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 Brazil's 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 household surveys from 2007, 2008, 2009, and 2011 to uncover the long term impacts of this intervention, comparing outcomes of the cohorts who were age 14 just before and just after the law passed, using regression discontinuity design. Since the estimates are performed for all individuals in the two cohorts, the parameter the estimates identify the *intent-to-tre*at (ITT). Estimates are provided for the whole period and allow for heterogeneous time effects. To check whether the change in the law affected groups with different socio-economic backgrounds, estimates are provided for whites and non-whites separately. Unconditional quantile treatment effects (QTE) are also estimated to check whether the child labour ban has had distributive effects. The main results suggest that the law affected white and non-white males differently. Preventing the whites from entering the labour force at age 14 resulted in better long run outcomes. The opposite is observed for non-whites. QTE estimates indicate that the law had distributive effects. Most of the estimates are robust to different bandwidth sizes. Finally, a placebo test is performed for two cohorts presumably unaffected by the law. None of the estimates are statistically significant. The results suggest that accumulated experience is likely the mechanism underlying the impact of the law.

Keywords: Child labour, long-term effects, treatment effects, and returns to experience.

JEL: C21, J08, J31, J38, K31.

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 the ‘rules of the game’ 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). The research question can be twisted to also investigate the effect of early exposure to the labour market on long-term outcomes. This question parallels the literature on the impact of youth employment on an individual’s long run outcomes. Most papers that use date of birth to estimate the long-term effects of a law or intervention focus on the impact of early school enrolment. The literature outlines the educational channel as the main mechanism linking date of birth to labour market outcomes. Empirical papers that provide causal estimates for long-term effects of youth employment (or child labour) and

[1] Angrist and Krueger (1991) were the first to use date of birth to identify eligible and ineligible groups for a treatment. After Angrist and Krueger (1991) many other authors have used date of birth as an instrumental variable. See, for instance, Dobkin and Ferreira (2010), Bedard and Dhuey (2011), and Black, Devereux, and Salvanes (2011). Bound et al. (1995) and Bound and Jaeger (2000) showed that quarter of birth can be a weak instrument, but more recently Buckles and Hungerman (2013) casted doubts on the validity of quarter of birth as instrumental variables at least for the US as mothers who give birth during the winter and summer seem to have very different socio-economic backgrounds. This does not seem to be a problem in the present paper as suggested by placebo test that compares other two cohorts.

outlines the potential experience in the labour market as the plausible mechanism through which such law can affect individuals' outcomes 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 have been justified on theoretical grounds (see e.g. Baland and Robinson, 2000; Dessy and Knowles (2008), though some also argue that depending on the context of the household lives, a ban can actually backfire[4]. The

[2] There is plenty of evidence of the impact of vocational training on youth outcomes. The question addressed in this paper is different, as it aims to discover the impact of hindering children aged 14 from participating in the labour market for up to 2 years.

[3] The literature on returns to education has shown heterogeneous effects due to ethnicity as well (see e.g. Angrist and Krueger, 1991 for the US; and Stefani and Biderman, 2006 for Brazil). One alternative way of estimating heterogeneous effects would be splitting the sample according to household income per capita, but this would have at least two implications for the empirical exercise. First, splitting the sample in quartiles, for instance, would reduce the sample in such a way that the first stage regressions would be likely underpowered. As will be discussed below, the first stage regressions are reported using a household survey from 1999 and the sample size with 3 and 6 months bandwidth is relatively small. Second, using household income per capital could result in biased estimates because it is very likely to affect time allocation of household members. To circumvent the issue of low power quantile treatment effects are provided instead. The advantage of quantile regression in the present case is that it provides estimates for the impact of the intervention in different quantiles of the earnings distribution. However, with quantile regressions we are unable to have a distribution of average treatment effects. For a discussion, see Abbring and Heckman (2007).

[4] Basu and Van (1998) and Basu (2005) theorize that child labour bans can backfire. The theoretical model developed by Dessy and Knowles (2008) also implies that a child labour ban is more likely to affect the not-so-poor and end up harming the poorest.

consequences of banning individuals from entering the formal labour force at age 14 in the short and long runs are ultimately an empirical question.

Emerson and Souza (2011) show that child labour harms individuals' outcomes in adult life. They use the Brazilian household survey *Pesquisa Nacional por de Domicílios* (PNAD) of 1996 to show that the wage earned by the cohort of adults who worked during their youth is lower compared to those who did not work during that period of their lives. Using the number of schools and teachers per 1,000 children in the state of their birth as instruments for participation in the labour market and school attendance, they show that child labour has a short run negative effect, with lower investment in human capital, and a long run negative effect, with lower (adult) earnings. However, their findings suggest that the negative effects vanish around age 30.

Lee and Orazem (2010) borrow Emerson and Souza's (2011)[5] identification strategy to estimate the long run effects of child labour on health outcomes of adults Brazilians using PNAD 1998[6]. The estimates suggest that a simultaneous effect of an early entrance into the labour force and premature school dropout resulted in higher probability of back problems, arthritis, and reduced stamina. Despite using an IV strategy, the authors are incapable of disentangling the effects of child labour and more time spent in school on adults' health outcomes.

Beegle et al. (2009) use an instrumental variable approach to investigate the medium-term consequences of child labour on schooling, labour market, and health outcomes in rural Vietnam. They use two waves of a panel data collected in 1992-93 and 1997-98 and rice price and community shocks as instruments to identify the causal impact of child labour in individual outcomes five years later. They consider the sample of individuals aged 8-13 as the baseline. Their findings suggest that child labour had a negative effect on school attendance and educational attainment, but a positive effect on labour market outcomes, such as employability in paid work and earnings. They found no impact on health outcomes. Based on these mixed results, Beegle et al. (2009) argue that for some individuals the returns to experience seem to overcome the returns to education, at least in the medium term in rural Vietnam. These results help explain why child labour

[5] In fact, Lee and Orazem (2010) refer to Emerson and Souza's (2011) working paper.
[6] PNAD 1998 has a special supplement on health outcomes.

exists and cast doubt on the hypothesis that parents are myopic or that children who enter the labour force relatively early do so due to credit constraints or lack of information on the returns to education.

This paper uses regression discontinuity design to investigate the impact of the ban of December 1998 on the following adult outcomes: hourly wage, likelihood of being employed, likelihood of being employed in the formal sector, and likelihood of either holding or pursuing a college degree. Cohorts of individuals born in the first half of 1985—age 14 in the first half of 1999—are compared to the cohorts of males born in the second half of 1984 and were age 14 in the second half of 1998. Estimates are provided for a 6-month bandwidth on each side of cutoff point (the date of the law). To check robustness, estimates are also provided with controls and a bandwidth of three months.

Unconditional quantile treatment effects (QTE) are estimated to shed light on the distributive impacts of the change in the law on hourly wages. To test whether the law affected most strikingly those of disadvantaged backgrounds, estimates are provided for whites and non-whites.

The main results show that the ban had long-lasting effects on the groups of white and non-white males, contributing to increase wage differentials between these two groups. There is some indication that the affected cohort of white males benefited from higher wages, higher probability of being employed and having a formal occupation, and higher probability of holding a college degree. For non-white males, the results suggest the opposite—that is, the ban implied lower wages for the non-whites and lower probability of being employed and having a formal occupation. Unconditional quantile treatment effects point to distributive effects among white and non-white males. Under rank preserving assumption, it could be argued that the ban harmed non-white males at the median of earnings distribution but benefited white males at the lower end of the hourly wage distribution. Results suggest that accumulated experience in the labour force might be the main driver of the results.

It is important to emphasise two things though. First, the results are valid for the cohort who turned age 14 in the first half of 1999. In other words, the results and conclusions cannot be extrapolated to other different age groups and even cohorts.

Second, one should not read the results among non-whites as an implicit advocacy towards child labour as the counterfactual are children allowed to work in the formal sector at age 14[7].

This paper is organised as follows. The next section discusses briefly the 1998 law. A theoretical framework is introduced in the third section, and the fourth section outlines the empirical strategy. Section five presents the dataset and descriptive statistics. Section six presents the empirical results, and section seven discusses the robustness check. The conclusion summarises the main findings of the paper and outlines policy recommendations.

2 THE INTERVENTOIN: THE LAW OF DECEMBER 1998

The Brazilian Constitution of 1988 set the minimum legal age of entry to the labour market at 14, and in 1990 a federal rule named 'The Statute of Children and Adolescents'[8] established children's and youth rights beyond regulating the conditions of entry to the formal labour market. Complementary to the Constitution of 1988, the statute is considered the legal framework for children and youth in the labour market.[9] From 1988 to November 1998, the minimum legal working age in Brazil was 14 and individuals under age 17 were prohibited from working in hazardous activities.

As a consequence of comprehensive modifications approved for the pension system in December 12th 1998, the Constitutional Amendment No. 20 also increased the minimum legal age for entry to labour market from 14 to 16[10]. According to the law,

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[7] The International Labour Organization (ILO) definition of child labour is very broad as it considers children individuals aged 5 to 17 and 'child labour' any type of 'illegal' work done by individuals in that age range. It means that any type of work performed by individuals under the minimum employment age legislation would be considered illegal and therefore being computed as child labour. Although we do not think this definition is too accurate, particularly in the present case where the work done by those who turned age 14 before the law passed would not be considered illegal but that done by those turned age 14 after the change in the law would, we adopt it in this paper.

[8] *Lei do Estatuto e do Adolescente*, Law No.8069 from 07/13/1990. Complementary to the Constitution of 1988, the statute is considered the legal framework for children and youth in the labour market.

[9] Although ILO considers as child an individual 17 years old or younger, in this paper terms 'children', 'teenagers' and 'youth' are used interchangeably.

[10] The law passed on December 15th and was made effective in the following day.

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 (‘treatment group’) 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 children’s participation rate. If the law were fully enforced in the 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 the ban, the effect of the law on children’s participation rate would have 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 probably reduce participation rate in the informal sector as well.

The main question this paper aims to investigate is how these two cohorts who turned age 14 close to the change in the minimum legal age, facing different constraints to participate into the labour force, performed in the long term.

3. THEORETICAL FRAMEWORK

This section develops a theoretical framework that helps rationalise the effect of the law on labour supply. Although drawing on a standard static labour supply model, 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 $u_i(C, l; \varepsilon)$ 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 ε[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 $u_i(C, l; \varepsilon)$ subject to the budget constraint: $C = V + wL$, 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 $24 - l$, 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: $l_i = l(V, w; \varepsilon)$. By symmetry, the labour supply function is $L_i = L(V, w; \varepsilon)$.

Individual *i* will participate in the labour force if the market wage rate is at least equal to his/her reservation wage, that is: $L_i > 0$ if $w_m > w_i$, where w_i is the individual's *i* reservation wage. Assume that the wage rate paid in the formal labour market, w_F, is higher than the wage rate paid in the shadow economy, w_{Inf}[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 $w_j < w_i$. 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 with the ultimate objective to identify a rationale for children's decisions regarding time allocation.
[15] This vector can also include individuals' backgrounds, such as parents' education. In other words, the individuals have, on average, the same skills and socio-economic characteristics, but are allowed to differ in terms of reservation wage.
[16] The price of a unit of C is $1.
[17] To simplify, we assume the labour market is perfect so that individuals are price takers. This is a plausible assumption for individuals who have little accumulated experience in the labour market and are just beginning their career.
[18] Figure 2 shows that this was the case for children aged 14 in 1998. In this paper, informal sector and shadow economy are used interchangeably.

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 w_m from $w_m^{'}$ 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 ($w > w_F > w_{Inf}$) – 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 w_F to w_{Inf} 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].

With the 'fall' 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

[19] Assume that the market wage rate drops from w_m 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 w_m to 10 can be seen as the shift from w_F to w_{Inf}.

[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. Consequently, they are less likely to allocate more time to household chores and/or school. If the market rewards experience (work history) accumulated in the formal sector rather than workers' productivity[23], the worse off hindered from participating in the formal labour force at age 14 may end up earning less in the long run than their counterparts. This cohort may face difficulties proving their experience accumulated in

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[22] To simplify, we assume that the wage rate is the only variable affecting individuals' decisions regarding labour force participation and number of working hours. In reality, there are many other variables that can affect individuals' decisions, such as stigma effect. The theoretical framework can be made more sophisticated with the inclusion of the stigma effect on individuals' reservation wage. Consequently, many who are supposed to shift from the formal to informal labour market would rather drop the labour force once the stigma effect is taken into account. Notice that this would not affect the main conclusion of the model.

[23] There is a significant body of literature on the effect of education as a credible signal to overcoming problems of adverse selection in the labour market. Employers may also use work history to select workers as a way of dealing with the same agency problem. Thus, individuals who accumulate experience in the informal sector would be less likely to be selected, and would probably be offered lower wages if selected. The evidence from Brazil suggests that after controlling for educational levels and self-selection into the formal sector, informal workers from ages 24 to 54 have higher wage rates than their formal counterparts (see Menezes Filho et al. 2004). This is an interesting finding, as it suggests that work experience in the formal and informal sectors may have similar effects on adults' earnings.

the shadow economy, as it is not formally registered in personal records[24]. However, if what counts is workers' productivity and this is, on average, similar regardless the sector 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 theoretical framework and to help outline the plausible channels through which the ban might affect long run outcomes of individuals. This analysis uses skin colour as a proxy for individuals' backgrounds. Skin colour is highly correlated with individuals' backgrounds, as shown in Table A.1, and is an exogeneous variable. The table compares whites and non-whites across several socio-economic characteristics. As can be seen, non-whites lag behind in all cases with the differences in means being statistically significant except in one case.

4 EMPIRICAL STRATEGY

The objective of this chapter is to estimate the long run effects of being hindered from participating in the (formal) labour force at age 14. The problem is that the participation decision is endogeneous. An individual may participate in the labour force for a number of reasons, e.g., to complement the household income, because s/he is talented enough to abdicate formal education, or because parents are not fully aware of the returns to education. Whatever the explanation, individuals may enter the labour force at a certain age for a variety of reasons. This paper uses the ban of December 1998 to identify the long run consequences of an exogenous variation in labour force participation at age 14.

As in Angrist and Krueger (1991)[25], the identification strategy relies on the individual's date of birth. The change of the minimum legal working age in December

[24] This dichotomy is similar to the role played by education in the labour market. People with higher levels of education can be rewarded, because they are more productive or because education is seen as a signal of an employee`s potential.

[25] Many other authors used a similar approach after the publication of this seminal paper. There is an increasing body of literature on weak instruments showing that the instrumental variable used by Angrist and Krueger (1991), the quarter of birth, may be weak. Differently from Angrist and Krueger, we estimate reduced form regressions.

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,

$$y_i = a + r D_i + h(Z) + b X_i^{'} + u_i, \tag{1}$$

where y_i 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 "smooth function." 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, Z_i takes the value of 1 for the first week of January 1999, 2 for the second week, and so on. Analogously, Z_i takes the value of -1 for the third week of December 1998, -2 for the second week, and so on. X_i is a vector of controls that includes skin colour and elements of family background such as parents' years of schooling, and u_i is the error term. Most of the regressions are estimated without controls.

The parameter of interest, ρ, 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

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[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.

on exogenous variations in the labour force participation rate of some 14-year-old individuals in the first half of 1999, so as they become less likely to participate in the labour force compared to their counterparts[28]. If the law of December 1998 implied a reduction in labour force participation, then the outcomes of the cohort who were 14 years old just before December 1998 can be used as counterfactual for the cohort who turned 14 just after the law passed[29].

With hourly wage in natural log in the left hand side of eq. (1), the model becomes very similar to the Mincer equation. However, note that eq. (1) does not include years of schooling as in the original Mincer equation. This is because in the Mincer equation the potential experience and the years of schooling are endogenous variables. It is a common practice to replace potential experience with an individual's age, leaving the researcher with the problem of dealing with the endogeneity of years of schooling. In the present case, the intent-to-treat estimates exclude the school attenders for all labour market outcomes. The empirical exercise involves identifying the most plausible mechanism through which the law affects adults' wages. As mentioned, this paper suggests that experience is likely driving the effect of the ban on labour market outcomes.

If the labour force participation rate varies according to individuals' backgrounds, the law might have had heterogeneous and distributive effects on wages[30]. Given the exogeneity of the law, unconditional quantile treatment effects are estimated to check if that was the case. As with the ITT, estimates are provided by pooling the years and then allowing for different year effects.

To check robustness, eq. (1) is estimated with controls and with a bandwidth size of three months. A placebo test is also performed, comparing two cohorts that supposedly

[28] The condition is called the monotonicity assumption. See, for instance, Imbens and Angrist (1994).

[29] As discussed in chapter one, according to the Brazilian Constitution the apprenticeship programme was available for youth aged 14 even before the increase in the legal minimum working age. Thus, the apprenticeship programme should have a common effect in the eligible and ineligible cohorts. However, since the programme remained an alternative to youth entering the formal labour force at age 14, the impact of a ban could have been furthered attenuated had the number of 14-year-old apprentices been high. Courseuil *et al.* (2012) shows that the number of apprentices in Brazil before December 2000 was below a hundred.

[30] We look at heterogeneous effects across gender and explore distributional impacts through unconditional quantile treatment effects. Unconditional quantile treatment effects are estimated only for hourly wage since the other outcomes variables are binary. The heterogeneity in the wage distribution justifies the estimation of the effect of the ban in different points of the wage distribution.

would not be affected by the law. For this exercise, the comparison is between individuals who turned 14 in the first and second halves of 1999.

5 DATA

This paper uses several years of the Brazilian household survey (*Pesquisa Nacional por Amostra de Domicilios* – PNAD). Data from 1998 and 1999 are used for descriptive statistics and short run estimates. For the long run analysis, we drop rural areas and pool the surveys from 2007, 2008, 2009, and 2011[31]. Because the survey is not collected in Census years, 2010 could not be considered.

The PNAD has been conducted annually by the Brazilian Bureau of Statistics (*Instituto Brasileiro de Geografia e Estatística,* IBGE) since the end of 1970s and covers around 100,000 households and 320,000 individuals. The survey is collected between October and December each year and it constitutes one of the main sources of microdata in Brazil[32]. It is nationally representative, containing information on household socioeconomic characteristics, demographic data, household sources of income, and labour force status.

The purpose of pooling several years of the household survey is twofold. First, covering several waves of the survey is important if one aims to investigate the impact of the ban on schooling and labour market outcomes when individuals are transitioning from school to work. Second, pooling allows for a better understanding of the mechanisms underlying individuals' decisions regarding the accumulation of human capital through formal education or labour market experience.

The subsample of interest is given by two cohorts of individuals aged 14. The first cohort includes individuals who turned 14 between July and December of 1998—before the increase in the minimum legal age for work. This cohort is used as a comparison group. The eligible group is the group of individuals who turned 14 between January and June of 1999.

[31] Rural areas are under-represented in the PNADs. See www.ibge.gov.br.

[32] The survey documents provides the month (September), week (last of the month) and day (usually 27th) of reference of when the survey was collected. According to emails exchanged with members from the Brazilian Bureau of Statistics, the survey is actually collected between October and December each year.

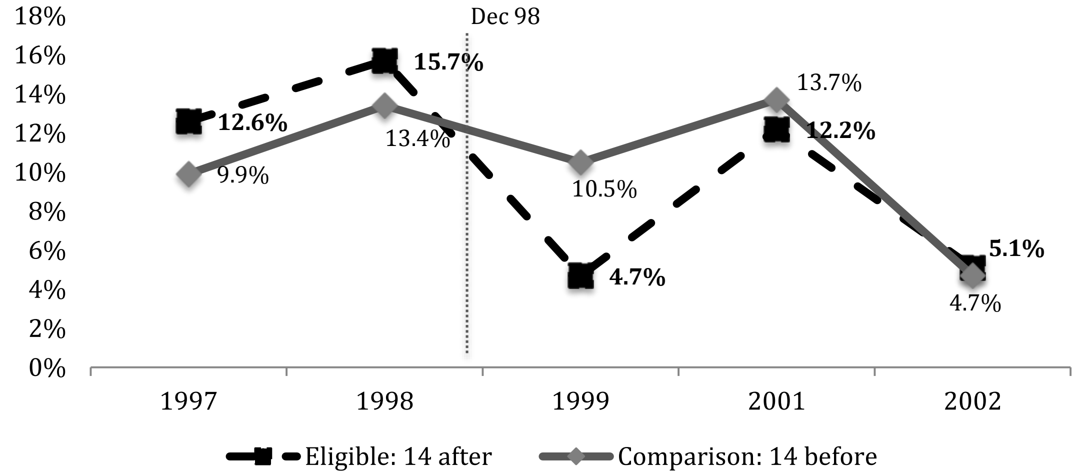
The estimates are initially obtained with a six-month bandwidth, but are also provided within a three-month bandwidth to check robustness. The same cohorts are compared from ages 22 and 23 to ages 26 and 27. The empirical analysis is performed in urban areas, because the law might not be fully enforced in rural areas, and because rural areas lack well-developed school systems and labour markets.

5.1 DESCRIPTIVE STATISTICS

The impact on participation rate in the labour force is not straightforward, as child workers can move to the informal economy. If children have moved to the informal economy, it might be difficult to argue that the accumulated experience in the labour market is the mechanism underlying the impact of the law on adults' outcomes, unless the returns to experience differ according to the sector in which experience was accumulated. However, if labour force participation drops and completed years of schooling remains the same between eligible and ineligible groups, then it can be argued that experience is the main driver.

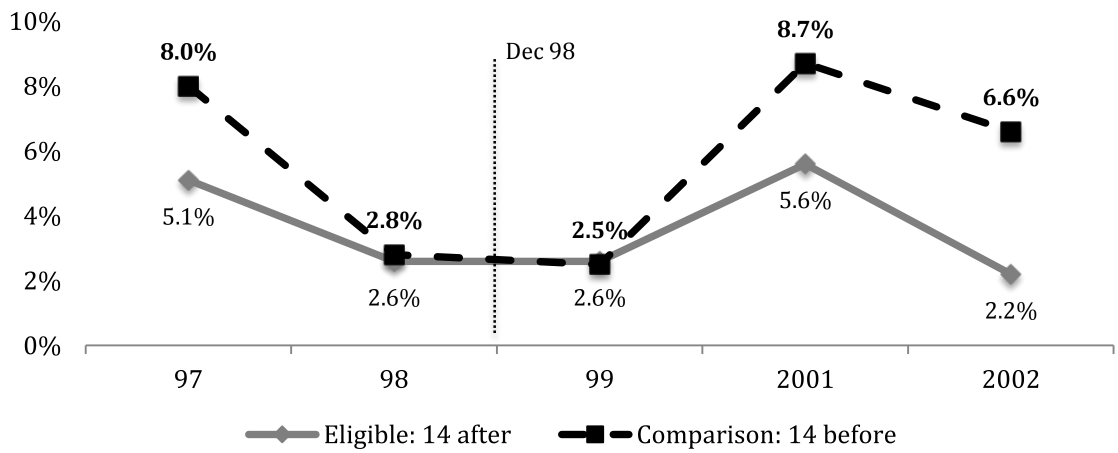
Figures 1 and 2 show the participation rate in the labour force for males and females in the eligible and ineligible groups. A three-month bandwidth is used in both figures so that the comparison is made between children who were 14 years old between October and December 1998 and children who turned 14 between January and March 1999. The figures plot the participation rate (in any sector) for different cohorts at five points in time.

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 observed before the ban passed. Because we cannot find any effect of ban on participation rate of females, long-term estimates will be provided for males only.

It is difficult to explain the differences in level observed in both figures with seasonal events. One could think of individuals who turned 14 before December 1998 more likely to participate in the labour force due to seasonal events that create temporary work demand, such as Christmas and New Year's Eve. Those events could therefore inflate the participation rate of the comparison group and the impact of the ban on the participation rate of the eligible group. However, the figures show that around December 1996 and December 1997 the participation rate was higher among the youngest cohort. The pattern reverses after the ban though.

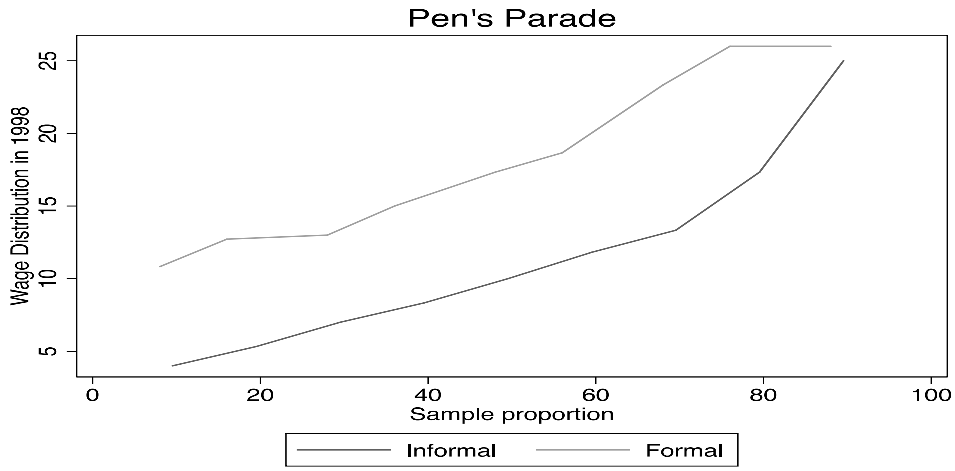
With the ban, similar individuals would receive different wage rates. Figure 3 indicates that individuals aged 14 before the ban received a higher wage rate than those who turned 14 after the ban was enacted, as they could still participate in the formal labour force[35]. This is consistent with the assumption made in the theoretical framework and can be used to rationalise children's decisions to leave the labour force after December 1998.

[33] Using 1995 as year base, in 1998 and 1999 the Brazilian real growth rate was 0.2% and -1.23% respectively. Data available at www.ipeadata.gov.br

[34] While boys dropped formal and informal labour force, girls seem to have shifted to informal sector. Because the participation rate of girls is small, the analyses do not have sufficient power to detect whether the effects are statistically different from zero or not.

[35] A t-test for difference in means rejects the null hypothesis of equal means at the one percent level. The wage paid in the formal sector was, on average, about 46 percent higher (R$ 187.5 vs. R$ 128.5). According to the PNAD 1999, the monthly wage in the informal sector was even lower than in 1998 (R$ 86.4). This could be partially explained by the economic recession in that year.

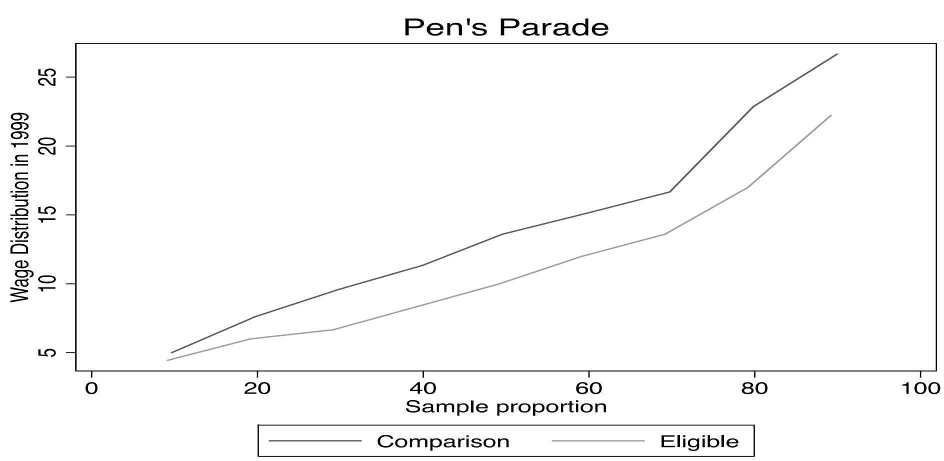
Figure 3 – First Order Stochastic Dominance: Hourly Wage Distributions for Formal and Informal Workers at Age 14 in 1998



Source: PNAD 1998.
Note: The figure corresponds to the Penn's Parade and is an alternative way of reporting the 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 individuals' participation in 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 the 'same' 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 difference in participation rate among males was 6 percentage points (pp.). In this case, a decrease of 16% in wage rate is associated with a decrease in participation rate of 6 pp. (or 60%, taking the participation of the comparison group as reference). This suggests a rough estimate of the elasticity of the labour supply of 0.375 (0.06/0.16). In other words, a 10% decrease in the hourly wage would be associated with a fall in participation rate of 3.75 percent. To get a better sense of the elasticity of boys' labour supply, we estimate the following reduced-form equation,

$$\ln whw_i = a + b_1 \ln wage_i + b_2 \ln wage_i * D_i + b_3 h(Z_i) + u_i \qquad (2)$$

where ln*whw* holds for weekly hours worked in natural log, *lnwage* is the natural log of hourly wage of boy *i*, and *h(.)* is defined as before. For the sake of simplicity, eq. (2) is fitted with 3-months bandwidth and with the smooth function specified as polynomials of 0 to 3 degrees and as linear, quadratic, and cubic splines. The parameter of interest is b_2. Table A.2 shows the results. The coefficient for the elasticity of labour supply is about -0.3 and statistically significant at 1% level in all cases, indicating that a decrease in hourly wage of 10% would increase hours worked by 3%. The negative coefficient suggests that leisure is a normal good, as demand for leisure reduces as consequence of a negative income shock. In addition, it suggests that the labour supply of male youth is not

[36] The average wage rate of the comparison group was 15.7 reais, whereas the eligible group faced an average wage rate of 14.15 reais. The difference in means was not statistically significant, but the Kolmogorov-Smirnov test rejects the null of equal distributions at the 5 percent level (p-value of 0.049) with a 6-month bandwidth.

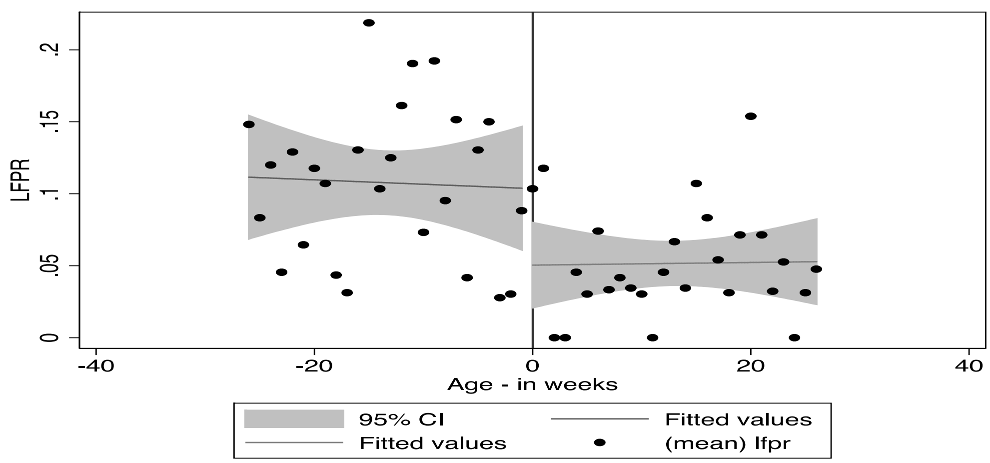
very responsive to variations in wage rate, but it means that boys have to work harder to compensate for a reduction in wage. This estimate is similar to that which is considered the benchmark in the literature[37]. This result is consistent with the hypothesis that child labour is influenced by poverty 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



[37] For an extensive survey of this literature, see Blundell and Macurdy (1999). Recent evidence includes Ziliak and Kniesner (2005) and Bargain et al. (2012). The estimate of -0.3 for young males is within the range found in the empirical literature and is almost identical to the estimate found by Bhalotra (2007) in rural Pakistan.

[38] Bhalotra (2007) argues that wage elasticity of child labour supply should be negative under the hypothesis that child labour is compelled by poverty. Using data from Pakistan, she finds support for this hypothesis for boys and mixed results for girls.

[39] Age can be defined in days, but it would create extra noise in the data.

[40] Figures A.2 to A.4 in the appendix show no discontinuity in the previous year the law passed.

Figure 5b –Labour Force Participation Rate in 1999

White Males – 26 Weeks Bandwidth

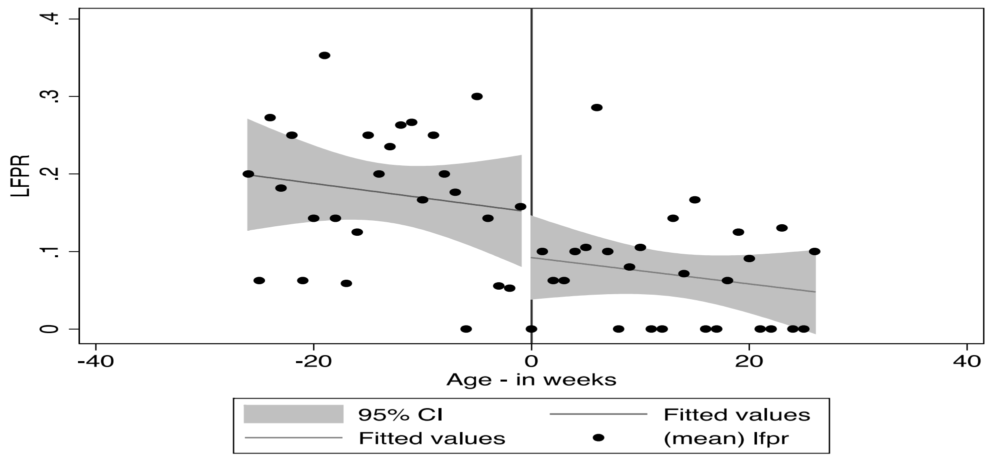


Figure 5c –Labour Force Participation Rate in 1999

Non-white Males – 26 Weeks Bandwidth

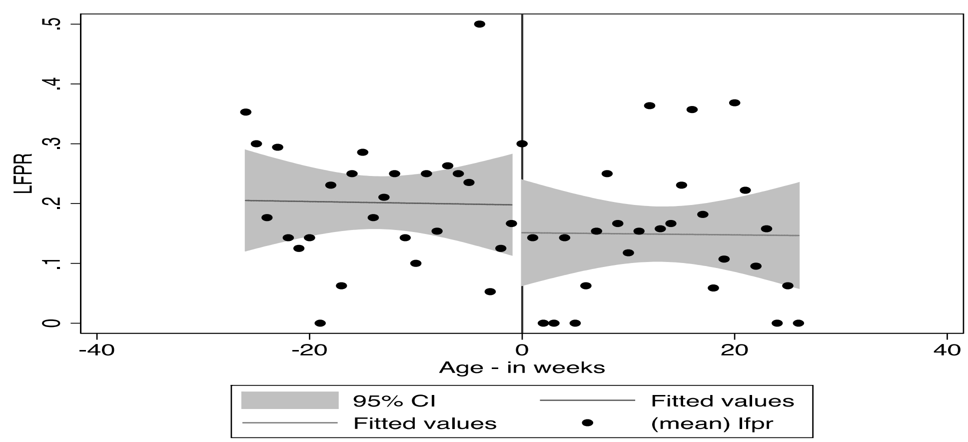


Figure 5a shows a sharp fall in participation rate among boys whereas figures 5b and 5c suggest that the ban affected mostly the participation rate of white males. The decrease in the labour force participation rate among the eligible group might be explained by a combination of three forces: (a) a downward shift in labour demand as employers would have to pay a fine for employing children illegally, (b) lower wage rate faced by the eligible group in the informal sector, and (c) a *stigma effect* associated with informal occupations.

Working at age 14, regardless the sector (formal or informal), became illegal after December 1998, and some individuals may have dropped out of the work force to avoid being seen as lawbreakers. It is interesting to note that the two regression lines in figure

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 – 26 Weeks Bandwidth

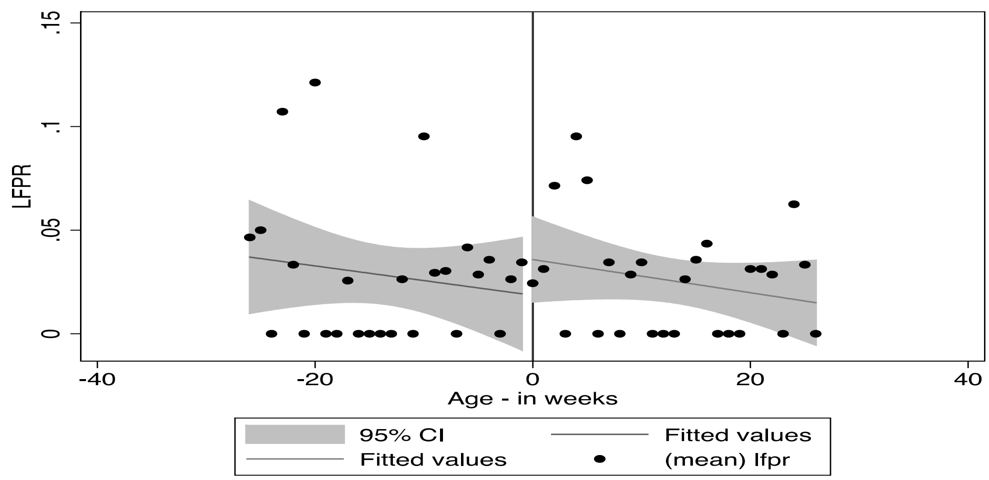


Figure 6b –Labour Force Participation Rate in 1999

White Females – 26 Weeks Bandwidth

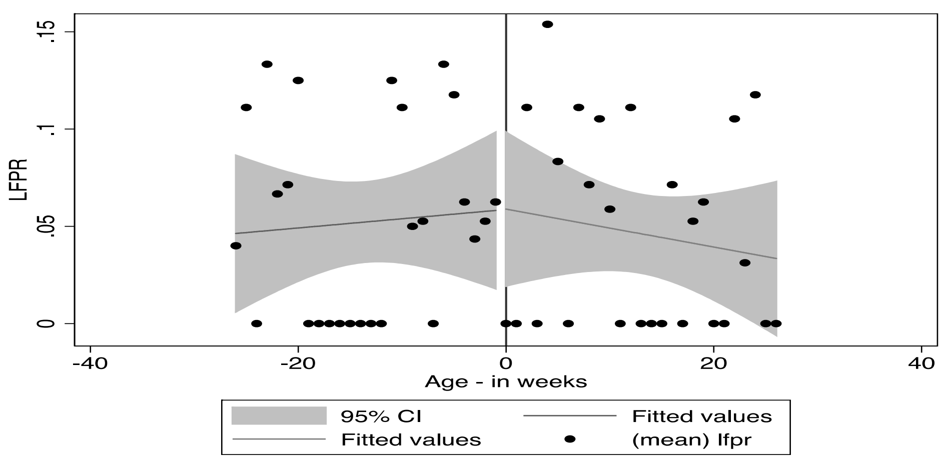
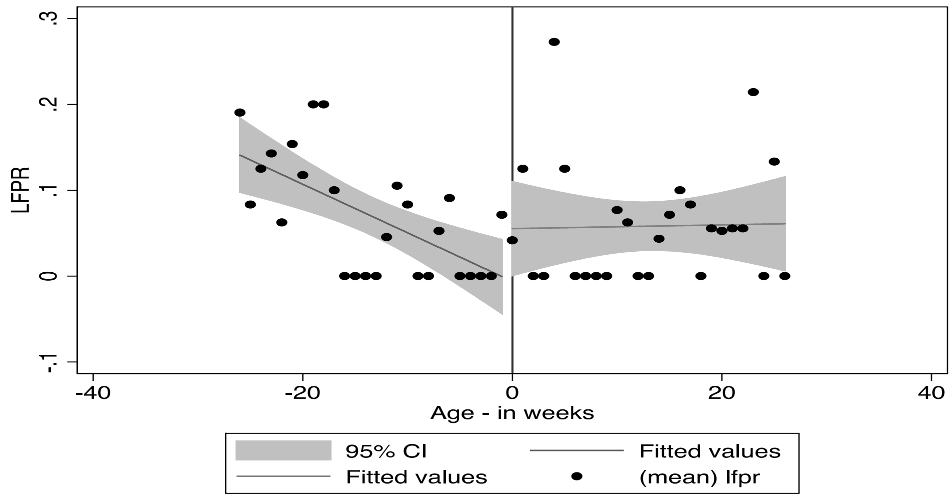


Figure 6c –Labour Force Participation Rate in 1999

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.

The samples seem to be very well balanced around the cutoff point, as the null hypothesis of equal means is rejected in only one case.

Figures 7a to 10b illustrate what may have happened to the cohorts in the long run. The figures are plotted with the pooled data from 2007 to 2011 (excluding 2010).

Figure 7a – Predicted Log Wage – Long Run

White Males – 26 Weeks Bandwidth

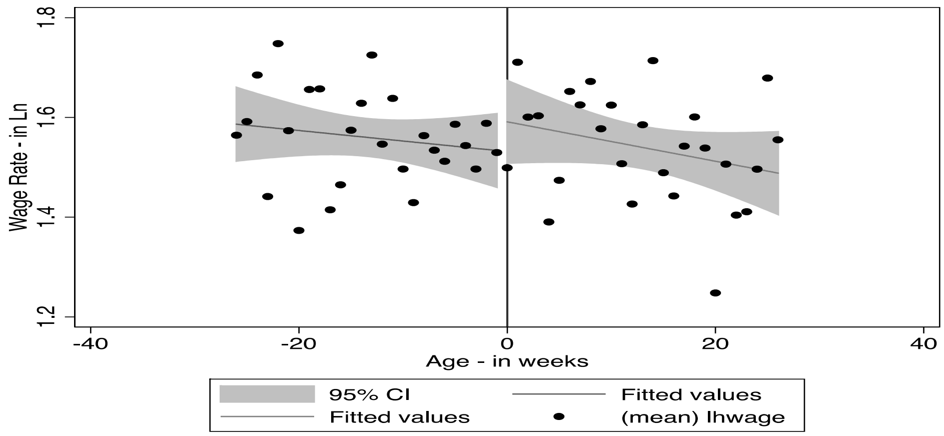


Figure 7b – Predicted Log Wage – Long Run

Non-white Males – 26 Weeks Bandwidth

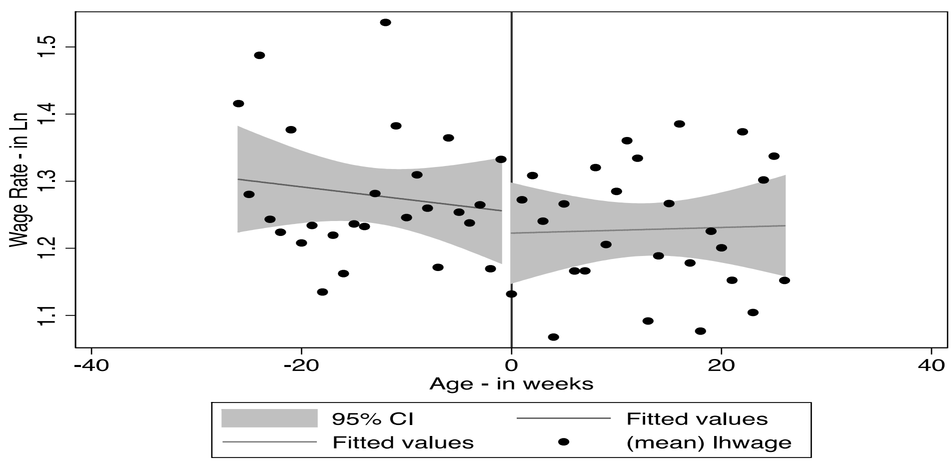


Figure 8a – LFPR – Long Run

White Males – 26 Weeks Bandwidth

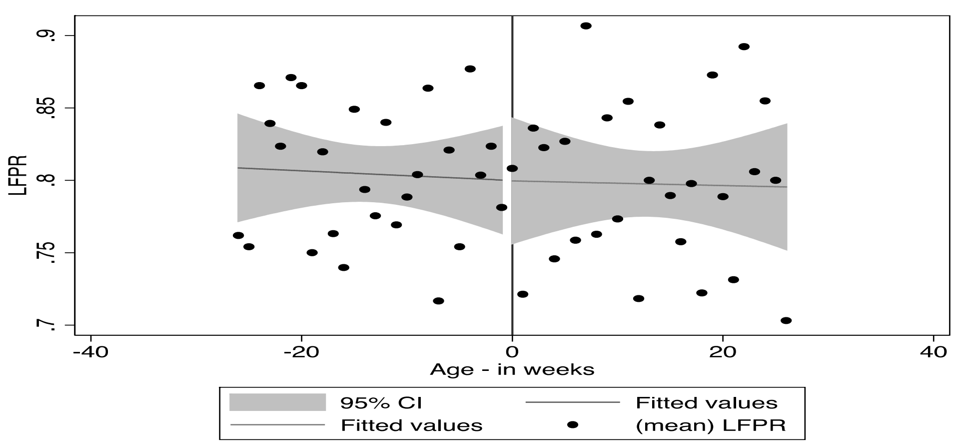


Figure 8b – LFPR – Long Run

Non-white Males – 26 Weeks Bandwidth

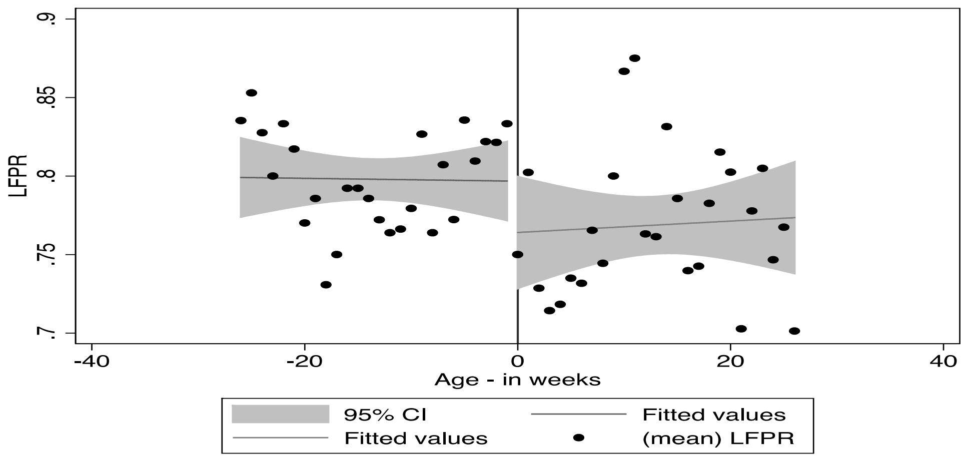


Figure 9a – Participation Rate in the Formal Labour Force – Long Run

White Males – 26 Weeks Bandwidth

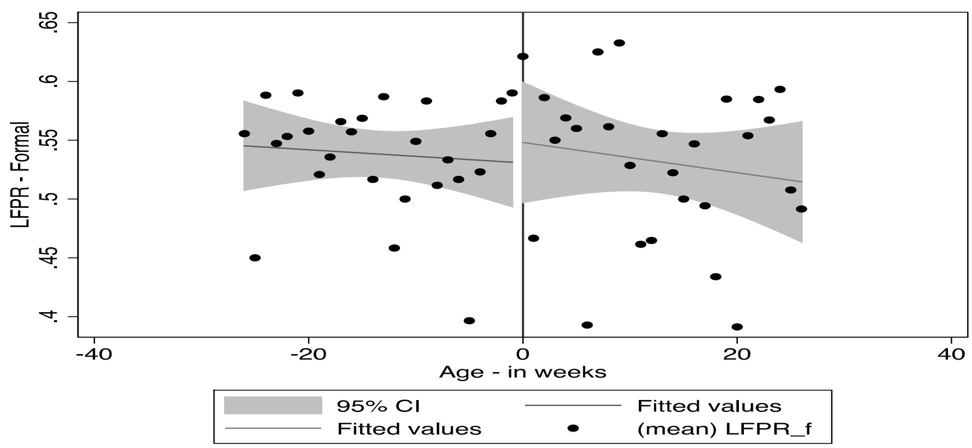


Figure 9b – Participation Rate in the Formal Labour Force – Long Run

Non-white Males – 26 Weeks Bandwidth

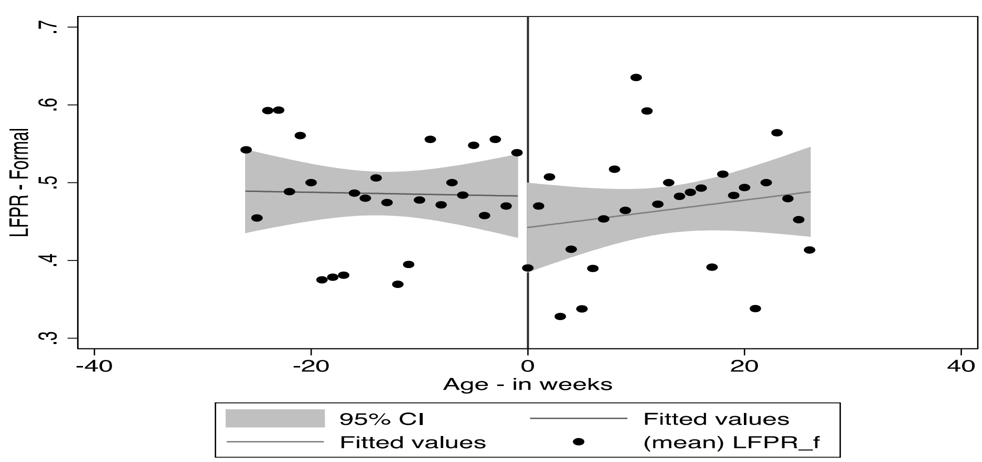


Figure 10a – Probability of Pursuing or Holding College Degree – Long Run

White Males – 26 Weeks Bandwidth

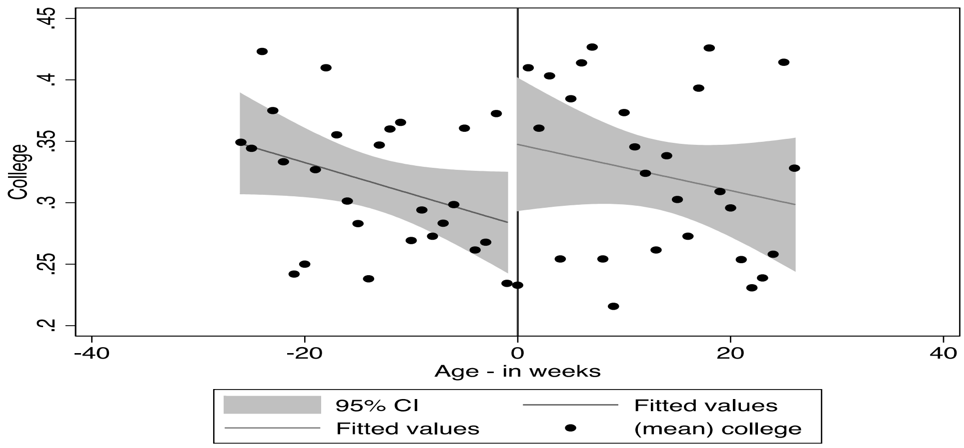
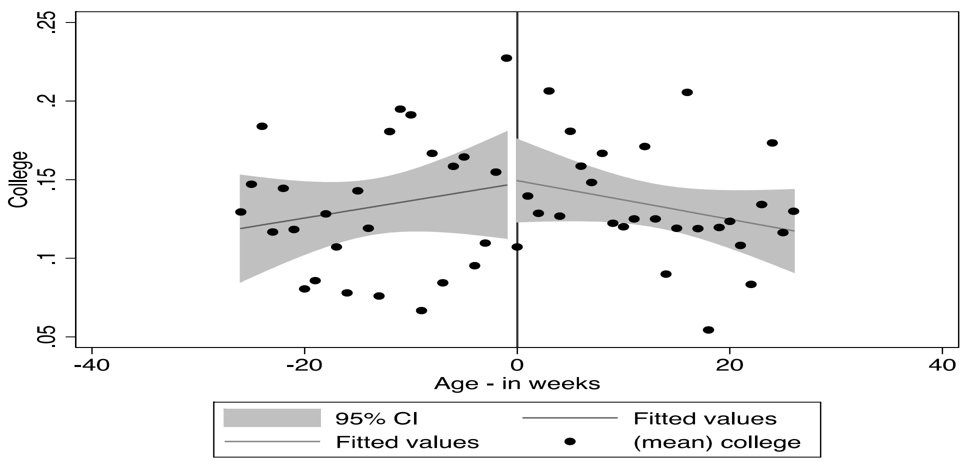


Figure 10b – Probability of Pursuing or Holding College Degree – Long Run

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 for age effects, are not capturing potential nonlinearities and heterogeneity in earnings distribution. The latter point suggests that a mean estimate on earnings might average out a heterogeneous distribution. The consequence might be an imprecise estimate as well as an estimate that is not very accurate for the whole distribution. For that reason estimates will be provided for other quantiles of earnings distribution as discussed below.

Based on the figures, the ban seem to have impacted the wages of white males and their likelihood of being enrolled in college, and non-white males' likelihood of being employed. The magnitude and precision of the estimates may be sensitive to the specification of the smooth function and is also likely to differ according to individuals' socio-economic backgrounds.

To check whether the sample of eligible and ineligible cohorts is balanced around the cutoff point, a t-test for difference in means for the outcomes and some covariates are reported using the pooled sample of 2007 to 2011 (excluding 2010). The t-test shows no difference in means for the covariates, except for school attendance. This is interesting for two reasons: it shows that eligible and ineligible youth have similar socio-economic backgrounds, and it strongly suggests that the ban did not affect human capital through education, as youth around the cutoff point have the same number of completed years of schooling.

To further check whether education could be a channel through which the ban affected long-term outcomes, we conducted a Kolmogorov-Smirnov test of the null hypothesis of equal distributions of completed years of schooling between eligible and ineligible groups. The null cannot be rejected for the pooled sample of males or for the subsamples of white and non-white males[41]. We will argue below that accumulated experience is likely the main driver underlying the results.

The next section examines whether such discontinuities are statistically and economically significant. Estimates assume common and heterogeneous time effects and are provided for two bandwidth sizes and a range of specifications of the smooth function.

6 RESULTS

6.1 SHORT TERM EFFECTS OF THE BAN OF DECEMBER 1998

To check whether the discontinuities illustrated in figures 5b, 5c, 6b and 6c are statistically significant, parametric regressions are estimated for white and non-white males and females respectively, as follows:

[41] The *p-values* are 0.85, 0.40, and 0.99 respectively.

$$y_i = a + dD_i + h(Z_i) + e_i, \qquad (3)$$

where D_i is a dummy that takes on the value of 1 for individuals who turned 14 after December 1998 and 0 otherwise, and Z_i is the forcing variable *age* defined in weeks as explained in section 4. Analogously, eq. (2) is estimated with $h(Z_i)$ defined as polynomials of degree 0 to 3 and as linear and quadratic piecewise polynomials. For the sake of robustness, eq. (3) is estimated with 3- and 6-month bandwidths. The parameter of interest d captures the (local) *intent-to-treat* of the ban. Table 3 shows the estimates for white and non-white males and females.

Given that the ban is supposed to reduce participation rate in the labour force of boys and girls, we can actually look at one-sided alternative to increase power. The coefficients for the impact of the ban on white and non-white males are similar. With the larger bandwidth size, several coefficients become statistically significant against a one-sided alternative, particularly for white males[42]. The results are clearly sensitive to the specification of the smooth function but consistent with the visual check.

Another notable finding is the absence of the effect of the law on girls. In fact, few coefficients for non-white girls are positive and relatively large, suggesting that they shift to informal sector. Because the coefficients for girls are not statistically significant even against one-sided alternative, we will look at long run outcome of boys only.

With regard to long run consequences of the ban, if the decrease in participation rate affected individuals' work histories, one can expect an effect on employability. Along the same lines, if the wage rate of youth is somehow responsive to accumulated experience in the labour market, one can also hypothesise that the cohort of males affected by the law will have a different (lower?) wage in the long run compared to the other group.

[42] Chapter one shows the results for the pooled sample of males are more precise and stable. Most of the estimates are statistically significant at standard levels and point to a fall in overall participation rate of about 6-7 percentage points for boys. See Table 10a in chapter one.

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 sensitive to the specification of the smooth function and most of them are not statistically significant, the pooled estimates suggest that postponing entrance into the labour force may have resulted in higher wages in the long run for white males.

For non-whites, the opposite is observed. Most of the coefficients are negative, but only in 2009 they are robust to different specifications of the smooth function and more precisely estimated. For 2009, the cohort of non-white males earned about 12% less than the comparison group. It is difficult to justify such an effect in that particular year.

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[43] The estimates exclude the school attenders as the groups differ in terms of school attendance rates and this could confound the results. Tables A.6 to A.9 in the appendix show the same set of estimates with controls.

[44] In fact, table 2 shows school attendance is higher among the eligible group and the difference is statistically significant at the 1% level.

One possible explanation might be the contraction of the economy in the aftermath of the financial crisis. The Brazilian gross domestic product grew only 0.33% in real terms in 2009[45]. It might be that, for this group, more years of experience in the labour market helped smooth the negative macroeconomic shock. It is important to bear in mind that these results are likely imprecise due to measurement error in the outcome variable and underestimating the actual impact of the ban as these are intent-to-treat estimates.

Taking the statistically significant point estimates for white and non-white males at face value and interpreting these results as returns to experience, the results suggest that up to two years of extra experience in the labour market implied a lower wage rate for whites but a higher hourly wage for non-whites. The magnitude of the estimates is, in fact, similar to what the empirical literature has reported for different countries. Despite being lower-bound estimates[46], they are very similar to those of Angrist (1990) and Bratsberg and Terreall (1998) for the case of the US, and Imbens and van der Klaauw (1995) for Netherlands[47].

For the sake of illustration, Table A.3 shows the returns to experience estimated for the same cohorts without considering selection biases. The regression model is fitted as follows:

$$\ln w_i = a + b_1 \exp_i + b_2 \exp_i^2 + d\,educ_i + e_i \tag{4}$$

where *exp* is the work experience of the individual *i* and *educ* is years of schooling. Work experience corresponds to the individual's potential experience: *age-educ-6*.[48] Note that because this measure of experience uses actual age as a reference, it will be different for the eligible and comparison groups by construction. Therefore, to compute the returns to experience the median of years of experience is used instead of the average.

[45] Data available at www.ipeadata.gov.br.

[46] The estimates consider the eligible cohort rather than those who actually drooped out of the labour force as consequence of the law.

[47] Angrist (1990) looked at the impact of serving in Vietnam on adults' earnings and found that two years of serving implied an adult wage of 15 percent lower than that of non-servers. Imbens and van der Klaauw (1995) looked at the impact of conscription in the Netherlands and found that one year of military service reduced the servers' annual wage by 5 percent. Both authors interpret these results through the effect of being recruited on potential experience.

[48] Light and Ureta (1995) show that the potential experience measure tends to understate the returns to experience of young workers compared to the worker's work history. If the same holds for the Brazilin context, the returns of experience estimates will be understated.

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 than non-whites. Interestingly, the median of years of experience for the eligible group of white males show that they have one year less experience than their counterparts in the comparison group. Using the median of years of experience of the comparison group as counterfactual, white males affected by the law would have wages about one percentage point higher had the ban not passed (14.1% vs. 13.3%).

Even though the ITT estimates do not provide the average effect of one extra year of experience on the treated, they can be contrasted with the naïve OLS to illustrate how misleading estimates can be when the selection biases are not controlled for. The ITT estimates suggest that white males prevented from working are better off due to the ban. This result contrasts with those shown in Table A.3. In addition, Table A.3 suggests that the eligible group of non-white males would be as successful as their counterparts in the comparison group, whereas ITT estimates indicate that they would have ended up with lower wages had they not been prevented from entering the (formal) labour force at age 14.

EMPLOYABILITY AND EDUCATION

The next two tables show the long-term effects of the ban on the probability of being employed and on being employed in the formal sector. As before, the estimates exclude school attenders, except for those pursing a college degree.

[49] Lemieux (2006) shows that for the US the traditional Mincer regression model tends to underestimate the observed wage of young workers and overstates the wages of those at the mid-career level. The best fit seems to come from a model in which experience enters as a quartic order polynomial and education as a second order polynomial.

The ITT estimates suggest that the employability of the cohort of white males was not affected by the ban, whereas non-white males became less likely to be employed and employed in the formal sector. Although only a few coefficients are statistically significant, most of the coefficients are positive for the cohort of whites and negative for the cohort of non-whites.

Table 7 shows that white males are more likely to hold or be in pursuit of a college degree[50]. Putting the results on employability and education together, it may be the case that some of the whites in the eligible group are in fact employed in higher skilled occupations[51].

Tables A.4 and A.5 in the appendix show pooled and heterogeneous linear probability model estimates for nine different groups of occupations. The occupation dummies are regressed on a constant, the indicator *D*, a piecewise linear function of the forcing variable, *h(z)*, and year dummies for 2008, 2009, and 2011. The standard errors are clustered at week level as before.

The results in Table A.4 point to an increase of about 5 pp. in participation rate in skilled occupations among white males, a decrease of about 2 pp. in participation rate in the armed forces, and a weak indication of a decrease in participation rate in civil construction. The coefficients in Table A.5 tell a similar story, but are less precisely estimated.

These results are striking. They suggest that the law had a positive effect on the better off (white males) and remarkable negative impact on the worse off (non-white males). While these are local estimates for a very specific cohort, the results indicate that an earlier entrance into the labour force benefits non-whites. This could be due to the fact that this group faces more constraints in constraints in life, such as low quality public education, problems of self-control that imply a sub-optimal accumulation of human capital, or even myopic parents who underestimate the returns to education.

[50] In recent years, access to college degrees for people with relatively poor backgrounds was made much easier. Student loans and scholarships have been fully or partially subsidised by the federal government. However, most of the universities these people manage to attend do not have good reputations. Note that the estimates showed in table 7 include school attenders.

[51] Tables A.6-A.9 in the appendix show the estimates with controls. The coefficients are very similar and there is very little gain in precision in adding controls.

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 experience are higher for more highly educated workers regardless of the occupation[52].

In this paper, skin colour is used as a proxy for individuals' backgrounds, the characteristics of which might be difficult to observe, such as quality of school and other educational outcomes unavailable in the data. If white males hindered from working reallocated more time and effort towards education, one could then expect a higher return to experience for white males than their counterparts who were not affected by the ban. For non-whites, on the other hand, one should not expect much difference in returns to experience between eligible and ineligible groups given that the reduction in participation rate was lower and, consequently, a smaller proportion of non-white males may have ended up studying harder or more intensively.

The estimation of the average effect on the eligible group (ITT) is very informative from a policy perspective, but might be of limited interest if the ban had different effects in different quantiles of the wage distribution. The next section provides unconditional quantile treatment effects of the ban to check whether it had distributive effects. The objective is to deepen the understanding of the impact of the ban taking into account the asymmetry in the wage rate distribution.

6.3 DISTRIBUTIVE EFFECTS OF THE LAW

To estimate the distributive effects of the increase in the minimum legal age, the unconditional quantile regression method proposed by Firpo et al. (2009) is used. The estimation of the unconditional quantile treatment effects takes advantage of the

[52] Brasterg and Terrell (1998) used several rounds of the National Longitudinal Survey of Youth to investigate whether the returns to experience are different between white and black workers in the US. They found that the return to experience is higher among whites but the return to tenure is higher for blacks.

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 first decile of the hourly wage distribution for white males, but a large and negative effect for non-whites at the median of the hourly wage distribution. Under rank preserving condition, the results indicate that the law benefited white males at the lower end of hourly wage distribution, reducing earnings inequality among white males. However, for non-white males the law would have harmed those at the median of hourly wage distribution probably increasing earnings inequality among non-white males. Consequently, the law may have widened the wage gap across race.

These results have to be linked to individuals' participation rates in the labour force. The drop in participation rate among white males was stronger than among non-white males. Whites were more likely to dedicate more time to school than non-whites. Accounting for individuals' backgrounds, it is also more likely that white males banned from labour force attended better schools compared to non-white males.

Table 9 presents the QTE estimates with heterogeneous time effects. Most of the estimates are positive for whites and negative for non-whites. The coefficients for white males are positive and statistically significant at the bottom decile and first quartile of the hourly wage distribution. With regard to non-white males, there is an indication of a negative effect at the median of the hourly wage distribution, although the effects become larger and more precisely estimated in 2009. The results suggest that the returns to work experience (human capital) are negative for white males as long as the eligible group of white males faces higher wages despite having less potential experience, but positive for non-white males.

These findings are similar to what Bratsberg and Terrell (1998) found in their study of the US economy. They used 12 years of the National Longitudinal Survey of Youth (1979-1991) to estimate returns to experience and job tenure for white and black 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's (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 household income and the children's opportunities for education. There is no evidence that the Brazilian ban reduced children's education in the short run in terms of the distributions of completed years of schooling of the eligible and ineligible groups. On the other hand, the raw data show that the ban reduced sharply the total household income of non-whites by 28%, but did not affect the household income of whites[53]. In that sense, at least for the group of non-white

[53] This number was obtained by dividing the difference in average monthly wages between eligible and ineligible individuals by the household net income of the ineligible group. A T-test for difference in means shows that the difference in monthly wage for non-white males was -28.7 reais and statistically significant,

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 present the effects of employability. The results for the labour force participation rate are very similar to those obtained with a larger bandwidth. There is no indication of impact on white males but there is a stronger evidence of a negative effect on non-white males[55]. Most of the estimates for 2008 and 2011 are statistically significant at 10 percent level and several coefficients in 2009 are statistically significant against a one-sided alternative. A similar pattern is observed in table 12. Table 12 suggests that the law had a very local negative effect on the labour force participation rate in the formal sector for non-white males. Non-white males became about 12 percentage points less likely to participate in the formal labour force and the negative effect. These results reinforce the previous findings and suggest, once again, that the law affected negatively children from disadvantaged backgrounds.

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but insignificant for white males. Over an average household net monthly income of 100.7 reais, this represented about 28.5%. The analysis considered a 6-month bandwidth.

[54] Most of the estimates for white males are large, stable and statistically significant only against a one-sided alternative.

[55] Some point estimates are statistically significant against a one-sided alternative.

Table 13 presents ITT estimates for the impact of the ban on educational outcome. The treatment effects on attaining a college degree are very similar to those reported in Table 7, but they are less precisely estimated as one would expect.

Tables 15 and 16 show the QTE estimates with a narrower bandwidth. The point estimates are slightly lower and less precise. None of the estimates for white males are statistically significant. Although the negative impact at the median of the hourly wage distribution for non-whites remains, there is an indication that the ban positively affected non-whites at the top decile of the wage distribution.

The heterogeneous effects presented in table 16 are similar to those estimated with a 6-month bandwidth, with few coefficients for non-white males being statistically significant.

7.1 PLACEBO TEST

This section presents a placebo test using the cohorts of individuals who turned 14 between January and December of 1999. Eq. (3) is re-estimated with the dummy *D* replaced by a placebo variable that takes on the value of 0 if the individual turned 14 between January 1st and June 30th 1999 and 1 if s/he turned 14 between July 1st and December 31st 1999. Tables 17 to 20 show the results for white and non-white males.

None of the coefficients of the placebo variable are statistically significant in tables 17, 18, and 19.

Table 20 shows statistically significant coefficients for 2011. According to the estimates, white males are more likely to hold or be in pursuit of a college degree. Although the coefficients are significant for 2011, an F-test cannot reject the null that the coefficients for the impact of the placebo are jointly equal to zero.

The placebo estimates provide further support for the main long run effects of the ban of December 1998. The ban that hindered individuals from participating in the formal labour force at age 14 had heterogeneous and distributive effects, as it affected mostly the subsample of non-white males, particularly those at the lower end of the hourly wage distribution. The evidence suggests that the law resulted in a higher wage gap between white and non-white males, and probably resulted in a more concentrated earnings 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 among white males and widened the wage gap across race. The results were robust to different bandwidth sizes and specifications of the smooth function.

The results on non-white males suggest that allowing this group to participate in the formal force at age 14 may pay off if the returns to experience actually overcomes the returns to education. One should not read the results among non-whites as an implicit advocacy towards child labour as the counterfactual are children allowed to work in the formal sector at age 14. Thus, incentivise children at this age group to enroll in the Brazilian apprenticeship programme may help them accumulate experience in the formal labour force and perhaps have better long term outcomes.

The results indicate that policy makers should take into account long run consequences of decisions on changes in law that can potentially have heterogeneous effects on individuals with distinct backgrounds.

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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			
White	0.016	*Na*	*Na*
	(1.45)	*Na*	*Na*
N	*7471*	*3248*	*4223*
School Attendance	0.038***	0.055***	0.022*
	(3.52)	(3.07)	(1.74)
N	*7471*	*3248*	*4223*
Years of Schooling (*exclude school attenders*)	-0.077	-0.13	-0.050
	(-0.85)	(-0.90)	(-0.37)
N	*5879*	*2367*	*3512*
Father's Education	-0.17	-0.065	-0.28*
	(-1.65)	(-0.48)	(-1.96)
N	*7471*	*3248*	*4223*
Mother's Education	-0.083	-0.12	-0.083
	(-1.01)	(-1.01)	(-0.72)
N	*7471*	*3248*	*4223*
Father's Age	0.089	0.73	-0.49
	(0.18)	(1.06)	(-0.80)
N	*7471*	*3248*	*4223*
Mother's Age	0.33	-0.053	0.57
	(0.94)	(-0.11)	(1.23)
N	*7471*	*3248*	*4223*
Metropolitan Region	-0.0083	-0.022	0.0020
	(-0.87)	(-1.51)	(0.15)
N	*7471*	*3248*	*4223*

Source: PNADs 2007, 2008, 2009 and 2011.

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 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**	0.0012	-0.0067	-0.021	0.028
	(-1.57)	**(-1.54)**	(-0.21)	**(-1.31)**	(0.030)	(-0.11)	(-0.64)	(0.60)
Observations	422	412	891	948	439	434	934	933

Source: PNAD 1999.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively. The bold coefficients are statistically significant against a one-sided alternative at conventional levels.

Table 4 – Long Run Effects on Hourly Log Wages – 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)	-0.011	0.099	0.096	0.18*	0.097	0.21*	-0.036	0.078	0.076	0.16	0.086	0.19
	(-0.33)	(1.38)	(1.33)	(1.84)	(1.34)	(1.84)	(-0.60)	(0.89)	(0.87)	(1.45)	(0.97)	(1.58)
D*2008							0.028	0.027	0.024	0.023	0.011	0.011
							(0.32)	(0.31)	(0.28)	(0.27)	(0.12)	(0.12)
D*2009							0.010	0.0013	0.0013	0.0080	-0.0025	0.0043
							(0.12)	(0.016)	(0.015)	(0.097)	(-0.030)	(0.052)
D*2011							0.048	0.043	0.042	0.046	0.031	0.037
							(0.50)	(0.46)	(0.44)	(0.49)	(0.32)	(0.38)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1966	1966	1966	1966	1966	1966	1966	1966	1966	1966	1966	1966
	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)	-0.029	0.0078	0.0014	-0.074	-0.0057	-0.065	-0.016	0.024	0.017	-0.059	0.015	-0.046
	(-1.29)	(0.16)	(0.030)	(-1.09)	(-0.12)	(-0.82)	(-0.38)	(0.38)	(0.28)	(-0.74)	(0.24)	(-0.50)
D*2008							0.052	0.051	0.049	0.052	0.042	0.045
							(0.89)	(0.88)	(0.85)	(0.89)	(0.71)	(0.75)
D*2009							-0.11*	-0.12*	-0.12*	-0.11*	-0.13*	-0.12*
							(-1.76)	(-1.79)	(-1.80)	(-1.75)	(-1.93)	(-1.92)
D*2011							0.0065	0.0052	0.0080	0.0094	0.0069	0.0076
							(0.11)	(0.086)	(0.13)	(0.16)	(0.11)	(0.12)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2831	2831	2831	2831	2831	2831	2831	2831	2831	2831	2831	2831

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 5 – Long Run Effects on Being Employed – 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	-0.00054	-0.010	-0.012	-0.018	-0.017	-0.022	0.013	0.0022	0.0012	-0.0040	0.000038	-0.0038
	(-0.033)	(-0.29)	(-0.34)	(-0.40)	(-0.47)	(-0.42)	(0.33)	(0.042)	(0.022)	(-0.066)	(0.00072)	(-0.057)
D*2008							-0.044	-0.044	-0.045	-0.045	-0.056	-0.056
							(-0.83)	(-0.83)	(-0.86)	(-0.86)	(-1.05)	(-1.06)
D*2009							0.0024	0.0034	0.0031	0.0026	0.0053	0.0049
							(0.043)	(0.061)	(0.055)	(0.048)	(0.096)	(0.090)
D*2011							-0.012	-0.012	-0.013	-0.013	-0.021	-0.021
							(-0.24)	(-0.24)	(-0.26)	(-0.27)	(-0.41)	(-0.40)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2367	2367	2367	2367	2367	2367	2367	2367	2367	2367	2367	2367
	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	-0.0045	-0.017	-0.017	-0.071*	-0.021	-0.079*	0.031	0.019	0.019	-0.036	0.015	-0.043
	(-0.30)	(-0.59)	(-0.60)	(-1.88)	(-0.71)	(-1.78)	(1.02)	(0.47)	(0.46)	(-0.73)	(0.36)	(-0.80)
D*2008							-0.043	-0.043	-0.043	-0.042	-0.041	-0.039
							(-1.03)	(-1.03)	(-1.03)	(-0.99)	(-0.97)	(-0.92)
D*2009							-0.039	-0.039	-0.039	-0.037	-0.041	-0.039
							(-0.88)	(-0.87)	(-0.87)	(-0.83)	(-0.92)	(-0.88)
D*2011							-0.056	-0.055	-0.055	-0.053	-0.054	-0.052
							(-1.35)	(-1.34)	(-1.33)	(-1.28)	(-1.29)	(-1.24)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3512	3512	3512	3512	3512	3512	3512	3512	3512	3512	3512	3512

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

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	0.035	0.082	0.032	0.054	0.053	0.099	0.064	0.11
	(0.33)	(0.61)	(0.58)	(1.25)	(0.74)	(1.21)	(0.61)	(0.80)	(0.79)	(1.26)	(0.93)	(1.27)
D*2008							-0.038	-0.038	-0.039	-0.039	-0.054	-0.054
							(-0.52)	(-0.53)	(-0.55)	(-0.55)	(-0.74)	(-0.75)
D*2009							-0.044	-0.047	-0.047	-0.043	-0.040	-0.038
							(-0.65)	(-0.68)	(-0.68)	(-0.63)	(-0.58)	(-0.55)
D*2011							-0.012	-0.012	-0.013	-0.011	-0.017	-0.014
							(-0.18)	(-0.18)	(-0.19)	(-0.16)	(-0.25)	(-0.20)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2283	2283	2283	2283	2245	2245	2283	2283	2283	2283	2245	2245
	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)	0.011	-0.018	-0.020	-0.080*	-0.019	-0.095*	0.031	0.0019	-0.000013	-0.062	0.0017	-0.076
	(0.58)	(-0.49)	(-0.54)	(-1.69)	(-0.51)	(-1.72)	(0.82)	(0.038)	(-0.00026)	(-1.01)	(0.033)	(-1.11)
D*2008							-0.021	-0.020	-0.021	-0.019	-0.019	-0.017
							(-0.39)	(-0.38)	(-0.39)	(-0.36)	(-0.36)	(-0.32)
D*2009							-0.023	-0.022	-0.022	-0.020	-0.027	-0.025
							(-0.41)	(-0.39)	(-0.39)	(-0.35)	(-0.48)	(-0.44)
D*2011							-0.033	-0.033	-0.032	-0.030	-0.031	-0.029
							(-0.64)	(-0.63)	(-0.61)	(-0.58)	(-0.59)	(-0.55)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3403	3403	3403	3403	3403	3403	3403	3403	3403	3403	3403	3403

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 7 – Long Run Effects on Holding or Being Pursuing a College Degree –Whites and Non-whites Males

26 Weeks Bandwidth

	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)	0.022	0.12***	0.12***	0.11**	0.12***	0.11**	0.034	0.13**	0.13**	0.13**	0.13**	0.13**
	(1.12)	(3.15)	(3.13)	(2.47)	(3.13)	(2.07)	(0.94)	(2.55)	(2.54)	(2.20)	(2.57)	(2.00)
D*2008							-0.015	-0.014	-0.014	-0.015	-0.0076	-0.0079
							(-0.29)	(-0.27)	(-0.28)	(-0.28)	(-0.15)	(-0.15)
D*2009							-0.020	-0.026	-0.025	-0.026	-0.024	-0.025
							(-0.38)	(-0.48)	(-0.48)	(-0.48)	(-0.45)	(-0.46)
D*2011							-0.012	-0.013	-0.013	-0.013	-0.024	-0.025
							(-0.25)	(-0.25)	(-0.26)	(-0.26)	(-0.49)	(-0.49)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3248	3248	3248	3248	3248	3248	3248	3248	3248	3248	3248	3248
	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)	-0.0034	0.015	0.016	0.00066	0.019	0.0086	-0.00053	0.018	0.019	0.0034	0.021	-0.0014
	(-0.27)	(0.58)	(0.64)	(0.020)	(0.75)	(0.24)	(-0.025)	(0.56)	(0.61)	(0.094)	(0.65)	(-0.034)
D*2008							-0.013	-0.013	-0.013	-0.013	-0.011	-0.010
							(-0.47)	(-0.48)	(-0.46)	(-0.45)	(-0.38)	(-0.37)
D*2009							0.0061	0.0057	0.0059	0.0063	0.0068	0.0072
							(0.17)	(0.16)	(0.17)	(0.18)	(0.19)	(0.20)
D*2011							-0.0039	-0.0040	-0.0050	-0.0049	-0.0024	-0.0020
							(-0.11)	(-0.12)	(-0.15)	(-0.14)	(-0.069)	(-0.058)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	4223	4223	4223	4223	4223	4223	4223	4223	4223	4223	4223	4223

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 8 – Long Run QTE on Hourly Log Wages – White and Non-White Males

26 Weeks Bandwidth – Exclude School Attenders – Homogeneous time effects

	Q10	Q25	Q50	Q75	Q90
			Whites		
D	0.19**	0.15	0.14	0.23	0.20
	(2.04)	(1.54)	(1.28)	(1.42)	(0.82)
			Non-Whites		
D	0.027	-0.092	-0.24***	-0.054	0.18
	(0.39)	(-1.38)	(-2.88)	(-0.49)	(1.02)

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 9 – Long Run QTE on Hourly Log Wages –White and Non-White Males

26 Weeks Bandwidth – Exclude School Attenders – Heterogeneous time effects

	Whites					Non-Whites				
	Q10	Q25	Q50	Q75	Q90	Q10	Q25	Q50	Q75	Q90
D	0.22*	0.21*	0.16	0.18	0.097	0.092	-0.13	-0.23**	-0.039	0.10
	(1.75)	(1.81)	(1.33)	(1.03)	(0.41)	(1.08)	(-1.58)	(-2.52)	(-0.35)	(0.55)
D*2008	-0.023	-0.034	0.043	0.13	0.014	-0.020	0.064	0.022	0.038	0.093
	(-0.19)	(-0.33)	(0.41)	(1.02)	(0.087)	(-0.28)	(0.86)	(0.29)	(0.46)	(0.85)
D*2009	-0.054	-0.045	-0.0025	0.12	0.34*	-0.16**	-0.047	-0.17**	-0.17*	-0.094
	(-0.52)	(-0.45)	(-0.024)	(0.87)	(1.74)	(-2.38)	(-0.72)	(-2.30)	(-1.93)	(-0.73)
D*2011	0.013	-0.081	0.0040	0.057	0.13	-0.083	0.017	-0.0072	-0.057	0.14
	(0.13)	(-0.92)	(0.043)	(0.41)	(0.64)	(-1.45)	(0.29)	(-0.11)	(-0.63)	(0.94)
Dummies for years?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1966	1966	1966	1966	1966	2831	2831	2831	2831	2831

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 10 – Long Run Effects on Hourly Log Wages – Whites and Non-whites 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)	0.063	0.16	0.16	0.12	0.15	0.096	0.11	0.21	0.21	0.17	0.20	0.14
	(1.20)	(1.44)	(1.43)	(0.84)	(1.39)	(0.54)	(1.42)	(1.60)	(1.60)	(1.04)	(1.56)	(0.72)
D*2008							-0.076	-0.078	-0.077	-0.081	-0.078	-0.072
							(-0.62)	(-0.63)	(-0.62)	(-0.66)	(-0.64)	(-0.59)
D*2009							-0.12	-0.12	-0.12	-0.13	-0.12	-0.12
							(-1.07)	(-1.09)	(-1.08)	(-1.11)	(-1.07)	(-1.05)
D*2011							0.0037	-0.00037	-0.00038	-0.0013	-0.00036	-0.00040
							(0.025)	(-0.0025)	(-0.0025)	(-0.0087)	(-0.0024)	(-0.0027)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	881	881	881	881	881	881	881	881	881	881	881	881
	Non-White Males											
	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)	-0.026	-0.014	-0.026	0.020	-0.030	0.054	0.028	0.040	0.027	0.069	0.024	0.11
	(-0.75)	(-0.19)	(-0.34)	(0.20)	(-0.40)	(0.45)	(0.41)	(0.39)	(0.27)	(0.53)	(0.23)	(0.72)
D*2008							-0.021	-0.022	-0.023	-0.020	-0.024	-0.017
							(-0.25)	(-0.26)	(-0.27)	(-0.23)	(-0.28)	(-0.20)
D*2009							-0.18*	-0.18*	-0.18*	-0.18*	-0.18*	-0.18*
							(-1.77)	(-1.77)	(-1.75)	(-1.74)	(-1.74)	(-1.76)
D*2011							-0.0096	-0.010	-0.0068	-0.0086	-0.0077	-0.0085
							(-0.11)	(-0.11)	(-0.076)	(-0.096)	(-0.087)	(-0.095)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

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*
							(-1.97)	(-1.85)	(-1.86)	(-1.80)	(-1.86)	(-1.77)
D*2009							-0.094	-0.091	-0.091	-0.091	-0.091	-0.091
							(-1.43)	(-1.38)	(-1.38)	(-1.37)	(-1.38)	(-1.38)
D*2011							-0.12*	-0.12*	-0.12*	-0.12*	-0.12*	-0.13*
							(-2.39)	(-2.38)	(-2.37)	(-2.38)	(-2.37)	(-2.44)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1591	1591	1591	1591	1591	1591	1591	1591	1591	1591	1591	1591

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 12 – Long Run Effects on Being a Formal Employee – 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)	0.044	0.0071	0.014	-0.011	0.019	-0.017	0.095	0.058	0.063	0.035	0.069	0.031
	(1.26)	(0.11)	(0.23)	(-0.15)	(0.30)	(-0.19)	(1.22)	(0.56)	(0.61)	(0.33)	(0.66)	(0.26)
D*2008							-0.090	-0.090	-0.087	-0.090	-0.088	-0.093
							(-0.83)	(-0.83)	(-0.80)	(-0.83)	(-0.81)	(-0.85)
D*2009							-0.065	-0.064	-0.064	-0.068	-0.065	-0.070
							(-0.65)	(-0.63)	(-0.63)	(-0.65)	(-0.64)	(-0.67)
D*2011							-0.043	-0.042	-0.040	-0.041	-0.040	-0.040
							(-0.42)	(-0.41)	(-0.39)	(-0.39)	(-0.38)	(-0.38)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1028	1028	1028	1028	1028	1028	1028	1028	1028	1028	1028	1028
	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)	0.0044	-0.11**	-0.11**	-0.0097	-0.12**	0.043	0.11**	0.0018	-0.0053	0.10	-0.0100	0.16
	(0.16)	(-2.00)	(-2.16)	(-0.15)	(-2.22)	(0.56)	(2.00)	(0.023)	(-0.070)	(1.19)	(-0.13)	(1.62)
D*2008							-0.13*	-0.12	-0.12	-0.11	-0.12	-0.11
							(-1.68)	(-1.54)	(-1.57)	(-1.48)	(-1.58)	(-1.46)
D*2009							-0.12	-0.11	-0.11	-0.11	-0.11	-0.11
							(-1.39)	(-1.34)	(-1.35)	(-1.34)	(-1.34)	(-1.36)
D*2011							-0.17**	-0.16**	-0.17**	-0.17**	-0.17**	-0.18**
							(-2.21)	(-2.15)	(-2.16)	(-2.25)	(-2.17)	(-2.27)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 14 – Long Run Effects on Holding or Being Pursuing a College Degree –White and Non-White Males
12 Weeks Bandwidth

	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)	0.073**	0.12**	0.12**	0.16**	0.12**	0.24***	0.069	0.12	0.12	0.16*	0.12	0.19*
	(2.48)	(2.39)	(2.32)	(2.46)	(2.27)	(2.81)	(1.20)	(1.61)	(1.59)	(1.93)	(1.57)	(1.94)
D*2008							0.029	0.030	0.029	0.032	0.030	0.030
							(0.36)	(0.37)	(0.36)	(0.39)	(0.36)	(0.37)
D*2009							-0.0081	-0.0078	-0.0078	-0.0046	-0.0078	-0.0061
							(-0.10)	(-0.099)	(-0.098)	(-0.057)	(-0.098)	(-0.077)
D*2011							-0.0032	-0.0042	-0.0043	-0.0053	-0.0043	-0.0054
							(-0.043)	(-0.057)	(-0.059)	(-0.073)	(-0.058)	(-0.074)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1485	1485	1485	1485	1485	1485	1485	1485	1485	1485	1485	1485

	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)	0.0052	0.014	0.012	-0.032	0.011	-0.058	0.040	0.051	0.050	0.0047	0.049	-0.014
	(0.27)	(0.37)	(0.33)	(-0.72)	(0.30)	(-1.22)	(1.14)	(1.08)	(1.06)	(0.089)	(1.03)	(-0.25)
D*2008							-0.059	-0.060	-0.060	-0.063	-0.060	-0.064
							(-1.25)	(-1.28)	(-1.29)	(-1.35)	(-1.30)	(-1.38)
D*2009							-0.037	-0.038	-0.038	-0.038	-0.038	-0.038
							(-0.66)	(-0.67)	(-0.68)	(-0.68)	(-0.68)	(-0.67)
D*2011							-0.044	-0.044	-0.045	-0.042	-0.045	-0.042
							(-0.85)	(-0.85)	(-0.86)	(-0.81)	(-0.86)	(-0.81)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1938	1938	1938	1938	1938	1938	1938	1938	1938	1938	1938	1938

Source: PNADs 2007, 2008, 2009, and 2011.
Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 15 – Long Run QTE on Hourly Log Wages –White and Non-White Males

12 Weeks Bandwidth – Exclude School Attenders

	Q10	Q25	Q50	Q75	Q90
			Whites		
D	0.28*	0.095	0.14	0.079	-0.053
	(1.94)	(0.64)	(0.84)	(0.28)	(-0.13)
			Non-Whites		
D	0.068	0.0055	-0.21*	-0.026	0.50**
	(0.64)	(0.059)	(-1.83)	(-0.18)	(2.03)

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 16 – Long Run QTE on Hourly Log Wages –White and Non-White Males

12 Weeks Bandwidth – Exclude School Attenders

	Whites					Non-Whites				
	Q10	Q25	Q50	Q75	Q90	Q10	Q25	Q50	Q75	Q90
D	0.32	0.19	0.22	0.11	-0.044	0.14	0.029	-0.21*	-0.0038	0.42
	(1.56)	(1.03)	(1.18)	(0.38)	(-0.11)	(1.00)	(0.25)	(-1.65)	(-0.025)	(1.59)
D*2008	0.049	-0.080	-0.012	0.029	-0.16	0.0037	-0.077	0.028	-0.023	-0.14
	(0.24)	(-0.51)	(-0.076)	(0.14)	(-0.61)	(0.033)	(-0.69)	(0.26)	(-0.23)	(-1.09)
D*2009	-0.14	-0.076	-0.11	0.014	0.040	-0.14	-0.13	-0.22**	-0.15	-0.25
	(-0.82)	(-0.50)	(-0.74)	(0.066)	(0.13)	(-1.31)	(-1.38)	(-2.08)	(-1.37)	(-1.41)
D*2011	-0.062	-0.14	-0.089	-0.045	0.083	-0.088	-0.029	0.072	-0.057	0.32
	(-0.35)	(-0.95)	(-0.64)	(-0.21)	(0.28)	(-0.89)	(-0.35)	(0.77)	(-0.47)	(1.52)
Dummies for years?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	881	881	881	881	881	1294	1294	1294	1294	1294

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 17 –Placebo Effects on Hourly Log Wages – 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)	-0.048*	0.025	0.025	-0.024	0.026	0.013	-0.047	0.025	0.026	-0.024	0.027	-0.046
	(-1.74)	(0.46)	(0.47)	(-0.35)	(0.49)	(0.21)	(-0.94)	(0.36)	(0.37)	(-0.30)	(0.39)	(-0.55)
D*2008							0.021	0.019	0.019	0.020	0.019	0.020
							(0.29)	(0.27)	(0.27)	(0.28)	(0.27)	(0.28)
D*2009							-0.056	-0.055	-0.055	-0.058	-0.056	-0.062
							(-0.78)	(-0.78)	(-0.78)	(-0.81)	(-0.79)	(-0.87)
D*2011							0.034	0.035	0.035	0.033	0.034	0.031
							(0.44)	(0.46)	(0.45)	(0.44)	(0.45)	(0.41)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2613	2613	2613	2613	2613	2613	2613	2613	2613	2613	2613	2613
	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)	-0.026	-0.049	-0.050	-0.038	-0.049	-0.055	-0.013	-0.037	-0.037	-0.023	-0.037	-0.022
	(-1.14)	(-1.08)	(-1.09)	(-0.63)	(-1.08)	(-0.92)	(-0.31)	(-0.65)	(-0.66)	(-0.33)	(-0.65)	(-0.30)
D*2008							-0.040	-0.040	-0.039	-0.040	-0.039	-0.040
							(-0.64)	(-0.65)	(-0.64)	(-0.65)	(-0.64)	(-0.65)
D*2009							0.027	0.026	0.026	0.025	0.026	0.025
							(0.42)	(0.41)	(0.40)	(0.40)	(0.40)	(0.39)
D*2011							-0.033	-0.035	-0.035	-0.035	-0.035	-0.036
							(-0.56)	(-0.58)	(-0.59)	(-0.60)	(-0.59)	(-0.60)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3271	3271	3271	3271	3271	3271	3271	3271	3271	3271	3271	3271

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

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	0.0038	-0.0012	0.0038
	(-0.21)	(-0.87)	(-0.87)	(-0.52)	(-0.86)	(-1.17)	(0.75)	(-0.030)	(-0.035)	(0.080)	(-0.033)	(0.072)
D*2008							-0.036	-0.036	-0.035	-0.036	-0.036	-0.036
							(-0.92)	(-0.94)	(-0.92)	(-0.92)	(-0.92)	(-0.93)
D*2009							-0.046	-0.046	-0.046	-0.046	-0.046	-0.047
							(-1.22)	(-1.22)	(-1.22)	(-1.22)	(-1.22)	(-1.24)
D*2011							-0.013	-0.014	-0.014	-0.014	-0.014	-0.015
							(-0.32)	(-0.35)	(-0.36)	(-0.36)	(-0.35)	(-0.38)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 19 – Placebo Effects on Being a Formal Employee – 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)	-0.051**	-0.0019	-0.0031	-0.024	-0.0032	0.0082	-0.056	-0.0075	-0.0089	-0.034	-0.0090	-0.043
	(-2.46)	(-0.049)	(-0.079)	(-0.47)	(-0.082)	(0.16)	(-1.36)	(-0.14)	(-0.17)	(-0.56)	(-0.17)	(-0.66)
D*2008							0.015	0.013	0.013	0.014	0.013	0.014
							(0.25)	(0.22)	(0.22)	(0.23)	(0.22)	(0.23)
D*2009							-0.037	-0.036	-0.036	-0.037	-0.036	-0.039
							(-0.66)	(-0.64)	(-0.63)	(-0.65)	(-0.63)	(-0.68)
D*2011							0.043	0.044	0.044	0.043	0.044	0.042
							(0.73)	(0.74)	(0.75)	(0.73)	(0.75)	(0.71)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2649	2649	2649	2649	2649	2649	2649	2649	2649	2649	2649	2649
	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)	-0.013	-0.016	-0.015	0.025	-0.015	-0.0049	0.0058	0.0020	0.0020	0.045	0.0022	0.052
	(-0.65)	(-0.39)	(-0.39)	(0.47)	(-0.38)	(-0.095)	(0.16)	(0.040)	(0.039)	(0.73)	(0.043)	(0.76)
D*2008							-0.024	-0.024	-0.022	-0.025	-0.022	-0.024
							(-0.44)	(-0.44)	(-0.41)	(-0.46)	(-0.42)	(-0.45)
D*2009							-0.012	-0.012	-0.013	-0.013	-0.013	-0.014
							(-0.24)	(-0.24)	(-0.25)	(-0.26)	(-0.25)	(-0.27)
D*2011							-0.038	-0.039	-0.039	-0.039	-0.038	-0.039
							(-0.74)	(-0.74)	(-0.74)	(-0.74)	(-0.74)	(-0.74)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3274	3274	3274	3274	3274	3274	3274	3274	3274	3274	3274	3274

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table 20 – Placebo Effects on Holding or Being Pursuing a College Degree –White and Non-White Males

26 Weeks Bandwidth

	White Males											
	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)	-0.0045	0.012	0.011	0.0022	0.010	-0.023	-0.050	-0.034	-0.035	-0.046	-0.037	-0.050
	(-0.23)	(0.32)	(0.30)	(0.046)	(0.27)	(-0.52)	(-1.38)	(-0.70)	(-0.73)	(-0.81)	(-0.75)	(-0.82)
D*2008							0.074	0.073	0.073	0.073	0.073	0.073
							(1.41)	(1.39)	(1.39)	(1.39)	(1.39)	(1.40)
D*2009							0.024	0.024	0.025	0.025	0.026	0.027
							(0.46)	(0.46)	(0.48)	(0.47)	(0.50)	(0.52)
D*2011							0.086*	0.085*	0.086*	0.085*	0.086*	0.086*
							(1.70)	(1.70)	(1.70)	(1.70)	(1.71)	(1.70)
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)	0.0078	0.022	0.022	0.018	0.022	-0.0012	0.0054	0.020	0.020	0.015	0.020	0.010
	(0.65)	(1.00)	(1.01)	(0.64)	(1.00)	(-0.044)	(0.26)	(0.74)	(0.75)	(0.45)	(0.76)	(0.29)
D*2008							0.033	0.033	0.032	0.032	0.032	0.032
							(1.11)	(1.12)	(1.10)	(1.11)	(1.09)	(1.09)
D*2009							-0.022	-0.022	-0.022	-0.022	-0.022	-0.023
							(-0.64)	(-0.63)	(-0.63)	(-0.63)	(-0.63)	(-0.65)
D*2011							-0.0014	-0.00061	-0.00050	-0.00037	-0.00065	-0.0014
							(-0.044)	(-0.019)	(-0.015)	(-0.011)	(-0.020)	(-0.042)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308	4308

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Appendix

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

	Non-whites	Whites	*P-value*
Log of hourly wage	2.21	2.90	0.00
Labour force participation rate	0.21	0.15	0.00
Labour force participation rate – Formal	0.00	0.01	0.03
Occupation rate – Formal	0.05	0.15	0.01
Informal	0.07	0.06	0.12
Domestic work	0.69	0.67	0.14
School attendance	0.90	0.94	0.00
Mother's Education	4.60	6.30	0.00
Father's Education	3.60	5.50	0.00
Household size	5.00	4.60	0.00

Source: PNAD 1998.

Table A.2 – Short Run ITT Estimates for Elasticity of Labour Supply
12 Weeks Bandwidth

	h(z) specifications						
	0	Linear	Quadratic	Cubic	Spline linear	Spline quadratic	Spline cubic
Ln WHW	-0.45***	-0.53***	-0.53***	-0.52***	-0.53***	-0.50***	-0.52***
	(-5.31)	(-7.12)	(-7.19)	(-6.63)	(-7.17)	(-6.22)	(-6.14)
Ln WHW*D1	0.024	0.23***	0.23***	0.17**	0.23***	0.19*	0.15
	(0.50)	(2.99)	(3.00)	(2.01)	(3.04)	(1.96)	(1.24)
Elasticity	*-0.43*	*-0.3*	*-0.3*	*-0.35*	*-0.3*	*-0.31*	*-0.37*
F-test (Ln WHW + Ln WHW*D1 =0)	30.39	8.96	9.68	15.82	9.55	10.22	14.54
P-value	0.000	0.006	0.005	0.005	0.005	0.004	0.001
Observations	72	72	72	72	72	72	72
Adjusted R2	0.18	0.27	0.27	0.27	0.27	0.28	0.29

Note: ****, **, * Statistically significant at 1, 5 and 10 percent respectively.
Source: PNAD 1999.

Table A.3 – Returns to Experience – White vs. Non-White Males
OLS Estimates

	Median Years of Experience Comparison Group (D=0)	Median Years of Experience Eligible Group (D=1)	Return D=0	Return D=1
Whites	7	6	14.1%	13.3%
Non-whites	8	8	9.3%	9.3%

Note: The estimated equation for white males is: lnwage = 3.24 + 0.085*exper + 0.004*exper^2 + 0.24*educ. For non-white males the estimated equation is: lnwage = 3.62 + 0.062*exper + 0.0039*exper^2 + 0.21*educ. All coefficients are statistically significant at 1 percent level.

Table A.4 – Effect of the Ban on Occupation of Adult Males – ITT Estimates
26 Weeks Bandwidth – Homogeneous Time Effects

	Directors in General	Science & Arts	Technicians	Administrative Services	Service Sector	Commerce Sector	Agricultural Sector	Civil Construction	Army Force	Undefined
					White Males					
D	0.027	0.047*	0.032	-0.014	0.0015	-0.010	0.0099	-0.076	-0.020*	0.0030
	(1.20)	(1.93)	(0.98)	(-0.35)	(0.044)	(-0.27)	(1.30)	(-1.56)	(-1.81)	(1.04)
Observations	*1978*	*1978*	*1978*	*1978*	*1978*	*1978*	*1978*	*1978*	*1978*	*1978*
					Non-White Males					
D	0.0054	0.015	-0.028	0.013	-0.030	-0.0034	0.011	0.010	0.0048	0.0030
	(0.35)	(0.86)	(-1.02)	(0.35)	(-0.91)	(-0.11)	(1.19)	(0.23)	(0.59)	(1.03)
Observations	*2851*	*2851*	*2851*	*2851*	*2851*	*2851*	*2851*	*2851*	*2851*	*2851*

Source: PNADs 2007, 2008, 2009, and 2011.
Note: Clustered T-statistics in parenthesis. * Statistically significant at 10 percent level.

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 Force	Undefined
					White Males					
D	0.053**	0.059	-0.00027	0.015	-0.0025	-0.0060	0.0088	-0.12	-0.0091	0.0051
	(2.05)	(1.46)	(-0.0063)	(0.25)	(-0.051)	(-0.14)	(0.77)	(-1.63)	(-0.45)	(1.05)
D*2008	-0.068**	-0.023	0.065	-0.026	-0.00067	0.016	0.0028	0.041	0.00052	-0.0083
	(-2.49)	(-0.58)	(1.40)	(-0.47)	(-0.012)	(0.37)	(0.29)	(0.56)	(0.024)	(-1.02)
D*2009	-0.011	-0.013	0.026	0.0091	-0.017	-0.017	-0.0038	0.047	-0.021	-0.00044
	(-0.41)	(-0.29)	(0.52)	(0.17)	(-0.30)	(-0.38)	(-0.24)	(0.70)	(-0.96)	(-0.87)
D*2011	-0.022	-0.011	0.032	-0.079	0.028	-0.0096	0.0046	0.075	-0.017	-0.00039
	(-0.78)	(-0.25)	(0.79)	(-1.60)	(0.58)	(-0.21)	(0.49)	(1.11)	(-1.07)	(-0.95)
Observations	1978	1978	1978	1978	1978	1978	1978	1978	1978	1978
					Non-White Males					
D	0.0012	0.025	-0.0013	0.039	-0.061	0.017	0.016	-0.042	-0.00070	0.0066
	(0.069)	(1.41)	(-0.038)	(0.92)	(-1.63)	(0.45)	(0.98)	(-0.71)	(-0.049)	(1.04)
D*2008	0.011	0.013	-0.026	-0.065*	0.047	0.0051	-0.016	0.021	0.014	-0.0043
	(0.79)	(0.74)	(-0.74)	(-1.69)	(1.08)	(0.15)	(-1.21)	(0.42)	(0.95)	(-1.01)
D*2009	0.00069	-0.014	-0.040	-0.031	0.051	-0.046	-0.00046	0.091	-0.0061	-0.0045
	(0.033)	(-0.63)	(-1.21)	(-0.71)	(1.13)	(-1.23)	(-0.037)	(1.65)	(-0.39)	(-1.02)
D*2011	0.0045	-0.032	-0.035	-0.011	0.024	-0.031	-0.0044	0.078	0.012	-0.0045
	(0.24)	(-1.67)	(-1.13)	(-0.31)	(0.67)	(-0.76)	(-0.28)	(1.17)	(0.90)	(-1.03)
Observations	2851	2851	2851	2851	2851	2851	2851	2851	2851	2851

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. **, * Statistically significant at 5 and10 percent respectively.

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**	-0.064	-0.012	-0.0093	0.11	-0.0089	0.14
	(-0.52)	(0.64)	(0.66)	(2.14)	(0.65)	(2.23)	(-1.11)	(-0.15)	(-0.12)	(1.31)	(-0.11)	(1.49)
D*2008							0.0027	0.0017	-0.0025	-0.0057	-0.0033	-0.0064
							(0.034)	(0.022)	(-0.032)	(-0.074)	(-0.043)	(-0.082)
D*2009							0.068	0.064	0.061	0.071	0.060	0.068
							(0.89)	(0.83)	(0.80)	(0.92)	(0.79)	(0.88)
D*2011							0.10	0.099	0.097	0.11	0.097	0.10
							(1.22)	(1.20)	(1.18)	(1.29)	(1.18)	(1.27)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1793	1793	1793	1793	1793	1793	1793	1793	1793	1793	1793	1793
	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)	-0.024	-0.027	-0.029	-0.11*	-0.029	-0.12	-0.0011	-0.0018	-0.0043	-0.091	-0.0043	-0.098
	(-1.10)	(-0.56)	(-0.60)	(-1.71)	(-0.60)	(-1.60)	(-0.025)	(-0.028)	(-0.068)	(-1.13)	(-0.069)	(-1.09)
D*2008							0.022	0.022	0.020	0.024	0.020	0.023
							(0.38)	(0.38)	(0.34)	(0.41)	(0.34)	(0.40)
D*2009							-0.11*	-0.11*	-0.11*	-0.11*	-0.11*	-0.11*
							(-1.77)	(-1.77)	(-1.77)	(-1.69)	(-1.77)	(-1.71)
D*2011							-0.0087	-0.0087	-0.0051	-0.0035	-0.0048	-0.0042
							(-0.14)	(-0.14)	(-0.086)	(-0.059)	(-0.079)	(-0.071)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2653	2653	2653	2653	2653	2653	2653	2653	2653	2653	2653	2653

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table A.7 – Long Run Effects on Being Employed – 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)	0.0053	-0.0036	-0.0033	-0.0016	-0.0035	-0.0048	0.020	0.0096	0.011	0.015	0.011	0.011
	(0.30)	(-0.093)	(-0.087)	(-0.032)	(-0.090)	(-0.090)	(0.46)	(0.17)	(0.21)	(0.23)	(0.21)	(0.17)
D*2008							-0.047	-0.046	-0.049	-0.049	-0.049	-0.049
							(-0.82)	(-0.82)	(-0.87)	(-0.87)	(-0.87)	(-0.87)
D*2009							0.0027	0.0036	0.0018	0.0020	0.0016	0.0017
							(0.046)	(0.061)	(0.030)	(0.034)	(0.028)	(0.030)
D*2011							-0.013	-0.013	-0.014	-0.014	-0.014	-0.014
							(-0.24)	(-0.24)	(-0.27)	(-0.27)	(-0.27)	(-0.27)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174

	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)	-0.0056	-0.024	-0.024	-0.079**	-0.024	-0.092**	0.036	0.019	0.018	-0.037	0.018	-0.050
	(-0.37)	(-0.80)	(-0.80)	(-2.01)	(-0.80)	(-2.09)	(1.15)	(0.44)	(0.44)	(-0.74)	(0.44)	(-0.91)
D*2008							-0.037	-0.037	-0.037	-0.035	-0.037	-0.035
							(-0.88)	(-0.88)	(-0.88)	(-0.83)	(-0.88)	(-0.82)
D*2009							-0.050	-0.049	-0.049	-0.046	-0.049	-0.047
							(-1.07)	(-1.05)	(-1.05)	(-0.98)	(-1.05)	(-1.00)
D*2011							-0.073*	-0.072*	-0.072*	-0.070	-0.072*	-0.071*
							(-1.70)	(-1.69)	(-1.68)	(-1.63)	(-1.68)	(-1.65)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively

Table A.8 – Long Run Effects on Being a Formal Employee – 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)	0.0098	0.048	0.048	0.10*	0.048	0.10*	0.026	0.066	0.067	0.12	0.067	0.12
	(0.40)	(1.04)	(1.04)	(1.76)	(1.04)	(1.66)	(0.49)	(0.96)	(0.98)	(1.54)	(0.97)	(1.47)
D*2008							-0.053	-0.053	-0.055	-0.056	-0.054	-0.055
							(-0.73)	(-0.74)	(-0.77)	(-0.78)	(-0.76)	(-0.77)
D*2009							-0.025	-0.028	-0.030	-0.025	-0.029	-0.025
							(-0.36)	(-0.41)	(-0.43)	(-0.37)	(-0.42)	(-0.37)
D*2011							0.0093	0.0086	0.0075	0.011	0.0079	0.010
							(0.13)	(0.12)	(0.11)	(0.16)	(0.11)	(0.15)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174	2174
	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)	0.0085	-0.029	-0.029	-0.095**	-0.029	-0.11**	0.028	-0.0097	-0.0100	-0.078	-0.0099	-0.092
	(0.45)	(-0.79)	(-0.80)	(-2.01)	(-0.79)	(-2.07)	(0.72)	(-0.19)	(-0.20)	(-1.26)	(-0.20)	(-1.37)
D*2008							-0.013	-0.013	-0.013	-0.011	-0.013	-0.011
							(-0.25)	(-0.24)	(-0.25)	(-0.19)	(-0.24)	(-0.19)
D*2009							-0.035	-0.034	-0.034	-0.030	-0.034	-0.031
							(-0.60)	(-0.59)	(-0.58)	(-0.52)	(-0.58)	(-0.53)
D*2011							-0.027	-0.026	-0.025	-0.023	-0.025	-0.024
							(-0.52)	(-0.50)	(-0.49)	(-0.45)	(-0.49)	(-0.46)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298	3298

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively.

Table A.9 – Long Run Effects on Holding or Being Pursuing a College Degree – White and Non-White Males

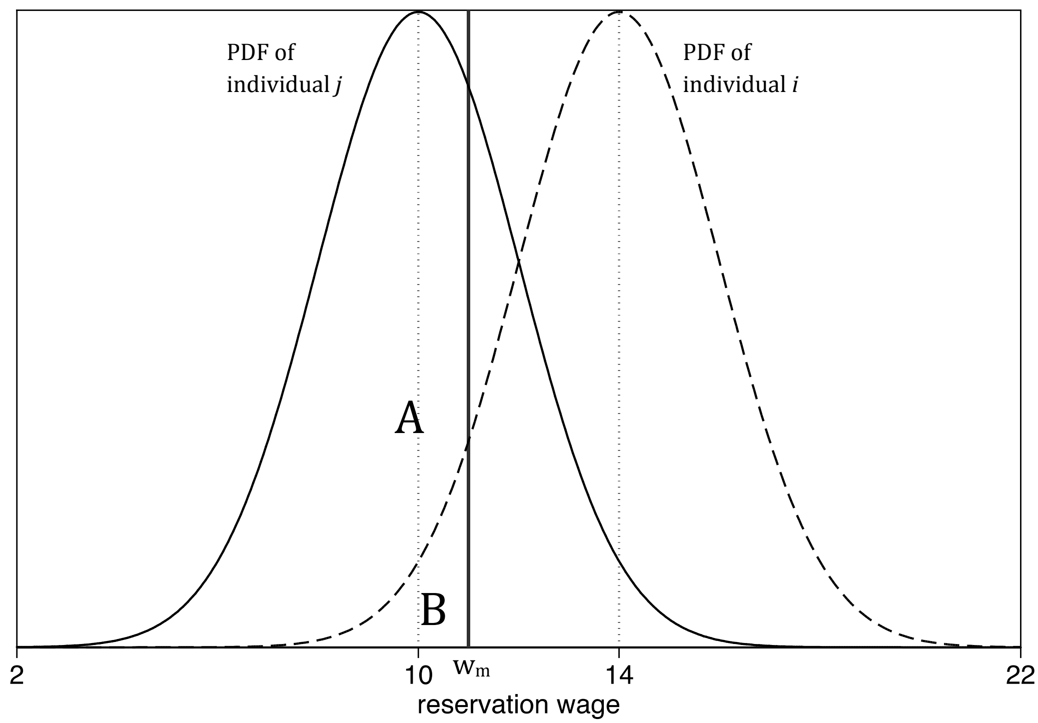
26 Weeks Bandwidth – with controls

	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)	0.023	0.089**	0.091**	0.064	0.091**	0.059	0.020	0.087*	0.089*	0.063	0.089*	0.058
	(1.15)	(2.30)	(2.36)	(1.30)	(2.37)	(1.11)	(0.55)	(1.68)	(1.73)	(1.06)	(1.73)	(0.91)
D*2008							0.0098	0.011	0.0098	0.0095	0.010	0.011
							(0.19)	(0.22)	(0.19)	(0.19)	(0.20)	(0.21)
D*2009							-0.0017	-0.0051	-0.0058	-0.0083	-0.0057	-0.0076
							(-0.032)	(-0.095)	(-0.11)	(-0.15)	(-0.11)	(-0.14)
D*2011							0.0045	0.0054	0.0048	0.0027	0.0050	0.0039
							(0.089)	(0.11)	(0.096)	(0.054)	(0.10)	(0.078)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	2972	2972	2972	2972	2972	2972	2972	2972	2972	2972	2972	2972
	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)	-0.0012	-0.00086	-0.0010	-0.010	-0.0012	-0.015	-0.0051	-0.0048	-0.0047	-0.014	-0.0047	-0.019
	(-0.097)	(-0.032)	(-0.039)	(-0.30)	(-0.045)	(-0.40)	(-0.24)	(-0.15)	(-0.14)	(-0.37)	(-0.14)	(-0.46)
D*2008							-0.00087	-0.00087	-0.00015	0.000059	-0.000094	0.00016
							(-0.031)	(-0.031)	(-0.0055)	(0.0021)	(-0.0034)	(0.0058)
D*2009							0.0043	0.0043	0.0044	0.0048	0.0043	0.0046
							(0.12)	(0.12)	(0.12)	(0.13)	(0.12)	(0.13)
D*2011							0.011	0.011	0.0096	0.0097	0.0092	0.0093
							(0.34)	(0.34)	(0.29)	(0.29)	(0.28)	(0.28)
Dummies for years	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	3936	3936	3936	3936	3936	3936	3936	3936	3936	3936	3936	3936

Source: PNADs 2007, 2008, 2009, and 2011.

Note: Clustered T-statistics in parenthesis. ***, **, * Statistically significant at 1%, 5% and 10% respectively.

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 w_F to w_{Inf} 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

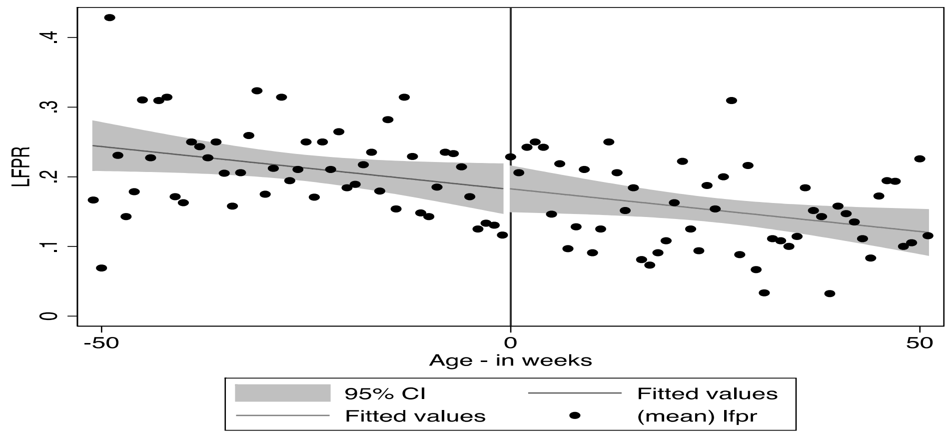


Figure A.3 –Labour Force Participation Rate in 1998

White Males – 51 Weeks Bandwidth

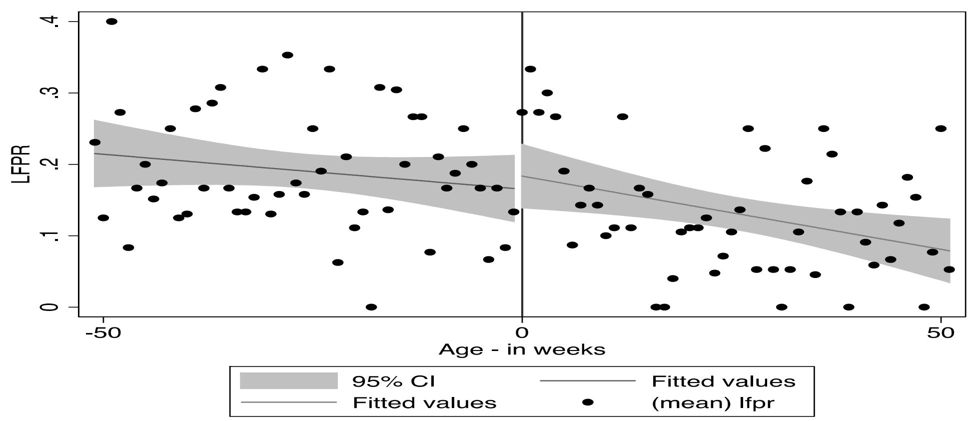


Figure A.4 –Labour Force Participation Rate in 1998

Non-white Males – 51 Weeks Bandwidth