LONG-TERM EFFECTS OF A CHILD LABOUR BAN ON ADULTS' OUTCOMES: EVIDENCE FROM BRAZIL

Caio Piza – PhD candidate in the Economics Department at the University of Sussex, UK and an economist in the Development Impact Evaluation Unit (DIME) at the World Bank in Washington, DC.

April, 12th 2014 – Draft Version (please don't quote)

Abstract

In December 1998 the Brazilian minimum legal age of entering the labour market increased from 14 to 16. This event 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 of 2007, 2008, 2009 and 2011 to uncover the long run impacts of this intervention comparing outcomes of the cohorts who were age 14 just before and just after the law passed. Since the estimates are performed for all individuals in the two cohorts, the parameter the estimates will be identifying is the *intent-to-tre*at (ITT). Estimates are provided for the whole period as well as allowing for heterogeneous time effects. To check whether the change in the law affected groups with different socio-economic background, estimates are provided for whites and non-whites separately. Unconditional quantile treatment effects (QTE) are also estimated to check whether the child labour ban 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. For the non-whites the opposite is observed. QTE estimates indicate that the law had distributive effects. Most of the estimates are robust to different bandwidths. Finally, a placebo test is performed for two cohorts presumably unaffected by the law. None of the estimates are statistically significant.

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 of the policy makers are shortsighted in that they do not take into consideration long-term consequences of their decisions. Indiscriminate changes in the 'rules of the game' can affect individuals in different manners particularly when the effectiveness of the rules depends somehow on the individuals' background. The purpose of this paper is to assess the long-term consequences of a law that affected cohort who born in the first semester of 1985.

In December 1998, in the midst of changes in the social pension system, the Brazilian federal government passed a Constitutional Amendment increasing the minimum legal age of entry into the labour market from 14 to 16. Individuals who turned age 14 before the law and were hold a work permit remained eligible to enter the formal labour force at age 14. Those who turned aged 14 just after the law passed were hindered from doing so. Thus, the change in the minimum legal age gave rise to a natural experiment as the individuals' eligibility status to participate into the formal labour force depends on their exact date of birth. This paper belongs to the strand of literature that uses the date of birth to compare outcomes of two cohorts who, despite of having very close age, are assigned to different treatment arms.

Angrist and Krueger (1991) were the first to use the date of birth to identify the eligible and ineligible groups for a treatment[1]. They addressed the impact of the compulsory school attendance law in US using the quarter of birth as an instrumental variable to estimate the returns to education and long run effects of human capital on adult earnings.

Most of the papers that use date of birth to estimate long-term effects of a law or intervention turn attention to the impact of an early entrance in school. Consequently, the empirical literature outlines the educational channel as the main mechanism linking date of birth to labour market outcomes.

[1] 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).

This paper contributes to this literature by using date of birth to investigate the effects of postponing the entrance into the formal labour force in up to two years on long-term outcomes. The research question can be twisted to answer what is the effect of an early exposure to labour market on long-term outcomes. This question has a direct parallel with the literature that turns to the impact of youth employment on individuals' long run outcomes. Empirical papers that provide plausible estimates for long run effects of youth employment (or child labour) are scant[2].

This research question has several policy implications. For instance, (1) it can inform policy makers the long run effects of across the board changes in legislation, (2) it can inform whether there are returns to experience of an earlier entrance into to the labour force, (3) it can show whether the returns to experience depends on individuals' background, and (4) it can shed some light on long run unintended consequences of such decisions and signals whether this type of policy should be accompanied by compensating policies for those more likely to be harmed by it.

The common sense may suggest that an early exposure to labour market on adult life is likely harmful. In fact, child labour ban has been justified on theoretical grounds (see e.g. Baland and Robinson, 2000; Dessy et al. 2008), though some have also argue that depending on the context the household lives in, a ban can actually backfire[3]. The consequences of banning individuals to enter the formal labour force at age 14 in the short and long runs are thus ultimately an empirical question.

Emerson and Souza (2011), for instance, 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 birth as instruments for participation in the labour market and school attendance, they show that child labour have a short run negative effect in terms of

[2] There are plenty of evidences of the impact of vocational training on youth outcomes. The question addressed in this paper is different as one is interested in uncovering the impact of hindering children aged 14 from participating in the labour market up 12 years later.

[3] Basu and Van (1998) and Basu (2005) show theoretically that child labour ban can sometimes backfire. The theoretical model developed by Dessy et al. 2008 also imply that a child labour ban is more likely to affect the not-so-poor and end up harming the poorest.

lower investment in human capital and a long run negative effect in terms of lower (adult) earnings. However, their findings suggest that the negative effects evanish around age 30.

Lee and Orazem (2010) borrow the same identification strategy from Emerson and Souza (2011)[4] to estimate the long run effects of child labour on health outcomes of adults Brazilians using the PNAD 1998[5]. The estimates suggest that a simultaneous effect of an early entrance into the labour force and a premature school dropout resulted in higher probability of back problems, arthritis and 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 child labour to identify the causal impact of child labour in individuals' outcomes five years later. The look at the sample of individuals who were aged 8-13 in the baseline. Their findings suggest that child labour had negative effect on school attendance and educational attainment, but 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, they 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 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 adopts a sharper identification strategy to investigate the impact of the ban of December 1998 on the following adults' outcomes: hourly wage, the likelihood of being employed, the likelihood of being employed in the formal sector, and the likelihood of either holding or being pursuing a college degree. Cohorts of individuals who born in

---

[4] In fact, Lee and Orazem (2010) used as reference the working paper version of Emerson and Souza (2011).

[5] The PNAD 1998 has a special supplement material on health outcomes.

the first semester of 1985 and were therefore aged 14 in the first semester of 1999 are compared to the cohorts of males who born in the second semester of 1984 and were aged 14 in the second semester of 1998. Estimates are therefore provided for a 6 months bandwidth in each side of cutoff point (the date of the law). To check robustness, estimates are also provided with controls and with a bandwidth of three months.

Unconditional quantile treatment effects (QTE) are estimated to shed light on distributive impacts of the change in the law. To test whether the law affected most strikingly those with disadvantaged background, estimates are provided for whites and non-whites. Skin color is used as a proxy for individuals' background since firstly descriptive statistics show that it correlates with several individuals' socio-economic characteristics, and secondly short run estimates show that the ban of 1998 affected whites and non-whites distinctly[6].

The main results show that the ban had long-lasting effects on the groups of white and non-white males contributing to increase the wage differential between these two groups. There is some indication that the affected cohort of white males was benefited with 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, 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 but benefited white males at the lower end of hourly wage distribution.

Beyond this introduction, this paper is organised as follows. Next section discusses the law of 1998. A theoretical framework is introduced in the third section, whereas section fourth turns to the empirical strategy. Section five presents the dataset and some descriptive statistics. Section six brings the empirical results, and section seven discusses the robustness check. The conclusion then summarises the main findings of the paper and outlines some policy recommendations.

[6] 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).

2 THE CHILD LABOUR BAN OF DECEMBER 1998

The Brazilian Constitution of 1988 and the Statute of Children and Adolescents are considered the legal framework for entry to the labour market[7]. From 1988 to December 15 of 1998, the minimum legal working age in Brazil was 14 and individuals under 17 were prohibited from working except as apprentices[8].

The Brazilian Congress enacted the Constitutional Amendment No. 20 on 12/15/1998 which increased the minimum legal age for entry to labour market from 14 to 16. Individuals under age 16 could work only as apprentices, whereas individuals younger than 18 were prohibited from hazardous and night work. Though the Brazilian Constitution of 1988 and the Constitutional Amendment of 1998 make reference to an apprenticeship programme that dates back to 1940s, this programme was institutionally formalised only with the Law No. 10.091 enacted on 19/12/2000. Notice that the apprenticeship programme would be an alternative to the youth aged 14 participate in the formal labour force, the official statistics suggest that the take-up rate was very low before December 2000[9].

Because the Constitutional Amendment was enacted in the second half of December 1998 it affected mostly the cohorts who turned age 14 from January 1999 onwards. The cohorts who turned age 14 before the law was passed were not affected by it. To comply with the new law, the Ministry of Labour Brazil had to stop issuing working permits for individuals younger than 16 from December 16th 1998 onwards, unless individuals were participating in the apprenticeship program. The main question this paper aims to investigate is how these two cohorts who turned age 14 close to the

[7] The Law No.8069 from 07/13/1990 named 'Statute of Children and Adolescents Law' (*Lei do Estatuto e do Adolescente*) complements the Constitution in that it establishes children's and youth rights beyond regulating the conditions of entry to the formal labour market.

[8] Although ILO considers as child an individual 15 years old or younger, in Brazil a child is someone aged 12 or less and a youth someone aged 13-18. In this paper, children, teenagers and youth are used interchangeably.

[9] 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.

change in the minimum legal age, but faced different constraints regarding labour force participation, 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 very standard static labour supply model, this framework is useful as it sheds some light on how the other outcomes of interest can be affected by the intervention under study[10].

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[11]. For the sake of simplicity, *C* is expressed in monetary units, and *l* in hours per day[12]. 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[13]. 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 into the labour force if the market wage rate is at least equal to his reservation wage, that is: $L_i > 0$ if $w_m > w_i$, where w_i is individual's *i* reservation wage. Assume that the wage rate paid in the formal labour market, w_F, is

---

[10] See, for instance, Borjas (2013). 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 once the ultimate objective is to bring some rationale for children's decisions regarding time allocation.

[11] This vector can also include individuals' background, such as parent's 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.

[12] The price of a unit of C is therefore \$1.

[13] To make things simple, we assume the labour market is perfect so that individuals are price takers. This is a plausible assumption for individuals who how low accumulated experience in the labour market and are just beginning their career.

higher than the wage rate paid in the shadow economy, w_{Inf} [14]. 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 disadvantageous background, assume that $w_j < w_i$. This implies that individuals with poorer background are less likely to dropout 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 simplicity the figure assumes log-normal distributions with the same variance. The figures differ only in terms of averages. For the sake of illustration, the average reservation wage of individual *i* is assumed to be 14 and for individual *j* 10. Notice that this implies that individuals with disfavoured background are less likely to dropout the labour force for an exogeneous reduction in market wage rate w_m from $w_m^{'}$ than individual *i*[15].

Given that the government passed a law preventing children who turn 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, ε [16], but will face different wage rates and hence incentives to participate in the labour force. This simple framework results in three groups of individuals with similar average characteristics ε: (1) one not affected by the law ($w > w_F > w_{Inf}$), (2) one that is affected by the law and will shift to the informal labour force ($w_F > w_{Inf} > w$), and (3) one affected by the law who will dropout the labour force ($w_F > w > w_{Inf}$).

Assuming that individuals with age close to the cutoff point 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 the same individual – individuals with similar observed and non-

---

[14] 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.

[15] 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}.

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

observed characteristics – faces 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[17].

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 dropout the labour force, and an ambiguous effect on the intensive margin of labour supply for those who move to the informal economy[18].

3.1 WHO ARE MORE LIKELY TO BE BENEFITED AND HARMED BY THE LAW?

The impact of the law on labour supply depends on the substitution and income effects. Based on the assumption outlined above, individuals can be separated into two groups: the better off and the worse off. The group (3) above could be seen as the better off whereas the group (2) the worse off.

Thus, the better off will dropout the labour force and will 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 away work by 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

[17] A fall in wage will imply less hours of work due to the substitution effect and less hours of work due to the income effect if leisure is a normal good. The total effect of a wage fall on labour supply will be negative. However, if leisure if inferior the fall in the wage rate could have a nil effect or even result in an increase in the intensive margin of labour supply. See, for instance, Borjas (2013).

[18] To make things easier, we are assuming that the wage rate is the only variable affecting individuals' decision regarding labour force participation and number of working hours. In reality, there are many other variables that can affect individuals' decision, such as stigma effect. The theoretical framework could be sophisticated a little further with the inclusion of the stigma effect on individuals' reservation wage. Consequently, many who are supposed to shift from formal to informal labour market would rather drop the labour force once stigma effect is taken into account. Notice that it would not affect the main conclusion of the model.

school. If the market rewards experience (work history) accumulated in the formal sector rather than workers' productivity[19], the worse off hindered from participating in the formal labour force at age 14 may end up earning less in the long run than his counterparts because they might face difficulty to prove the experience accumulated in the shadow economy as it is not formally registered in his records[20]. However, if what counts is workers' productivity and this is, on average, similar regardless the sector it was accumulated, then those who shifted to the informal sector will not be jeopardised by the ban.

Short run estimates will be 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 affected by it. This analysis uses skin color is used as proxy for individuals' background. Skin color is highly correlated with individuals' backgrounds, as shown in Table A.1 and is an exogeneous variable. The table compares white 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 THE 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 into the labour force, for instance, to complement households' income, because (s)he is talented enough

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

[20] This dichotomy is similar to the role played by education in the labour market. More educated people can be rewarded because they are indeed more productive or because education is seen as a signal of employees' potentialities by the employers.

to abdicate formal education, or because parents are not fully aware of the returns to education. Whatever is the explanation, individuals may enter the labour force at certain age for plenty 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)[21], the identification strategy relies on the individuals' date of birth once the change of the minimum legal age in December 1998 affected only the individuals who turned 14 from Jan 1999 onwards. The analysis of the long-term effects of the law on individuals' outcomes consists of comparing the cohorts who turned 14 in the second semester of 1998 with individuals who turned 14 in the first semester of 1999. However, unlike Angrist and Krueger (1991), and many other authors who combined date of birth with school entry or exit ages, parents could not anticipate the law and its effects[22].

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

$$y_i = \alpha + \rho D_i + h(Z) + \beta X_i^{'} + u_i \,, \tag{1}$$

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 semester of 1999 and therefore could not participate in formal labour market due to the ban, and 0 if he turned 14 in the second semester of 1998 and was thus allowed to do so. The function *h(.)* depends on age, the forcing variable and will be referred to as "smooth function". The variable age, *Z*, is defined in weeks and set to 0 for individuals who turned age 14 in December 31st 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, it 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 color and some

[21] Many other authors used a similar approach after the publication of this seminal paper. There is an increasing literature on weak instruments that show 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.

[22] 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).

family background, such as parents' years of schooling, and u_i the error term. Most of the regressions are estimated without controls though.

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 effectively affected by the law (those who stopped participating in the labour market or was *de facto* prevented from doing so because of the increase in the minimum legal age[23]. The identification of this parameter depends on some exogeneous variation in the labour force participation rate for some individuals 14 years old in the first semester of 1999 so as they become less likely to participate in the labour force compared to his counterparts[24]. If the law of December 1998 implied a reduction in the labour force participation, then the outcomes of the cohort who were age 14 just before December 1998 can be used as counterfactual for the cohort who turned age 14 just after the law passed. It is worth noting that the apprenticeship programme was available for youth aged 14 even before the increase in the legal minimum age. Thus, it is supposed to have a common effect in the eligible and ineligible cohorts. However, since the programme remained as an alternative to the youth enter the formal labour force at age 14, the impact of ban could be furthered attenuated had the number of apprentices aged 14 been high.

With hourly wage in natural log in the left hand side of eq. (1), it becomes very similar to Mincer equation. Notice, however, 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 the potential experience by 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 is capturing the effect of experience and education altogether.

The second part of the empirical exercise consists of verifying what is the most plausible mechanism through which the law is affecting adults' wages. As mentioned previously, the paper will show that the experience is the most likely to be driving the effect of the ban.

[23] For a comprehensive introduction to different treatment effects parameters, see Heckman, Lalonde and Smith, 1999.
[24] The condition is called monotonicity assumption. See, for instance, Imbens and Angrist (1994).

If the labour force participation rate varies according to individuals' background, the law might have had distributive effects on wages[25]. 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 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 were not be affected by the law. For this exercise, the comparison is between individuals who turned 14 years old in the first and second semesters of 1999.

5 DATA

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

The PNAD is annually collected by the Brazilian Bureau of Statistics (*Instituto Brasileiro de Geografia e Estatística,* IBGE) since the end of 1970's and covers around 100,000 households and about 320,000 individuals. It constitutes one of the main sources of microdata in Brazil, and is a nationally representative, containing information on household's socioeconomic characteristics, demographic data, household sources of income and labour force status.

The purpose of pooling together 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 in schooling and labour market outcomes when individuals are transitioning from school to work. Second, by doing so one can have a better understanding of the mechanisms underlying individuals' decisions regarding the

[25] Quantile treatment effects are estimated only for hourly wage since the other outcomes variables are binary.

accumulation of human capital through formal education or experience accumulated in the labour market.

The subsample of interest is given by two cohorts of individuals aged 14. The first cohort comprehends individuals who turned age 14 between July and December of 1998, hence before the increase in the minimum legal age. This cohort will be used as comparison group. The eligible group will therefore be the group of individuals who turned age 14 between January and June of 1999.

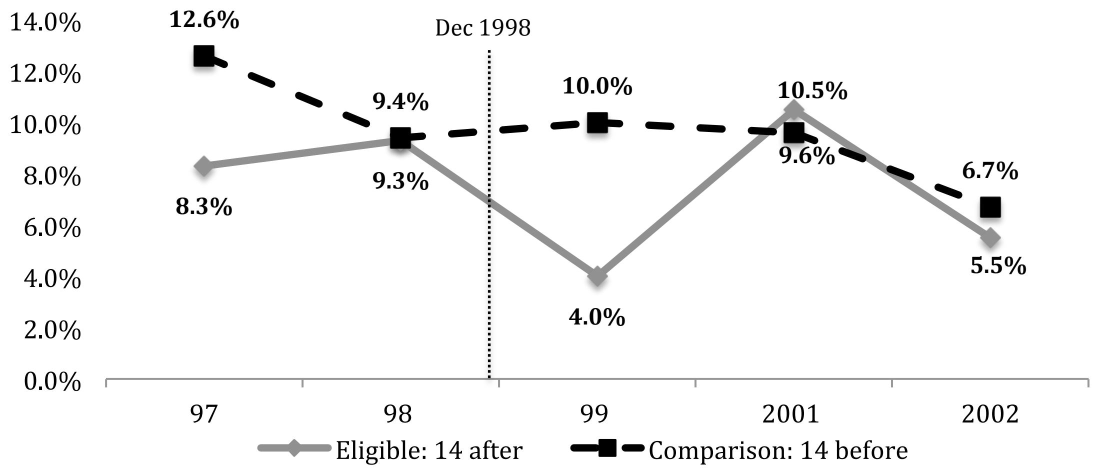
The estimates are initially obtained with a six months bandwidth but are also provided with three months bandwidth to check robustness. The same cohorts are compared from ages 22 and 23 to ages 26 and 27. Finally, the empirical analysis is performed in urban areas because the law might not be fully enforced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

5.1 DESCRIPTIVE STATISTICS

The impact on participation rate in the labour force is not straightforward as children could move to the informal economy. If children 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 experience was accumulated. However, if labour force participation drops and completed years of schooling is the same between eligible and ineligible groups, then it can be argued that experience is the main driver.

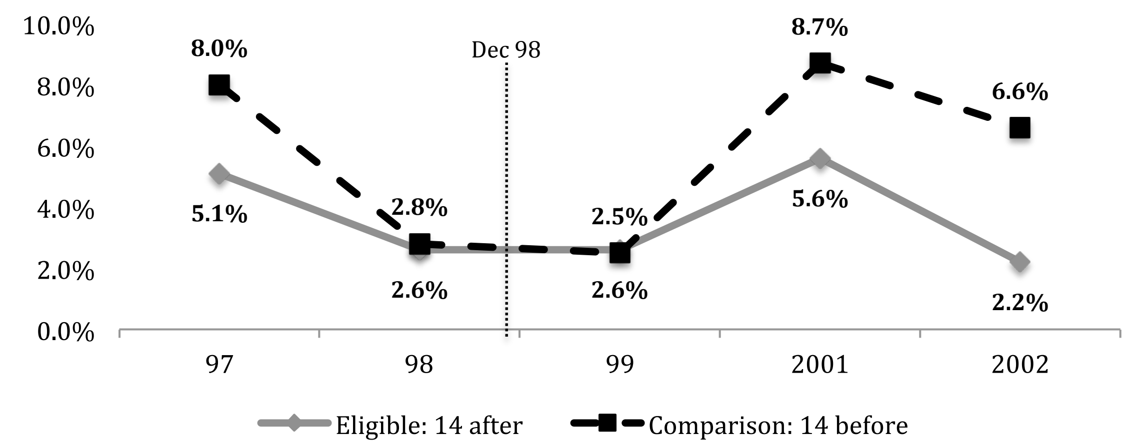
Figures 1 and 2 show the participation rate in the labour force for males and females in the eligible and ineligible groups. A three months bandwidth is used in both figures so that the comparison is made between children who were 14 years old between October and December 1998 and children who turned age 14 between January and March 1999. The figures plot the participation rate (in any sector) for different cohorts in 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 sharply among males who turned age 14 after December 1998 but remained constant for the cohort who turned 14 before. This is an interesting result because it suggests that (1) the ban affected only 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[26]. Figure 2 suggests that the ban did not affect participation rate of girls since the drop observed in the eligible group seems to be due to common macro shocks. As it will be showed below, short run estimates support the descriptive evidence.

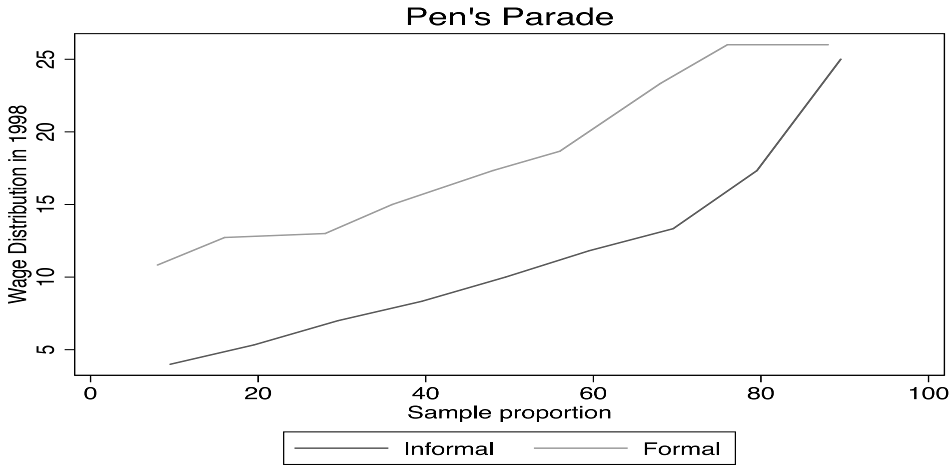
The differences in level observed in in both figures can be explained by seasonal events. Individuals who turned age 14 up to December 1998 were more likely to participate in the labour force due to seasonal events that create temporary work such as Christmas and New Eve. Those events would inflate the participation rate of the comparison group and the impact of the ban on participation rate of the eligible group. The function $h(Z_i)$ in equations (1) and (2) control for such age effects in various ways.

With the ban, similar individuals would face different wage rates. Figure 3 indicates that individuals aged 14 before the ban faced a higher wage rate than those who turned age 14 after as they could still participate in the formal labour force[27]. This is consistent with the assumption made in the theoretical framework and can be used to rationalise children's decision of leaving the labour force after December 1998.

[26] 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

[27] A T-test for difference in means rejects the null hypothesis of equal means at 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.

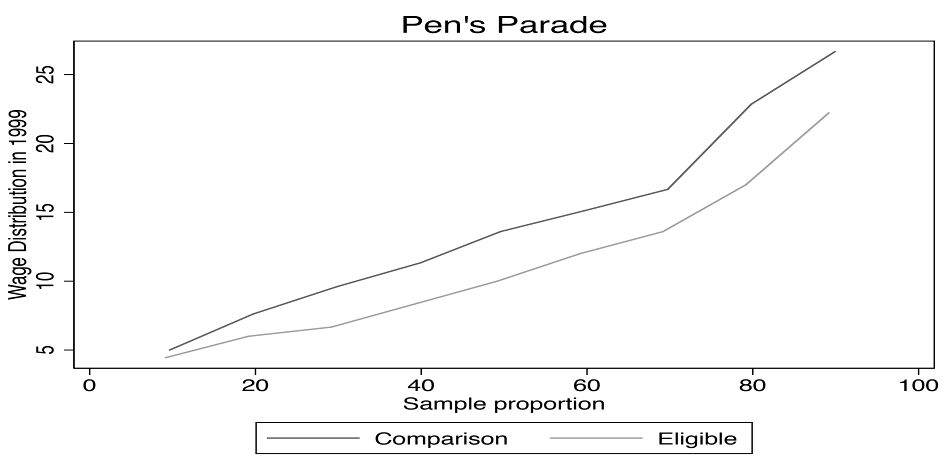
Figure 3 – First Order Stochastic Dominance: Hourly Wage Distributions for Formal and Informal Workers at Age 14 in 1998



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 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 faced by the ineligibles.

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 get a rough estimate of the effect of a change in wage rate on individuals' participation in the labour. Since individuals with very close ages are likely to have similar observed and unobserved characteristics, the ban gave rise to a natural experiment where the 'same' individual faced two different wage rates. It is thus plausible that a fraction of individuals who have a reservation wage above the wage rate paid in the informal sector dropped out the labour force after the ban.

The difference in wage rate between the eligible and ineligible groups in 1999 was, on average, of about 16%[28]. Figure 1 shows that the difference in participation rate among males was 6 pp. In this case, a fall of 16% in wage rate is associated to a fall in participation rate of 6 pp. (or 60% taking the participation of the comparison group as reference). This suggests a rough estimate for the elasticity of labour supply of 3.75 (0.6/0.16). In other words, a ten percent fall in the hourly wage would be associated to a fall in participation rate of 3.75 percent. To get a better sense of the elasticity of labour supply, we estimate the following reduced-form equation,

$$\ln whw_i = \alpha + \beta_1 \ln wage_i + \beta_2 \ln wage_i * D_i + \beta_3 h(Z_i) + u_i \qquad (2)$$

where ln*whw* holds for weekly hours worked in natural log, *lnwage* is the natural log of hourly wage, 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 degree 0 to 3 and as linear, quadratic and cubic splines. The parameter of interest is β_2. Table A.2 shows the results. The coefficient for the elasticity of labour supply is about -0.3 and statistically significant at 1 percent level in all cases. It indicates that a fall in hourly wage of 10% would increase hours worked in 3%. The negative coefficient suggests that leisure is a normal good as the income effect dominates the substitution effect. In addition, it suggests that the labour supply of youth males is not too responsive to

[28] 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 5 percent level (p-value of 0.049) with a 6 months bandwidth.

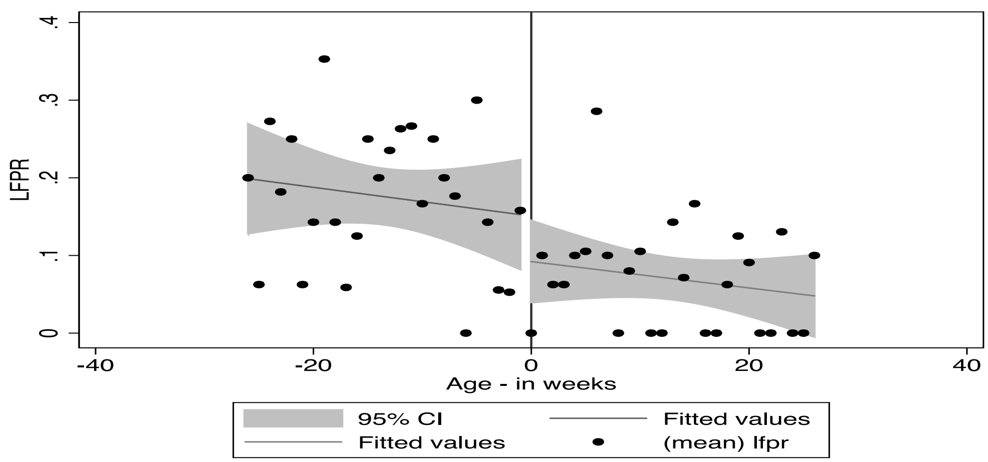
variations in wage rate. This estimate is actually not too far from what has been considered the benchmark in the literature[29].

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 date of birth of each individual, age was defined in weeks to mitigate excess of noise and standard errors clustered at the week level[30].

Figures 5a and 5b show a fall in labour force participation rate for white and non-white males in the year of 1999[31].

Figure 5a –Labour Force Participation Rate in 1999

White Males – 26 Weeks Bandwidth



---

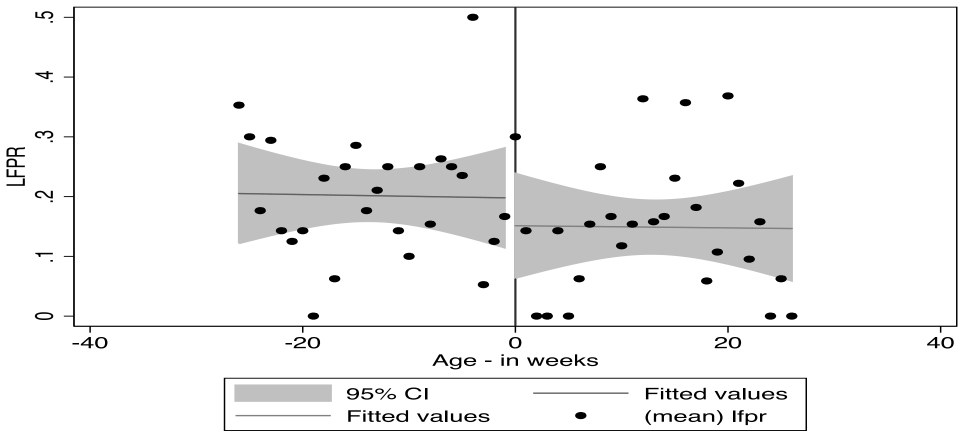
[29] For an extensive survey of this literature, see Blundell and Macurdy (1999). Recent evidence includes Ziliak and Kniesner (2005) and Bargain et al. (2012). The estimate of -0.3 for young males is within the range found in the empirical literature.

[30] Age could be defined in days but it would create some extra noise in the data.

[31] Participation rate for girls are not shown because the short run estimates discussed in chapter one were not statistically significant.

Figure 5b –Labour Force Participation Rate in 1999

Non-white Males – 26 Weeks Bandwidth



The figures suggest that the ban might have had local effects on the participation rate of white and non-white males. The fall in the labour force participation rate among the eligible group might be associated with the lower wage rate but is also consistent with a *stigma effect*. Working at age 14, regardless the sector (formal or informal), became illegal after December 1998 and some individuals may have dropped out to avoid being seen as lawbreakers. Interesting to note that the two regression lines in figure 3a indicate that participation rate was following a downward trend among white males. In figure 3b the regression lines are very flat.

Figures 6a and 6b illustrate the effect of the ban on females.

Figure 6a –Labour Force Participation Rate in 1999

White Females – 26 Weeks Bandwidth

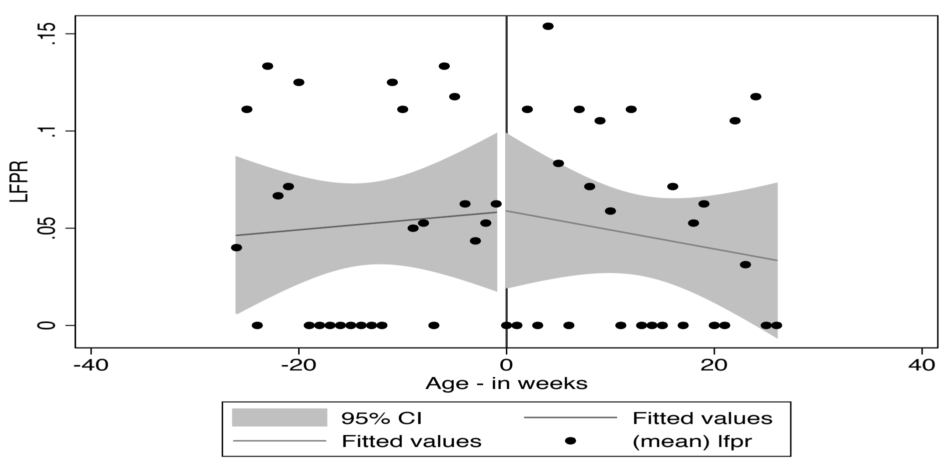
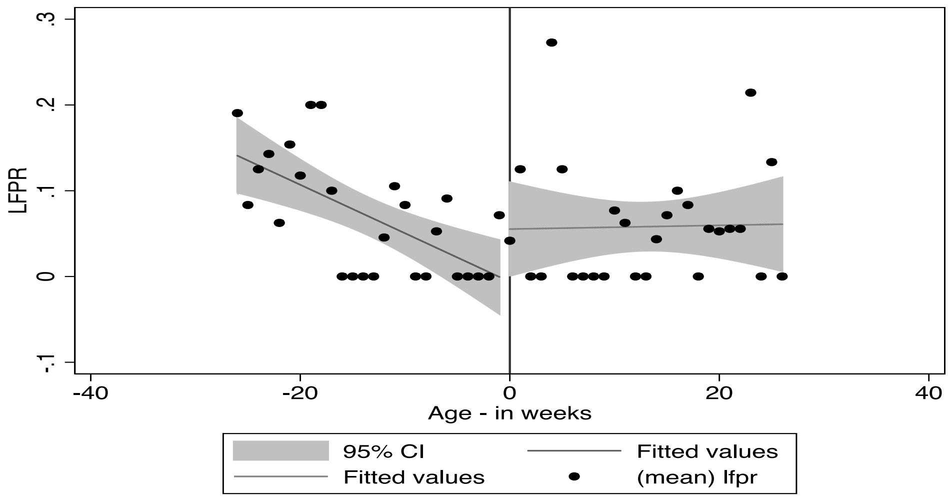


Figure 6b –Labour Force Participation Rate in 1999

Non-white Females – 26 Weeks Bandwidth



Figures 6a shows no discontinuity, but figure 6b suggests that the ban might have increased participation of non-white females.

If the ban gave rise to a natural experiment for individuals with close dates of birth, the observed characteristics of individuals just on the left and on the 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.

[TABLE 1 HERE]

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.

Figure 7a – Predicted Log Wage – Long Run

White Males – 26 Weeks Bandwidth

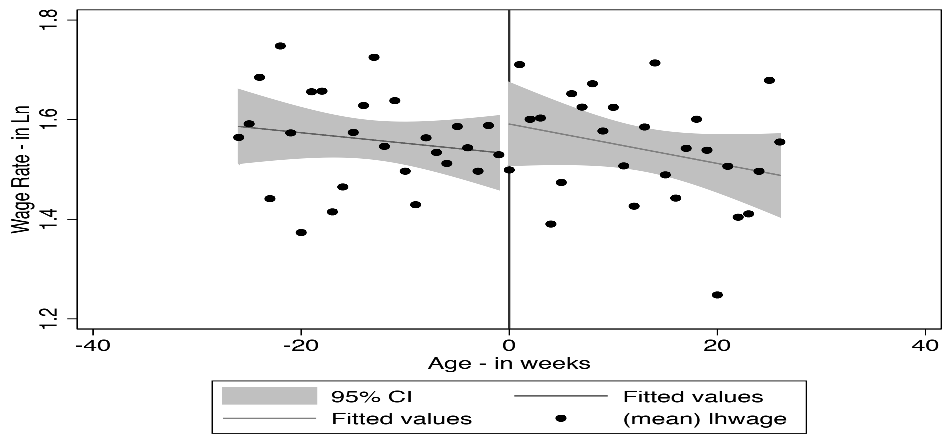


Figure 7b – Predicted Log Wage – Long Run

Non-white Males – 26 Weeks Bandwidth

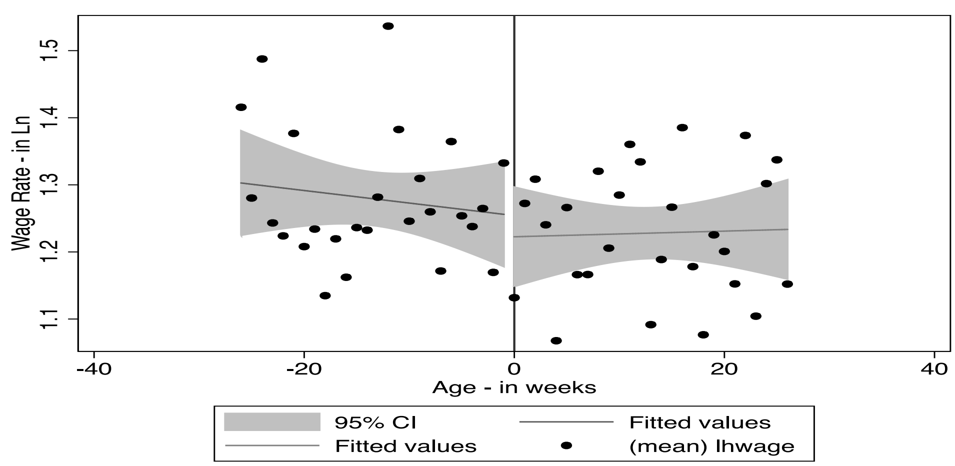


Figure 8a – LFPR – Long Run

White Males – 26 Weeks Bandwidth

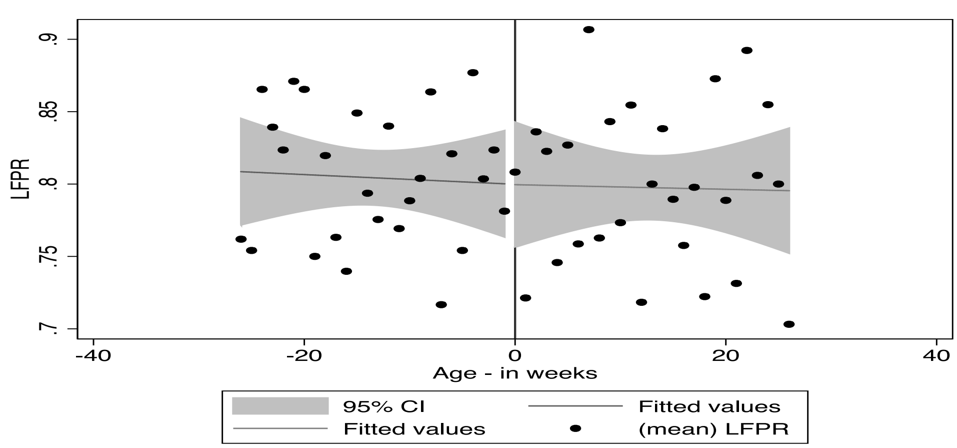


Figure 8b – LFPR – Long Run

Non-white Males – 26 Weeks Bandwidth

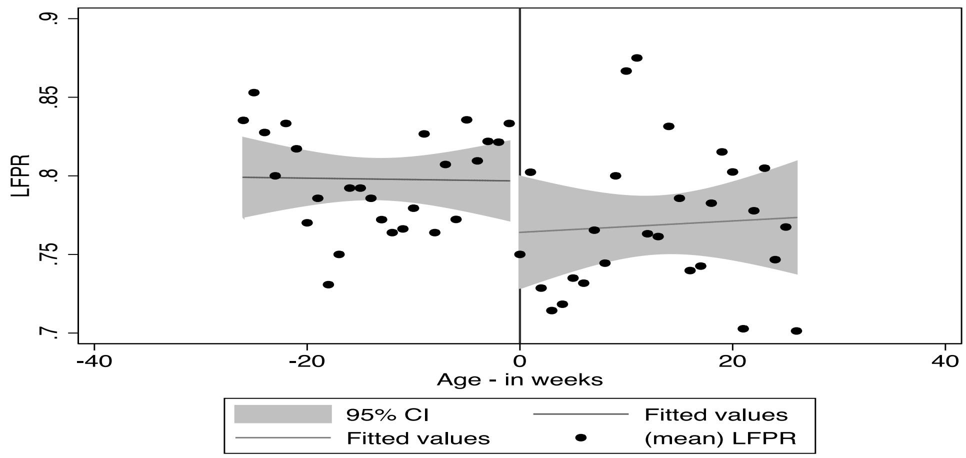


Figure 9a – Participation Rate in the Formal Labour Force – Long Run

White Males – 26 Weeks Bandwidth

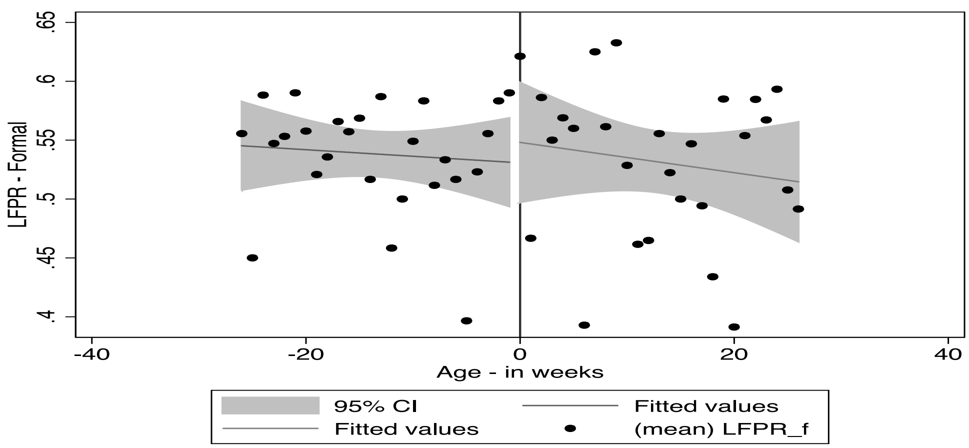


Figure 9b – Participation Rate in the Formal Labour Force – Long Run

Non-white Males – 26 Weeks Bandwidth

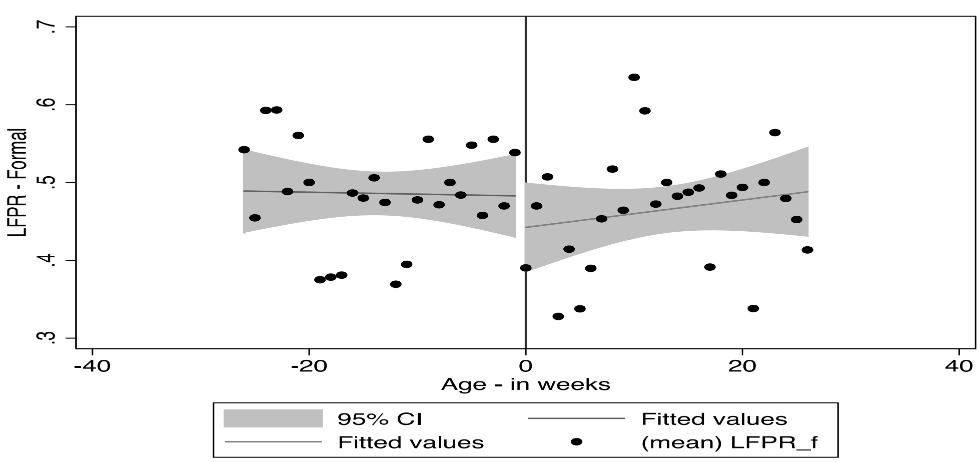


Figure 10a – Probability of Being Pursuing or Holding College Degree – Long Run

White Males – 26 Weeks Bandwidth

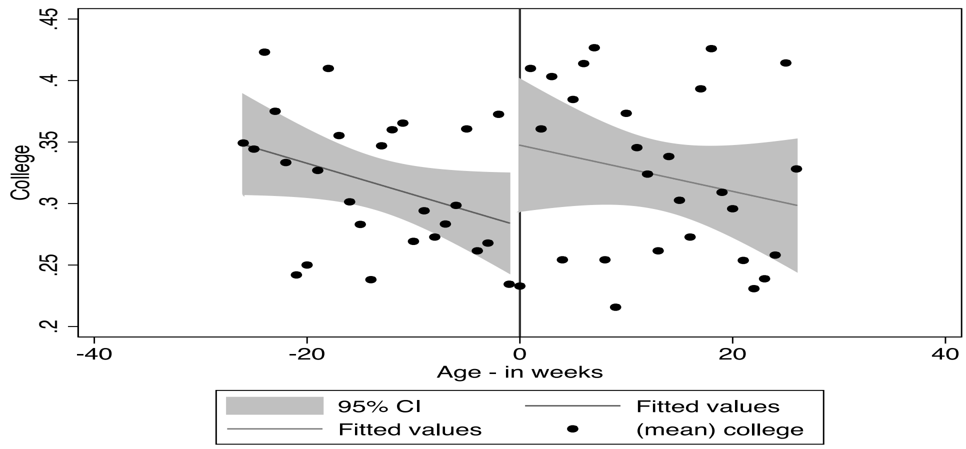
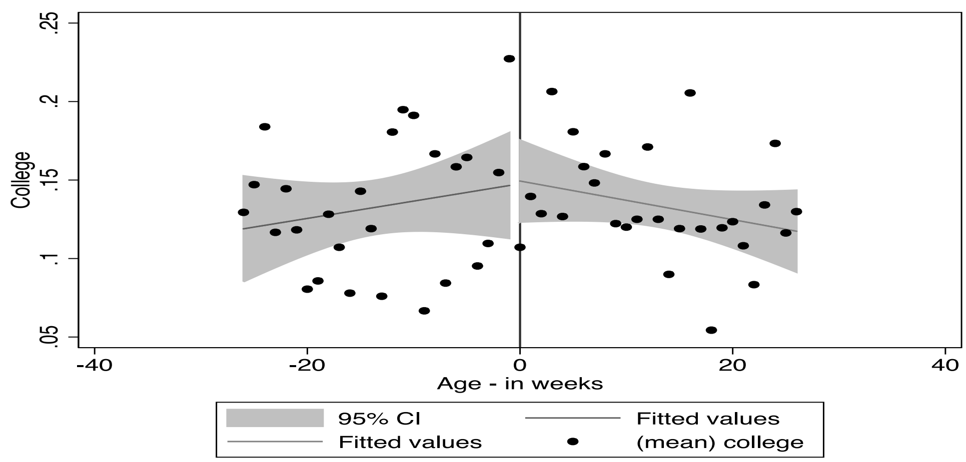


Figure 10b – Probability of Being Pursuing or Holding College Degree – Long Run

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 impacted many outcomes, but the effect and its magnitude might be sensitive to the specification of the smooth function and is likely to differ according to individuals' socio-economic background.

Based on the figures, the ban impacted white males' wage and likelihood of being in a college, and non-white males' likelihood of being employed.

To check whether the sample of eligible and ineligible cohorts is balanced around the cutoff point, t-test for difference in means for the outcomes and some covariates are reported using the pooled sample of 2007-2011. The t-test shows no difference in means for the covariates, except for school attendance. This is interesting for two main reasons. First, it shows that the eligible and ineligible youth have similar socio-economic background, and second it strongly suggests that the ban did not affected individuals' human capital through education as youth around the cutoff point have the same completed years of schooling.

[TABLE 2 HERE]

To completely rule out the education channel, a Kolmogorov-Smirnov test the null hypothesis of equal distributions of completed years of schooling between eligigle and ineligible groups. The null cannot be rejected for the pooled sample of males and for the subsamples of white and non-white males[32].

Next section examines whether such discontinuities are statistically and economically significant. Estimates assume common and heterogeneous time effects, are provided for two bandwidth sizes and for 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 the figures 3a-5b are statistically significant, parametric regressions are estimated for white and non-white males and females respectively as follow:

$$y_i = \alpha + \delta D_i + h(Z_i) + \varepsilon_i , \qquad (3)$$

where D_i is a dummy that takes on the value of 1 for individuals who turned age 14 after December 1998 and 0 otherwise. Z_i is the forcing variable *age* defined in weeks and centered in zero in the last week of December 1998. Thus, Z_i equals 1 for the first week of January 1999, 2 for the second week and so on. Analogously, it equals -1 for the third week of December 1998, -2 for the second week and so on. $h(Z_i)$ is the smooth function

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

defined as in eq. (1). 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 months bandwidths. The parameter of interest δ captures the (local) *intent-to-treat* of the ban. Table 3 shows the estimates for white and non-white males and females.

[TABLE 3 HERE]

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. The results are clearly sensitive to the specification of the smooth function. The results are fairly consistent with the visual check.

Another finding that stands out is the absence of effect of the law on girls. This is consistent with the visual check and with results obtained in the first chapter. It thus provides further support to the evidence that the law affected exclusively the males.

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

6.2 LONG RUN EFFECTS OF THE BAN

ITT ESTIMATES ON WAGES: RETURNS TO EXPERIENCE?

This section turns the long run effects of the ban of 1998. It starts reporting the impact of the law of 1998 on the average hourly wage of the cohort prevented from working due to the ban. Table 4 presents the ITT estimates without controls and a six months 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 such assumption and allows for heterogeneous time effects. Since contemporaneous education can have a direct effect on earnings, the estimates exclude school attenders[33].

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.

[TABLE 4 HERE]

The estimates suggest that white males were positively affected by the ban. Although the point estimates are sensitive to the specification of the smooth function, the pooled estimates suggest that postponing the entrance into the labour force resulted in higher wages in the long run.

For the non-whites, the opposite is observed. Most of the coefficients are negative, but only in 2009 they are robust to different specification of the smooth function and more precisely estimated. For the year of 2009 the cohort of non-white male earned about 12 percent less than the comparison group. It is not easy to justify such an effect in that particular year. 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 percent in real term in 2009[34]. It might be that, for this group, more years of experience in the labour market helped smooth the negative macroeconomic shock.

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. Even

[33] In fact, table 2 shows school attendance is higher among the eligible group and the difference is statistically significant at 1 percent level.
[34] Data available at www.ipeadata.gov.br.

though being lower-bound estimates[35], they are very similar to those found by Angrist (1990) and Bratsberg and Terreall (1998) for the case of the US, and Imbens and van der Klaauw (1995) for Netherlands[36].

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

$$\ln w_i = \alpha + \beta_1 \exp_i + \beta_2 \exp_i^2 + \delta educ_i + \varepsilon_i \qquad (4)$$

where *exp* is the work experience of the individual *i* and *educ* is his years of schooling. The work experience corresponds to individuals' potential experience: *age-educ-6*.[37] Notice that because this measure of experience use actual age as 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.

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[38]. According to the OLS estimates shown in Table A.3, returns to experience seems to be higher for whites than for non-white males. The difference of about four percentage points could be reflecting unobserved individuals' background. One could think of white males having better occupations or 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

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

[36] Angrist (1990) looked at the impact of serving at Vietnam on adults' earnings and found that two years of serving implied an adult wage 15 percent lower than the 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.

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

[38] Lemieux (2006) shows that for the US the traditional Mincer regression model tends to underestimate the observed wage of young works and overstates the wage of those at mid careers. 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.

comparison group as counterfactual, white males affected by the law would have wages about one percentage 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 the that the eligible group of non-white males would be as good 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 college degree.

[TABLES 5 AND 6 HERE]

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

Table 7 shows that white males are more likely to hold or being pursuing a college degree[39]. Putting the results on employability and education together, it might be that some of the whites in the eligible group are in fact employed in higher skilled occupations[40].

[39] In recent years, access to college degree for people with relatively poor background was made much easier. Student loans as well as 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 reputation.
[40] Tables A.6-A.9 in appendix show the estimates with controls. The coefficients are very similar and there is very little gain in precision in adding controls.

Tables A.4 and A.5 in appendix show pooled and heterogenous 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 occupation among white males and show a fall of about 2 pp. in participation rate in army forces, and an weak indication of fall in participation rate in civil construction. The coefficients in Table A.5 tell a similar story, but are less precisely estimated.

[TABLE 7 HERE]

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

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 and according to their results the returns to experience are higher for more-educated workers regardless the occupation[41].

In this paper skin color is used as a proxy for individuals' background some of which might be difficult to observe such as quality of school and other educational outcomes not available in the data. Thus, if the white males hindered from working reallocated more time and effort towards education one could then expect higher return to

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

experience for white males than they counterparts not affected by the ban. For no-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 average effect on the eligible group (ITT) is very informative for a policy perspective but might be of limited interest if the ban had different effects in different quantiles of the wage distribution. Next section provides unconditional quantile treatment effects of the ban to check whether it had distributive effects. The objective is to deep the understanding of the impact of the ban taking into account the asymmetry in wage distribution.

6.3 DISTRIBUTIVE EFFECTS OF THE LAW

To estimate the distributive effects of the increase in the minimum legal age, the unconditional quantitle regression method proposed by Firpo et al. (2009) is used. The estimation of the unconditional quantiles treatment effects takes advantage of the 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 wage gap of the two groups in different points of the unconditional hourly log wage distribution assuming common time effects.

[TABLE 8 HERE]

The results suggest that the ban had a large positive effect at the first decile of hourly wage distribution for white males, but a large and negative effect for non-whites at the median of hourly wage distribution. These results somehow corroborate the ITT estimates and are consistent with the theoretical framework. Under the rank preserving assumption, it suggests that the law benefited the whites at the lower end of hourly wage distribution, but might have harmed the worse-off.

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

[TABLE 9 HERE]

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 hourly wage distribution. With regard to non-white males, there is an indication of negative effect at the median of hourly wage distribution, although the effects become larger and more precisely estimated in 2009. The results are suggesting that the returns to work experience (human capital) are negative for white males as long as the eligible group of white males face higher wages despite having less potential experience, but positive for non-white males.

These findings are somewhat similar to what Bratsberg and Terrell (1998) found in their study for the US economy. They used twelve years of the National Longitudinal Survey of Youth (1979-1991) to estimate the returns to experience and job tenure for white and black workers. Their main 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 data of the Brazilian household survey (PNAD) of 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 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 suggest that the returns to experience might be in fact higher than the returns to education for individuals in the lower end of wage distribution. Although returns to education are not provided here, they are unlikely to reach 20 percent. If this is the case, the 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 individuals' socio-economic background as well as along the unconditional distribution of hourly wage.

This finding has an immediate implication for public policy. It shows that prohibiting households to send young boys to the formal labour force at age 14 may not pay off if the returns to education for individuals with poor background that have to attend low quality public schools and carry on working informally might not be high. While returns to education are high for better off males who face less constraints to attend high quality schools, returns to experience might be more relevant to those with 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 household total income and the children's education. There is no evidence that the ban reduced children's education in the short run as 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 the household total income of non-whites by 28% but did not affect household income of whites[42]. In that sense, at least for the group of non-white males, the ban seems to have affected households' welfare through its impact on households' total 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 therefore 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 age 14 between October and December of 1998 and 0 if he turned age 14 in the first three months of 1999. If the effect was very local, then one expects a slightly higher impact in absolute terms. Table 10 shows the ITT estimates for the impact of the law on log of hourly wage. The reduction in precision resulted in statistically insignificant point

[42] This number was obtained by dividing the difference in average monthly wages between eligible and ineligibles by the household net income of the ineligible group. A T-test for difference in means show that the difference in monthly wage for non-white males was -28.7 reais and statistically significant 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 six months bandwidth.

estimates. Although qualitatively very similar to those obtained with a larger bandwidth size, most of the estimates are statistically significant only against a one-sided alternative.

[TABLE 10 HERE]

Tables 11 and 12 present the effects employability. The results for labour force participation rate are very similar to those obtained with a larger bandwidth. There is no indication of impact on white males and some weak evidence of negative effect on non-white males[43]. On the other side, unlike the estimates with a broader bandwidth, the results of Table 12 strongly suggest that the law had a very local negative effect on the formal labour force participation rate for non-white males. Non-white males became about 12 percentage points less likely to participate in the formal labour force. These results reinforce the previous findings and suggest once again that the law affected mostly children with unfavorable backgrounds.

[TABLES 11 AND 12 HERE]

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

[TABLE 13 HERE]

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 hourly wage distribution for non-whites remains, there is an indication that non-whites at the top decile of wage distribution were positive affected by the law.

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

[TABLES 15 AND 16 HERE]

7.1 PLACEBO TEST

This section presents a placebo test using the cohorts of individuals who turned age 14 between January and December of 1999. Eq. (3) is re-estimated with the dummy

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

D being replaced by a placebo variable that takes on the value of 0 if the individual turned age 14 between January and July 1999 and 1 if (s)he turned 14 between August and December. Tables 17 to 20 show the results for white and non-white males.

[TABLES 17-20 HERE]

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

Table 21 shows statistically significant coefficients for the year of 2011. According to the estimates, white males are more likely to be pursing or holding a college degree. Although the coefficients are significant for 2011, a 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 into 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 in a more concentrated earnings distribution by increasing the wage gap between those at the bottom and top of 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 to the labour market from ages 14 to 16. To our knowledge, this paper contributes to the scarce evidence of the long run effects of an early participation into the labour force on adult outcomes. Apart from that, 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 in 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 suggested 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. The main results indicate that the law benefited white males but harmed the 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 non-affected 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 distributive impact of the law. The results suggested that non-white males in the lower end of wage distribution was negatively affected by the law but those in the top decile of wage distribution were positively affected. Under rank preserving condition, this indicates that the law led to an increase in earnings inequality. The results were robust to different bandwidth sizes and specifications of the smooth function.

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

REFERENCES

Angrist, J. (1990), Lifetime Earnings and the Vietnam Era Draft Lottery: Evidence from Social Security Administrative Records, *American Economic Review*, Vol. 80, pp. 313-35.

Angrist, J. and Chen, S. H. (2008), Long-Term Economic Consequences of Vietnam-Era Conscription: Schooling, Experience and Earnings, Royal Holloway University of London, Discussion Paper 2009-2.

Angrist, J. and Krueger, A. (1991), Does Compulsory School Attendance Affect Schooling and Earnings?, *The Quarterly Journal of Economics*, Vol. 106, No. 4, pp. 979-1114.

Angrist, J. and Krueger, A. (1994), Why Do World War II Veterans Earn More than Nonveterans?, *Journal of Labor Economics*, Vol. 12, pp. 74-97.

Bargain, O., Orsini, K. and Peichl, A. (2012), Comparing Labor Supply Elasticities in Europe and the US: New Results, IZA DP No. 6735, July.

Basu, K. (2005), Child Labor and the Law: Notes on Possible Pathologies, *Economics Letters*, Vol. 87, pp. 169-174.

Basu, K. and Van, P. H. (1998), The Economics of Child Labour, *The American Economic Review,* Vol. 88, n. 3, pp. 412-427.

Basu, Kaushik, e Zafiris Tzannatos. (2003), The Global Child Labour Problem: What do We Know and What Can we Do?, *World Bank Economic Review*, Vol. 17, n. 2, pp.147-173.

Basu, K. (1999), Child Labour: Cause, Consequence, and Cure, *Journal of Economic Literature*, Vol. 37, n. 3, pp. 1083-1119.

Becker, G. S., (1993), *Human Capital*, The University of Chicago Press, Third Edition.

Beegle, K., Dehejia, R. and Gatti, R. (2004), Why Should We Care About Child Labor? The Education, Labor Market, and Health Consequences of Child Labor, NBER Working Paper 10980.

Black, S., Devereux, P. J., Salvanes, K. G. (2011), Too Young to Leave the Nest? The Effects of School Starting Age, *The Review of Economics and Statistics*, Vol. 93, No. 2, pp. 455-467.

Bratsberg, B. and Terrell, D. (1998), Experience, Tenure, and Wage Growth of Young Black and White Men, *Journal of Human Resources*, Vol. 33, No. 3, pp. 658-682.

Blundell, R. and MaCurdy, T. (1999), Labour Supply: A Review of Alternative Approaches, in Ashenfelter, O. and Card, D. (ed.) *Handbook of Labor Economics*, Vol. 3A, pp. 1559-1695. Amsterdan: Elsevier.

Connolly, H. and Gottschalk, P. (2006), Differences in Wage Growth by Education Level: Do Less Educated Workers Gain Less from Work Experience?, Boston College, Working Paper 473.

Dessy, S. and Knowles, J. (2008), Why is Child Labor Illegal?, *European Economic Review*, Vol. 52, pp. 1275-1311.

Dobkin, C. and Ferreira, F. (2010), Do School Entry Laws Affect Educational Attainment and Labour Market Outcomes?, *Economics of Education Review*, Vol. 29, pp. 40-54.

Emerson, P. M. and Souza, A.P. (2003), Is There a Child Labor Trap? Intergenerational Persistence of Child labor in Brazil, *Economic Development and Cultural Change,* pp. 375-398.

Emerson, P. M. and Souza, A.P. (2007), Child labor, School Attendance, and Intrahousehold Gender Bias in Brazil, *The World Bank Economic Review*, Vol. 21, No. 2, pp. 301-316.

Emerson, P. M. and Souza, A.P. (2011), Is Child Labor Harmful? The Impact of Working Earlier in Life on Adult Earnings, *Economic Development and Cultural Change*, Vol. 59, No. 2, pp. 345-385.

Ferro, A. R. and Kassouf. A.L. (2005), Efeitos do Aumento da Idade Mínima Legal no Trabalho dos Brasileiros de 14 e 15 Anos, *Revista de Economia e Sociologia Rural*, vol.43, No.02, pp.307-329.

Firpo, S. (2007), Efficient Semiparametric Estimation of Quantile Treatment Effects. *Econometrica*, Vol. 75, No.1, pp. 259–276.

Firpo, S., Fortin, N. M., and Lemieux, T. (2009), Unconditional Quantile Regressions, *Econometrica*, Vol. 77, No.3, pp. 953-973.

Fortin, N. M., Lemieux, T., and Firpo, S. (2010), Decomposition Methods in Economics, NBER Working Paper # 16045.

Grogger, J. (2009), Welfare, Returns to Experience, and Wages: Using Reservation Wages to Account for Sample Selection Bias, *Review of Economics and Statistics*, Vol. 91, No. 3, pp. 490-502.

Hahn, J., Todd, P. and Van der Klaauw, W. (2001), Identification and Estimation of Treatment Effects with Regression-Discontinuity Design, *Econometrica*, Vol.69, No.1, pp. 201-209.

Heckman, J., Lalonde, R. J. and Smith, J. A. (1999), The Economics and Econometrics of Active Labor Market Programs, in: Card, D. and Ashenfelter, O. (ed.) *Handbook of Labor Economics*, North-Holland.

Imbens, G. and van der Klaauw, W. (1995), Evaluating the Cost of Conscription in the Netherlands, *Journal of Business & Economic Statistics*, Vol. 13(2), pp. 207-15.

Imbens, G. W., and Lemieux, T. (2008), Regression Discontinuity Designs: A Guide to Practice, *Journal of Econometrics*, Vol. 142, pp.615-635.

Kassouf, A. L. (2001), Trabalho infantil, In: Marcos de Barros Lisboa; Naércio Aquino Menezes-Filho (Org.), *Microeconomia e Sociedade no Brasil*. Rio de Janeiro: Fundação Getulio Vargas, pp. 117-150.

Lee, C. and Orazem, P. F. (2010), Lifetime Health Consequences of Child Labor in Brazil, *Research in Labor Economics,* Vol. 31, pp. 99-133.

Lee, D. S. and Card, D. (2008), Regression Discontinuity Inference with Specification Error, *Journal of Econometrics*, Vol. 142, No. 2, pp. 655-674.

Lee, D. and Lemieux, T. (2009), Regression Discontinuity Design in Economics, NBER Working Paper 14723.

Lemieux, T. (2006), "The Mincer Equation" Thirty Years of Schooling, Experience and Earnings, in S. Grossbard-Shechtman (ed.) *Jacob Mincer*, *A Pioneer of Modern Labor Economics*, Springer Verlag.

Light, A. and Ureta, M. (1995), Early-Career Work Experience and Gender Wage Differentials, *Journal of Labor Economics*, Vol. 13, No. 1, pp. 121-154.

Looney, A. and Manoli, D. (2011), Are There Returns to Experience at Low-Skill Jobs? Evidence from Single Mothers in the United States over the 1990s, Mimeo.

Margo, R. A. and Finegan, T. A. (1996), Compulsory Schooling Legislation and School Attendance in Turn of the Century America: A ‘Natural Experiment’ Approach, *Economics Letters*, Vol. 53, pp. 103–110.

Marner, V., Feir, D. and Lemieux, T. (2011), Weak Identification in Fuzzy Regression Discontinuity Designs, UBC Working Paper.

McCrary, J. and Royer, H. (2011), The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth, *American Economics Review,* Vol. 101, No. 1, pp. 158-195.

McCrary, J. (2008), Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test, *Journal of Econometrics*, Vol. 142, pp. 698-714.

Menezes Filho, N., Mendes, M. and Almeida, E. S. de, (2004), O Diferencial de Salários Formal-Informal no Brasil: Segmentação ou Viés de Seleção?, *Revista Brasileira de Economia*, Vol. 58, No. 2, pp. 235-48.

Moehling, Carolyn M. (1999) State Child Labour Laws and the Decline of Child Labour, *Explorations in Economic History*, Vol. 36, pp. 72-106.

Munansighe, L, Reif, T., and Henriques, A. (2008), Gender Gap in Wage Returns to Job Tenure and Experience, *Labour Economics*, Vol. 15, pp. 1296-1316.

Oreopoulos, P. (2006a), The Compelling Effects of Compulsory Schooling: Evidence from Canada, *The Canadian Journal of Economics*, Vol. 39, No. 1, pp. 22-52.

Oreopoulos, P. (2006b), Estimating Average and Local Average Treatment Effects of Education when Compulsory School Laws Really Matter, *American Economic Review*, Vol. 96, No. 1, pp. 152-175.

Smith, J. (2009), Can Regression Discontinuity Help Answer an Age-Old Question in Education? The Effect of Age on Elementary and Secondary School Achievement, The B.E. Journal of Economic Analysis & Policy, Vol. 9, No. 1, pp. 1-28.

Tyler, J. H. (2003), Using State Child Labor Laws to Identify the Effect of School-Year Work on High School Achievement, *Journal of Labour Economics*, Vol.21, No.2, pp.381-408.

Ziliak, J. P. and Kniesner, T. J. (2005), The Effect of Income Taxation on Comsuption and Labor Supply, *Journal of Labor Economics*, Vol. 23, pp. 769-796.

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

Bandwidth of 6 months

	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 – Difference in Means for the Outcome Variables and Some Covariates – Males

Bandwidth of 6 months

	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	-0.031	-0.028	-0.080
	(-0.37)	(-0.23)	(-0.61)
N	*7432*	*3234*	*4198*
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)
3	-0.092	0.063	-0.024	-0.15	-0.011	0.019	-0.0094	0.035
	(-1.51)	(-0.63)	(-0.45)	(-1.53)	(-0.28)	(0.36)	(-0.32)	(0.82)
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.

Table 4 – Long Run Effects on Hourly Log Wages – Whites and Non-whites Males

Bandwidth of 6 months – 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	1932	1932	1966	1966	1966	1966	1932	1932
	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	2787	2787	2831	2831	2831	2831	2787	2787

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

Bandwidth of 6 months – 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	2325	2325	2367	2367	2367	2367	2325	2325
	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	3452	3452	3512	3512	3512	3512	3452	3452

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

Bandwidth of 6 months – 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	3344	3344	3403	3403	3403	3403	3344	3344

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

Bandwidth of 6 months

	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	3184	3184	3248	3248	3248	3248	3184	3184
	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	4146	4146	4223	4223	4223	4223	4146	4146

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

Bandwidth of 6 months – 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

Bandwidth of 6 months – 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

Bandwidth of 3 months – 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

Bandwidth of 3 months – 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

Bandwidth of 3 months – 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

Bandwidth of 3 months

	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

Bandwidth of 3 months – 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

Bandwidth of 3 months – 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

Bandwidth of 6 months – 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)	-0.048*	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 Dec 1998; =0 if 14 before Dec 1998)	-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

Bandwidth of 6 months – 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)	-0.032**	-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 Dec 1998; =0 if 14 before Dec 1998)	-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

Bandwidth of 6 months – 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)	-0.051**	-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 Dec 1998; =0 if 14 before Dec 1998)	-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

Bandwidth of 6 months

	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)	-0.0045	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 Dec 1998; =0 if 14 before Dec 1998)	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
3 Months 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

Homogeneous Time Effects – 6 Months Bandwidth

	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

Heterogeneous Time Effects – 6 Months Bandwidth

	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)
Dt2	-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)
Dt3	-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)
Dt4	-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)
Dt2	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)
Dt3	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)
Dt4	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

Bandwidth of 6 months – 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

Bandwidth of 6 months – 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

Bandwidth of 6 months – 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

Bandwidth of 6 months – with controls

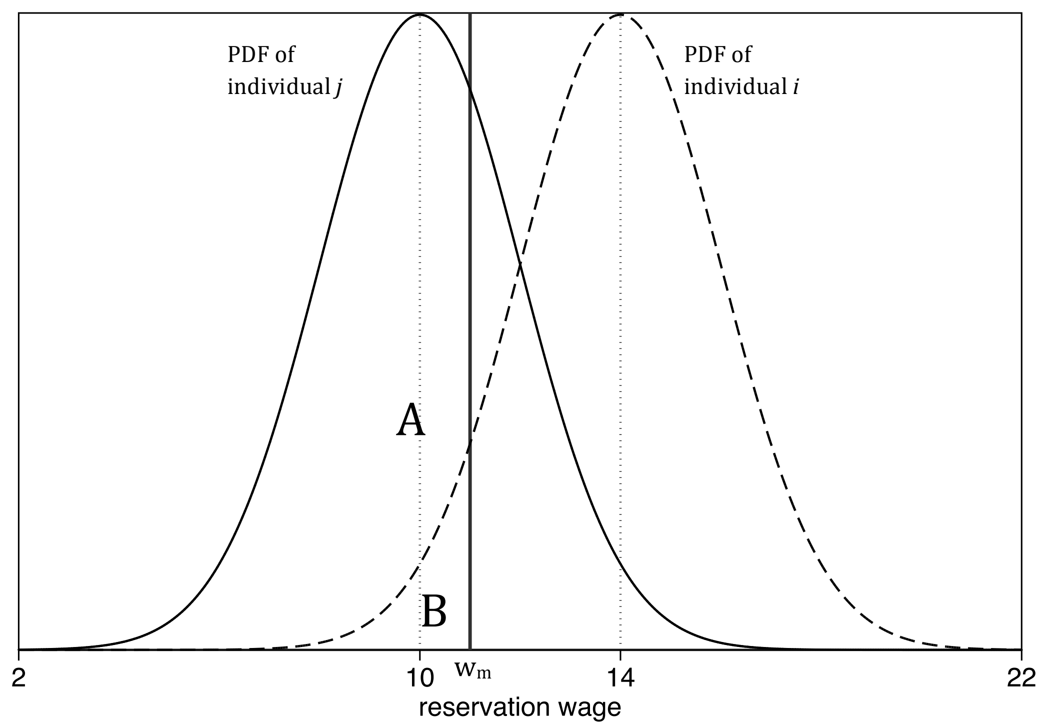
	White Males											
Polynomial degree	0	1	2	3	spline linear	quadratic spline	0	1	2	3	spline linear	quadratic spline
D (=1 if 14 after Dec 1998; =0 if 14 before Dec 1998)	0.023	0.089**	0.091**	0.064	0.091**	0.059	0.020	0.087*	0.089*	0.063	0.089*	0.058
	(1.15)	(2.30)	(2.36)	(1.30)	(2.37)	(1.11)	(0.55)	(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*



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