Paper title: The long-run impact of minimum wages on workers' careers: evidence from two decades of longitudinal linked employer-employee data*

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Abstract: We analyze the impact of high youth minimum wages relying on two decades of linked employer-employee data and a major law change. Alternative treatment/control groups follow from two strands of the literature, namely: the one tracking low-skilled workers employed before the law change, who are eligible to a large wage increase; the one tracking employment of full cohorts, whether working or in school when the law changed. High minimum wages led to a short-run wage gain, which faded over time. They did not jeopardize employment prospects. Changes in hours worked by part-timers point to increased job attachment.

Keywords: teenage labor markets; wage growth; employment; hours of work

JEL codes: J08, J31, J24

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I Introduction

There is limited evidence on the impact of exposure to high youth minimum wages on long-term labor market fortunes. Yet, economic theory identifies several outcomes that are likely to be influenced by an increase in the mandatory minimum wage. In particular, employers might reduce the quantity of labor demanded and undercut the provision of training. Likewise, individuals may change their labor supply, if the minimum wage influences job attachment or schooling decisions. These changes would impact the accumulation of human capital and thus labor market performance over the life cycle. However, depending on the theoretical setting, we are confronted with contradictory predictions on the operation of these mechanisms, and the scarcity of empirical evidence limits our understanding of the process.

This paper analyses the long-term impact of exposure to high youth minimum wages on workers' careers, focusing on the wage profile and employment —both at the intensive and extensive margins —over the life cycle.

Two sets of remarkable conditions combine for the analysis of this topic. First, a longitudinal matched employer-employee dataset of unusual quality is available, which tracks the worker's employment and wage history since early ages and over two decades. Crucially, it covers the population of workers and firms in the private sector in Portugal. The few existing studies that dealt with the longer-term impact of the minimum wage (Dube et al., 2010; Neumark and Nizalova, 2007; Dolton et al., 2012) could not rely on individual longitudinal data.

Secondly, a sharp change in the legislation took place in mid-1980s, leading to a very large wage increase for a very specific group of workers. Indeed, on January 1st, 1987 the minimum wage for 17 year olds increased by 50%; for 18 and 19 year olds, it increased by 33%. These legislative changes were exploited by Pereira (2003), who investigated the impact of minimum wages on firm level employment, and by Portugal and Cardoso (2006), who analyzed worker flows and firms' recruitment and dismissal policies. Either study concentrated on firm-level employment (stocks or flows) and was restricted to the short-run, at most two years after the change in the legislation.

We follow up on those studies, complementing their analyses into the long-run and for

a wide set of outcomes. The core identification strategy follows from Portugal and Cardoso (2006). Workers aged 16 to 18 in 1986 were eligible in January 1987 to a major increase in the applicable minimum wage and they were thus defined as the treatment group. Given that those cohorts were eligible to the high minimum wage at different ages, for different durations, and with different intensities, we will also quantify the impact of the minimum wage separately on each treated cohort. The control group are workers aged 19 in 1986, who were never exposed to the high youth minimum wage. In line with Portugal and Cardoso (2006), we start out defining the control and treatment groups conditional on being employed in 1986, a strategy that has both advantages and limitations. The advantage of this identification strategy is, first of all, that the treatment group was actually exposed early in its working life to the high minimum wage regime. Secondly, it guarantees that both the treatment and control groups were observed in the labor market, having left school, before any change in the law. Hence schooling is not endogenous. However, this strategy raises concern about potential sample selection, as individuals working at early ages (16, 17 or 18) may be different from those working at age 19, potentially leading to different wage and employment patterns over the life cycle. In particular, those who drop out of school at age 16 are most likely different from those who join the labor market later. We therefore also perform the analysis by defining control and treatment groups irrespective of the employment status before the change in the legislation. We compare individuals aged 19 and employed in 1986 (control group) to those aged 19 and employed in 1987, 1988, and 1989 (the treatment groups, potentially exposed for different durations to the high minimum wage). These two empirical strategies are in line with different strands of the previous literature. Whereas the bulk of the analysis has aimed at detecting the short-run impact of the minimum wage on aggregate employment, studies with access to individual longitudinal data have aimed at detecting its impact on the probability of remaining employed for low-skill individuals already holding a job, whose wages would have to increase to comply with the new minimum (Stewart, 2004, 2004a; Stewart and Swaffield, 2008; Zavodny, 2000; Currie and Fallack, 1996). Either approach —concentrating on overall employment for the workers potentially affected or on subsequent employment for low-wage workers who hold a job and are actually affected

by the minimum wage increase —has an interest of its own. We will explore both research avenues, as we define alternative sets of treatment and control groups.

We further check whether the schooling achievement of the cohorts aged 16 to 19 in 1986 could have been influenced by the change in the law. This analysis, though constrained by data limitations, suggests that the sharp rise in youth minimum wages contributed to reinforce the rise in university completion rates that was at the time taking place in the country. This finding reinforces the interest in comparing treatment and control groups already observed in the labor market before the change in the law, whose decisions to leave school had thus not been influenced by the level of the youth minimum wage.

Section II describes the mechanisms that may lead youth minimum wages to have a long-run impact on workers' careers. Section III clarifies the institutional and economic setting in Portugal. In particular, it provides evidence on changes in the teenage wage distribution after the increase in the minimum wage, showing the high degree of compliance with the new legislation. Section IV describes the data and provides details on the treatment and control groups. Section V addresses the question: Do high youth minimum wages jeopardize future wage growth? It devotes special attention to tackling the problem of separate identification of cohort, age and year effects in a wage regression. Section VI addresses the question: Do high youth minimum wages undermine the long-run chances of employment? It first concentrates on the probability of working part-time, conditional on working. It then models the number of hours worked, separately for full- and part-timers. It progresses to model the probability of holding a job in the private sector of the economy. The following section replicates the analysis for the second set of treatment and control groups, as it drops the constraint that individuals must be observed employed before the change in the law. The exploratory analysis on the impact of the minimum wage on schooling achievement is presented next. Finally, section IX concludes.

II Long-run impact of youth minimum wages: the mechanisms

Setting a statutory minimum wage can trigger several mechanisms that influence hu-

man capital accumulation and workers' long-run labor market outcomes. The most widely studied mechanism is labor demand. A standard competitive model concludes that firms will reduce the quantity demanded of low-productivity workers, possibly replacing them with more productive ones, under the assumptions that there is perfect mobility of factors and adjustments can take place instantaneously (Mincer, 1976).

Alternative models highlight the existence of market frictions, in particular job search and other mobility costs or information asymmetries. In the equilibrium wage dispersion model by Burdett and Mortensen (1998), imperfect information on wage opportunities and on workers' skills may lead firms into making wage offers that are below the worker's reservation wage and are therefore rejected, even though the worker productivity may be higher than his reservation wage. This would result in inefficient unemployment. Therefore, imposing a minimum wage would shift the wage offer distribution and reduce inefficient unemployment. Given that wages play a crucial role determining the size of the pool of job applicants and the quit rate by employed workers, a higher minimum wage would motivate firms to move from a policy of low wages and small workforce with high worker turnover, into a policy of higher wages and larger workforce with lower turnover. The overall employment level would thus increase. In general, models with market frictions show that the imposition of a minimum wage within a certain range will lead firms to choose a higher employment level (Gavrel, 2015; Manning, 2003; Rebitzer and Taylor, 1995).

Empirical work has concentrated on the short-term impact of the minimum wage on employment. After a heated debate decades long, which gained new impetus with the extensive evidence summarized in Card and Krueger (1995), a relative consensus emerged in late 1990's around the idea that such impact, whether negative or positive, is very small (see the overview in Brown, 1999). The exceptions that addressed the longer-run impact of minimum wages on employment are the studies by Dube et al. (2010), Neumark and Nizalova (2007), and Dolton et al. (2012). It should be stressed that none of these studies relied on individual-level data and they reached conflicting results. This line of literature has been mainly concerned with the magnitude of the aggregate change in employment. However, studies relying on individual longitudinal data have analyzed the impact of the

minimum wage on low-skill workers who hold a job, as they are actually exposed to the increase in the minimum wage and their wages must increase to comply with the new law. For an analysis of their subsequent employment prospects, see Currie and Fallack (1996) and Stewart (2004, 2004a); for their subsequent hours of work, see Stewart and Swaffield (2008); Zavodny (2000) analyzed both of those outcomes.

Firms could as well adjust along the intensive margin of employment. To the extent that hours of work change, accumulation of labor market experience will be affected. The little available empirical evidence on hours of work is mixed and, in any case, it refers to countries where the minimum wage is set on an hourly basis. Stewart and Swaffield (2008) found a reduction in hours of work by the group affected by the introduction of the hourly minimum wage in the UK. Zavodny (2000) finds mostly insignificant impacts in the US, whereas Gramlich (1976) finds a transfer of workers affected by the increase in hourly minimum wages from full-time to part-time. Hyslop and Stillman (2007) documented an increase in hours worked in New Zealand, following a change in the legislation similar to the one in Portugal. This evidence points to the possibility that employment may adjust as well along the intensive margin, following a change in the minimum wage. The analysis of hours of work, in particular in a setting where the minimum wage is defined as a monthly rate, can thus yield new insights.

The second mechanism linking minimum wages and workers' human capital is on-the-job training, with obvious consequences in the long-run. Under a competitive setting, a statutory minimum wage would undermine firms' ability to provide training and, as a consequence, the affected workers would suffer a wage penalty and possibly lower chances of employment later in life (Becker, 1993: 30-51, first published 1964; Mincer, 1984; Hashimoto, 1982). Instead, under market frictions, the firm can appropriate part of the worker's productivity and this gap between the value of the worker productivity and the wage is increasing with the level of skill or training embodied in the worker. As a result, firms would be willing to invest in general skills. Therefore, a higher minimum wage would lead them into upgrading the type of jobs offered and the general skills of their workforce (Acemoglu and Pischke, 1999, 2003). This upgrading in the quality of the workforce could also be observed if a higher minimum wage enabled firms that face an adverse selection

problem to attract a better pool of job applicants (Drazen, 1986). This reasoning of “upgrading the quality of jobs” or “high wage strategy” is often implicit in the aims of policy makers, when claiming that minimum wages will raise the living standards of the low paid while contributing to develop general skills.

Given the scarcity of data on training, the empirical literature has most often taken an indirect approach, by analyzing workers’ wage profiles over the life cycle. Leighton and Mincer (1981) and Hashimoto (1982) found that workers subject to minimum wages have a flatter age-earnings profile. Papps (2014) took a longer-run perspective, concluding that wages will grow more slowly for exposed workers if they are not covered by collective bargaining when young; otherwise, their wages will grow as fast as the non-exposed. The scarce direct testing of the impact of minimum wages on firm provided training points to mixed results (Leighton and Mincer, 1981; Grossberg and Sicilian, 1999; Acemoglu and Pischke, 2003). We do not tackle this mechanism directly, given lack of data on training. Instead, it will be embodied in the long-run employment and wage profiles under analysis.

Yet another mechanism linking minimum wages to human capital formation is investment in formal schooling. Higher minimum wages could raise the skill threshold to be hired, motivating youngsters to stay longer in school (for different reasonings leading to this outcome, see Cahuc and Michel (1996), Agell and Lommerud (1997), Ravn and Sorensen (1999), and Mincer (1976)). On the contrary, teenagers could drop out of school to start working attracted by a high minimum wage, if the reduction in the probability of employment were not enough of a deterrent (Leighton and Mincer, 1981). When analyzing the impact of minimum wages on schooling, one naturally has in mind the full cohort of potentially exposed individuals and not only those actually employed when the minimum wage increases, given that the latter had already dropped out of school. Empirical analysis of this issue has also provided mixed results —see Mattila (1978, 1981), Campolieti et al. (2005), Cunningham (1981), Neumark and Wascher (1996), Hyslop and Stillman (2007), and Ehrenberg and Marcus (1982).

All of these mechanisms would naturally combine into an impact of early exposure to minimum wages on long-term careers, in a direction not known a-priori, despite the emphasis most often placed on the negative long-run consequences that minimum wages

might have.

III Institutional and macroeconomic setting

The minimum wage was introduced in Portugal in 1974 as a monthly rate for full-time work; for part-timers, it applies pro-rata. Its level is updated annually by the parliament under government proposal and the mandated changes take effect on January 1st.² Decisions on the level of the minimum wage are usually preceded by a consultation with employers' and workers' representatives.

In 1987, the youth minimum wage underwent a major change: for workers aged 17, it was raised from 50 to 75 percent of the full rate, thus increasing by 50 percent due to changes in the legislation *ceteris paribus*; for workers aged 18 and 19, it was raised from 75 percent to the full minimum wage, therefore increasing by 33 percent.³ These were very pronounced changes, moreover if one keeps in mind that the adult minimum wage was approximately 60% of the mean base wage in the economy.

The policy change can be seen as exogenous, yielding good conditions for economic analysis. In fact, the stated aim of the legislator was to promote social justice (Portugal, 1987). Also, according to Pereira (2003), this law generalized to the minimum wage the change in citizenship regulations that had taken place a few years earlier on, when adulthood and its associated rights and duties were set at age 18, down from 21. Her thorough review of the national press reveals that the change in the minimum wage law was unexpected: "In August 1986 there was for the first time news in the Portuguese press about changing the starting age for full MW [minimum wage] entitlement. The change to the law was announced in the daily papers on the last day of that year" (Pereira, 2003: 231). It took effect the following day. Under this setting, the change in the legislation was not driven by any attempt by the government to respond to past or predicted youth labor market outcomes, as it was instead prompted by changes in civil rights regulations that

²During the period under analysis, the exceptions were 1982, when the minimum wage was not updated, and 1989, when it was updated twice.

³The following year, workers aged 14 to 16 were also entitled to an increase in the minimum wage, from 50 to 75 percent of the full rate. However, this change in the legislation affected a less sizable working population for whom, moreover, it is hard to build a control group. Indeed, younger individuals are not allowed to work and slightly older ones were as well exposed to increases in minimum wages at later ages, which are in fact the object of the current analysis.

had been taking place in the country over the decade since the end of the dictatorship regime. Also, there was no previous knowledge of the policy change that economic agents could have used to influence outcomes of interest before the legislation changed.

These changes in the legislation had a remarkable impact on the wage distribution. Figure 1 depicts two cross-section wage distributions: before and after the change in the legislation for the affected group of 17 to 19 year olds. In 1986, the bi-modal wage distribution presented a peak at the minimum wage for 18-19 year olds (75 percent of the full minimum) and another peak at the full minimum wage. By 1987, instead, there was a major peak, at the new full mandatory minimum, reflecting a clear shift in the distribution as the minimum wage increased. The lower peak that year refers to the minimum wage for 17 year olds.

Figure 1: DISTRIBUTION OF MONTHLY WAGES, BEFORE AND AFTER THE CHANGE IN THE LEGISLATION.

Source: Computations based on QP cross-section data for 1986 and 1987. Notes: Real monthly wage for teenagers aged 17 to 19 working full-time. The first vertical line indicates 75% of the national minimum wage in 1986; the second vertical line indicates the full national minimum wage in 1987.

Those changes in the minimum wage took place as the economy was growing at a fast pace: 7.6% in 1987, and 5.3%, 6.7%, 7.9%, and 3.4%, each of the following years up to 1991 (Pordata, 2016). Portugal had joined the European Community one year ago, in January 1986. The unemployment rate was declining, from 8.3% in 1986 to 4.1% in 1991 (Pordata, 2016).

During this period, compulsory education lasted in Portugal for 6 years. Regular school abandonment could take place upon obtaining the diploma of completion of 6

years of education or, if not reaching it, at the end of the academic year when the student turned 14. The legal minimum age to work was 14.⁴

The regulations on hours of work also remained stable around the time the minimum wage changed. The maximum duration of the normal workweek was 48 hours. Later, in 1990, it was reduced to 44 hours; starting in 1997, a gradual reduction took place, until 40 hours by 1998.

Unlike other Southern European countries, female labor force participation is very high in Portugal, similar to the UK. By 2009, the female activity rate stood in Portugal at 56%, like the UK, slightly lower than the US or Sweden, at 59%; two decades earlier, in 1990, the Portuguese female activity rate, at 48%, compared to 53% in the UK (ILO, 2016) (for more details on female labor force participation in Portugal, see Card et al., 2016). The similarity in participation rates of males and females could render any potential impact of the minimum wage also similar across gender, an issue that will be subject to scrutiny, as the analysis will be performed separately for males and females.

IV Dataset and concepts used

Data source

The analysis relies on administrative longitudinal data on workers and their firms, *Quadros de Pessoal* (QP), gathered annually by the Ministry of Employment. Every firm with wage earners is legally obliged to report information on all the personnel working for the firm in a reference week (in March until 1993 and in October since 1994). Civil servants, household employees, and the self-employed are not covered, and the coverage of agriculture is low, given its low share of wage earners. For manufacturing and the services private sector, the survey covers virtually the population of wage earners.

Reported data include the worker's gender, date of birth, highest completed level of education, date of hire into the firm and, for wage-earners, monthly earnings and monthly hours of work. The birth date is reported as month-year information and the first day of

⁴Later changes affected cohorts that are not the object of analysis here. Compulsory schooling was raised to 9 years for students entering in 1987 their first year of primary education, thus affecting the cohorts born October 1980 and later. The working age was raised in 1992 to 15 and in 1997 to 16.

the month has been assumed for the computation of age. Information on the employer includes the industry, location, and share of public and foreign capital ownership. A long period of over two decades of information, from 1986 to 2009, is used. However, no worker data are available for 1990 or 2001.

A worker identification code, based on a transformation of the social security number, enables tracking him over time. The legal requirement for the data to be posted in a space inside the firm accessible to all its workers contributes to data reliability, reducing measurement errors. Also, the data producer has been adopting increasingly refined checks on each unit over time to guarantee data accuracy. Nevertheless, given the relevance of the longitudinal component for the current analysis, additional extensive checks on the panel data from 1986 to 2009 have been performed. The checked panel keeps workers with valid identification code, non-duplicate yearly data, and consistent longitudinal information on birth date, gender, and highest schooling level achieved. The final panel dataset includes approximately 45 million observations on over 7 million workers, corresponding to 82% of the initial total number of observations. Note that inconsistencies were identified for a much smaller share of the database, but whenever the source of the mistake in these key variables could not be detected and corrected, the whole worker history was dropped and only consistent records were kept (for a detailed description of the complete procedure, see Card and Cardoso, 2012: 87-89).⁵

First treatment and control groups

The definition of the first treatment group follows from Portugal and Cardoso (2006). Individuals aged 16 to 18 working in 1986 were eligible to a major wage increase the following year. In turn, the control group comprises individuals aged 19 working in 1986. This is the group that most closely resembles the treatment group, with the remarkable difference that it was just above the age threshold to be eligible to the high youth minimum wage the following year. We further restrict the groups to wage-earners (thus excluding unpaid family members, firm owners and members of cooperatives), given that wages and

⁵Data for early 1980s was once supplied by the data producer, even though its longitudinal nature on workers was compromised by very incomplete identification codes. The data currently supplied for research projects starts in 1986.

hours of work are reported only for wage-earners.⁶

Figure 2 highlights the basis for this comparison. It describes the nature of the treatment, based on a longitudinal perspective of the minimum wage trajectory as a function of age for the treated and control cohorts. Workers aged 16 in 1986 would have been entitled up to age 17 to 50% of the national minimum wage, under the old regime; instead, under the new regime, their minimum wage increased to 75% of the full minimum when they turned 17; it further increased to the full adult rate once they reached age 18. As a result, this group was exposed for the longest period (from ages 17 to 19) to the high youth minimum wage regime. In turn, individuals aged 17 in 1986 were entitled to 50% of the full minimum wage up to age 17 and, under the new regime, the full minimum wage from age 18 onwards, thus undergoing a sudden twofold increase in the applicable minimum wage. Therefore, even though this cohort was exposed for a shorter period to the high youth minimum wage (at ages 18 and 19), it underwent the most abrupt increase in minimum wages of all cohorts depicted. Finally, those aged 18 in 1986 were entitled to 50% of the adult rate up to age 17, to 75% at age 18 and, under the new regime, to the full adult rate at age 19. As such, this cohort is the most similar to the control cohort aged 19 in 1986, both in terms of its shorter period of exposure and its more gradual increase in the applicable minimum. The control cohort, aged 19 in 1986, was eligible to 50% of the adult minimum wage up to age 17, to 75% at ages 18 and 19, and to the full rate thereafter. In the regression analysis to be undertaken, it is informative to identify the impact of the high minimum wage on each treated cohort separately, given that the nature of the treatment is different across the three cohorts, both in terms of intensity and duration of exposure.

The analysis will focus on the long term impact of exposure to high youth minimum

⁶Portugal and Cardoso (2006) and Pereira (2003) adopted much broader control groups (up to ages 25 or 35), because their main analysis was performed at the firm level, to model the share of teenagers in the overall entry or exit flow of workers or the employment stock of youngsters within a broader stock. Given that a very small share of firms in the economy employ workers aged *exactly* 19, restricting those analyses to that control group would have led to a too narrow and most likely unrepresentative set of firms. The current analysis, instead, will be conducted at the individual worker level and such broad control groups would present a major drawback—it would be very hard to sustain the assumption that, in the absence of the minimum wage shock, the outcomes for the control and treated workers would have evolved similarly. This justifies the current option for a narrower control group, very similar to the treated one.

Figure 2: MINIMUM WAGE TRAJECTORY AS A FUNCTION OF AGE IN 1986 FOR THE TREATED AND CONTROL COHORTS.

Source: Portugal, legislation on the minimum wage.

wages over the longest overlapping age range feasible for the treatment and control groups —ages 19 to 39. Naturally, it should control for year effects when analyzing different birth cohorts over a common age range, as the cohorts reached each age at different calendar moments.

Sample sizes

Background information on the size of the birth cohorts 1966 to 1969, the key cohorts of interest for the current analysis, highlights the reliability of this dataset. The National Statistical Office reports approximately 800 thousand individuals born in Portugal during that period (Pordata, 2016). We would expect to follow a similar number of individuals once they reach the labor market, given that the dataset tracks the population of workers in the private sector of the economy (abstracting in this rough exercise from the existence of migration flows and individuals who never work for the private sector as wage-earners). Indeed, the QP panel tracks in the labor market, at any point in their careers, 85% of the individuals in these cohorts. Some may be tracked for long periods, if remaining employed in the private sector, others for short periods, if joining uncovered sectors or labor market states (in particular, civil servants, self-employed, which is a category that increased in the Portuguese economy over the period under analysis, unemployed, etc).

The population sizes for the male treatment group aged 16 in 1986 and the control group aged 19 are 5,591 and 15,031 individuals, respectively (see Table 1). The larger

size of the control group results from the accumulation of flows of labor market entrants from ages 14 to 19, who are potentially employed at age 19. Instead, at age 16 the group potentially employed includes only the entrants from ages 14 to 16. For that reason, the size of the treatment group increases monotonically from ages 16 to 18. These patterns on sample sizes hold for males and females alike.

Table 1: DESCRIPTIVE STATISTICS, CONTROL AND TREATMENT GROUPS CONDITIONAL ON WORKING IN 1986.

Variable	Moment	Males				Females			
		T16	T17	T18	C19	T16	T17	T18	C19
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Log) hrly wage	initial	0.258 (0.324)	0.378 (0.345)	0.569 (0.310)	0.684 (0.299)	0.273 (0.305)	0.378 (0.295)	0.532 (0.268)	0.615 (0.257)
	ages 19-39	1.312 (0.400)	1.281 (0.415)	1.267 (0.438)	1.227 (0.469)	1.047 (0.296)	1.017 (0.295)	1.009 (0.324)	0.989 (0.359)
Weekly hours	initial	41.495 (6.383)	41.373 (6.537)	41.139 (6.548)	40.814 (6.931)	42.597 (5.642)	42.558 (5.437)	42.104 (6.160)	41.686 (6.558)
	ages 19-39	39.477 (5.483)	39.853 (5.495)	39.909 (5.641)	40.073 (5.671)	39.404 (6.248)	39.876 (6.045)	39.927 (6.405)	40.114 (6.451)
Part-time (share)	initial	0.086	0.095	0.102	0.111	0.056	0.056	0.069	0.083
	ages 19-39	0.074	0.072	0.076	0.077	0.086	0.080	0.084	0.086
Education (share) 9 years	initial	0.034	0.039	0.052	0.067	0.025	0.027	0.043	0.060
	ages 19-39	0.077	0.077	0.084	0.094	0.051	0.048	0.062	0.075
high school	initial	0.000	0.000	0.016	0.026	0.000	0.000	0.018	0.039
	ages 19-39	0.014	0.011	0.032	0.044	0.011	0.009	0.029	0.051
university	initial	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	ages 19-39	0.001	0.001	0.001	0.002	0.001	0.001	0.001	0.001
Employed (share)	initial	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
	ages 20-39	0.445	0.453	0.455	0.471	0.491	0.490	0.495	0.505
N observ.	initial	5,591	8,988	12,413	15,031	3,811	5,616	7,444	8,919
	ages 19-39	44,596	73,447	101,918	132,074	33,106	49,036	65,919	83,146

Source: Computations based on QP (1986-2009). Notes: “Initial” is defined as before the change in the legislation, in 1986, i.e. at ages 16, 17, 18 or 19 for the three treatment groups and the control group, respectively. Employed means observed working as wage-earner in the private sector of the economy (see text for details). The descriptives refer to wage-earners with non-missing wage. Standard-deviations in parenthesis.

It follows that, also by design, workers in the treated groups are less educated than those in the control group, reflecting their earlier entry into the labor market. Note that the decision to leave school by these groups was not influenced by the increase in the minimum wage, as both the treatment and control groups were observed working already before the change in the legislation. Whereas no group was old enough to have completed college when initially observed, the share with a high school diploma is larger in the control group (3% or 4%) than in the treatment groups (0% to 2%); similar patterns hold for

the completion of 9 years of schooling.⁷ The education gap between treated and control groups tends to be larger for females than males. Regression analysis should account for this gap in schooling attainment between the treatment and control groups observed before the change in the minimum wage, when quantifying the long-run impact of actual exposure to high youth minimum wages.

Second treatment and control groups

We tackle a potential selection bias that might result from defining the treatment and control groups conditional on being employed before the change in the law, in 1986. Indeed, as discussed in the previous subsection, youngsters observed as wage-earners at age 16 (17 or 18) are less educated than those working at age 19. They are likely to be different also along other dimensions and could thus have different life cycle wage and employment patterns. Unfortunately, the set of feasible solutions to control for heterogeneity across the two groups is limited by the fact that the data are not available before 1986. Therefore, we have instead extended the groups to consider full cohorts, dropping the condition on employment before the change in the legislation, to include, those who enter the labor market at later ages. We therefore consider potential exposure to the high minimum wage, as the treatment group includes individuals who were out of the labor force when the law changed.

In any case, a trade-off must be faced. For the full cohort, the schooling outcome is likely to have been influenced by the increase in the youth minimum wage. As a matter of fact, section VIII discusses the evidence on changes in schooling achievement, indicating that the increase in the minimum wage may have contributed to reinforce the increase in the rate of completion of tertiary education that was taking place in the country. To reduce this potential endogeneity problem, we retain as treatment group those individuals aged 16 to 18 in 1986 who were observed working at age 19 (in 1989, 1988, and 1987, respectively); likewise, the control group includes those individuals aged

⁷The fact that the data are available from 1986 onward precludes the precise identification of the age at entry into the labor market for these cohorts. One could infer it for workers who remained with the same employer between entry and 1986, but this sample of stayers would not yield reliable information to compare the entry age across cohorts.

19 in 1986 observed working. This procedure tackles the potential selection bias in the initial analysis, by removing the employment constraint at early ages, while addressing also the potential endogeneity of education.

Outcomes under analysis

The outcomes under analysis over the life cycle are: real hourly wage; part-time versus full-time nature of the job, for those employed; weekly hours of work, separately for part- and full-timers; probability of holding a job.

Gross hourly wages were computed as $\frac{bw+reg}{hnorm}$, where *bw* stands for monthly base-wage, *reg* are other regularly paid benefits, such as meal allowances, and *hnorm* are normal monthly hours of work. Wages were deflated using the Consumer Price Index (base 2009) and wage outliers have been recoded to missing.⁸

Normal monthly hours of work are defined by collective bargaining or by firm regulations. They may vary across groups of workers, depending in particular on the industry, the occupation, or other workers' attributes. As such, they are not constrained to be uniform, even within the firm. They must, in any case, respect the national law on the maximum duration of work. Reported normal hours pertain to hours paid under the monthly base-wage and, as such, they include any paid time, whether actually worked or not (see public holidays). Weekly hours of work were computed as monthly hours divided by 4.3. Part-time work is coded as working less than 35 hours a week. Note that this definition of part-time and full-time is based on objective criteria and employers could not "relabel" workers into a different category, unless there was an actual change in the hours of labor supplied.⁹

To analyze employment over the life cycle, we code employment as one if the worker is observed as wage-earner in the reference week in the database and zero otherwise, for all years after the initial observation (in 1986 for the initial analysis and at age 19 for the full cohorts analysis).

⁸Defined as 10 times the wage of percentile 99 or half the wage of percentile 1 of the yearly wage distribution. We have as well applied a more widespread definition of outliers (lowest 1% and highest 1% wage earners each year) and the results, available from the author upon request, are very similar to the ones reported.

⁹The robustness of the results will be checked by using an alternative concept of hourly wages and hours of work, which includes overtime pay and work (see the online supplemental material).

V Do high youth minimum wages jeopardize future wage growth?

The profusion of responses that the minimum wage may generate, from workers and employers alike, could combine into a lifetime wage premium or penalty. Theoretical predictions point in either direction. The aim of this section is to quantify the overall long-run wage impact of exposure to high youth minimum wages. The outcome of interest is the individual wage profile over a common age range for the treatment and control groups of workers: a two-decade interval, from ages 19 to 39.

Figure 3 provides a longitudinal perspective on the raw real hourly wages of the treatment and control groups. In 1986, before the change in the legislation, the treated group aged 16 was eligible to 50% of the adult minimum wage, whereas the control group, aged 19, was eligible to 75% (or 179 Euro versus 268 Euro a month, at 2009 prices). A large gap in the actual hourly real wage thus prevailed between the two groups (43 log points for males), reflecting in particular the institutional setting and the older age of the control group. The two following years this gap narrowed substantially (to 33 and 18 log points), as the treatment group was eligible to 75% and 100% of the adult minimum wage under the new regime in 1987 and 1988, respectively, and the control group was entitled to the full minimum. For females, the wage gap between treated and control groups was smaller (see the second panel in the first row of Figure 3). Nevertheless, the convergence trend is similar, as the raw gap reduced from 34 log points in 1986 to 11 points two years later. The cohort aged 17 in 1986 was subject to the most abrupt increase in minimum wages. Accordingly, the wage gap with respect to the control group narrowed substantially between 1986 and 1987. Finally, the cohort aged 18 in 1986 is the most similar to the control group. The convergence in raw wages between treatment and control groups lingered on throughout the period under analysis, even though its pace slowed down.

For all age and gender groups, the raw evidence on wage convergence over two decades does not point to the existence of a wage penalty later in life for the group exposed to high youth minimum wages. However, given that the definition of treatment and control

Figure 3: EVOLUTION OF (LOG) REAL HOURLY WAGE.

Source: Computations based on QP. Note: Considers wage-earners only.

groups is based on the different exposure to high youth minimum wages across cohorts, a key concern that the identification strategy must tackle is that cohort, age, and time effects are not separately identified in a linear regression. Only two of these effects are not perfectly collinear. The omitted effect —time or age —is thus bound to confound the comparison of treatment and control cohorts. If controlling for time effects while omitting age, the treated cohort would benefit from being younger, as wages are known to have a concave life-cycle profile, growing faster at younger ages. If, in turn, controlling for age while omitting calendar time, the younger cohort would benefit from rising real wages in the economy, as it reached each age at a later calendar moment than the control cohort. Also, business cycle effects that may have a differential impact across cohorts at each age would not be accounted for.

The identification strategy addresses these concerns. First of all, the analysis compares the different cohorts at the same ages —the common bracket 19 to 39, as well as subsets of this range. Secondly, time effects are accounted for, to net out labor market trends and business cycle effects over the two decades. To identify time effects, information on other birth cohorts in the economy is brought into the analysis sample, when they are aged 40 to 49. This age bracket was chosen considering that the age-earnings profile has a concave profile that reaches a plateau around those ages, thus providing an age interval clean of age effects —see in particular Card et al. (2016) or Angrist and Krueger when stating “we mainly focus on 40-49 year-old men, whose wages are hardly related to age” (Angrist and Krueger, 1991: 995).¹⁰

Each regression will therefore be ran on the following pooled dataset: i) the treated cohorts from ages 19 to 39; ii) similarly, the control cohort from ages 19 to 39; iii) all other birth cohorts in the economy, from ages 40 to 49 in each year included in the analysis. The latter is the group that will enable identification of the year effects in each regression. Given that the control group is common across treated cohorts, each regression pools data on all treated cohorts, though identifying separately the impact of the minimum wage on each of the treated groups.

More formally, the following equation is estimated separately for males and females, using pooled data for the treated and control cohorts at ages 19-39 and all other cohorts in the economy at ages 40-49, from 1986 to 2009:

$$\ln w_{it} = \alpha + \sum_{t=1987}^{2009} \gamma_t + \alpha_2 \text{age}_{i,19-39} + \sum_{m=16}^{18} \alpha_{3m} \cdot \text{treated}_m \cdot \text{age}_{i,19-39} + \mathbf{X}'_{it} \boldsymbol{\lambda} + \varepsilon_{it} \quad (1)$$

where $\ln w_{it}$ is the natural logarithm of the real hourly wage of individual i in year t ; γ_t stands for calendar year fixed effect; age_{19-39} is a dummy variable equal to one at ages 19 to 39 and zero otherwise (i.e. one for the treatment and control cohorts and zero for all other); treated_m is a set of three dummy variables, each equal to one for each treated cohort $m = 16, 17, 18$ years old in 1986 and zero otherwise. α_{3m} are the parameters of interest, capturing the long-run wage differential between the control and each treatment

¹⁰Nevertheless, we test the robustness of the results by considering an alternative age bracket (45-55), reported in the online supplemental material.

group. Whereas the coefficient α_2 summarizes the wages of the control group over the two decades, the coefficients α_{3m} capture the long-run difference between the control and each treated group. γ_t ($t=1987, \dots, 2009$) captures the evolution over time of wages in the economy, identified relying on workers aged 40 to 49, while the constant captures their average wage in 1986. $\mathbf{X}_{it}$ is a vector of control variables, including the education level of the worker (four dummy variables), the region (2 dummy variables, for the Lisbon and Oporto areas), whether the firm is public or foreign, and a set of 24 industry dummies. Standard-errors are clustered by person.

We are further interested in quantifying the time path of the wage gap between treated and control groups. Therefore, we follow an approach common in the unemployment literature, when aiming at detecting whether a spell of involuntary unemployment leads to permanent "scars" or only temporary "blemishes" in a worker's career (Ruhm, 1991; Jacobson et al., 1993; Couch and Placzek, 2010). Each coefficient of interest in equation 1, α_{3m} , will unfold into its age components: 19, 20-29 and 30-39. The equation under estimation therefore becomes:

$$\begin{aligned} \ln w_{it} = & \alpha + \sum_{t=1987}^{2009} \gamma_t + \sum_{a=19,20-29,30-39} \lambda_a \text{age}_{i,a} + \\ & + \sum_{m=16}^{18} \sum_{a=19,20-29,30-39} \zeta_{am} \cdot \text{treated}_m \cdot \text{age}_{i,a} + \mathbf{X}'_{it} \boldsymbol{\lambda} + \varepsilon_{it} \end{aligned} \quad (2)$$

where all variables keep their meaning from equation 1 and age_a stands for a dummy variable equal to one for each age $a = 19, 20-29, 30-39$, and zero otherwise. The differential growth in real hourly wages between the treatment and control groups is now captured by the set of estimates ζ_{am} .

Table 2 reports the results of the estimation of equations 1 and 2. The first column for each gender includes no controls, simply replicating the raw data reported in the descriptive statistics for ages 19-39. It summarizes a raw life-cycle wage advantage for the treated group over the control one, ranging from 2% for women in the oldest treated cohort to 8% for men in the youngest treated cohort. This wage advantage is confounded by time effects and the different schooling level of the groups upon entry into the labor market, before the change in the legislation. Column 2 thus accounts for time trends and

business cycle effects, by including year fixed effects, and for the educational level of the individual. It reveals, as expected, a considerable reduction in the premium of the treated group, which now ranges between 1% for the oldest female treatment group and 6% for the youngest male group.

We now inspect the time path of wage convergence across the two groups. Column 3 disaggregates the impact at ages 19-39 into three separate intervals: 19, 20 to 29, and 30 to 39. It is clear that the life cycle wage premium of the treated group is concentrated in the initial career years after exposure to high youth minimum wages. The wage push at young ages generated by the minimum wage was particularly strong for females, but it dissipated faster than for males (see columns 3 and 7, for treatment 17 and 18). At age 19, the wage premium of the treatment over the control group was for females 8% to 10%, whereas for males it was 5% to 9%. Subsequently, at ages 20-29, this wage advantage of the treated groups had partly or fully eroded: for males, it was down to 1% to 5%, whereas for females it had either vanished or was at most 3%. By ages 30-39, the wage premium had further eroded for males (to 3% for the longest exposed group and hardly any premium for the shortest exposed), but remained roughly stable for females (from 2% for the longest exposed group to no premium for the shortest exposed).

Columns 4 and 8 aim at shedding some light on the potential mechanisms generating the detected wage premium. They show that, once we account for the attributes of the firm, the wage premium declines. Therefore, access to better firms partly accounts for the lifetime wage premium for the groups exposed to high youth minimum wages.

In synthesis, analysis of wages profiles points, on one hand, to the existence of a wage premium over the life cycle for workers exposed to high minimum wages at youth. On the other hand, it shows that the premium was concentrated in the short-run, with wages converging over the longer-run across the treatment and control groups. The evidence gathered for this Portuguese setting does not indicate that high youth minimum wages jeopardize future wages resulting in an overall life cycle wage penalty.

Table 2: REGRESSION OF (LOG) HOURLY WAGES, TREATMENT GROUP (16, 17 OR 18 YEAR OLDS) VERSUS CONTROL GROUP (19 YEAR OLDS).

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment 16	.084*** (.005)	.059*** (.005)			.058*** (.005)	.036*** (.004)		
Treatment 17	.054*** (.005)	.046*** (.004)			.028*** (.004)	.025*** (.004)		
Treatment 18	.040*** (.004)	.027*** (.004)			.020*** (.004)	.013*** (.004)		
T16 * age 19			.089*** (.006)	.074*** (.006)			.085*** (.005)	.075*** (.005)
T16 * age 20-29			.046*** (.005)	.031*** (.005)			.025*** (.004)	.010** (.004)
T16 * age 30-39			.029*** (.007)	.017*** (.006)			.022*** (.006)	-.001 (.005)
T17 * age 19			.078*** (.005)	.067*** (.005)			.099*** (.005)	.089*** (.005)
T17 * age 20-29			.035*** (.004)	.028*** (.004)			.010*** (.004)	.003 (.003)
T17 * age 30-39			.019*** (.006)	.014*** (.005)			.013** (.005)	-.003 (.005)
T18 * age 19			.047*** (.004)	.039*** (.004)			.080*** (.005)	.065*** (.004)
T18 * age 20-29			.014*** (.004)	.014*** (.003)			-.001 (.004)	-.003 (.003)
T18 * age 30-39			.010* (.005)	.009* (.005)			.002 (.005)	-.005 (.004)
Const.	1.645*** (.0006)	1.214*** (.001)	1.244*** (.001)	.815*** (.002)	1.369*** (.0007)	.925*** (.001)	.952*** (.002)	.667*** (.002)
Controls for:								
year	no	yes	yes	yes	no	yes	yes	yes
education	no	yes	yes	yes	no	yes	yes	yes
industry	no	no	no	yes	no	no	no	yes
region	no	no	no	yes	no	no	no	yes
foreign co.	no	no	no	yes	no	no	no	yes
public co.	no	no	no	yes	no	no	no	yes
Obs.	5423097	5423097	5423097	5423082	3488358	3488358	3488358	3488347
R^2	0.025	0.414	0.419	0.542	0.026	0.48	0.482	0.606

Sources: Computations based on QP. Notes: Treatment 16, 17, or 18 refers to each of the three cohorts in the treatment group. Data are pooled for the three treatment groups and the control group at ages 19 to 39, and for all other birth cohorts at ages 40 to 49. Regressions in columns 1-2 and 5-6 include the main effect age_{19-39} ; regressions in columns 3-4 and 7-8 include the main effects for ages 19, 20-29 and 30-39. The control variables are: year (21 dummies), education (4 dummies), industry (24 dummies), region (2 dummies), foreign company (1 dummy), and public company (1 dummy). See equations 1 and 2 and section V for details on the specification. Standard errors clustered by person in parentheses.

VI Do high youth minimum wages undermine the long-run chances of employment? The intensive and extensive margins of adjustment

Employers could handle hours of work as a another margin to respond to the change

in minimum wages (Stewart and Swaffield, 2008; Gramlich, 1976; Hyslop and Stillman, 2007). Faced with an increase in the *monthly* minimum wage, which is in fact common across continental European countries, they could either increase the number of hours worked by full-timers, aiming at keeping the hourly labor cost unchanged, or reduce it, namely transferring workers into part-time, aiming at keeping the total wage bill unchanged, as the minimum wage applies pro-rata to part-timers.

This margin of adjustment has deserved little attention in the previous literature, when compared to the voluminous literature on employment adjustments. The current findings can therefore provide a contribution to overcome a relatively recent criticism: "Two problems with the empirical literature on the minimum wage [...] are that (1) it typically focuses on specific industries [...] and (2) it neglects potential effects of the minimum wage on hours rather than on persons employed." (Boeri and van Ours, 2013: 50)

The analysis to be undertaken will first shed light on hours of work, namely: the probability of working part-time; the number of weekly hours of work, conditional on working full-time or part-time. We then move to the extensive margin, to analyze the probability of being observed employed.

The treatment group worked longer hours in its early career than the control group (see Table 1 on the descriptive statistics). We also find that young women worked longer hours than men the same age. By ages 19-39, the weekly hours of work were lower than at the initial moment for all groups, reflecting the reduction over time in the maximum duration of work. Across treatment and control groups at ages 19-39, differences in average hours of work tend to be very small. Full-time work is overwhelming, both for males and females. Curiously, at young ages the share of part-time workers is lower for females than males, whereas at ages 19-39 it is only approximately 1 percentage point larger. Again, these patterns in the raw data must be evaluated accounting for the potentially confounding nature of time and schooling effects, given that the maximum duration of work was declining over the period under analysis and different educational levels tend to work different weekly hours (in particular, reflecting the blue- or white-collar nature of the job).

The regression setting parallels the analysis of wage profiles in the previous section, in terms of the definition of control and treatment groups and model specification. Differently from the previous section, though, the probability of working part-time is modeled as a probit instead of a linear regression model. The results are reported in Tables 3 to 5, for the probability of working part-time, and the number of hours worked by full- and part-timers, respectively. All this first set of regressions are, of course, conditional on observing the individual employed.

Concerning part-time work, columns 2 and 6 in Table 3 show that the probability of working part-time over the life cycle (ages 19 to 39) is not significantly different for workers in the treatment and control groups.¹¹ As for the evolution over time, at age 19, the male treatment groups 16 and 18 were 2 p.p. less likely to work part-time than the corresponding control group; the same holds for the female treatment 18 group; for the other groups, results are not significant at the 1% significance level. From ages 20 to 39, we detect a total convergence in the probability of working part-time across treated and control groups.

Table 4 analyzes hours of work by full-timers. Columns 1 and 6, again with no controls, simply reflect the consecutive declines in the weekly duration of work imposed by the law, which affected to a larger extent the younger cohorts. Columns 2 and 6 reveal that the difference in hours of work between treatment and control group is either non-significant or economically negligible (3 to 11 minutes a week less for the treated than the control group). Interestingly, columns 3 and 7 reveal that at age 19 all the treatment groups who were working full-time worked longer hours than the respective control group, a gap that is larger for females than males (ranging from a low average 9 minutes, for males in the oldest treatment group, to over half an hour a week for females in the youngest treatment group). At later ages, there is either no difference in the hours of work by full-timers in the treatment and control groups, or the treated work slightly less. These results barely change once controls for industry and occupation are included in the analysis. The key insight from this analysis is that the longer hours worked by the treatment group may have contributed to accommodate in the short run the increase in the monthly minimum

¹¹At the 1% significance level.

Table 3: PROBABILITY OF WORKING PART-TIME, TREATMENT GROUP (16, 17 OR 18 YEAR OLDS) VERSUS CONTROL GROUP (19 YEAR OLDS), PROBIT MODEL MARGINAL EFFECTS.

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment 16	-.003 (.003)	-.002 (.003)			.001 (.004)	.010** (.004)		
Treatment 17	-.007*** (.002)	-.003 (.002)			-.009*** (.003)	-.001 (.004)		
Treatment 18	-.001 (.002)	-.0006 (.002)			-.003 (.003)	.0005 (.003)		
T16 * age 19			-.016*** (.005)	-.007 (.005)			-.024** (.010)	-.001 (.010)
T16 * age 20-29			-.001 (.003)	.004 (.003)			.013** (.005)	.020*** (.005)
T16 * age 30-39			.005 (.004)	.003 (.003)			.008 (.006)	.008 (.005)
T17 * age 19			.004 (.005)	.007 (.006)			-.01 (.009)	.007 (.009)
T17 * age 20-29			-.001 (.003)	.002 (.002)			-.0003 (.004)	.007* (.004)
T17 * age 30-39			-.001 (.003)	-.003 (.003)			-.005 (.005)	-.002 (.004)
T18 * age 19			-.019*** (.003)	-.014*** (.003)			-.020*** (.007)	-.009 (.007)
T18 * age 20-29			.003 (.003)	.005** (.003)			.0008 (.004)	.005 (.004)
T18 * age 30-39			.005 (.003)	.004 (.003)			.0005 (.005)	.001 (.004)
Controls for:								
year	no	yes	yes	yes	no	yes	yes	yes
education	no	yes	yes	yes	no	yes	yes	yes
industry	no	no	no	yes	no	no	no	yes
region	no	no	no	yes	no	no	no	yes
foreign co.	no	no	no	yes	no	no	no	yes
public co.	no	no	no	yes	no	no	no	yes
Obs.	5423097	5423097	5423097	5423082	3488358	3488358	3488358	3488347
Log Likelihood	-1748190	-1699924.8	-1699567	-1482586	-1509002	-1485985	-1485779	-1314784

Sources: Computations based on QP. Notes: Treatment 16, 17, or 18 refers to each of the three cohorts in the treatment group. Data are pooled for the three treatment groups and the control group at ages 19 to 39, and for all other birth cohorts at ages 40 to 49. Regressions in columns 1-2 and 5-6 include the main effect age_{19-39} ; regressions in columns 3-4 and 7-8 include the main effects for ages 19, 20-29 and 30-39. The control variables are: year (21 dummies), education (4 dummies), industry (24 dummies), region (2 dummies), foreign company (1 dummy), and public company (1 dummy). See section VI for details. Marginal effects computed as discrete change in the dummy variable from zero to one, at the mean of the other variables. Standard errors clustered by person in parentheses.

wage. Note that the minimum wage is a monthly rate for full-time work and, as a result, earnings remain unchanged once the normal duration of work is increased.

For part-timers, one magnitude clearly stands out in Table 5 —part-time workers

Table 4: REGRESSION OF WEEKLY HOURS OF WORK BY FULL-TIMERS, TREATMENT GROUP (16, 17 OR 18 YEAR OLDS) VERSUS CONTROL GROUP (19 YEAR OLDS).

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment 16	-.643*** (.021)	-.151*** (.020)			-.727*** (.024)	-.190*** (.021)		
Treatment 17	-.302*** (.018)	-.043** (.017)			-.411*** (.022)	-.121*** (.019)		
Treatment 18	-.167*** (.018)	-.010 (.016)			-.216*** (.022)	-.058*** (.019)		
T16 * age 19			.438*** (.055)	.443*** (.054)			.627*** (.054)	.612*** (.053)
T16 * age 20-29			-.204*** (.031)	-.197*** (.029)			-.344*** (.031)	-.328*** (.029)
T16 * age 30-39			-.141*** (.020)	-.114*** (.020)			-.106*** (.025)	-.076*** (.024)
T17 * age 19			.194*** (.049)	.184*** (.049)			.362*** (.052)	.350*** (.051)
T17 * age 20-29			.003 (.026)	-.001 (.025)			-.148*** (.028)	-.151*** (.027)
T17 * age 30-39			-.088*** (.017)	-.064*** (.017)			-.119*** (.022)	-.099*** (.022)
T18 * age 19			.141*** (.039)	.167*** (.038)			.267*** (.044)	.331*** (.043)
T18 * age 20-29			.059** (.025)	.043* (.024)			-.03 (.027)	-.039 (.026)
T18 * age 30-39			-.068*** (.016)	-.061*** (.015)			-.086*** (.021)	-.072*** (.020)
Const.	40.325*** (.002)	42.258*** (.007)	42.231*** (.008)	42.703*** (.013)	39.931*** (.003)	42.108*** (.012)	42.003*** (.013)	42.293*** (.018)
Controls for:								
year	no	yes	yes	yes	no	yes	yes	yes
education	no	yes	yes	yes	no	yes	yes	yes
industry	no	no	no	yes	no	no	no	yes
region	no	no	no	yes	no	no	no	yes
foreign co.	no	no	no	yes	no	no	no	yes
public co.	no	no	no	yes	no	no	no	yes
Obs.	4886917	4886917	4886917	4886903	2941582	2941582	2941582	2941574
R ²	0.007	0.292	0.294	0.377	0.026	0.297	0.302	0.388

Sources: Computations based on QP. Notes: Treatment 16, 17, or 18 refers to each of the three cohorts in the treatment group. Data are pooled for the three treatment groups and the control group at ages 19 to 39, and for all other birth cohorts at ages 40 to 49. Regressions in columns 1-2 and 5-6 include the main effect age_{19-39} ; regressions in columns 3-4 and 7-8 include the main effects for ages 19, 20-29 and 30-39. The control variables are: year (21 dummies), education (4 dummies), industry (24 dummies), region (2 dummies), foreign company (1 dummy), and public company (1 dummy). See equations 1 and 2 and section VI for details on the specification. Standard errors clustered by person in parentheses.

exposed to the sharpest increase in minimum wages (those aged 17 in 1986) were a year later supplying between 1.4 and 2.1 more hours of work than the control group (see the coefficients in columns 3 and 7). The fact that the sharpest wage increase led to the sharpest increase in hours of work by part-timers, with the associated increase in total

pay, is strong indication of the operation of a supply-side mechanism. Note that the minimum wage of part-timers is set pro-rata the monthly full-time minimum wage, such that an increase in hours worked by part-timers translates directly into an increase in earnings. This adjustment of hours of work corroborates the hypothesis by Portugal and Cardoso (2006) according to which a sharp increase in the minimum wage increased worker job attachment. It is also consistent with the analysis of transitions between labor market states in Canada by Brochu and Green (2013), who found lower rate of job separations in high minimum wage regimes, driven by a reduction in quit rates and an even larger reduction in layoff rates. Their theoretical model accounts for this pattern, as firms expect lower benefit from terminating a job match in a high minimum wage regime, namely if they are likely to replace the dismissed worker with another one on minimum wage or if (low) inflation is not expected to erode the real minimum wage.

The current insights on hours of work thus suggest, on one hand, rising labor force attachment by workers exposed to the sharpest increase in the applicable minimum wage, as those working part-time increased their labor supply a year after the increase in the minimum wage. On the other hand, the fact that full-timers in the treated group worked longer hours than workers in the control group may have helped firms manage the rising minimum wage, in a period of fast economic growth.

We now check whether workers exposed to high youth minimum wages were less likely to be observed employed later in the life cycle. By design the treated groups are observed working at ages 16, 17 or 18, whereas the control group is observed working at age 19. Therefore, the comparison of the long-term employment outcome is restricted to the age bracket 20 to 39. Workers not observed in the dataset may be in other labor market states, such as unemployment or out of the labor force, or they may have joined an uncovered sector —civil servants comprised almost 20% of the Portuguese workforce and contract workers accounted for a growing share of employment in the economy (Rebelo, 2003). Also in the case of the outcome probability of employment, regression analysis must account for macroeconomic trends and business cycle effects that may have impacted differently the groups at each age. The analysis once again parallels that in the previous tables, in terms of sample under analysis and model specification. Table 6 reports the results of a

Table 5: REGRESSION OF WEEKLY HOURS OF WORK BY PART-TIMERS, TREATMENT GROUP (16, 17 OR 18 YEAR OLDS) VERSUS CONTROL GROUP (19 YEAR OLDS).

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment 16	-.637*** (.208)	-.268 (.210)			-.427* (.248)	-.396 (.247)		
Treatment 17	-.414** (.174)	-.006 (.171)			.306 (.229)	.444** (.226)		
Treatment 18	-.287* (.161)	-.081 (.154)			.199 (.209)	.204 (.202)		
T16 * age 19			.130 (.653)	-.161 (.651)			.900 (.901)	.031 (.886)
T16 * age 20-29			-.797*** (.296)	-.554* (.284)			-.371 (.332)	-.607* (.323)
T16 * age 30-39			-.0009 (.306)	-.183 (.289)			-.474 (.361)	-.882** (.355)
T17 * age 19			2.105*** (.432)	1.311*** (.433)			1.427** (.703)	.328 (.694)
T17 * age 20-29			-.411* (.237)	-.242 (.228)			.455 (.303)	.202 (.296)
T17 * age 30-39			-.082 (.258)	-.156 (.246)			.369 (.326)	.165 (.306)
T18 * age 19			.002 (.357)	.101 (.349)			.577 (.549)	.254 (.533)
T18 * age 20-29			-.081 (.206)	-.083 (.199)			.411 (.269)	.269 (.262)
T18 * age 30-39			-.215 (.239)	-.228 (.226)			-.013 (.300)	-.029 (.282)
Const.	26.374*** (.019)	24.323*** (.070)	24.327*** (.073)	20.387*** (.127)	23.259*** (.022)	20.073*** (.084)	20.026*** (.086)	16.865*** (.115)
Controls for:								
year	no	yes	yes	yes	no	yes	yes	yes
education	no	yes	yes	yes	no	yes	yes	yes
industry	no	no	no	yes	no	no	no	yes
region	no	no	no	yes	no	no	no	yes
foreign co.	no	no	no	yes	no	no	no	yes
public co.	no	no	no	yes	no	no	no	yes
Obs.	536180	536180	536180	536179	546776	546776	546776	546773
R ²	0.002	0.126	0.127	0.265	0.00003	0.137	0.137	.235

Sources: Computations based on QP. Notes: Treatment 16, 17, or 18 refers to each of the three cohorts in the treatment group. Data are pooled for the three treatment groups and the control group at ages 19 to 39, and for all other birth cohorts at ages 40 to 49. Regressions in columns 1-2 and 5-6 include the main effect age_{19-39} ; regressions in columns 3-4 and 7-8 include the main effects for ages 19, 20-29 and 30-39. The control variables are: year (21 dummies), education (4 dummies), industry (24 dummies), region (2 dummies), foreign company (1 dummy), and public company (1 dummy). See equations 1 and 2 and section VI for details on the specification. Standard errors clustered by person in parentheses.

probit model.

The comparison of the raw data on the likelihood of the treatment and control groups being employed over the lifecycle points to an advantage of the control group (columns 1 and 5). However, once we account for business cycle effects or time trends and the

different level of education of the cohorts upon entry into the labor market, before the change in the legislation, we find that exposure to high youth minimum wages did not have an impact on the lifetime chances of finding a job (estimates in columns 2 and 6). Once we look at the profile over time (columns 3 and 7), there is clearly no impact on the chances of employment in the medium or long run for females; likewise, for males at ages 20-29 there is no impact. Over the longer time horizon, at ages 30 to 39, for males only, actual exposure in the labor market to high teenage minimum wages is associated with slightly lower chances of finding a job, though never significant at the 1% level. A potential explanation for this weak result could be lower investment on on-the-job training early in the career of these workers, which would be worth testing directly if training data were available. An alternative explanation is the low level of human capital these workers possess, as they entered the labor market at very young ages. The analysis on wider treatment cohorts, dropping the condition on employment at very young ages, may help shed further light on this issue.

Table 6 does not cover the probability of employment in the short-run after the change in the minimum wage, as it concentrates on the longer run, from ages 20 to 39. This complements the detailed analysis by Portugal and Cardoso (2006) of the short-run impact of the minimum wage on employment, which exploited the same institutional change and data source. A comparison of employment for the treated and control cohorts over 1987-1989 systematically reveals that the groups exposed to high youth minimum wages were *more* likely to be found employed than the corresponding control group, which is naturally consistent with the analysis in Portugal and Cardoso (2006).

In synthesis, the analysis of the likelihood of holding a job over the age range 20-39 following actual exposure to high youth minimum wages points to a main result: exposure to high youth minimum wages did not have an impact on the probability of holding a job later on. These findings are in line with the new economics of the minimum wage, which have relied on fine-tuned empirical strategies and found no employment impact of minimum wages on the medium-run, either in the US (Dube et al., 2007, 2010) or the UK (Stewart, 2004, 2004a). These result are also, as anticipated, remarkably consistent with Portugal and Cardoso (2006). Their analysis of firm worker flows in the short-run

Table 6: PROBABILITY OF BEING OBSERVED AS WAGE-EARNER, TREATMENT GROUP (16, 17 OR 18 YEAR OLDS) VERSUS CONTROL GROUP (19 YEAR OLDS), PROBIT MODEL MARGINAL EFFECTS.

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment 16	-.026*** (.005)	-.008 (.005)			-.013** (.006)	.006 (.006)		
Treatment 17	-.018*** (.004)	-.004 (.003)			-.015*** (.005)	.0009 (.005)		
Treatment 18	-.016*** (.004)	-.007* (.004)			-.009* (.005)	.00009 (.005)		
T16 * age 20-29			-.005 (.005)	-.014*** (.005)			.009 (.006)	.0009 (.006)
T16 * age 30-39			-.011* (.006)	-.025*** (.006)			.001 (.007)	-.010 (.007)
T17 * age 20-29			.004 (.004)	-.001 (.004)			.007 (.006)	.003 (.006)
T17 * age 30-39			-.013** (.005)	-.022*** (.005)			-.006 (.006)	-.015** (.006)
T18 * age 20-29			-.004 (.004)	-.007* (.004)			-.001 (.005)	-.004 (.005)
T18 * age 30-39			-.010** (.005)	-.014*** (.004)			.0007 (.006)	-.003 (.006)
Controls for:								
year	no	yes	yes	yes	no	yes	yes	yes
education	no	yes	yes	yes	no	yes	yes	yes
industry	no	no	no	yes	no	no	no	yes
region	no	no	no	yes	no	no	no	yes
foreign co.	no	no	no	yes	no	no	no	yes
public co.	no	no	no	yes	no	no	no	yes
Obs.	9137832	9137832	9137832	9137541	5861933	5861933	5861933	5861738
Log Likelihood	-6047128	-5967258	-5967189	-5812499	-3860025	-3814145	-3812874	-3770050

Sources: Computations based on QP. Notes: Treatment 16, 17, or 18 refers to each of the three cohorts in the treatment group. Data are pooled for the three treatment groups and the control group at ages 20 to 39, and for all other birth cohorts at ages 40 to 49. Regressions in columns 1-2 and 5-6 include the main effect age_{20-39} ; regressions in columns 3-4 and 7-8 include the main effects for ages 20-29 and 30-39. The control variables are: year (21 dummies), education (4 dummies), initial industry (24 dummies), initial region (2 dummies), whether the initial firm was foreign (1 dummy) or public (1 dummy). See section VI for details on the specification. Marginal effects computed as discrete change in the dummy variable from zero to one, at the mean of the other variables. Standard errors clustered by person in parentheses.

concluded that the increase in youth minimum wage led to a reduction in job separations, which compensated for the reduction in job accessions.

VII Results on the second treatment and control groups

We now aim at tackling a potential bias resulting from the selection of individuals working at early ages, 16 to 18, into the treatment group. Analysis of the full cohort, irrespective of whether the individuals were already working in 1986, would thus have an interest of

its own. However, there is a trade-off to consider. On one hand, the cohorts working in 1986 consist of a potentially selected sample, but guarantee that decisions on the level of education are not endogenous, as these individuals were observed as wage earners already before the law on youth minimum wages changed. On the other hand, the full cohort overcomes the potential selection problem, at the expense of creating a sample where education is likely endogenous. Therefore, we perform an analysis on the cohort of individuals aged 16 to 18 in 1986 who were observed working at age 19 (in 1989, 1988, or 1987, respectively). We thus relax the constraint that individuals must be observed as wage-earners at very young ages, before the minimum wage increased. Table 7 reports the descriptive statistics. As the selection condition was eliminated, the sample sizes are now very similar across groups, with each treatment group comprising approximately 25 thousand individuals (males and females together), just like the treatment group. We also observe that the schooling level of the treatment group is now higher than in the initial treatment groups, as we include individuals who left school to enter the labor market later.

Table 7: DESCRIPTIVE STATISTICS, FULL COHORTS WORKING AT AGE 19.

Variable (at ages 19-39)	Males				Females			
	T16	T17	T18	C19	T16	T17	T18	C19
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Log) hrly wage	1.307 (0.443)	1.274 (0.446)	1.255 (0.457)	1.227 (0.469)	1.079 (0.361)	1.038 (0.354)	1.017 (0.348)	0.989 (0.359)
Weekly hours	39.511 (5.530)	39.845 (5.626)	39.911 (5.749)	40.073 (5.671)	39.360 (6.126)	39.823 (6.111)	39.976 (6.238)	40.114 (6.451)
Part-time (share)	0.080	0.077	0.080	0.077	0.090	0.083	0.084	0.086
Education (share)								
9 years	0.121	0.103	0.100	0.094	0.116	0.089	0.082	0.075
high school	0.062	0.049	0.045	0.044	0.076	0.061	0.054	0.051
university	0.003	0.002	0.002	0.002	0.003	0.002	0.001	0.001
Employed (share)	0.494	0.486	0.477	0.471	0.518	0.512	0.513	0.505
N obs at age 19	16,297	14,809	14,717	15,031	11,883	10,240	9,466	8,919
N obs ages 19-39	150,946	135,322	131,199	132,074	113,357	96,959	89,421	83,146

Source: Computations based on QP (1986-2009). Notes: Employed means observed working as wage-earner in the private sector of the economy (see text for details). The descriptives refer to wage-earners with non-missing wage. Standard-deviations in parenthesis.

Table 8 reports the regression results. It confirms an overall wage premium for the treated groups (columns 1 and 3). Controlling for time, worker and firm attributes also confirms the short-run wage gain for most treated groups. However, that premium fades

away over time faster than in the previous analysis. Thus the positive impact of the minimum wage on wages is shorter-lasting for the overall cohort potentially affected than for those individuals actually affected because they held a job already before the increase in the minimum wage. The mechanism could be the following. The improvement in education and possibly other worker attributes for the wider cohort was not matched by a proportional increase in wages over the life cycle.

The next result worth discussing concerns the probability of holding a job in the private sector of the economy. Columns 11 and 12 show that for every treated group the likelihood of holding a job at ages 20-29 is larger than for the control group; at older ages, it is either larger as well for the treated group or the gap between treatment and control is non-significant. The timing of the impact on employment for the wider cohorts is thus smoother, pointing unambiguously to a short- and medium-run positive impact of potential exposure to high youth minimum wages. This more positive impact could reflect better (unobservable) human capital in the treated cohorts at large than in their counterparts observed working at very young ages, as hypothesized in section VI.

The evidence on hours of work is in general in line with the analysis on the treatment and control groups working before the change in the law, even though the magnitudes are smaller. In particular, the following key results are confirmed: hours worked at age 19 by full-timers in the treatment group tend to be larger than in the control group; part-timers exposed to the most abrupt increase in youth minimum wages present the sharpest positive impact on hours worked, interpreted as an indication of increased labor market attachment. In any case, the push for part-timers to work longer hours was stronger for those who already held a job before the change in the minimum wage than for their cohort counterparts at large.

Table 8: REGRESSIONS FOR FULL COHORTS WORKING AT AGE 19, TREATMENT GROUP (16, 17 OR 18 YEAR OLD IN 1986) VERSUS CONTROL GROUP (19 YEAR OLD IN 1986).

	(Log) hourly wages				Prob. part-time (*)		Hours full-time		Hours part-time		Prob. employed (*)	
	Males		Females		Males	Females	Males	Females	Males	Females	Males	Females
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment 16	.080*** (.004)		.090*** (.004)									
Treatment 17	.046*** (.004)		.049*** (.004)									
Treatment 18	.027*** (.004)		.028*** (.004)									
T16 * age 19		.027*** (.004)		-.0007 (.004)	.010*** (.003)	.033*** (.007)	.300*** (.034)	.142*** (.038)	.452 (.306)	-.026 (.426)		
T16 * age 20-29		-.0006 (.003)		-.019*** (.003)	.004** (.002)	.013*** (.004)	-.170*** (.021)	-.439*** (.023)	-.202 (.186)	-.132 (.233)	.042*** (.004)	.033*** (.004)
T16 * age 30-39		.001 (.004)		-.008** (.004)	.003 (.002)	-.002 (.004)	-.024* (.014)	.023 (.018)	.074 (.200)	-.132 (.252)	.022*** (.004)	.018*** (.005)
T17 * age 19		.032*** (.004)		.021*** (.004)	.014*** (.003)	.030*** (.007)	.081** (.037)	.155*** (.041)	.875*** (.310)	.217 (.446)		
T17 * age 20-29		.003 (.003)		-.019*** (.003)	.003 (.002)	.007** (.004)	-.047** (.022)	-.224*** (.024)	.084 (.188)	.109 (.245)	.038*** (.004)	.024*** (.004)
T17 * age 30-39		.002 (.004)		-.011*** (.004)	.001 (.002)	-.003 (.004)	-.027* (.015)	-.047** (.019)	-.092 (.204)	.166 (.258)	.009* (.004)	.006 (.005)
T18 * age 19		.024*** (.004)		.024*** (.004)	-.006* (.003)	.0002 (.006)	.084** (.034)	.157*** (.039)	-.206 (.292)	.014 (.443)		
T18 * age 20-29		.004 (.003)		-.008*** (.003)	.007*** (.002)	.006* (.004)	.029 (.023)	-.104*** (.025)	-.065 (.188)	.272 (.247)	.021*** (.004)	.018*** (.004)
T18 * age 30-39		.005 (.004)		-.004 (.004)	.002 (.002)	-.003 (.004)	-.048*** (.015)	-.030 (.019)	-.252 (.212)	.148 (.266)	.001 (.004)	.008 (.005)
Const.	1.645*** (.0006)	.819*** (.002)	1.369*** (.0007)	.668*** (.002)			42.699*** (.013)	42.287*** (.018)	20.424*** (.125)	16.880*** (.114)		
Controls for:												
year	no	yes	no	yes	yes	yes	yes	yes	yes	yes	yes	yes
education	no	yes	no	yes	yes	yes	yes	yes	yes	yes	yes	yes
industry	no	yes	no	yes	yes	yes	yes	yes	yes	yes	yes	yes
region	no	yes	no	yes	yes	yes	yes	yes	yes	yes	yes	yes
foreign co.	no	yes	no	yes	yes	yes	yes	yes	yes	yes	yes	yes
public co.	no	yes	no	yes	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	5620603	5620588	3640040	3640028	5620588	3640028	5067814	3079778	552774	560250	9465739	6118017
R ²	0.035	0.546	0.034	0.607			0.379	0.396	0.26	0.232		
LL					-1536712	-1359029					-6040639	-3944621

Sources: Computations based on QP. Notes: (*) For probability of working part-time and probability of being employed in the private sector, columns 5-6 and 11-12 report the marginal impacts of a probit model. Treatment 16, 17, or 18 refers to each of the three cohorts in the treatment group. Data are pooled for the three treatment groups and the control group at ages 19 to 39, and for all other birth cohorts at ages 40 to 49, except for the model on the probability of employment, which pools data for the treatment and control groups at ages 20-39 and on all other birth cohorts at ages 40-49. Regressions in columns 1 and 3 include the main effect *age*₁₉₋₃₉; columns 2, 4, and 5-10 include the main effects for ages 19, 20-29 and 30-39; in columns 11-12, the main effects for ages 20-29 and 30-39. The other control variables, except in columns 1 and 3, are: year (21 dummies), education (4 dummies), industry (24 dummies), region (2 dummies), foreign company (1 dummy), and public company (1 dummy); in the model on employment, the firm attributes refer to the initial firm. See the text for details. Standard errors clustered by person in parentheses.

VIII Schooling achievement of the full cohorts

A complete analysis of the impact of the minimum wage on schooling decisions is beyond the reach of this paper, given data limitations. In fact, lack of information on individual unemployment or other indicators of job search precludes a full analysis of potential changes in labor supply. Also, given that the available data starts in 1986, it is not feasible to quantify the share of each treatment and control cohort that was working at each age from 16 to 19. We can nevertheless provide exploratory evidence on the schooling achievement of the affected cohorts.

Figure 4 reports the proportion of each cohort born 1959 to 1979 that completed high school or college, separately by gender. It reveals a steady increase in schooling attainment over time in Portugal. This trend is approximately linear for high school completion. On the contrary, the rate of growth of university completion rates for males accelerated for the cohorts born 1967 to 1970. Those were precisely the cohorts first exposed to high minimum wages from ages 16 to 18. For the younger cohorts the trend reverts back to a slower pace. For females, the rate of growth in the university graduation rate also increased starting with cohort 1967 and afterwards that pace did not slow down as much as for males.

Regression analysis further explored the significance of these patterns for college attainment, relying on a linear spline model. Consider the following knots in birth cohort: k_{1967} and k_{1971} . Before 1967, individuals were never eligible to high youth minimum wages. The interval from 1967 to 1971 includes the first cohorts ever exposed. Such exposure took place gradually, from age 18 only for those born 1967, to ages 16 to 18 for those born in 1969 or 1970; after birth year 1971, individuals started being eligible to high youth minimum wages at ages 14 and 15 as well. For each knot k_y the dummy variable D_y achieves value 1 if the birth year ($byear$) is larger than or equal to y and zero otherwise. The following equation will be estimated separately for each gender:

$$ed_{iat} = c_0 + b_0 byear_i + \sum_{y=1967,1971} b_y \cdot D_y \cdot (byear_i - k_y) + d_t + \mathbf{Z}_i' \boldsymbol{\psi} + \epsilon_i \quad , \quad (3)$$

where ed_{iat} stands for college degree (equal to one if completed and zero otherwise) for

Figure 4: EDUCATIONAL ATTAINMENT.

Source: Computations based on QP. Note: Considers the first observation for each individual observed between ages 30 and 50.

individual i , observed at age a and year t ; d_t refers to the year of observation, capturing potential changes in the way the variable higher education is reported over time. $\mathbf{Z}$ includes the control variables birth quarter (three dummies), a cubic on age when the worker is observed, and a set of region dummies. This specification can be seen as an adaptation of Card and Lemieux (2001).

Columns 1 and 2 in Table 9 report the results of the linear spline model, confirming that the growth in college graduation rates accelerated considerably for the first cohorts eligible to high youth minimum wages at ages 16 to 18. Indeed, for the oldest cohorts of males, born 1959 to 1967, the share completing higher education was increasing by 0.3 percentage points per cohort; remarkably, for the cohorts born 1967 to 1970, it was increasing by 1.4 p.p. per cohort. This faster rate of growth in college attainment by males was not sustained, as it slowed down to 0.7 p.p. per cohort for the younger cohorts, born 1971 onward.¹² For females, regression analysis uncovers an equally sharp and more sustained acceleration in college completion rates for the cohorts first exposed to high youth minimum wages: 0.4 p.p., 1.6 p.p., and 1.4 p.p. per cohort, for the oldest,

¹²A formal test rejects the hypothesis of equality in the pace of growth of college completion rates for cohorts born before 1967 and after 1971.

intermediate and youngest group, respectively.

Table 9: PROBABILITY OF COLLEGE COMPLETION, COHORTS BORN 1959 TO 1979.

	linear probability model		probit marginal impact	
	Males	Females	Males	Females
	(1)	(2)	(3)	(4)
main birth year effect	.003** (.001)	.004*** (.001)	.003** (.001)	.006*** (.002)
change at cohort 1967	.010*** (.0004)	.012*** (.001)	.008*** (.0003)	.007*** (.0008)
change at cohort 1971	-.006*** (.0006)	-.002* (.001)	-.007*** (.0004)	-.005*** (.0009)
Obs.	7807720	6266735	7807720	6266735
R^2	0.043	0.076		
Log Likelihood			-2463764.4	-2248813.1

Sources: Computations based on QP. Notes: All regressions include: a full set of dummies for year of observation; birth quarter (three dummies); a cubic on age at the moment the individual was observed; and a set of region dummies. See equation 3 and the text in section VIII for details. Standard errors clustered by birth cohort in parentheses.

Overall, this exploratory evidence on intercohort trends in educational attainment suggests that high minimum wages may have had a role speeding up the pace of increase in university graduation rates that was taking place in the Portuguese economy. Different theoretical models have addressed the potential mechanisms driving this result and some of them could apply in the current context. Indeed, the threshold of skills required to access jobs may be higher under a high minimum wage regime and individuals may increase their investment in schooling, so as to raise their productivity and compete for jobs (Cahuc and Michel, 1996) or to compensate for the decline in wages due to a reduction of on-the-job training (Ravn and Sorensen, 1999).¹³ The current result reinforces the interest in a comparison of treatment and control groups observed working before the change in the law, whose decision to leave school had not been influenced by the minimum wage.

IX Conclusion

Relying on over two decades of longitudinal linked employer-employer data of remark-

¹³In settings different from the one under analysis, they could increase schooling to avoid employment in a secondary sector where no minimum wage is enforced (Agell and Lommerud, 1997) or as a result of the decline in the opportunity cost of schooling, when higher minimum wages lead to lower employment opportunities (Mincer, 1976).

able quality, the current analysis of the impact of the minimum wage shifted away from the traditional emphasis on the short-run, into the long-run and a wide set of outcomes: wages and employment, both at the extensive and intensive margins.

The findings highlight three main points. First of all, the distributional impact of the minimum wage was concentrated in the short to medium run, as the premium to exposure to high youth minimum wages faded over time. Secondly, adjustment of hours of work points to increased worker job attachment. Indeed, the most striking adjustment at the intensive margin concerns the increase, one year after the law change, in hours worked by part-timers exposed to the sharpest rise in the minimum wage. Given that the minimum wage level is defined as monthly pay for full-time work and it applies pro-rata to part-timers, any increase in hours of work by part-timers translates directly into an increase in earnings. For full-timers, instead, earnings remain unchanged if the normal duration of work increases. As a result, the slightly longer duration of work by full-timers in the treatment group than in the control one may have helped firms accommodate the increase in the minimum wage, under the good economic conditions that prevailed at the time. Thirdly, the evidence on employment indicates that high youth minimum wages did not jeopardize employment prospects: for females already employed before the increase in the minimum wage, no difference can be detected between the treatment and control groups in their chances of employment over the medium and long run; for males, such chances are slightly lower for the treated at ages 30-39; instead, for either gender, dropping the condition on working at very young ages (before the law change) to focus on cohorts at large, points to better chances of employment over the life cycle for the cohorts exposed to high youth minimum wages.

Statements on the external validity of this exercise must consider that the minimum wage underwent a sharp increase, but applied to a very specific group of workers, in an economy undergoing at the time a remarkable boom.

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Supplementary Material

The long-run impact of minimum wages on workers' careers: evidence from two decades of longitudinal linked employer-employee data

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Robustness checks

The first robustness check considers overtime work, which could bring a difference to the wage and hours profiles analyzed. Given that overtime work is paid at a premium indexed to the wage of regular hours of work, workers whose wages increased could be granted less frequent access to overtime work. Accordingly, their wages would change. Table 1 reports the initial and final model specifications from the analysis in sections V and VI. It shows that the results on wages remain virtually unchanged. On hours of work, results for females are very similar in the initial analysis and including overtime hours. We would indeed expect results for females to be similar, given that they traditionally do less paid overtime work. For males, the main change is the non-significance, at age 19, of the hours gap between those treated at age 17 or 18 and the control group. Whereas these two control groups worked, at age 19, on average 10 to 11 minutes more than the control group, once we include overtime hours that small difference vanishes. Therefore, we infer that, at age 19, the male treated worked full time slightly longer hours than the control group, but had slightly less access to overtime work.

A second robustness check consisted on pooling data on cohorts outside the treatment and control birth cohorts at ages other than the bracket 40-49 considered in sections V and VI. Table 2 reports the results for the final specification reported in the main text, which undergo very little change.

Table 1: REGRESSIONS INCLUDING OVERTIME HOURS AND PAY, TREATMENT GROUP (16, 17 OR 18 YEAR OLD IN 1986) VERSUS CONTROL GROUP (19 YEAR OLD IN 1986).

	(Log) hourly wages				Weekly hours work by full-timers			
	Males		Females		Males		Females	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment 16	.084*** (.005)		.059*** (.005)		-.634*** (.031)		-.692*** (.026)	
Treatment 17	.054*** (.005)		.028*** (.004)		-.265*** (.028)		-.387*** (.024)	
Treatment 18	.040*** (.004)		.020*** (.004)		-.163*** (.025)		-.199*** (.023)	
T16 * age 19		.070*** (.006)		.075*** (.005)		.269*** (.058)		.547*** (.055)
T16 * age 20-29		.031*** (.005)		.010** (.004)		-.207*** (.036)		-.356*** (.031)
T16 * age 30-39		.017*** (.006)		-.002 (.005)		-.133*** (.042)		-.059* (.032)
T17 * age 19		.063*** (.005)		.088*** (.005)		-.014 (.053)		.299*** (.054)
T17 * age 20-29		.028*** (.004)		.003 (.003)		.005 (.031)		-.174*** (.028)
T17 * age 30-39		.014*** (.005)		-.003 (.005)		-.045 (.040)		-.089*** (.029)
T18 * age 19		.036*** (.004)		.064*** (.004)		-.061 (.042)		.336*** (.047)
T18 * age 20-29		.014*** (.004)		-.003 (.003)		.049* (.029)		-.045* (.027)
T18 * age 30-39		.009* (.005)		-.005 (.004)		-.075** (.033)		-.074*** (.026)
Const.	1.654*** (.0006)	.813*** (.002)	1.372*** (.0007)	.667*** (.002)	41.052*** (.003)	42.545*** (.017)	40.163*** (.003)	42.339*** (.022)
Controls for:								
year	no	yes	no	yes	no	yes	no	yes
education	no	yes	no	yes	no	yes	no	yes
industry	no	yes	no	yes	no	yes	no	yes
region	no	yes	no	yes	no	yes	no	yes
foreign co.	no	yes	no	yes	no	yes	no	yes
public co.	no	yes	no	yes	no	yes	no	yes
Obs.	5423097	5423082	3488358	3488347	4886917	4886903	2941582	2941574
R ²	0.025	0.542	0.026	0.606	0.001	0.185	0.019	0.291

Sources: Computations based on QP. Notes: Treatment 16, 17, or 18 refers to each of the three cohorts in the treatment group. Data are pooled for the three treatment groups and the control group at ages 19 to 39, and for all other birth cohorts at ages 40 to 49. Regressions in columns 1-2 and 5-6 include the main effect age_{19-39} ; regressions in columns 3-4 and 7-8 include the main effects for ages 19, 20-29 and 30-39. The control variables are: year (21 dummies), education (4 dummies), industry (24 dummies), region (2 dummies), foreign company (1 dummy), and public company (1 dummy). See equations 1 and 2 and section V for details on the specification. Standard errors clustered by person in parentheses.

Table 2: REGRESSIONS POOLING COHORTS AGED 45-55, TREATMENT GROUP (16, 17 OR 18 YEAR OLD IN 1986) VERSUS CONTROL GROUP (19 YEAR OLD IN 1986).

	(Log) hourly wages		Prob. part-time (*)		Hours full-time		Hours part-time		Prob. Employed (*)	
	Males	Females	Males	Females	Males	Females	Males	Females	Males	Females
	(1)	(2)	(3)	(4)	(7)	(8)	(5)	(6)	(9)	(10)
T16 * age 19	.065*** (.006)	.072*** (.005)	-.010* (.005)	-.004 (.011)	.454*** (.054)	.592*** (.053)	-.031 (.654)	.091 (.887)		
T16 * age 20-29	.018*** (.005)	-.004 (.004)	.006* (.003)	.025*** (.005)	-.138*** (.029)	-.274*** (.029)	-.679** (.285)	-.726** (.324)	-.008 (.005)	.004 (.007)
T16 * age 30-39	.016*** (.006)	-.006 (.005)	.003 (.003)	.013** (.006)	-.104*** (.020)	-.065*** (.024)	-.236 (.290)	-.982*** (.355)	-.031*** (.006)	-.017** (.007)
T17 * age 19	.061*** (.005)	.086*** (.005)	.005 (.004)	.007 (.010)	.193*** (.049)	.338*** (.052)	1.463*** (.435)	.506 (.698)		
T17 * age 20-29	.020*** (.004)	-.005 (.003)	-.004 (.002)	.009** (.004)	.037 (.025)	-.113*** (.027)	-.318 (.229)	.106 (.296)	.003 (.005)	.003 (.006)
T17 * age 30-39	.013** (.005)	-.007 (.005)	-.004 (.003)	.001 (.005)	-.058*** (.017)	-.089*** (.021)	-.196 (.246)	.103 (.306)	-.024*** (.005)	-.017*** (.007)
T18 * age 19	.035*** (.004)	.067*** (.005)	-.014*** (.003)	-.0100 (.007)	.183*** (.038)	.294*** (.044)	.285 (.351)	.402 (.537)		
T18 * age 20-29	.010*** (.004)	-.007** (.003)	.006** (.002)	.006 (.004)	.065*** (.024)	-.019 (.026)	-.110 (.199)	.229 (.263)	-.003 (.004)	-.003 (.005)
T18 * age 30-39	.008 (.005)	-.007 (.004)	.003 (.003)	.003 (.005)	-.058*** (.015)	-.067*** (.020)	-.252 (.227)	-.065 (.283)	-.014*** (.005)	-.002 (.006)
Const.	.771*** (.002)	.627*** (.002)			42.833*** (.012)	42.347*** (.020)	20.156*** (.127)	16.791*** (.118)		
Controls for:										
year	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
education	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
industry	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
region	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
foreign co.	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
public co.	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	5284945	2977411	5284945	2977411	4759260	2469534	525685	507877	9315371	5304033
R ²	0.55	0.597			0.385	0.392	0.286	0.228		
LL			-1435131	-1184643					-6051457	-3495105

Sources: Computations based on QP. Notes: (*) For probability of working part-time and probability of being employed in the private sector of the economy, columns 3-4 and 9-10 report the marginal impacts of a probit model. Treatment 16, 17, or 18 refers to each of the three cohorts in the treatment group. Data are pooled for the three treatment groups and the control group at ages 19 to 39, and for all other birth cohorts at ages 40 to 49, except for the model on the probability of employment, which pools data for the treatment and control groups at ages 20-39 and on all other birth cohorts at ages 40-49. The models include the main effects for ages 19, 20-29 and 30-39, with the exception of the model on the probability of employment, which includes only the main effects for ages 20-29 and 30-39. The control variables are: year (21 dummies), education (4 dummies), industry (24 dummies), region (24 dummies), foreign company (1 dummy), and public company (1 dummy); in the model on employment, the firm attributes refer to the initial firm. See the text for details. Standard errors clustered by person in parentheses.