# LONG-TERM EFFECTS OF CHILD LABOUR BANS ON ADULT OUTCOMES: EVIDENCE FROM BRAZIL

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## Abstract

In December 1998, the minimum legal age to enter labour market increased from 14 to 16. This change gave rise to a natural experiment, as it prevented children who turned age 14 in January 1999 or after from participating in the formal labour force. This paper uses exact date of birth and cros1 0 0 1 413.83 457.27 Tm4.94 Tm[C)-2(aio Pi)-v 166.82 443.47 Tm[c

## INTRODUCTION

It is a plausible assumption that most policy makers are shortsighted in that they might not take into consideration long-term consequences of their decisions. When changes in th are made indiscriminately, policy makers may not care if the changes can affect individuals differently, particularly in cases in which the effectiveness of the rules somehow depends on the individual s background. The purpose of this paper is to assess the long-term consequences of a child labour ban on labour market and schooling outcomes of males affected by the ban.

In December 1998, Brazil passed a Constitutional Amendment increasing the minimum legal age of entry into the labour market from 14 to 16. The change in the minimum working age gave rise to a natural experiment, as an individual s eligibility status to participate in the formal labour force depends on individual s date of birth. This paper belongs to the strand of literature that uses birth date to compare outcomes of two cohorts who, despite being very close in age, are assigned to different treatment arms[1].

This paper uses the law of 1998 to investigate the long-term effects of postponing entrance into the formal labour force by up to two years (from 14 to 16).

outlines the potential experience in the labour market as the plausible mechanism through

are scant[2].

To assess whether the law affected differently individuals with different socio-economic background, the cohort affected by the ban is split groups of white and non-whites males. Skin color (or race) is used because it correlates well with several socio-economic indicators (including income poverty) and is exogenous[3]. Thus, we compare long-term outcomes of white males affected and unaffected and non-white males affected and unaffected by the ban.

The research question addressed in this paper has several policy implications: (1) it informs policy makers of the long run effects of across the board changes in legislation; (2) it reveals whether there are returns to experience of an earlier entrance into to the labour force; (3) it shows whether the returns to experience depend on the individual s socioeconomic background; and (4) it sheds light on long run unintended consequences of such decisions and signals whether this type of policy should be accompanied by compensating policies for those to whom it is more likely cause harm.

Common sense may suggest that early exposure to the labour market is likely harmful. In fact, child labour bans

individuals under 14 could work only as apprentices, whereas individuals younger than 18 were prohibited from hazardous and night work.

The law makes reference to apprenticeship status at the labour force despite the fact that the programme was institutionalised only in December 2000. Actually, this helps explain why the number of apprentices was so low before that year.[11] This ambiguity in the law seems to have generated some discussion in the Brazilian courts. The law is unclear about whether those who turned 14 before the law passed but were not participating into the labour force could still do so or not[12].

Anecdotal evidence suggests that some judges and labour lawyers interpreted the law differently. For one group, the law did not affect the eligibility status of individuals who turned age 14 before the increase in the minimum legal age. Therefore, those already working could carry on working in the formal labour market, and those not working could still participate into the formal labour force. Those who turned age 14 after the law passed were prevented from participating into the formal sector, but could do so as long as apprentices[13].

For the other group of experts the law should have become a binding constraint for all individuals who turned age 14 after its enactment, except for interested in taking up to the apprenticeship programme. The official statistics of participation rate w weekly hours worked for children at age 15 in 1999 show a still high participation rate with full-time job that year (more than 35 hours per week), suggesting that those who turned 14 before the ban were not affected by it.

Thus, the ban affected those who turned 14 years old in the second half of December 1998, that is, the law became a binding constraint only for a subgroup of

[11] According to Corseuil et al. (2011), who use the Brazilian Census of formal enterprises (*Relação Anual de Informações Sociais - RAIS*) to assess the impact of the Brazilian Apprenticeship Programme of 2000, the number of apprentices at age 14 in 1999 and 2000 was 82 and 99 respectively. On the other hand, the number of apprentices increases sharply from 2001 onwards. In 2002, for instance, the number of apprentices aged 14 reached 582.

[12] I consulted with few Labour Lawyers in Brazil and got different views on this regard.

[13] Given that the ban reduced the number of 14 year-old children in the formal labour market, it is unlikely that the law benefited those unaffected by the ban with higher wage rate as 14 year-old children are engaged in low-skilled jobs and are an input easy to substitute by the employees.

children who turned age 14 after December 15th 1998 and would participate in the formal labour force had the Amendment not been passed.

With the change in the law the Ministry of Labour stopped issuing work permits for individuals who turned age 14 after the law passed. Consequently, the law divided similar children into two groups: one banned from formal labour force (

) and one unaffected by the law (control group). Note that children affected by the ban who shifted to the informal sector automatically entered the child labour statistics whereas those with similar age (and plausibly other characteristics) but unaffected by the law did not.

The relatively large informal sector in Brazil can cast doubt on the effectiveness of such type of law. However, the effect of this intervention on participation rate of the treatment group depends on its enforceability but also on the size of problem it is trying to fix. The small participation rate in the formal labour force among teenagers under age 16 and the large informal sector in Brazil may suggest that the law would have had limited impact on chi

formal sector, the effect on participation rate would had been small, around 1-2 percentage points. If some of children participating in the formal sector simply shifted to informality after

been negligible or even positive. But, if some employers decided no longer to employ children under age 16 to avoid legal consequences such as paying fines , the law would proba this framework is useful as it sheds light on how outcomes of interest can be affected by the intervention under study[14].

To fix ideas, let be the utility function of individual *i* that depends on the consumption good, *C*, and leisure, *l*. The observed and unobserved characteristics of individual *i* are captured by the vector [15]. For the sake of simplicity, *C* is expressed in monetary units, and *l* in hours per day[16]. The problem of individual *i* is to maximise subject to the budget constraint: , where, *V* is the non-labour income, *w* is the hourly market wage (the wage rate), and *L* is the number of daily hours worked[17]. The number of daily hours worked is given by , that is, the total number of hours in a day minus the consumption of leisure, *l,* in a day. The Marshallian leisure demand function is given by: . By symmetry, the labour supply function is .

Individual *i* will participate in the labour force if the market wage rate is at least equal to his/her reservation wage, that is: if > , where is the individual *i* reservation wage. Assume that the wage rate paid in the formal labour market, , is higher than the wage rate paid in the shadow economy, [18]. It is therefore assumed that individuals with the same average observed and unobserved characteristics will have the same reservation wage distribution.

For an individual *j* with a disadvantaged background, assume that < . This implies that individuals with poorer backgrounds are less likely to drop out of the labour force than the better off for whatever market wage rate. Figure A.1 illustrates the hypothetical distributions of reservation wages of individuals *i* and *j*. For the sake of

[14] See for instance, Borjas (2012). The theoretical framework could be modified to include more complex household decisions as in different versions of household models. However, we opted to keep things simple

[15] This vector can also include individuals

simplicity, the figure assumes log-normal distributions with the same variance. The distributions differ only in terms of averages. For the sake of illustration, the average reservation wage of individual *i* is assumed to be 14 and 10 for individual *j*. This implies that individuals with disadvantaged backgrounds are less likely to drop out of the labour force for an exogeneous reduction in market wage rate from than individual *i*[19].

Given that the government passed a law preventing children turning age 14 after December 1998 from participating in the formal labour force, individuals just under and just above age 14 will have similar average observed and unobserved characteristics, $e$[20], but will face different wage rates and incentives to participate in the labour force. This simple framework results in three groups of individuals with similar average characteristics $e$: (1) one would not be affected by the law ( $>$ $>$ ) group one; (2) one that would be affected by the law and would shift to the informal labour force ($w_F$ $w_{Inf}$ $w$) group two; and (3) one that would be affected by the law and would drop out of the labour force ($w_F$ $w$ $w_{Inf}$) group three.

Assuming that individuals approaching the cutoff age face a positively inclined labour supply function, the exogeneous change in wage rate from to will discourage some individuals to stay in the labour force. It is as if the law generated two scenarios in which individuals with similar observed and non-observed characteristics face two different incentives to participate in the labour force. For those who stay in the market, one could expect a reduction or an increase in the weekly hours worked[21].

in wage rate from $w_F$ to $w_{Inf}$, one can then expect a negative effect on the extensive and intensive margins of labour supply for those who decide to drop out

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[19] Assume that the market wage rate drops from to 10. It can be easily seen in the figure that area B will shrink by about a half, whereas area A will reduce only marginally. Analogously, the shift from to 10 can be seen as the shift from to .

[20] This is consistent with the regression discontinuity design framework and will be shown in the data section.

[21] A fall in wage will imply fewer hours of work due to the substitution effect and more hours of work due to the income effect if leisure is a normal good. The total effect of a wage will be ambiguous. However, if leisure is inferior, the fall in the wage rate will be negative because the income effect will imply fewer hours of work. See, for instance, Borjas (2012).

of the labour force, and an ambiguous effect on the intensive margin of labour supply for those who move into the informal economy[22].

## 3.1 WHO ARE MORE LIKELY TO BENEFIT FROM AND BE HARMED BY THE LAW?

The impact of the law on labour supply depends on substitution and income effects. Based on the assumptions outlined above, individuals can be separated into two groups: the better off (group one) and the worse off (group three).

The better off will drop out of the labour force and consume more leisure, participate more actively in household chores, and/or study more. Whatever is the case, the better off will accumulate less work experience, but maybe more education. If there is an experience premium in the labour market, this group is expected to have lower wages than their counterparts in the long run. However, this negative effect could be at least partially counterbalanced if it turns out that the better off substituted work with school.

The worse off, on the other hand, are more likely to shift to the informal sector.

the shadow economy, as it is not formally registered in personal records[24]. However, if

in which it was accumulated, then those who shifted to the informal sector would not be jeopardised by the ban.

Short run estimates were provided to white and non-white males to check whether the results are consistent with the predictions of the theoreti

1998 affected only those who turned 14 from Jan 1999 onwards. The analysis of the long-term effects of the law on individual outcomes consists of comparing the cohorts who turned 14 in the second half of 1998 with individuals who turned 14 in the first half of 1999. However, unlike Angrist and Krueger (1991) and many other authors who combine birth date with school entry or exit ages, parents could not have anticipated this change in law and its effects[26].

Using the household surveys of 2007, 2008, 2009, and 2011, the impact of the ban on the outcomes of interest are estimated fitting the following reduced-form regression model,

$$= a + r \quad + \quad (\quad) + b \; ' + \quad , \qquad (1)$$

where  is the outcome of individual *i*, *D* is a dummy that takes on the value of 1 if the individual turned age 14 in the first half of 1999 and could not participate in formal labour market due to the ban, and 0 if s/he turned 14 in the second half of 1998 and was thus allowed to do so. The function *h(.)* depends on age, the forcing variable, and will be referred to as the . The variable age, *Z*, is defined in weeks and is set to 0 for individuals who turned 14 on the last week of December 1998. Thus,  takes the value of 1 for the first week of January 1999, 2 for the second week, and so on. Analogously,  takes the value of -1 for the third week of December 1998, -2 for the second week, and so on.  is a vector of controls that includes skin colour and elements of  and  is the error term. Most of the regressions are estimated without controls.

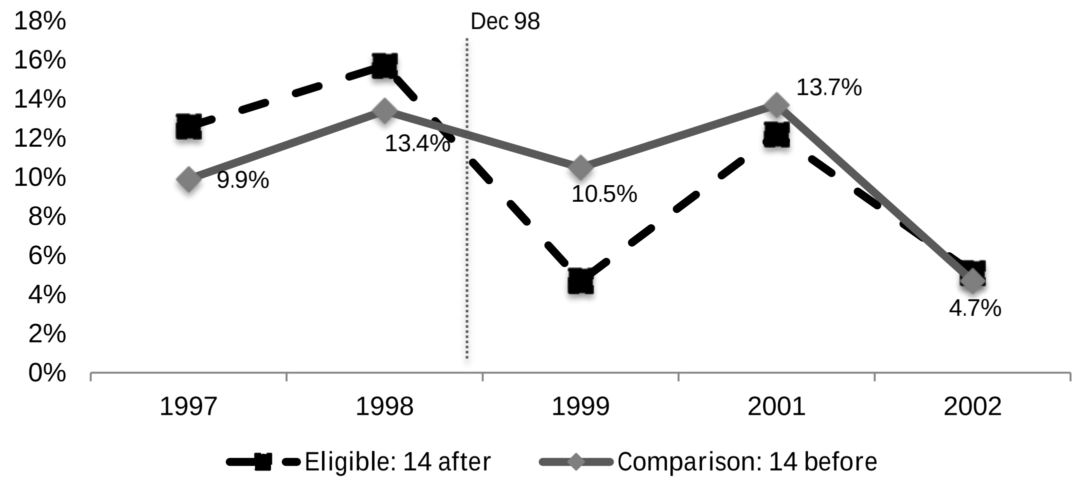
The parameter of interest, , corresponds to the *intent-to-treat* as long as the analysis is performed for all individuals who belong to the cohort affected by the law rather than the subgroup of individuals affected by the law (those who stopped participating in the labour market or who were *de facto* prevented from doing so because of the increase in the minimum legal age[27]. The identification of this parameter depends

[26] See, for instance, Smith (2009) and McCrary and Royer (2011), and Black et al. (2011). For criticisms on using date of birth as an instrumental variable to years of schooling, see Bound, Jaeger and Baker (1995) and Staiger and Stock (1997).
[27] For a comprehensive introduction to different treatment effects parameters, see Heckman, Lalonde and Smith, 1999.

would

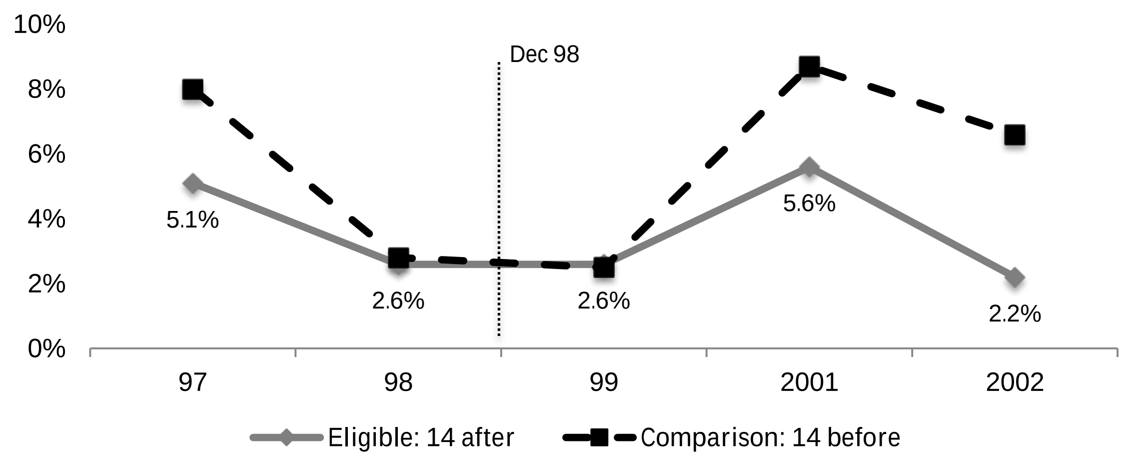
The estimates are initially obtained with a six

Figure 1 Trends in the Labour Force Participation Rate of Males in Urban Areas
*Different Cohorts – 3 Months Bandwidth*




Source: PNADs of 1997, 1998, 1999, 2001, and 2002.

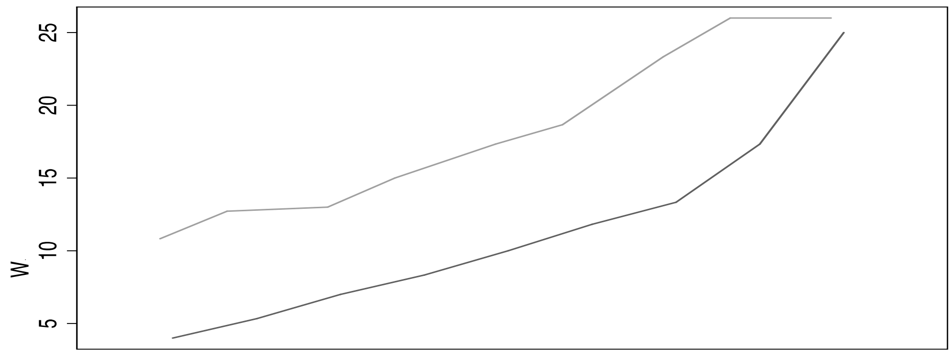
Figure 2 Trends in the Labour Force Participation Rate of Females in Urban Areas
*Different Cohorts – 3 Months Bandwidth*




Source: PNADs of 1997, 1998, 1999, 2001, and 2002.

The trends in the labour force participation rate show that participation rate dropped among males 14, but more sharply among those who turned 14 after December 1998. This is an interesting result, because it suggests that (1) the ban affected mostly the eligible group; (2) the effect of the ban went beyond the formal sector; and (3) the fall in the Brazilian GDP in 1998 is unlikely to be driving the results[33]. Figure 2 suggests that the ban did not affect the participation rate of girls, since the drop observed in the eligible group seems to be due to common macro shocks. As shown below, short run estimates support the descriptive evidence and the findings discussed in chapter one[34]. It is interesting to note that for both boys and girls the figures return to a similar level

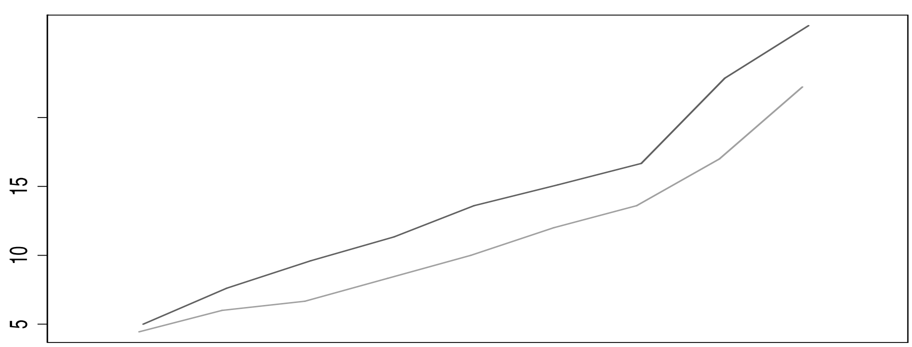
Figure 3 First Order Stochastic Dominance: Hourly Wage Distributions for Formal and Informal Workers at Age 14 in 1998




Source: PNAD 1998.
Note:
FOSD (see e.g. Jenkins and Van Kerm, 2009). In 1998, the Brazilian monthly minimum wage was R$ 130.

The lower wage rate in the informal sector may have contributed to the fall in the labour force participation rate, since the wage rate in the informal economy would be lower than the reservation wage for some individuals. In fact, figure 4 shows that the hourly wage of the eligible group was below the wage rate received by those who were ineligible.

Figure 4 First Order Stochastic Dominance: Hourly Wage Distributions for Children Aged 14 Before and After December 1998
*52 Weeks Bandwidth*




Source: PNAD 1999.

Taking this set of descriptive results into account, it is possible to roughly estimate the effect of a change in wage rate on the labour force. Since individuals who are very close in age are likely to have similar observed and unobserved characteristics, the ban gave rise to a natural experiment wherein individual faced two different wage rates. Thus, it is plausible that a fraction of individuals who have a reservation wage above the wage rate paid in the informal sector dropped out of the labour force after the ban.

The difference in wage rate between the eligible and ineligible groups in 1999 was, on average, about 16%[36]. Figure 1 shows that the very responsive to variations in wage rate, but it means that boys have to work harder to compensate for a reduction in wage. This estimate is similar to that which is considered the benchmark in the literature[37]. This result is consistent with the hypothesis that child labour is influenced by poverty status of the household (Bhalotra, 2007)[38].

The figures below present the visual checks of the short run effects of the ban. Linear regressions are fitted in each side of the cutoff point. Since the survey provides the exact birth date of each individual, age was defined in weeks to mitigate excess noise and standard errors clustered at the week level[39].

Figures 5a, 5b and 5c show a decrease in the labour force participation rate for males, white and non-white males in 1999 respectively[40].

Figure 5a Labour Force Participation Rate in 1999
*Males – 26 Weeks Bandwidth*

Figure 5b   Labour Force Participation Rate in 1999
*White Males – 26 Weeks Bandwidth*

Figure 5c   Labour Force Participation Rate in 1999
*Non-white Males – 26 Weeks Bandwidth*

Figure 5a shows a sharp fall in participation rate among boys whereas figures 5b

5b indicate that the participation rate followed a downward trend among white males. In figure 5c the regression lines are very flat. Figures 6a, 6b and 6c illustrate the effect of the ban on females, white and non-white females respectively.

Figure 6a Labour Force Participation Rate in 1999

*Females – 2[illegible]andwid[illegible]*



BT 6a TJE1 0 33.34 Tm[ )]TJETBT1 0 0 1 274.13 633.34 Tm[(F)6(or)-6(c)4(5117.961-691.. ) 5 BDC BT[TJET EMC /7n 199

Figure 6c Labour Force Participation Rate in 1999
*Non-white Females – 26 Weeks Bandwidth*

Figures 6a points to some increase in participation rate among girls, but figures 6a and 6b suggests that participation may have increased only among non-white females. Regression analysis below will inspect the discontinuities further testing different specifications of the forcing variable.

If the ban gave rise to a natural experiment for individuals with close dates of birth, the observed characteristics of individuals just to the left and right sides of the cutoff point should be statistically similar.

Table 1 presents the t-test for difference in means for some covariates with a six months (or 26 weeks) bandwidth. The table reports the coefficients of simple regressions of each covariate on a constant and the indicator function *D*, with *D* defined as in eq. (1). The estimates consider the same cohorts that are used in the estimates of the long run effects of the ban.

Figure 7a  Predicted Log Wage  Long Run

*White Males – 26 Weeks Bandwidth*

Figure 7b  Predicted Log Wage  Long Run

*Non-white Males – 26 Weeks Bandwidth*

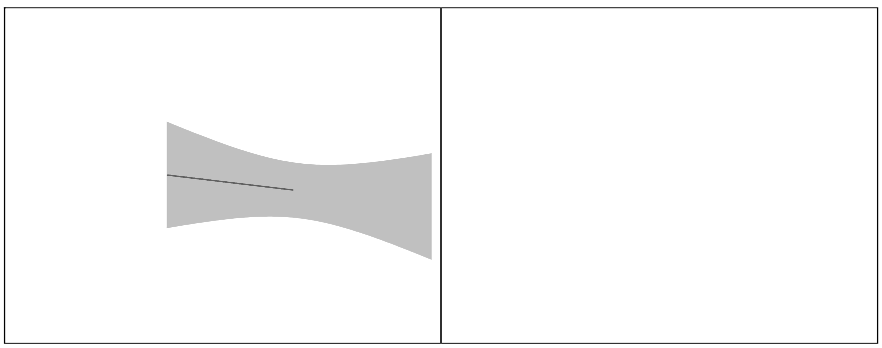


Figure 8a  LFPR  Long Run

*White Males – 26 Weeks Bandwidth*

Figure 8b LFPR Long Run

*Non-white Males – 26 Weeks Bandwidth*

Figure 9a

Figure 10a Probability of Pursuing or Holding College Degree Long Run
*White Males – 26 Weeks Bandwidth*

Figure 10b Probability of Pursuing or Holding College Degree Long Run
*Non-white Males – 26 Weeks Bandwidth*

As before, the figures provide linear estimates for the long run effect of the law. Assuming a common time effect, the ban seems to have influenced many outcomes, but only marginally. It is worth saying that these linear regressions lines are not controlling

## 6.2 LONG RUN EFFECTS OF THE BAN

### ITT ESTIMATES ON WAGES: RETURNS TO EXPERIENCE?

This section considers the long run effects of the ban of 1998. It starts reporting the impact of the law on the average hourly wage of the cohort prevented from working due to the ban[43].

Table 4 presents the ITT estimates without controls and a 6-month bandwidth. The table shows two sets of estimates. In the first set (columns 1-6), the ban is assumed to have a constant effect during the period. The second set of estimates (columns 7-12) relaxes this assumption and allows for heterogeneous time effects. Since contemporaneous education can have a direct effect on earnings, the estimates exclude school attenders[44].

Estimates are provided with different specifications of the smooth function. The first row of the table shows six distinct specifications, with the first column consisting of a difference in means (polynomial of degree zero), whereas in the second, third, and fourth columns the smooth function is specified as polynomials of degree one, two, and three respectively. The last two columns consist of linear and quadratic splines. In these two cases, the slope of the functions fitted in each side of the cutoff point is permitted to differ.

Although the point estimates are sensitiþgETBT0 arecific is

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Eq. (4) is the traditional Mincer equation, in which the log wage is specified as a quadratic function of the potential experience and as a linear function of the years of schooling[49]. According to the ordinary least squares (OLS) estimates shown in Table A.3, returns to experience seem to be higher for whites than for non-white males. The difference of about four percentage points could be reflecting unobserved background characteristics. One could think of white males as having better occupations or as more likely to accumulate experience in the formal sector

Although drawing on a different method and country, these results are qualitatively similar to some evidence found for the US. Connolly and Gottschalk (2006), for instance, use ten years (1986-1996) of the Survey of Income and Program Participation, a panel that collected monthly continuous information of workers for a period of up to 48 months. They use this long panel to investigate whether the less educated gain less from returns to experience. According to their results, the returns to exogeneity of the law of 1998, and it consists of comparing the horizontal distance of two unconditional wage distributions for any given quantile.

Table 8 presents the impact of the law on the wage gap of the two groups at different points of the unconditional hourly log wage distribution, assuming common time effects.

The results suggest that the ban had a significant positive effect at the workers. Their results indicated a higher return to general experience for whites than blacks, but black workers experienced higher returns to tenure than white workers.

Using a different approach and PNAD data from 1996, Emerson and Souza (2011) show that, on average, the returns to experience tend to be lower than the returns to education up to age 31. Given that the cohorts followed in this study are in their mid-20s, this seems consistent with the results for white males. However, the impact of the ban on the wages of the cohort of non-white males suggests that the returns to experience might be higher than the returns to education for individuals at the lower end of the wage distribution. Although returns to education are not provided here, they are unlikely to reach 20%. If this is the case, Emerson and Souza (2011) findings may not hold across the board. Our estimates show that the impact of an early entrance into the formal labour force varies with the individual s socio-economic background and along the unconditional distribution of hourly wage.

This finding has immediate implications for public policy. It shows that prohibiting households from sending young boys to the formal labour force at age 14 may not pay off if the returns to education for poor individuals who have to attend low quality public schools and carry on working informally might not be high. Conversely, returns to education are high for better off males who face fewer constraints to attending high quality schools. Returns to experience might be more relevant to those from disadvantaged backgrounds.

The main findings of this paper are also supported by theoretical predictions. Dessy and Knowles (2008) use a theoretical model to argue that a child labour ban can make the not-so-poor better off. However, their model shows that a ban can jeopardise the poorest households by reducing the total

opportunities for education. There is no evidence that the Brazilian ban reduced males, the ban seems to have affected household welfare through its impact on total household income.

## 7 ROBUSTNESS CHECK

In this section, the same regressions are re-estimated with a bandwidth of 3 months. The disadvantage of using a narrower bandwidth is that it increases the sampling variance and reduces the estimates precision (power). The small sample size increases the chances of type II error, i.e., one might not be able to reject the null when it is false.

The eligibility dummy *D* is redefined so as to take the value of 1 if an individual turned 14 between October and December of 1998 and 0 if s/he turned 14 in the first three months of 1999. If the effect were very local, then one would expect a slightly higher impact in absolute terms. Table 10 shows the ITT estimates for the impact of the law on the log of hourly wage. Although qualitatively similar to those obtained with a larger bandwidth size, the reduction in precision resulted in statistically insignificant point estimates[54].

Tables 11 and 12



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distribution by increasing the wage gap between those at the bottom and top of the earnings distribution.

## CONCLUSION

This paper investigated the long-run effects of a Brazilian law from December 1998 that increased the minimum legal age of entry into the labour market from age 14 to 16. To our knowledge, this paper contributes to the scarce evidence of the long run effects of early participation in the labour force on adult outcomes. In addition, to the best of our knowledge this is the first paper to provide long run causal estimates for the impact on the cohort affected by a change in the minimum legal age of entry into the labour market.

This paper drew on Angrist and Krueger (1991) and explored dates of birth around the date the law was enacted to estimate local treatment effects. The results suggest that the law had heterogeneous effects across gender and race. Short run estimates suggest that the law affected only boys and long run estimates confirmed that hypothesis. The main results indicate that the law benefited white males but harmed non-whites. Except for the impact on the probability of holding a high school degree, all estimates indicate that white males prevented from entering the labour force at age 14 had better outcomes compared to those unaffected by the law. On the other had, the estimates indicate that non-white males prevented from working at age 14 had worse outcomes in adult life compared to the control group.

The ITT estimates on wages were interpreted as lower bound for the returns to experience as long as the eligible and comparison groups have the same distribution of completed years of schooling, and estimates were obtained for non-school attenders. Unconditional quantile treatment effects were estimated to shed light on the distributive impact of the law. The results showed higher earnings for white males at the bottom decile of earnings distribution and negative effects for non-white males at the median of earnings distribution. Under rank preserving condition, this indicates that the law increased earnings inequality among non-white males, decreased earnings inequality

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Table 1 T-test for Difference in Means in 1999 Males

*26 Weeks Bandwidth*

| | All | Whites | Non-whites |
|---|---|---|---|
| Mother's education | 0.15 | -0.072 | 0.38 |
| | (0.68) | (-0.22) | (1.41) |
| N | 1839 | 891 | 948 |
| Father's education | -0.0041 | -0.038 | 0.051 |
| | (-0.019) | (-0.12) | (0.19) |
| *N* | *1839* | *891* | *948* |
| Mother's age | -0.22 | -0.95 | 0.48 |
| | (-0.23) | (-0.71) | (0.35) |
| *N* | *1839* | *891* | *948* |
| Father's age | -1.09 | -0.98 | -1.23 |
| | (-1.15) | (-0.72) | (-0.92) |
| *N* | *1839* | *891* | *948* |
| Household size | 0.034 | 0.085 | -0.020 |
| | (0.46) | (0.91) | (-0.18) |
| *N* | *1839* | *891* | *948* |
| Land title | -0.013 | -0.034* | 0.0080 |
| | (-0.91) | (-1.88) | (0.37) |
| *N* | *1456* | *707* | *749* |
| Household non-labour income | -0.0014 | -0.19 | 0.21 |
| | (-0.0013) | (-0.10) | (0.22) |
| *N* | *1839* | *891* | *948* |
| Monthly earnings | -23.5* | 10.4 | -28.7*** |
| | (-1.84) | (0.36) | (-2.63) |
| *N* | 163 | 67 | 96 |
| Monthly household net income (net of children's income) | 19.3 | 43.4 | 1.22 |
| | (0.49) | (0.61) | (0.035) |
| *N* | *1839* | *891* | *948* |

Source: PNAD 1999.

Note: The T-test is performed through simple regressions with each covariate *X* being regressed on a constant and the indicator variable *D*. T-statistic in parenthesis. ***, **, * Statistically significant at 1, 5 and 10 percent respectively.

Table 2 T-test for Difference in Means Males

*26 Weeks Bandwidth*

| | All | Whites | Non-whites |
|---|---|---|---|
| ***Covariates*** | | | |

Table 3 Short Run Effects of the Ban on Labour Force Participation Rate

| Functional Formal of *h(z)* | White Males | Non-white Males | White Males | Non-white Males | White Females | Non-white Females | White Males | Non-white Females |
|---|---|---|---|---|---|---|---|---|
| | *3 Months Bandwidth* | | *6 Months Bandwidth* | | *3 Months Bandwidth* | | *6 Months Bandwidth* | |
| 0 | -0.085*** | -0.071* | -0.11*** | -0.059** | -0.00087 | 0.0042 | -0.012 | **-0.023** |
| | (-2.87) | (-1.64) | (-4.86) | (-2.14) | (-0.047) | (0.18) | (-0.95) | **(-1.31)** |
| 1 | 0.0059 | -0.091 | **-0.054** | -0.041 | -0.014 | 0.048 | 0.012 | 0.040 |
| | (-0.1) | (-0.88) | **(-1.37)** | (-0.66) | (-0.46) | (1.03) | (0.49) | (1.15) |
| 2 | 0.0076 | -0.089 | **-0.054** | -0.043 | -0.015 | 0.047 | 0.012 | **0.045** |
| | (-0.14) | (-0.87) | **(-1.34)** | (-0.68) | (-0.46) | (1.01) | (0.48) | **(1.37)** |
| Spline linear | 0.01 | -0.09 | **-0.053** | -0.042 | -0.014 | 0.046 | 0.011 | **0.047** |
| | (-0.18) | (-0.88) | **(-1.32)** | (-0.68) | (-0.44) | (0.97) | (0.45) | **(1.40)** |
| Spline quadratic | **-0.12** | **-0.12** | -0.013 | **-0.15** | | | | |

Table 6 Long Run Effects on Being a Formal Employee Whites and Non-whites Males

*26 Weeks Bandwidth – Exclude School Attenders*

| | *White Males* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Polynomial degree** | 0 | 1 | 2 | 3 | spline linear | quadratic spline | 0 | 1 | 2 | 3 | spline linear | quadratic spline |
| D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998) | 0.0083 | 0.028 | 0.027 | 0.075 | | | | | | | | |

Table 7

Table 10

Table 11 Long Run Effects on Being Employed White and Non-white Males

*12 Weeks Bandwidth – Exclude School Attenders -*

| | ***White Males*** | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Polynomial degree** | 0 | 1 | 2 | 3 | spline linear | quadratic spline | 0 | 1 | 2 | 3 | spline linear | quadratic spline |
| D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998) | -0.0021 | -0.027 | -0.028 | -0.048 | -0.028 | -0.054 | 0.028 | 0.00050 | -0.00068 | -0.023 | -0.00089 | -0.028 |
| | (-0.082) | (-0.54) | (-0.56) | (-0.77) | (-0.55) | (-0.72) | (0.43) | (0.0061) | (-0.0082) | (-0.26) | (-0.011) | (-0.28) |
| D*2008 | | | | | | | -0.10 | -0.10 | -0.10 | -0.10 | -0.10 | -0.10 |
| | | | | | | | (-1.14) | (-1.15) | (-1.16) | (-1.18) | (-1.15) | (-1.18) |
| D*2009 | | | | | | | -0.024 | -0.023 | -0.023 | -0.026 | -0.023 | -0.027 |
| | | | | | | | (-0.28) | (-0.27) | (-0.27) | (-0.31) | (-0.27) | (-0.31) |
| D*2011 | | | | | | | 0.0036 | 0.0046 | 0.0043 | 0.0037 | 0.0044 | 0.0041 |
| | | | | | | | (0.048) | (0.060) | (0.056) | (0.048) | (0.057) | (0.054) |
| *Dummies for years* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| Observations | 1074 | 1074 | 1074 | 1074 | 1074 | 1074 | 1074 | 1074 | 1074 | 1074 | 1074 | 1074 |

| | ***Non-White Males*** | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Polynomial degree** | 0 | 1 | 2 | 3 | spline linear | quadratic spline | 0 | 1 | 2 | 3 | spline linear | quadratic spline |
| D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998) | -0.0082 | -0.077* | -0.081** | -0.022 | -0.083** | 0.0091 | 0.081 | 0.013 | 0.0095 | 0.069 | 0.0075 | 0.099 |
| | (-0.39) | (-1.89) | (-1.99) | (-0.41) | (-2.01) | (0.15) | (1.65) | (0.21) | (0.15) | (0.95) | (0.12) | (1.25) |
| D*2008 | | | | | | | -0.13* | -0.12* | -0.12* | -0.12* | -0.12* | -0.12* |

Table 15

Table 18 Placebo Effects on Being Employed White and Non-White Males

*26 Weeks Bandwidth – Exclude School Attenders*

| | *White Males* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Polynomial degree** | 0 | 1 | 2 | 3 | spline linear | quadratic spline | 0 | 1 | 2 | 3 | spline linear | quadratic spline |
| D (=1 if 14 after June 1999; =0 if 14 before June) | -0.032** | -0.029 | -0.028 | -0.017 | -0.028 | -0.0017 | -0.049 | -0.047 | -0.046 | -0.034 | -0.045 | -0.035 |
| | (-2.04) | (-0.95) | (-0.93) | (-0.43) | (-0.91) | (-0.043) | (-1.51) | (-1.17) | (-1.15) | (-0.72) | (-1.13) | (-0.67) |
| D*2008 | | | | | | | 0.057 | 0.057 | 0.057 | 0.057 | 0.057 | 0.057 |
| | | | | | | | (1.24) | (1.24) | (1.24) | (1.24) | (1.24) | (1.24) |
| D*2009 | | | | | | | 0.0087 | 0.0087 | 0.0082 | 0.0085 | 0.0076 | 0.0065 |
| | | | | | | | (0.20) | (0.20) | (0.19) | (0.20) | (0.18) | (0.15) |
| D*2011 | | | | | | | 0.0050 | 0.0049 | 0.0048 | 0.0051 | 0.0047 | 0.0046 |
| | | | | | | | (0.11) | (0.11) | (0.11) | (0.12) | (0.11) | (0.11) |
| *Dummies for years* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* | *Yes* |
| Observations | 3386 | 3386 | 3386 | 3386 | 3386 | 3386 | 3386 | 3386 | 3386 | 3386 | 3386 | 3386 |

| | *Non-White Males* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Polynomial degree** | 0 | 1 | 2 | 3 | spline linear | quadratic spline | 0 | 1 | 2 | 3 | spline linear | quadratic spline |
| D (=1 if 14 after June 1999; =0 if 14 before June) | -0.0030 | -0.025 | -0.025 | -0.021 | -0.025 | -0.044 | 0.020 | -0.0011 | -0.0013 | | | |

Table 19 Placebo Effects on Being a Formal Employee White and Non-

**Appendix**

Table A.1 T-test for Difference in Means in 1998 White vs. Non-White Males

| | Non-whites | Whites | *P*- |
|---|---|---|---|

Table A.5 Effect of the Ban on Occupation of Adult Males ITT Estimates

*26 Weeks Bandwidth – Heterogeneous Time Effects*

| | Directors in General | Science & Arts | Technicians | Administrative Services | Service Sector | Commerce Sector | Agricultural Sector | Civil Construction | Army |
|---|---|---|---|---|---|---|---|---|---|

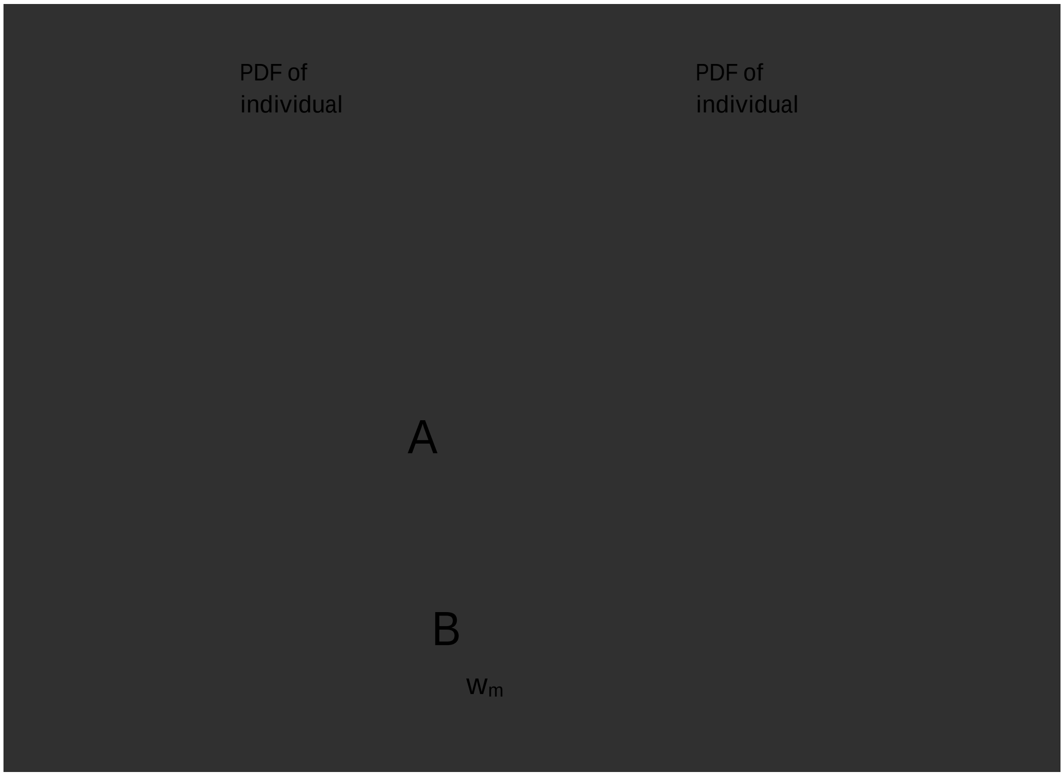
Table A.6 Long Run Effects on Hourly Log Wages White and Non-White Males

*26 Weeks Bandwidth – with controls*

| | ***White Males*** | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Polynomial degree** | 0 | 1 | 2 | 3 | spline linear | quadratic spline | 0 | 1 | 2 | 3 | spline linear | quadratic spline |
| D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998) | -0.016 | 0.038 | 0.038 | 0.16** | 0.038 | 0.19** | | | | | | |

Table A.

Figure A.1 Theoretical Framework Reservation Wages Distributions for Individuals *i* and *j*




Note: The solid PDF corresponds to the reservation wage distribution of the worse-off whereas the dashed PDF is the reservation wage distribution of the better off. To keep things simple, the distributions are assumed to be normally distributed and to differ only with respect to the averages. The figures show that the proportion of individuals with reservation wage below than the hypothetical market wage, $w_m$, is larger among the worse-off. This can be seen comparing the areas A and B. Consequently, an exogenous reduction in the market wage from to will affect more the participation of the better off than the worse-off.

Figure A.2 Labour Force Participation Rate in 1998

*Males – 51 Weeks Bandwidth*

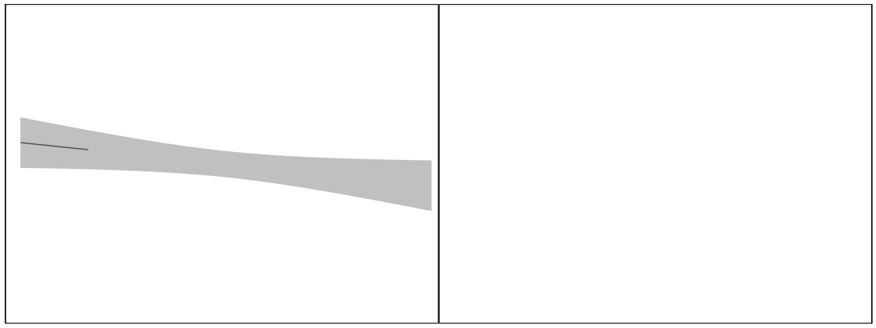


Figure A.3 Labour Force Participation Rate in 1998

*White Males – 51 Weeks Bandwidth*

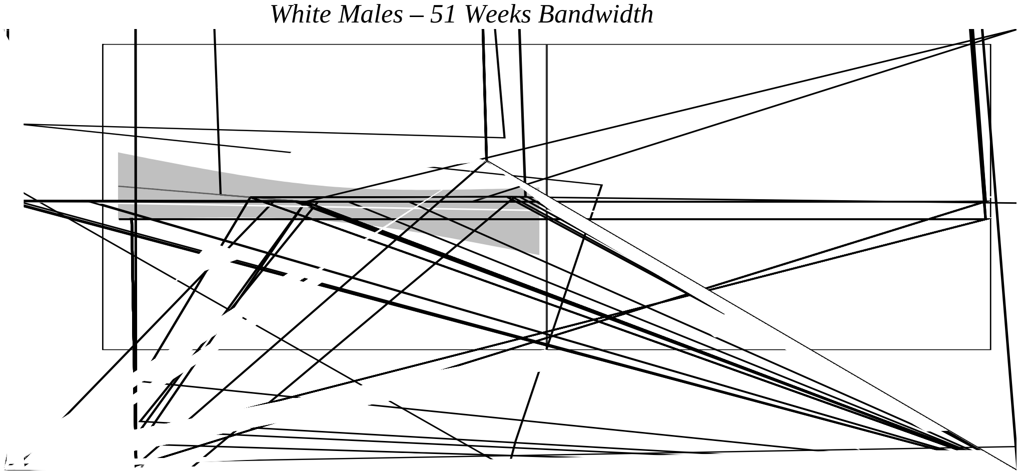


Figure A.4 Labour Force Participation Rate in 1998

*Non-white Males – 51 Weeks Bandwidth*