	0.180	0.181
Hindu Household	0.776	0.782	0.774	0.767	0.770	0.767
Urban Area	0.318	0.321	0.317	0.328	0.327	0.329
Real Monthly Expenditure per capita	131.9	136.8	130.9	137.4	134.9	138.0
Food Expenditure per capita	69.2	69.8	69.1	81.7	80.2	82.0
Calories per capita	2159.9	2130.8	2165.8	2173.6	2115.8	2185.8
Staple Share of Calories	0.713	0.708	0.713	0.680	0.676	0.681
Asset Index	-0.715	-0.707	-0.716	0.137	0.106	0.144
Principal Industry is Banned	0.114	0.121	0.113	0.120	0.122	0.120
Number of observations	50232	8458	41774	92423	16491	77945

"Treated" households are defined as having at least 1 child aged 10-13. Real values (expressed in 1982 rupees) are nominal values deflated by the average wholesale price index reported by the Government of India for the respective year. Sample consists of all households with at least one child aged 6-13 who also has at least one sibling in the age range 6-17.

TABLE 2B. Summary statistics: Means of child variables by sibling treatment status and period

	Ages 10-13					
	1983			1987-8, 1993-4		
	All	Control	Treated	All	Control	Treated
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Children						
Male	0.528	0.535	0.522	0.532	0.542	0.524
Has a working sibling under age 14	0.068	0.016	0.116	0.038	0.006	0.068
(Principally) Attending School	0.613	0.594	0.630	0.737	0.728	0.746
(Principally Engaged in) Any Economic Activity	0.115	0.125	0.105	0.066	0.070	0.062
(Principally Performing) Unpaid Household Services	0.121	0.126	0.116	0.065	0.068	0.062
Days Spent in Econ Act in Past Week	0.758	0.821	0.699	0.461	0.486	0.437
Days Spent in School in Past Week	4.191	4.064	4.308	5.032	4.975	5.085
Days Spent in Unpaid HH Services in Past Week	0.886	0.926	0.850	0.473	0.500	0.447
Number of observations	57263	27599	29664	106937	51374	55563
Panel B: Children Principally Engaged in Economic Activities						
Engaged in Unpaid Economic Activity	0.660	0.667	0.653	0.627	0.632	0.623
Engaged in Paid Employment	0.340	0.333	0.347	0.373	0.368	0.377
Employment in banned industry	0.080	0.079	0.080	0.099	0.096	0.101
Days Spent in Any Econ. Act. in Past Week	6.095	6.116	6.072	6.279	6.267	6.291
Days Spent in School in Past Week	0.017	0.011	0.024	0.036	0.049	0.022
Days Spent in Unpaid HH Services in Past Week	0.457	0.457	0.457	0.274	0.276	0.273
Real Daily Wages (1982 Rupees)	3.732	3.871	3.581	2.826	2.838	2.814
Number of observations	6563	3440	3123	7044	3549	3445

"Treated" children are defined as having at least 1 sibling aged 10-13. Real values (expressed in 1982 rupees) are nominal values deflated by the average wholesale price index reported by the Government of India for the respective year. Sample consists of all individuals related to the household head with at least 1 other (related) household member age 6-17.

TABLE 2C. Summary statistics: Means of child variables by sibling treatment status and period

	Ages 6-9					
	1983			1987-8, 1993-4		
	All	Control	Treated	All	Control	Treated
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Children						
Male	0.519	0.513	0.521	0.521	0.521	0.521
Has a working sibling under age 14	0.118	0.019	0.153	0.067	0.008	0.091
(Principally) Attending School	0.579	0.587	0.577	0.586	0.616	0.574
(Principally Engaged in) Any Economic Activity	0.016	0.022	0.014	0.006	0.008	0.006
(Principally Performing) Unpaid Household Services	0.021	0.026	0.020	0.006	0.008	0.006
Days Spent in Econ Act in Past Week	0.112	0.156	0.097	0.059	0.074	0.053
Days Spent in School in Past Week	4.026	4.101	4.000	4.820	4.930	4.775
Days Spent in Unpaid HH Services in Past Week	0.161	0.192	0.150	0.064	0.082	0.056
Number of observations	54291	14145	40146	98591	28532	70059
Panel B: Children Principally Engaged in Economic Activities						
Engaged in Unpaid Economic Activity	0.817	0.820	0.815	0.712	0.662	0.740
Engaged in Paid Employment	0.183	0.180	0.185	0.288	0.338	0.260
Employment in banned industry	0.042	0.049	0.038	0.078	0.069	0.083
Days Spent in Any Econ. Act. in Past Week	6.330	6.339	6.324	6.342	6.222	6.409
Days Spent in School in Past Week	0.085	0.122	0.063	0.193	0.156	0.214
Days Spent in Unpaid HH Services in Past Week	0.244	0.281	0.223	0.122	0.189	0.085
Real Daily Wages (1982 Rupees)	2.983	3.577	2.659	3.072	3.536	2.735
Number of observations	863	311	552	625	225	400

"Treated" children are defined as having at least 1 sibling aged 10-13. Real values (expressed in 1982 rupees) are nominal values deflated by the average wholesale price index reported by the Government of India for the respective year. Sample consists of all individuals related to the household head with at least 1 other (related) household member age 6-17.

TABLE 3. Simple Estimates of the Effects of the Ban on Child Time Allocation

	Any Economic Activity (1)	Any Economic Activity (2)	Employment in Banned Occ. (3)	Employment in Non-Banned Occ. (4)	Unpaid Economic Activity (5)	Paid Employment (6)	Attending School (7)	Unpaid Household Services (8)
Under14XPost	0.024	0.026***	0.004***	0.023***	0.007*	0.019***	0.008	-0.009**
	(0.040)	(0.005)	(0.001)	(0.005)	(0.003)	(0.002)	(0.007)	(0.004)
Pre-Ban Mean of Dep. Var.	0.118	0.118	0.009	0.108	0.077	0.041	0.604	0.124
Observations	327,233	327,233	326,768	326,768	327,233	327,233	327,233	327,233
R-squared	0.055	0.182	0.030	0.162	0.093	0.099	0.303	0.211
Controls?	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*** p<0.01, ** p<0.05, * p<0.1 "Under14" is a dummy variable that takes the value of 1 if the child is under 14 years of age. Column 1 includes only a Post-ban dummy, the "Under14" dummy, and an interaction between "Under14" and Post. Controls: gender, gender of household head, age of household head, urban status, number of adult females, number of male children, number of female children, number of children under 5, number of children ages 6-9 as well as the following fixed effects: age, family size, household head's education level, religion, survey round, survey quarter, state. Sample consists of all individuals related to the household head aged 10-17. Standard errors are clustered by age-survey round. Pre-Ban mean is for children under the age of 14 only. Columns 4 and 5: Smaller sample sizes are due to missing NIC codes. Employment in non-banned occupations includes all unpaid economic activity within the household and paid employment in non-banned occupations.

TABLE 4. Sibling-based Estimates of the Effects of the Ban on Child Time Allocation

Panel A: Children Ages 6-9							
	Any Economic Activity (1)	Employment in Banned Occ. (2)	Employment in Non-Banned Occ. (3)	Unpaid Econ. Activity (4)	Paid Employment (5)	Attending School (6)	Unpaid Household Services (7)
SibAge10-13XPost	0.006***	0.000	0.006***	0.006***	0.000	-0.027***	0.004**
	(0.002)	(0.000)	(0.001)	(0.001)	(0.001)	(0.006)	(0.002)
Pre-Ban Mean of Dep. Var.	0.016	0.001	0.015	0.013	0.003	0.579	0.021
Observations	152,882	152,850	152,850	152,882	152,882	152,882	152,882
R-squared	0.025	0.003	0.024	0.020	0.007	0.324	0.024
Panel B: Children Ages 10-13							
	Any Economic Activity (1)	Employment in Banned Occ. (2)	Employment in Non-Banned Occ. (3)	Unpaid Econ. Activity (4)	Paid Employment (5)	Attending School (6)	Unpaid Household Services (7)
SibAge10-13XPost	0.008***	0.001	0.008***	0.007***	0.001	-0.007	-0.000
	(0.003)	(0.001)	(0.003)	(0.003)	(0.002)	(0.005)	(0.003)
Pre-Ban Mean of Dep. Var.	0.115	0.009	0.105	0.076	0.039	0.613	0.121
Observations	164,200	164,084	164,084	164,200	164,200	164,200	164,200
R-squared	0.105	0.015	0.099	0.064	0.052	0.272	0.131

*** p<0.01, ** p<0.05, * p<0.1 "SibUnder14" is a dummy variable for whether the child has at least 1 sibling age 10-13. Controls: gender, gender of household head, age of household head, urban status, number of adult females, number of male children, number of female children, as well as the following fixed effects: (own) age, family size, household head's education level, religion, survey round, survey quarter, state. Additionally we include controls for sibling age as follows: for *each* age 0-25, we create a separate variable which counts the number household members of that specific age. Sample consists of all individuals related to the household head with at least 1 other (related) household member age 6-17. Standard errors are clustered by household.

TABLE 5. Sibling-based Estimates using only Children with Siblings ages 10-17

Panel A: Children Ages 6-9							
	Any Economic Activity (1)	Employment in Banned Occ. (2)	Employment in Non-Banned Occ. (3)	Unpaid Econ. Activity (4)	Paid Employment (5)	Attending School (6)	Unpaid Household Services (7)
SibAge10-13XPost	0.006**	0.000	0.006**	0.007***	-0.001	-0.001	0.004
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.008)	(0.003)
Pre-Ban Mean of Dep. Var.	0.015	0.001	0.014	0.012	0.003	0.577	0.021
Observations	125,314	125,288	125,288	125,314	125,314	125,314	125,314
R-squared	0.022	0.003	0.022	0.018	0.006	0.319	0.022
Panel B: Children Ages 10-13							
	Any Economic Activity (1)	Employment in Banned Occ. (2)	Employment in Non-Banned Occ. (3)	Unpaid Econ. Activity (4)	Paid Employment (5)	Attending School (6)	Unpaid Household Services (7)
SibAge10-13XPost	0.007*	0.000	0.007**	0.006**	0.000	-0.001	-0.005
	(0.004)	(0.001)	(0.003)	(0.003)	(0.002)	(0.005)	(0.003)
Pre-Ban Mean of Dep. Var.	0.109	0.009	0.100	0.072	0.037	0.626	0.116
Observations	131,435	131,346	131,346	131,435	131,435	131,435	131,435
R-squared	0.102	0.015	0.096	0.064	0.050	0.264	0.128

*** p<0.01, ** p<0.05, * p<0.1 "SibUnder14" is a dummy variable for whether the child has at least 1 sibling age 10-13. Controls: gender, gender of household head, age of household head, urban status, number of adult females, number of male children, number of female children, as well as the following fixed effects: (own) age, family size, household head's education level, religion, survey round, survey quarter, state. Additionally we include controls for sibling age as follows: for *each* age 0-25, we create a separate variable which counts the number household members of that specific age. Sample consists of all individuals related to the household head with at least 1 other (related) household member age 10-17. Standard errors are clustered by household.

TABLE 6. Sibling-based Estimates using only Children with Siblings ages 6-13

Panel A: Children Ages 6-9							
	Any Economic Activity (1)	Employment in Banned Occ. (2)	Employment in Non-Banned Occ. (3)	Unpaid Econ. Activity (4)	Paid Employment (5)	Attending School (6)	Unpaid Household Services (7)
SibAge10-13XPost	0.006***	0.001	0.005***	0.006***	0.001	-0.035***	0.004**
	(0.002)	(0.000)	(0.002)	(0.002)	(0.001)	(0.007)	(0.002
Pre-Ban Mean of Dep. Var.	0.015	0.001	0.015	0.013	0.003	0.578	0.021
Observations	144,221	144,194	144,194	144,221	144,221	144,221	144,221
R-squared	0.024	0.003	0.024	0.019	0.007	0.325	0.024
Panel B: Children Ages 10-13							
	Any Economic Activity (1)	Employment in Banned Occ. (2)	Employment in Non-Banned Occ. (3)	Unpaid Econ. Activity (4)	Paid Employment (5)	Attending School (6)	Unpaid Household Services (7)
SibAge10-13XPost	0.010***	0.001	0.009***	0.008***	0.002	-0.009*	0.003
	(0.003)	(0.001)	(0.003)	(0.003)	(0.002)	(0.005)	(0.003)
Pre-Ban Mean of Dep. Var.	0.116	0.009	0.106	0.077	0.040	0.607	0.123
Observations	140,575	140,483	140,483	140,575	140,575	140,575	140,575
R-squared	0.107	0.014	0.101	0.066	0.053	0.274	0.132

*** p<0.01, ** p<0.05, * p<0.1 " SibAge1013" is a dummy variable for whether the child has at least 1 sibling age 10-13. Controls: gender, gender of household head, age of household head, urban status, number of adult females, number of male children, number of female children, as well as the following fixed effects: (own) age, family size, household head's education level, religion, survey round, survey quarter, state. Additionally we include controls for sibling age as follows: for *each* age 0-25, we create a separate variable which counts the number household members of that specific age. Sample consists of all individuals related to the household head with at least 1 other (related) household member age 6-13. Standard errors are clustered by household.

TABLE 7. Geographical Heterogeneity

	Dependent Variable: Any Economic Activity					
	Importance of Banned Industries		Probability of Detection		Proportion of Region that is Urban	
	Simple Estimate	Sibling-based Estimate	Simple Estimate	Sibling-based Estimate	Simple Estimate	Sibling-based Estimate
	(1)	(2)	(3)	(4)		
AboveMedianXUnder14XPost	0.003	0.013**	0.015*	0.014**	0.010	0.004
	(0.008)	(0.006)	(0.008)	(0.006)	(0.009)	(0.007)
Under14XPost	0.025***	0.002	0.017***	0.004	0.021***	0.007*
	(0.006)	(0.004)	(0.005)	(0.005)	(0.006)	(0.004)
Age Group	10-17	10-13	10-17	10-13	10-17	10-13
Observations	327,233	164,200	298,357	150,021	327,233	164,200
R-squared	0.183	0.105	0.184	0.106	0.193	0.118

*** p<0.01, ** p<0.05, * p<0.1. Columns (1)-(2): Importance is measured as the 1983 proportion of households in the region whose primary industry is banned (as of 1986). Columns (3)-(4): The probability of detection under the ban is created using data on the number of inspections at the state-level during the period 1997-2005 (as reported by IndiaStat, www.indiastat.com), scaled by the number of children working in 1983 in occupations that would be banned under the 1986 Act. Inspection data is not available for the following states/UTs: Himachal Pradesh, Manipur, Nagaland, Tripura, Andaman & Nicobar Islands, Arunachal Pradesh, Delhi, Lakshadweep, and Mizoram (about 8.8% and 8.6% of the original overall and sibling samples, respectively). Columns (5)-(6): The proportion of the region that is classified as urban uses the 1983 round of data only. We separate states into high (above median) and low (below median) importance/probability states based on these measures and interact those indicators with our main treatment variables. Controls: gender, gender of household head, age of household head, urban status, number of adult females, number of male children, number of female children, number of children under 5, number of children ages 6-9 as well as the following fixed effects: age, family size, household head's education level, religion, survey round, survey quarter, state. All regressions also include interactions between all control variables (including "Under14" and "Post") and the measure of heterogeneity. Standard errors are clustered by at the region level for columns (1), (2), (5), and (6) and at the state level for columns (3)-(4). Columns (1) and (3): "Under14" is a dummy variable that takes the value of 1 if the child is under 14 years of age. Sample consists of all individuals related to the household head aged 10-17. Columns (2) and (4): "Under14" is a dummy variable that takes the value of 1 if the child has at least 1 sibling age 10-13. In addition to the controls already listed, we include controls for sibling age as follows: for *each* age 0-25, we create a separate variable which counts the number household members of that specific age. Sample consists of all individuals related to the household head with at least 1 other (related) household member age 6-17.

TABLE 8. Household Heterogeneity

	Dependent Variable: Any Economic Activity					
	HH Head Has Less than Sec. Educ		Non-Staple Share of Calories		Scheduled Caste Status	
	Overall Effect	Sibling Effect	Overall Effect	Sibling Effect	Overall Effect	Sibling Effect
	(1)	(2)	(3)	(4)	(5)	(6)
Heterog.XUnder14XPost	0.016***	0.009*	0.007*	0.010	0.024**	0.005
	(0.005)	(0.005)	(0.004)	(0.006)	(0.010)	(0.013)
Under14XPost	0.003	-0.000	0.015***	0.001	0.025***	0.008**
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)
Age Group	10-17	10-13	10-17	10-13	10-17	10-13
Observations	326,754	163,969	318,570	159,802	327,223	164,193
R-squared	0.197	0.106	0.198	0.111	0.192	0.110

*** p<0.01, ** p<0.05, * p<0.1. Heterogeneity Measures: Cols (1)-(2) – Dummy variable for whether the household head has educational achievement less than secondary school; Cols (3)-(4) –"Non-staple share of calories" is a dummy variable for whether the household's share of daily calories from sources other than cereals and cereal substitutes is *below* the pre-ban (1983) median share; Cols (5)-(6) – "Scheduled Caste" is a dummy variable for whether the household belongs to a scheduled caste. Controls for all columns: gender, gender of household head, age of household head, urban status, number of adult females, number of male children, number of female children, number of children under 5, number of children ages 6-9 as well as the following fixed effects: age, family size, household head's education level, religion, survey round, survey quarter, state. All regressions also include interactions between all control variables (including "Under14" and "Post") and the measure of heterogeneity. Columns (1),(3), (5): "Under14" is a dummy variable that takes the value of 1 if the child is under 14 years of age. Sample consists of all individuals related to the household head aged 10-17. Standard errors are clustered by age-survey round. Columns (2),(4),(6): "Under14' is a dummy variable that takes the value of 1 if the child has at least one sibling age 10-13. In addition to the controls already listed, we include controls for sibling age as follows: for each age 0-25, we create a separate variable which counts the number of household members of that specific age. Sample consists of all individuals related to the household head with at least one other (related) household member age 6-17. Standard errors are clustered by household.

TABLE 9. Effects of the Ban on Child Wages

	Dependent Variable: Log(Real Wage)					
	Ages 6-21	Ages 6-21	Ages 7-20	Ages 8-19	Ages 9-18	Ages 10-17
	(1)	(2)	(3)	(4)	(5)	(6)
Under14XPost	-0.100	-0.078***	-0.076***	-0.070***	-0.065**	-0.043
	(0.085)	(0.023)	(0.024)	(0.025)	(0.027)	(0.026)
Observations	33,731	33,731	30,566	23,648	20,696	14,848
R-squared	0.128	0.392	0.378	0.357	0.343	0.313
Controls?	No	Yes	Yes	Yes	Yes	Yes

*** p<0.01, ** p<0.05, * p<0.1 Each column uses individuals related to the household head in the specified age range. "Under14" is a dummy variable that takes the value of 1 if the child is under 14 years of age. Real values (expressed in 1982 rupees) are nominal values deflated by the average wholesale price index reported by the Government of India for the respective year. Wages are trimmed of the top and bottom 1% of values within each round. Controls: gender, gender of household head, age of household head, urban status, number of adult females, number of male children, number of female children, number of children under 5, number of children ages 6-9 as well as the following fixed effects: age, family size, household head's education level, religion, survey round, survey quarter, state, industry. Standard errors are clustered by age-survey round.

TABLE 10. Effect of the Ban on Household Outcomes

	Log Total Expenditure Per Capita	Log Food Expenditure Per Capita	Log Daily Calories Per Capita	(1-Staple Share of Calories)	Asset Index
	(1)	(2)	(3)	(4)	(5)
Child10-13	-0.005	-0.005	0.000	-0.003***	-0.034**
XPost	(0.004)	(0.003)	(0.003)	(0.001)	(0.014)
Pre-Ban Mean of Dep. Var.	4.593	4.177	7.631	0.292	-0.732
Observations	222,604	220,358	220,362	220,357	220,542
R-squared	0.382	0.364	0.185	0.498	0.548

*** p<0.01, ** p<0.05, * p<0.1. "ChildUnder14" is a dummy variable that takes the value of 1 if there is at least one child age 10-13 in the household. Sample consists of households with at least one household member age 6-17, trimmed of the top and bottom 1% of values of the dependent variable within each round. Robust standard errors reported. Controls: gender of household head, age of household head, urban status, number of adult females, number of male children, number of female children, number of children under 5, number of children ages 6-9 as well as the following fixed effects: family size, household head's education level, religion, survey round, survey quarter, state. Additionally we include controls for sibling age as follows: for *each* age 0-25, we create a separate variable which counts the number household members of that specific age.