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. 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 examine the effects of the ban at the household level. Using linked consumption and expenditure data, we find that along the margins of assets and share of staple goods in calorie consumption, households are *worse* off after the ban.

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1. INTRODUCTION

Legal interventions are a common tool used by societies seeking to bring about equality and justice. Bans against child marriage, racial segregation in schooling, and discriminatory hiring practices are prime examples of legal action intended to improve overall welfare and create equality of opportunity. While legal interventions have undoubtedly been effective in many situations, 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). This possibility is crucial when evaluating the consequences of legal action taken against a controversial yet pervasive aspect of developing societies: child labor.

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.[2] 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.[3]

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. 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 a difference in differences model 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.

[2]While there is a wealth of empirical and theoretical work examining the determinants (see excellent reviews by Basu (1999), Edmonds et al. (2010), Edmonds (2007)) and consequences (see for example Beegle et al. (2009)) of child labor there is little empirical evidence on the effectiveness of child labor *bans* in developing countries. Nonetheless, the *theoretical* research in this area is quite rich (see for example Baland and Robinson (2000), Basu and Van (1998), and Doepke and Zilibotti (2005)). There have been numerous evaluations of related policies like cash transfers that are directly intended to affect child labor (see Skoufias et al. (2001) among many others) and policies like trade liberalization that affect child labor indirectly (see Edmonds and Pavcnik (2005b)).

[3]A number of studies focus on the impact of child labor legislation in the U.S. (Moehling (1999), Lleras-Muney (2002), Manacorda (2006), Bugni (2012)) but to our knowledge the only other paper to consider the effect of such legislation on child work outcomes in the developing country context is Edmonds and Shrestha (2012a). Edmonds and Shrestha

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. To better understand the mechanisms behind this effect, we also examine how the employment of children under 14 changes when their *sibling* is under or over the age of 14. According to the theoretical model, when the ban depresses the wages of children under 14 (and thus the income of their families), it is the *siblings* of affected children who are most likely to be pushed into work.[4]

We find 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. Using the sibling-based difference in difference approach more closely tied to the theoretical model, 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 0.9 percentage points compared to a child of the same age with a sibling over the age of 14. This represents an increase of approximately 7.8% over the pre-ban employment rate for that age group and suggests that the channels in the model are quantitatively important. We then exploit state level variation to find that the employment effects are larger in areas more likely to be affected by the ban (measured along several dimensions, including the importance of banned industries in local labor markets, the likelihood of inspection, and the degree of labor market frictions). A key implication of the theory is that poorer households who are forced to rely on child labor to generate income will be most impacted by the ban. Though we do not observe household income in the data, we use education of the household head, scheduled caste status, and non-staple share of calories consumed as proxies for income and find that the increases in child labor due to the ban is concentrated among poorer families. We also find decreases in

[4]While it would be interesting to examine responses along the intensive margin, the data do not contain any information on hours worked. This strategy is similar to that used in Manacorda (2006), but differs in that we use siblings to identify which children were affected by the ban through the pathway of child wages and household income rather than using age eligibility of siblings to study the impact of one sibling's labor supply on another. As described in greater detail in a later section, we use sibling age to identify children whose siblings are both likely to be working (as in Manacorda (2006)) *and* likely to suffer a wage reduction due to the ban.

child participation in schooling, although this is only significant for very young children (ages 6-9). Hence, there is some suggestive evidence that the ban had negative impacts on human capital accumulation.

In line with the mechanism underpinning the theory, we find that child wages fall relative

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

The impetus for the 1986 law[5] came from multiple reports from Government committees that suggested weak implementation of prior laws against child labor (see descriptions of these committee reports, the Sanat Mehta Committee of 1986 and the Gurupadaswamy Committee on Child Labor of 1979, in Ramanathan (2009)). 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 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 exempted and family-run businesses were allowed to employ their own children without age restrictions.[6]

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."

Though enforcement of the 1986 law has been largely weak, 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

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

[6]For the industries/processes where child labor was not explicitly banned (including agriculture but excluding household enterprises), the 1986 law placed limits on how many hours and which hours children could work. For example, Section III of the law states that for every three hours of work, a child would get an hour of rest; no child shall work between 8pm and 7am; and no child shall be permitted or required to work overtime.

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 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 (see the online Appendix for details). Similarly, employers may have been able to work around the law through bribes paid to inspectors or other officials involved in the age authentication process: “Fake age certificates are produced in courts claiming the child’s age above 14 years. These certificates ban 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.

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 prosecutions (IndiaStat). While overall enforcement might have been weak, the anecdotal evidence on the increased threat of inspections and employers’ subsequent responses leads us to believe that the Act raised awareness of the law as the government put renewed effort into enforcing the Act.

3. Theoretical motivation

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 Theory Appendix, available from the authors upon request. 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).

In the one sector case, Basu (2005) and Basu and Van (1998) show that an imperfectly enforced ban on child labor could lead to lower wages and *increased* levels of child labor. The basic idea is that if child and adult labor are substitutable (up to a productivity shifter)[7], a ban lowers the pre-ban levels). Although all children will work in the non-banned sector, the marginal rate of substitution between child leisure (or schooling) and work has not changed from pre-ban levels and thus overall levels of child labor are unaffected by the ban (as long as there is sufficient adult labor to satisfy total labor demand in the banned sector). From a policy making point of view, while the fine does not achieve its goal of reducing child labor, it does not lead to the perverse outcome of increased child labor as in Basu (2005).

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.[8] As the 1986 ban largely applied to manufacturing jobs, to fix ideas we can think of the banned sector as manufacturing and the non-banned sector as including agriculture and household enterprises. In this partial mobility setting, an increased fine for hiring child labor in the manufacturing sector lowers wages in that sector. Unlike the full mobility case, labor cannot completely reallocate to "undo" the effects of the child labor ban; although children may flow out of the banned sector, adults cannot completely replace the children who exit that sector due to the barriers to entry. In response to lower manufacturing wages, more children must work to help households reach the subsistence target. Children that enter the workforce as a result of the ban may enter either manufacturing or agriculture; since there are no barriers to entry into agriculture we might actually expect larger increases in child employment in agriculture than in manufacturing. Thus, after the ban, more children are working overall and all earn lower wages. In summary, when we extend the canonical model of Basu and Van (1998) and Basu (2005) to two sectors with frictions, child labor may rise and child wages may fall in response to an imperfectly enforced ban on child labor.

[8] For example, the ability to find work outside the household (and in particular, child work) is likely to depend on networks based on religious, ethnic, caste, familial, or other social ties; since these are likely to be correlated with particular occupations and sectors, these types of network-based restrictions might significantly limit labor mobility. Additionally one sector may require skills that are costly or difficult to acquire; in our empirical context, many of the occupations listed in the 1986 Act were broadly considered to be within the manufacturing sector. Even relatively low-skill jobs may require a set of basic skills that are instilled during early schooling or training (see for example the discussion of garment factories in Heath and Mobarak (2014)).

the 3-digit NIC codes reported for each employed child.[15] These are matched to the list of processes and occupations listed as banned in the 1986 Act but the definition of banned occupations that we use explictly 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. Wages are increasing for children over time.

5. EMPIRICAL STRATEGY AND RESULTS

5.1. **Effect of the ban on child time allocation**

5.1.1. *Overall effects of the ban.* To assess the overall effects of the 1986 Act on child time allocation, we begin by running the following difference-in-difference specification:

$$(1) \qquad Y_{it} = {}_0 + {}_1 Under14_i + {}_2 Post1986_t$$
$$+ {}_3 (Under14_i \quad Post1986_t) + {}_X X_{it} + {}_t + {}_q + {}_{it}$$

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. X_{it} is a vector of household- and child-level covariates, such as characteristics of the household head and fixed effects for gender, age, locality (most often state[16]), household size, etc.[17] ${}_t$ represents survey round fixed effects and ${}_q$ captures quarterly seasonality through quarterly dummies. In our main results we cluster our standard errors by age-survey round though we consider other clustering methods in the section on robustness checks.

Our coefficient of interest is ${}_3$, 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

[15]Over time, additional occupations and processes have b7e of theth426(add674 -4.035o/F51 9.9626 Tf 6.9626 Td [(Ov)15(er)-391(t

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.[18] 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 sample those children between the ages of 10 and 17.

The results of estimating (1) on the the main sample are displayed in Table 2.[19] The first two columns report the results for main outcome of interest, "Any Economic Activity", with and without any controls, respectively. While the addition of controls greatly increases precision, the point estimate is very stable across specifications with and without covariates. Under the assumption of parallel trends, the coefficient reported in column 2 indicates that the ban increased the probability of child participation in economic activity by 2.6 percentage points (a 22% increase over the pre-ban mean for children ages 10-13). When we decompose the overall effect of the ban into its effect various categories of employment, we find that while the majority of the increase is in non-banned occupations (2.3 percentage point increase, column 5), there is also a small but statistically significant increase in work in banned occupations (0.4 percentage point increase, column 4). In other words, though the ban was introduced to lower child labor in a specific set of occupations, the overall impact of the ban was to increase the work of children *under* the age of 14 in exactly those occupations. Columns 6 and 7 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

[18]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 use an RD approach for our baseline estimates for several related reasons. First, the age gradient of work probabilities is relatively steep; for example, in our sample children ages 6-9 (the youngest children for whom we have employment information) only 2% are working in the pre-ban period whereas the proportion for children ages 14-17 is nearly 34%. This means that children in age bins that would normally be considered relatively "close" to the cutoff (e.g. children aged six would be within 8 age bins of the cutoff) are in fact quite different from those at the cutoff. Second, we observe age only in increments of an entire year. Once we restrict the age range to ages which appear to be more similar in the pre-ban period (10-17), we are left with only 4 age bins on either side of the cutoff. This leaves us with too few bins to implement the usual RD design. Nonetheless, in section 6, we show that our main estimates are robust to flexibly controlling for age trends (though with less precision). Finally, as Figures 1 and 2 of the Online Appendix illustrate, there is significant heaping at certain ages (though not in a way that is systematically related to the over- versus under-14 split). This heaping would prove problematic for an RD design.

[19]The.

child work as a result of the ban. There are small and statistically insignificant effects on school attendance (column 8), and the ban seems to have decreased the incidence of unpaid household services (column 9).[20]

One potential concern with estimating (1) on a wide range of ages is that the estimates may capture pre-existing or secular differences in the trends in employment (or schooling) for children ages 10-13 versus children ages 14-17. To address this concern, we re-estimate (1) using narrow age ranges. The idea is that the narrower the age band, the more likely we are to satisfy the parallel trends assumption. The results of this exercise are reported in Table 3a. Even with the narrow effect of the ban on child economic activity is smaller than in the main sample (0.3 percentage points, column 2) and not statistically significant. This is perhaps not surprising given that the sample size for this round is much smaller than in the main sample and that the effect of the ban may take time to set in and thus not be fully captured in this short time frame. Moreover, we may expect that the ban forces children to enter into the labor force in search of new jobs but that in the short run children may not be immediately successful in finding jobs. Turning to the results in column 3 we can see that indeed the effect on labor force participation (which includes *new* entrants into the labor force seeking work) is much larger, though still not statistically significant. The only statistically significant increase in child labor is in paid work, which rises by 0.6 percentage points (column 5). Schooling also seems to decline significantly for those under 14 (column 6), though there is no effect on unpaid household activities in the short run (column 7).

5.1.2. *Sibling-based effects of the ban.* While Tables 2, 3a and 3b give us estimates of the overall policy impact of the 1986 ban on child time allocation, they cannot isolate a specific channel through which the ban affects children. Thus we now turn to the mechanisms identified in the model in Section 3. According to the model, a ban can lead to increases in child employment because it reduces child wages and thus income for households reliant on child labor. 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.

To isolate this channel empirically, the basic design would be to compare the employment status of children with working *siblings* under 14 to those with working *siblings* over 14. However, the work status of siblings is endogenous to the ban, so we instead rely on the age of siblings as a proxy for endogenous "treatment" of having an underage working sibling. To capture children whose siblings are likely to be working, the first requirement of our treatment variable is that the child must have a sibling who is at least 10 years of age. Since only 1.6% of children under the age of 10 are working in 1983 as compared to 19.5% children ages 10-17, we think age 10 represents a reasonable lower limit of ages we consider for the definition of treatment based on sibling age. The second requirement of our treatment variable is that it capture only children whose siblings'

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expect these "floors" to be equally binding for those under and over 14. Additionally in over a short period of time (Table 3b) we do not see large decreases in child labor and thus child employment "floors" are not likely to be relevant.

wages would be impacted by the ban, i.e. whose siblings are under the legal working age of 14. Thus we define our sibling treatment variable to be 1 if a child has a sibling who is at least 10 but under 14 because we think this captures a child whose sibling is both likely to be working and to 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.[25] This strategy allows us to construct a much more similar "control" set of children who are less likely to be affected by the ban through other channels.[26] Moreover, because of the way we define "treated" children (having a sibling ages 10-13), our "control" set includes children with both younger *and* older siblings, so it is unlikely that our results are simply capturing changes over time that affect older and younger families differently.

Tables 4a and 4b display the results for estimating our baseline specification in (3) for both the very young (ages 6-9) and the young (ages 10-13) samples of children, respectively. We find that through the siblings channel, the ban increases the likelihood of a child engaging in any form of work by 0.4 percentage points for the very young and by 0.9 percentage points for young children (column 1 in both tables).[27] These represent 25% and 8% increases over the pre-ban mean, respectively. As with the overall effects, the sibling-based effects are stronger for work in non-banned occupations than in banned occupations (columns 3 and 4). 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 5). We observe negative effects on the ban on attending school, which are larger and significant only for the very young (column 7). Time spent in unpaid household services increases for the very young but not for the young (column 8).

5.2. **Heterogeneity**

One concern with the analysis so far is that in analyzing a national change using a difference in difference approach, we might be allowing other policies or events that happen to coincide with the law change to influence our results. In order to shed some additional light on whether

[25]Note that 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 as described in footnote 23.

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

[27]These results are robust to a number of alternate samples. Specifically Online Appendix Table 4 displays the results of estimating (3) on (i) the sample including additional consumption rounds of the NSS (ii) Round 42 (July 1986 - June1987) (iii) the sample that excludes children whose "treatment generating sibling" (i.e. sibling between the ages of 10-13) is *younger* and (iv) the sample of children with at least one sibling ages 6-17. These sibling effects are also robust to dropping Round 43; see column 2 of Online Appendix Table 9.

the ban is truly driving the overall effects, we use geographic and household level features that generate variation specific to the ban.

5.2.1. *Geographical Heterogeneity.* We examine geographic heterogeneity in the incidence of the ban along several dimensions. First, we measure the importance of the ban as the proportion of households in each state 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 state that derive income mostly from banned industries to capture the importance of banned industries to local labor markets. As can be seen in Table 1a, about 12% of households nationally are primarily engaged in activities listed under the 1986 Act but at the state level this ranges from 0.7% to 28.2%, indicating considerable geographic heterogeneity. Second, we measure the degree of pre-existing labor market frictions across sectors as the *pre-ban* differential in state-level median wages of banned versus non-banned sectors, conditional on observable characteristics.[28] As discussed in Section 3, in a multi-sector framework the model predicts the ban will affect levels of child labor only when there are frictions that limit the movement between occupations. Thus we expect to find stronger effects in states where these frictions are more binding, i.e. where workers in banned sectors enjoy a larger wage premium. Finally, we measure the probability of inspection under the ban using data on the number of inspections at the state-level during the period 1997-2005. We scale the number of inspections by dividing by the the number of children working in 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 channels (imprisonment or penalties) or through illicit channels (e.g. bribes extracted), the model in Section 3 suggests that a higher likelihood of inspection should lead to lower child wages and thus larger impacts of the ban on employment.

Table 5a displays the results of estimating equations (1) in states above and below the median level of importance, pre-existing labor market frictions, and probability of inspection.[29] In states where banned industries are likely to be more important to the local labor market, the overall effect of the ban is larger (significant at the 10% level). Similarly, in states where pre-existing labor market frictions produce larger wage gaps between sectors, we find stronger overall effects of the ban (marginally significant; p-value = 0.105). The overall effect of the ban is over 3.1 percentage points in states where the likelihood of inspection is relatively high (significantly larger than the effect in below median states at the 1% level). The patterns for sibling-based effects displayed in Table 5b are very similar. Children in states with higher exposure to the ban, greater degree of labor market frictions, and higher probability of inspection under the ban see larger sibling-based effects (differences across subsamples are significant, except when splitting the sample according to pre-ban labor market frictions). In above median states, the sibling-based effect can be quite sizeable, ranging from 12.2% over the pre-ban mean in states with high pre-ban wage differentials to 20.2% in states with high probability of detection.

5.2.2. *Household Heterogeneity.* Another important source of heterogeneity in the ban's potential impact comes from the economic status of the household. 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 are those with low incomes. Unfortunately we do not observe household income in the data and as we will show in a later section, measures such as household consumption are likely endogenous to the ban itself.[30] We therefore rely on several

[29]As there are a small number of states and union territories (31) we are limited in the state-level analysis we can perform. However, the results presented in Table 5 are robust to other state level measures such as splitting at the 75th percentile rather than the median and using a continuous measure of exposure (results available upon request).

[30]While we observe a statistically significant impact of the ban on staple share of calories, the effect is very small in magnitude so we continue to consider heterogeneity by this measure. Nonetheless we interpret these results with more caution than the other results using the other measures, which do not respond to the ban.

important proxies: education of the household head, non-staple share of foods consumed, and scheduled caste status. Additionally we consider heterogeneity by the child to adult ratio (number of children under 17 relative to adults 18 and over), which we believe captures the degree to which households may rely on child labor income relative to adult income.

The results after splitting the samples by household measures are presented in Tables 6a and 6b for the overall and sibling-based effects, respectively. In all but one sample split (staple share of calories, columns 3 and 4), we observe statistically larger overall effects of the ban in households that we expect to be poorer or possibly more dependent on child-generated income, i.e. households in which the head is less educated, that belong to scheduled castes, and with a higher child to adult ratio. Similarly we see that the sibling-based effects are generally larger for poorer households, though the differences are statistically significant for only two of the four measures and one works in the opposite direction as expected (child to adult ratio, columns 7 and 8; the difference is not statistically significant). 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 the measures of poverty we use 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.

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

models in Basu (2005) and Basu and Van (1998). The results of estimating equation (1) on various samples are displayed in Table 7. We see a substantial drop in child wages relative to adult wages; once we control for observable individual characteristics the effect of the ban is to reduce child wages by 7.8% on average (column 2).[31] 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. Online Appendix Table 6 illustrates 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 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. In Section 3 we assume that all children are equally productive and earn the same wage. In reality, we might expect households to withdraw the least economically productive children as their incomes rise, as these children are likely to be paid less and are therefore less important to household income. 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 changing selection into the workforce over time (results available upon request).[32]

[31] We do not analyze heterogeneous effects of the ban on wages by occupation. This is because selection into different occupations of work is likely to be shifting differentially over time for those over and under age 14. Online Appendix Table 5 illustrates this point. It displays the proportion of working individuals that report wages by sector and age group. While all children under 17 become less likely to report wages after the ban, the largest drop in wage reporting is for children under 14 in banned occupations. Thus we believe this differential selection in wage reporting will distort any estimated wage effects of the ban by sector.

[32] Another alternate explanation for our wage results could be that the results capture an average decline in wages for children due to skill-biased technical change. If skills are positively correlated with age, we 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, if anything we find that the return to education declines over this period for this sample (results available upon request), consistent with earlier

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.[35]

6. ROBUSTNESS CHECKS

6.1. **Falsification exercises**

The underlying assumption for our identification strategy is that the difference between children with siblings just above and below the legal working age should be steady across time in the absence of the ban. One way to test whether the changes in child employment were due to ban and not some other change occurring at the same time is to impose "false" age restrictions on our untreated sample. In Appendix Table A.1 we see that when we define treatment as having a sibling under the age of 5, 10, or 18, we find no such effect of the ban (columns 1-3). The results of this "placebo" test lead us to believe that our estimated effect of the ban is not simply picking up the effect of having an older or younger sibling. In column 4 we show that estimating our main specification but using 1987-8 as the "pre" and 1993-4 as the "post" period period does not lead to significant “effects” of the ban. Hence, it appears that the policy change specific to 1986 is driving our results.[36]

Another way to address concerns about the exogeneity of our ban variable is to see whether it can predict changes in any demographic variables. To test for this, we regress household demographic variables on the household-level treatment variable as defined in the previous section. The results in Table A.2 indicate that there is only one statistically significant endogenous response of household demographics to the ban (out of eight). Moreover, the effect is very small in magnitude; household size decreases by 0.029 members (0.5% of the pre-ban mean). Thus we do not find evidence that the ban had any meaningful effect on household characteristics.[37]

6.2. **Controlling for age-specific changes over time**

One potential concern with our findings is that factors other than the ban, such as schooling reforms or the rapid economic growth India experienced during the period under study, may have affected the time allocation of younger children differently relative to older children. To address this we perform several additional robustness checks. The first is to include more flexible age controls. We do this by including round-specific quadratic trends in age (i.e. quadratic trends in age that are estimated separately for 1983, 1987-8, and 1993-4), age-specific linear time trends (i.e. linear time trends separately for each age), or a complete set of age interactions (linear, quadratic and cubic age trends that are allowed to be different for those under and over 14, separately for the pre- and post-ban periods). The results of these exercises are displayed in Appendix Table A.3. While adding in such flexible age controls significant reduces the precision of our estimates – unsurprisingly, as there are a limited number of age groups on which to estimate these age trends and the effect of the ban – the 1 of the at conventional levels (columns 1, 2, 5 and 6) or marginally significant (the p-values for columns 3 and 4 are 0.144 and 0.137, respectively).

To specifically address the possible confounding effects of economic growth and/or state-level policies that may have affected younger children differently than older children, we include state-by-round fixed effects and interactions between time-varying state GDP measures and an indicator for being under 14 (or for having a sibling under 14 in our sibling-based regressions). As Appendix Table A.4 indicates, allowing for these additional factors does not change our estimates of the impact of the ban.

Finally, 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.[38] 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 slightly larger (though not significantly so);

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6.3. **Effects on other ages**

One of the main assumptions in the Basu (2005) and Basu and Van (1998) models is that adults supply labor inelastically. Hence, in response to lower child wages, we should not expect to see a response from adults (or in our framework, "young adults" who may be considered children by households but are classified as adults in the definitions set forth in the 1986 Act). In Appendix Table A.5, we show that this is precisely the case. Individuals above the age of 14 do not show any increases or decreases in labor supply in response to the ban.[40]

6.4. **Alternate clustering methods**

As discussed in the previous section, one issue we encounter when estimating the overall effects of the ban on child time allocation is the potentially low number of clusters.[41] Appendix Table A.6 displays the results of estimating equation (1) under various clustering regimes. Column 1 gives our baseline result, clustering at the age-round level. Columns 2 and 3 cluster by age only to allow for arbitrary correlation within ages over time; since this leads to 8 clusters, we estimate the standard errors using the standard cluster-robust procedure and a bootstrap procedure (described in the Online Appendix). The effect of the ban remains highly significant under these alternate methods. Finally we display the results for clustering at the most conservative level – by under 14 age group and post – in columns 4 and 5. Using the bootstrap method at this level yields a marginally significant p-value of 0.176.

6.5. **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. In particular parents may underreport the labor of their children due to social or other types of pressure. Moreover it is possible that this underreporting

(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."

In the particular case of the 1986 ban, this paper has shown that households are worse off along measures that reflect welfare. Not only is the ban ineffective in the short run (by not

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TABLES

TABLE 1A. Summary statistics: Means of household variables

	1983	1987-8, 1993-4
Family Size	6.27	6.10
Head Age	44.7	44.7
Head Is Male	0.914	0.916
Head Has No Education	0.517	0.429
Head Has Atleast Some Primary Eduction	0.259	0.265
Head Has Middle Eduction	0.102	0.117
Head Has Secondary Eduction or More	0.122	0.190
Hindu Household	0.783	0.779
Urban Area	0.323	0.342
Real monthly expenditure per capita	136.1	147.8
Food expenditure per capita	72.4	86.5
Calories per capita	2215.2	2230.3
Staple share of calories	0.7	0.7
Asset index	-0.7	0.2
Principal Industry is Banned	0.120	0.124
Number of observations	77613	151920

TABLE 1B. Summary statistics: Means of child variables

	1983		1987-8, 1993-4	
	Ages 10-13	Ages 14-17	Ages 10-13	Ages 14-17
Male	0.529	0.528	0.535	0.539
Attending School	0.604	0.430	0.733	0.549
Any Economic Activity	0.118	0.296	0.068	0.223
Labor Force Participation	0.121	0.319	0.070	0.242
Unpaid Household Services	0.124	0.214	0.067	0.166
Number of observations	63743	48481	118101	96908
Among economically active children				
Unpaid Economic Activity	0.654	0.565	0.625	0.573
Paid Employment	0.346	0.435	0.375	0.427
Number of observations	7511	14361	8049	21605
Among children in paid employment				
Employment in banned occupation	0.252	0.313	0.334	0.382
Employment in non-banned occupation	0.749	0.687	0.666	0.618
Real Daily Wages (1982 Rupees)	4.11	5.53	5.32	7.52
Number of observations	2000	4872	1404	4617

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.

TABLE 2. Overall Effects of the Ban on Child Time Allocation
Rounds: 1983, 1987-8, 1993-4

	Any Economic Activity (1)	Any Economic Activity (2)	Labor Force Participation (3)	Employment in Banned Occ. (4)	Employment in Non-Banned Occ. (5)	Unpaid Economic Activity (6)	Paid Employment (7)	Attending School (8)	Unpaid Household Services (9)
Under14XPost	0.024	0.026***	0.029***	0.004***	0.023***	0.007*	0.019***	0.008	-0.009**
	(0.040)	(0.005)	(0.005)	(0.001)	(0.005)	(0.003)	(0.002)	(0.007)	(0.004)
Pre-Ban Mean of Dep. Var.	0.118	0.118	0.121	0.009	0.108	0.077	0.041	0.604	0.124
Observations	327,233	327,233	327,233	326,768	326,768	327,233	327,233	327,233	327,233
R-squared	0.055	0.182	0.192	0.030	0.162	0.093	0.099	0.303	0.211
Controls?	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*** p<0.01, ** p<0.05, * p<0.1 Column 1 includes only a dummy for post-ban, a dummy for under 14, and an interaction between under 14 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, and fixed effects for age, family size, household head's education level, religion, survey round, survey quarter, state. "Under 14" is a dummy variable that takes the value of 1 if the child is under 14 years of age. Sample consists of all individuals 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 3A

TABLE 4A. Sibling-based Effects of the Ban on Child Time Allocation
Rounds: 1983, 1987-8, 1993-4. Ages: 6-9.

	Any Economic Activity (1)	Labor Force Participation (2)	Employment in Banned Occ. (3)	Employment in Non-Banned Occ. (4)	Unpaid Econ. Activity (5)	Paid Employment (6)	Attending School (7)	Unpaid Household Services (8)
SibUnder14	0.004***	0.004***	0.000	0.004***	0.004***	0.000	-0.030***	0.003**
XPost	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.005)	(0.001)
Pre-Ban Mean of Dep. Var.	0.016	0.016	0.001	0.015	0.013	0.003	0.576	0.022
Observations	179,399	179,399	179,363	179,363	179,399	179,399	179,399	179,399
R-squared	0.024	0.024	0.002	0.024	0.019	0.007	0.323	0.024

TABLE 4B. Sibling-based Effects of the Ban on Child Time Allocation
Rounds: 1983, 1987-8, 1993-4. Ages: 10-13.

	Any Economic Activity (1)	Labor Force Participation (2)	Employment in Banned Occ. (3)	Employment in Non-Banned Occ. (4)	Unpaid Econ. Activity (5)	Paid Employment (6)	Attending School (7)	Unpaid Household Services (8)
SibUnder14	0.009***	0.009***	0.001	0.008***	0.007***	0.002	-0.006	-0.001
XPost	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.002)	(0.005)	(0.003)
Pre-Ban Mean of Dep. Var.	0.112	0.115	0.009	0.103	0.074	0.038	0.609	0.121
Observations	158,522	158,522	158,409	158,409	158,522	158,522	158,522	158,522
R-squared	0.102	0.103	0.014	0.097	0.062	0.051	0.273	0.130

*** p<0.01, ** p<0.05, * p<0.1 "SibUnder14" is a dummy variable that takes the value of 1 if the child has at least 1 sibling age 10-13. Controls are as listed notes under Table 2, but we additionally include fixed effects for age of sibling closest to 14. Sample consists of all individuals related to the household head with at least 1 sibling age 25 or younger. Standard errors are clustered by household.

TABLE 6A. Heterogeneity in Overall Effects by Household Characteristics
Rounds: 1983, 1987-8, 1993-4

	Dependent Variable: Any Economic Activity							
	Head Education		Staple Share of Calories		Scheduled Caste		Child to Adult Ratio	
	Less Than Secondary Schooling (1)	At Least Secondary Schooling (2)	Above Median (3)	Below Median (4)	Sched. Castes (5)	Non Sched. Castes (6)	Above Median (7)	Below Median (8)
Under14 XPost	0.018***	0.003	0.022***	0.015***	0.048***	0.025***	0.029***	0.019***
	(0.006)	(0.002)	(0.006)	(0.004)	(0.013)	(0.004)	(0.006)	(0.005)
p-value for test of diff.	0.003		0.155		0.021		0.038	
Pre-Ban Mean of Dep. Var.	0.132	0.011	0.161	0.078	0.211	0.108	0.122	0.105
Observations	272,774	53,980	132,328	186,242	32,948	294,275	225,876	101,221
R-squared	0.179	0.037	0.203	0.155	0.271	0.174	0.188	0.175

TABLE 6B. Heterogeneity in Sibling Effects by Household Characteristics
Rounds: 1983, 1987-8, 1993-4

	Dependent Variable: Any Economic Activity							
	Head Education		Staple Share of Calories		Scheduled Caste		Child to Adult Ratio	
	Less Than Secondary Schooling (1)	At Least Secondary Schooling (2)	Above Median (3)	Below Median (4)	Sched. Castes (5)	Non Sched. Castes (6)	Above Median (7)	Below Median (8)
SibUnder14 XPost	0.009**	-0.002	0.013**	0.001	0.012	0.008***	0.008**	0.015**
	(0.004)	(0.003)	(0.005)	(0.004)	(0.012)	(0.003)	(0.004)	(0.007)
p-value for test of diff.	0.022		0.053		0.773		0.316	
Pre-Ban Mean of Dep. Var.	0.126	0.010	0.154	0.075	0.201	0.103	0.117	0.097
Observations	133,153	25,155	66,576	87,683	16,021	142,494	124,295	34,180
R-squared	0.100	0.014	0.119	0.075	0.180	0.095	0.106	0.090

*** p<0.01, ** p<0.05, * p<0.1. Table 6a: "Under14" is a dummy variable that takes the value of 1 if the child is under 14 years of age. See notes to Table 2. Table 6b: "SibUnder14" is a dummy variable that takes the value of 1 if the child has at least 1 sibling age 10-13. See notes to Table 4b. Pre-Ban mean is for children under the age of 14 only.

TABLE 7. Effect of the Ban on Child Wages
Rounds: 1983, 1987-8, 1993-4

	Dependent Variable: Log(Wages)	
	(1)	(2)
Under14XPost	-0.100	-0.078***
	(0.085)	(0.023)
Observations	33,731	33,731
R-squared	0.128	0.392
Controls?	No	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. See notes to Table 2. 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. Sample includes individuals aged 6-21.

TABLE 8. Effect of the Ban on Household Outcomes
Rounds: 1983, 1987-8, 1993-4

	Log Total Expenditure Per Capita (1)	Log Food Expenditure Per Capita (2)	Log Daily Calories Per Capita (3)	(1-Staple Share of Calories) (4)	Asset Index (5)
ChildUnder14 XPost	-0.005	-0.004	0.000	-0.003***	-0.032**
	(0.004)	(0.003)	(0.003)	(0.001)	(0.014)
Pre-Ban Mean of Dep. Var.	N/A	N/A	N/A	0.292	-0.732
Observations	222,590	220,342	220,346	220,341	220,526
R-squared	0.381	0.364	0.185	0.497	0.547

*** 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 1 child age 10-13 in the household. Sample consists of all households with at least 1 child ages 6-17. Each sample is trimmed of the top and bottom 1% of values within each round. Robust standard errors reported. Controls as listed in notes to Table 4b, excluding any own-age fixed effects and gender but maintaining fixed effects for the age of the child closest to the age cutoff.

Appendix A. Appendix Tables

Table A.1. Falsificiation Tests 1: Imposing False Ban Ages and Dates
Rounds: 1983, 1987-8, 1993-4

	Dependent Variable: Any Economic Activity				
	Sibling Effects			Overall Effects	Sibling Effects
	Eligible Age = 5	Eligible Age = 10	Eligible Age = 18	False Ban Date: Rounds 43 vs. 50	
	(1)	(2)	(3)	(4)	(5)
Under14XPost	-0.001	-0.003	0.004	0.006	-0.000
	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)
Ages	10-13	10-13	10-13	10-17	10-13
Observations	89,565	89,565	87,899	215,009	102,894
R-squared	0.102	0.102	0.102	0.172	0.091

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TABLE A.3. Including Flexible Age Controls
Rounds: 1983, 1987-8, 1993-4

	Dependent Variable: Any Economic Activity					
	Overall Effects			Sibling Effects		
	Round-Specific Quadratic Age Trends	Age-Specific Linear Time Trends	All Age Interactions	Round-Specific Quadratic Age Trends	Age-Specific Linear Time Trends	All Age Interactions
	(1)	(2)	(3)	(4)	(5)	(6)
Under14 XPost	0.013***	0.019***	0.030	0.005	0.009**	0.007*
	(0.004)	(0.005)	(0.020)	(0.003)	(0.004)	(0.004)
p-value	0.007	0.001	0.144	0.137	0.020	0.098
Observations	327,233	327,233	327,233	159,171	158,593	159,171
R-squared	0.182	0.182	0.182	0.103	0.102	0.103

*** p<0.01, ** p<0.05, * p<0.1. Each regression contains the additional age controls listed above each column (for own age in columns 1-3 and for sibling age for columns 4-6). “All Age Interactions”: age, age squared, age cubed, age*post, age sq.*post, age cub.*post, age*under14, age sq.*post, age cub.*post, age*post*under14, age*sq.*post*under14, age cub.*post*under14. Cols 1 and 2: “Under14XPost” is a dummy variable that takes the value of 1 if the child is under 14 years of age. See notes to Table 2. Cols 3 and 4: “Under14XPost” is a dummy variable that takes the value of 1 if the child has at least 1 sibling age 10-13. See notes to Table 4b.

TABLE A.4. Accounting for State Policies and Differential Effects of Economic Growth on Children
Rounds: 1983, 1987-8, 1993-4

	Dependent Variable: Any Economic Activity			
	State X Round FE		State GDP Index X Treat	
	Overall Effects	Sibling Effects	Overall Effects	Sibling Effects
	(1)	(2)	(3)	(4)
Under14XPost	0.027***	0.008**	0.025***	0.009**
	(0.005)	(0.003)	(0.006)	(0.004)
Observations	327,233	158,522	325,408	157,621
R-squared	0.184	0.105	0.182	0.101

*** p<0.01, ** p<0.05, * p<0.1 Columns 1 and 2 include state-by-round fixed effects. Columns 3 and 4 include an interaction between state GDP and Treat. State-level GDP (yearly) is calculated using state-level census data as reported by IndiaStat
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and 4: “Under14XPost” is a dummy variable thatTreat.
10-13. See notes(sibling)-250(age)-2to Table 4b.

TABLE

TABLE A.5. Effects on Other Ages
Rounds: 1983, 1987-8, 1993-4

	Dependent Variable: Any Economic Activity					
	Ages 14-17 (1)	Ages 18-25 (2)	Ages 26-35 (3)	Ages 36-45 (4)	Ages 46-55 (5)	Ages 56+ (6)
ChildUnder14	-0.007	-0.003	-0.001	-0.009**	-0.003	-0.005
XPost	(0.005)	(0.004)	(0.003)	(0.003)	(0.004)	(0.005)
Mean of Dep. Var.	0.293	0.511	0.645	0.676	0.638	0.383
Observations	135,954	253,116	185,328	122,158	116,877	
R-squared	0.195	0.348	0.502	0.509	0.505	0.399

*** 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 1 child age 10-13 in the household. Sample consists of all individuals related to the household head in households with at least 1 child age 25 or younger. Standard errors are clustered by household. Other controls as listed in notes to Table 2.

TABLE A.6. Alternate Clustering Methods
Rounds: 1983, 1987-8, 1993-4

	Dependent Variable: Any Economic Activity				
	Standard Cluster by Age-Round (1)	Standard Cluster by Age (2)	Wild Cluster Bootstrap by Age (3)	Standard Cluster by Under 14-Post (4)	Wild Cluster Bootstrap by Under 14-Post (5)
Under14XPost	0.026***	0.026***	0.026**	0.026***	0.026
	(0.005)	(0.007)	N/A	(0.000)	N/A
No. of clusters	24	8	8	4	4
p-value	0.000	0.005	0.010	0.000	0.176
Observations	327,233	327,233	327,233	327,233	327,233

Columns (3) and (6): Wild cluster bootstrap is implemented as in Cameron, Gelbach and Miller (2008) but using the 6-point distribution weights presented in Webb (2012) due to the low number of clusters. See the Online Appendix for full details on bootstrapping methods.