Supplementary material to "Is it Enough to Increase Compulsory Education to Raise Earnings? Evidence from French and British Compulsory Schooling Laws"*

(not intended for publication)

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This material supplements the paper "Is it Enough to Increase Compulsory Education to Raise Earnings? Evidence from French and British Compulsory Schooling Laws". It contains supplementary tests and discussions for section 2 (Institutional background), section 5 (Results), section 6 (An interpretation based on certification effects) and section 7 (Discussion of alternative explanations).

1 Appendix to section 2 (Institutional background)

1.1 Correspondence between date of birth and assignment to pre- and post-compulsory schooling regimes

The correspondence between date of birth and school entry is far from perfect is many countries. For example, a significant fraction of American children defer school entry by a year, making them the oldest students (Datar, 2006). The facts that some children do not comply with school entry dates and that these children cannot be considered as a random sample raise important identification issues in the estimation of age of school entry effects on educational outcomes (Bedard and Dhuey, 2006).

I believe that this specific problem is not a serious challenge to the identification of the impact of the French reform, because the cutoff was defined in terms of date of birth and not in terms of date of school entry. The French 1967 Berthoin edict stated that the new minimum school leaving age of 16 would be applicable for *"children who will turn six*

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from January 1, 1959 onwards" (i.e. born after January 1, 1953). Hence in principle, the assignment of individuals to the pre- or post-reform regime did not depend on whether they started school at the normal age, skipped or repeated a grade. In practice, however, this rule is unlikely to have applied to individuals who were born after 1952 but were ahead of their normal school cohort. Although I lack direct statistical information to evaluate the magnitude of this phenomenon for individuals born in the 1950s, existing sources suggests that among recent cohorts, only a very small minority of children start school earlier than expected or skip a grade before they reach the compulsory schooling age:

- The *Panel primaire de l'Éducation nationale* is a survey which was conducted by the French ministry of Education between 1997 and 2002 on a representative sample of 9,639 pupils who started primary school in September 1997. The survey indicates that 97% of the pupils started school the year they turned six; 1.5% started one year earlier while the remaining 1.5% started school one or two years later.
- The *Panel secondaire de l'Éducation nationale* was conducted between 1995 and 2001 on a representative sample of roughly 17,800 students who started junior high school in September 1995. By the end of junior high school, only 3% of these students were one or two years ahead of their year group.

My guess is that the picture was not very different at the time of the Berthoin reform, which leads me to conclude that in the French context, only few individuals are not accurately assigned to the pre- or post-reform regime based on their year of birth.

The British case is less straightforward since the minimum school leaving age of 16 came into effect on September 1, 1972, irrespective of students' date of birth. In principle, individuals born between September 1, 1957 and August 31, 1958 were the first to face the new compulsory schooling regime. However, it cannot be excluded that children born before September 1, 1957 who started school later or repeated a grade would have had to comply with the new requirement. Symmetrically, children born after that date would have probably been allowed to drop out of school before the age of 16 if they were ahead of their age group. Hence there might be some errors in assigning individuals to compulsory schooling regimes based on their date of birth.

Various pieces of evidence suggest that this potential misallocation problem is not a major concern in the British case:

- First, existing evidence on recent school cohorts indicates that in England & Wales, compliance with school entry requirement is almost perfect while grade repetition (or grade skipping) is almost non-existent. Information about the 1995 Third International Mathematics and Science Study (TIMSS) samples, published in table 1.8 of Martin et al. (1997, p. 39) shows that only 0.1% of English 9-year-olds were in grades above the upper grade tested and only 0.9% were in grades below the lower grade tested, i.e. 99% of English 9-year-olds were in the expected grades for their age. In the TIMSS sample of 13-year-olds, the fraction of English students in the expected grades was also 99% (Beaton et al., 1996, table A.3, p. 190).

- Second, if non-compliance with school entry dates in England and Wales was a less uncommon phenomenon in the 1970s, one would expect it to show up in the evolution of the average school leaving age by school cohort (school cohort t+1 being defined as being born between September 1 of year t and August 31 of year t+1). Suppose that some individuals born before September 1, 1957 delayed school entry or repeated a grade while a some of those born after that date were ahead of their year group because of early school entry or grade skipping. If these proportions were substantial, then one would expect to observe two "secondary" jumps in average schooling around the 1958 school cohort cutoff: the first jump would occur between the 1956 and 1957 school cohort cutoff and would be driven by individuals born between September 1, 1956 and August 31, 1957 who were behind their expected year level and complied with the new minimum leaving age of 16; the second jump would occur between the 1958 and 1959 school cohorts and would be driven by individuals born between September 1, 1957 and August 31, 1958 who were ahead of their expected year level and did not comply with the new compulsory regime. Yet the visual inspection of the evolution of the average school leaving age by school cohort in England and Wales (figure 4 in the paper) shows no evidence of such secondary "jumps" around the 1958 cutoff (see figure 4 in the paper). This suggest only marginal deviations from perfect correspondence between school cohort and assignment to the pre- or post-law change in the British case.

- Finally, the fact that some individuals are incorrectly assigned to their compulsory schooling regime would certainly attenuate the first stage estimates with respect to the reform's intention to treat but would not call into question the exogenous nature of the observed increase in average educational attainment between school cohorts 1957 and 1958. In the current RDD setting, the forcing variable (date of birth) cannot be manipulated by individuals. Deviations from perfect correspondence between date of birth and compulsory schooling regimes are therefore unlikely to bias the estimates through sample selection issues. Such deviations would only be only a serious concern if they were large enough to cancel out the reform's positive impact on average educational attainment, but this is not what is observed in the data.

2 Appendix to section 5 (Results)

2.1 Impact of compulsory schooling laws on the distribution of school leaving ages

Figures A.1 and A.2 show the distribution of school leaving ages across school cohorts 1944-1962 in France and 1949-1967 in England & Wales, with each curve indicating the fraction of individuals who left school by a given age.

Both graphs share similar patterns. First, they reveal that the new mandatory requirements introduced by the 1967 Berthoin reform in France and the 1972 ROSLA in England & Wales were binding for a significant fraction of the population[1]: in both countries, about a third of the individuals belonging to the last school cohort facing the previous compulsory schooling regime (i.e. 1952 in France and 1957 in England & Wales) left school before the age of 16. Second, both compulsory schooling laws did force some – but not all – individuals to stay in school for one or two years more than they would have if the reform had not been implemented. It can be inferred from the graphs that about half of the individuals "caught up" by the new minimum school leaving age in France complied with the new mandatory requirement, while the other half did not. In England & Wales, the fraction of compliers is slightly higher (about three quarters). Imperfect compliance can be observed in most countries studied in the literature and results from both gaps in the enforcement of compulsory schooling laws and measurement error in the school leaving age variable[2]. Finally, these graphs show some evidence that the 1967 Berthoin reform and the 1972 ROSLA triggered a limited "spillover" effect on educational attainment[3] by affecting the distribution of school leaving ages *beyond* the age of 16, as the fraction of individuals who left school by the age of 16 and 17 exhibits a slight discontinuity at the cutoff points.

Table B.1 examines the impact of the 1967 Berthoin reform on the distribution of school leaving ages in France based on a global fourth order polynomial approximation allowing for an intercept shift at the cutoff point. The results are reported for France (column 1) and England & Wales (column 2) using three distinct samples: women and men (panel A),

[1]Compulsory schooling laws are not necessarily binding. For instance, Oreopoulos (2007) shows that when the minimum school leaving age was extended from 14 to 15 in the Republic of Ireland (1972), only 10% of the children were leaving school before the age of 15, which explains that the reform had hardly any impact on the average educational attainment.

[2]It should be noted that these factors are unlikely to represent a serious threat to identification, as long as a clear discontinuity in average educational attainment can be observed across school cohorts. Furthermore, our instrumental variable strategy controls for potential biases arising from measurement error in the schooling variable.

[3]Lang and Kropp (1986) first noted that the positive impact of compulsory schooling laws on educational attainment could well extend beyond the subpopulation of early school dropouts. Indeed, if the school leaving age partly acts as a signal of individual productivity on the labour market, then extending compulsory education can give all students the incentive to increase their own schooling in order to preserve their relative educational premium over lower achievers.

women only (panel B) and men only (panel C).

Unsurprisingly, the RDD estimates for France (column 1) indicate that the 1967 Berthoin reform had a strong negative and significant impact on the fraction of individuals who were "caught up" by the new minimum school leaving age. The fraction of individuals who dropped out by the age of 14 and by the age 15 fell respectively by 9.5 and 9.4 percentage points, the effect being slightly larger for women (-11.0 and -11.3 percentage points) than for men (-8.0 and -7.5 percentage points). Our results also show that while most of the reform's impact was concentrated on the 14 to 16 school leaving age group, it also induced a small fraction of individuals to prolong their studies *beyond* the new compulsory requirement: the fraction of individuals who dropped out by the age of 16 fell by a signficant 1.5 percentage point, this phenomenon being driven essentially by the male population (the coefficient is small and insignificant for women). Interestingly, our estimates suggest that the Berthoin reform's "spillover" effect did not extend beyond secondary schooling: the coefficients are not significantly different from zero when we consider school leaving ages of 18 or more.

The coefficients for England & Wales (column 2) show that the bulk of the 1972 ROSLA was concentrated on the population of individuals who left school by the age of 15, the effect being slightly larger for women (-23.8 percentage points) than for men (-21.0 percentage points). The reform's impact on the distribution of post-compulsory school leaving ages was negligible.

2.2 Sensitivity of estimates to differences in the average age of respondents

A substantial difference in the average age of individuals included in the French vs. English/Welsh samples would bring into question the reliability of the comparison, since the returns to compulsory schooling are likely to differ by age and labour market experience levels. Fortunately, both the timing of the reforms and the time span covered by the French *Enquête Emploi* and the UK Quarterly Labour Force Survey are such that the average age at which individuals are observed in both samples is very close. In the British sample, the average age is 41 vs. 43 in the French sample. This small difference seems unlikely to be driving the results.

Imposing age restrictions might appear as an appealing solution to make the samples even more comparable. However, the problem is that in the current setting, these restrictions would lead the samples to no longer be balanced by year of observation. The age range that I use in the paper (25-60) is not "binding" in the sense that all of the school cohorts which I consider in the French and British samples (i.e. 1944-1962 and 1949-1967 respectively) meet this age restriction in each of the surveys (i.e. 1990-2002 and 1993-2006).

Tighter age restrictions would have the undesired consequence of excluding some survey years for some of the school cohorts considered in the RDD specifications. For instance, if we were to consider only individuals aged 30-55, then French workers born in the years 1944, 1945, 1946, 1961 and 1962 would no longer be observed in each of the 13 annual EE surveys spanning between 1990 and 2002. Symmetrically, the same age restriction would lead British workers born in 1949, 1950, 1965, 1966 and 1967 to no longer be observed in each of the 53 quarterly LFS surveys spanning between 1993q4 and 2006q4. Since age, cohort and survey year effects cannot be simultaneously controlled for in the RDD specifications (because of linear dependence between these variables), it is important for the consistency of the estimates to preserve the balancing of samples by year of observation.

To reduce the average age at which individuals are observed in the French sample without having to exclude some school cohorts due to missing years of observations, I estimated the impact of the 1967 Berthoin reform on the subsample of respondents who were surveyed between 1990 and 1999. The average age of individuals included in this subsample is the same as in the full British sample (i.e. 41).

The corresponding first stage, reduced form and 2SLS estimates are reported in **table B.2**, with and without controlling for a quartic in age. The coefficients are very close to those reported in table 3 of the paper. In particular, the returns to compulsory education remain insignificant and close to zero. These results suggest that the slight difference in the age at which individuals are observed in France vs. England & Wales is not driving the diverging estimates between both countries.

2.3 Non-parametric RDD estimations

2.3.1 Local linear regressions

Table B.3 is the analog of table 3 in the paper, except that the RDD estimations are performed non-parametrically using a local linear regression (LLR) estimator rather than a parametric global polynomial approximation. The LLR approach consists in fitting linear regression functions to observations located within a given distance h of the cutoff point and separately on each side. As shown by Fan and Gijbels (1996) and Hahn et al. (2001) among others, LLR displays a number of attractive features. In addition to producing estimates which are not too sensitive to outcome values for observations far away from the threshold, this approach minimizes the edge effects usually associated with kernel-type non-parametric estimators. Furthermore, the LLR specification does not force the slope coefficients to be the same on both side of the cutoff point, as in the global polynomial setting[4].

From a practical point of view, the local linear regression estimation of the impact of a compulsory schooling law on an outcome Y can be estimated by restricting the sample

[4] As emphasized by Imbens and Lemieux (2007), the main benefit of local linear regression is that the estimates of $E[Y(0)|X = x]$ rely exclusively on observations on $Y(0)$ from the left of the discontinuity point and do not depend on observations on $Y(1)$, and vice versa for the estimates of $E[Y(1)|X = x]$.

to individuals belonging to school cohorts in the interval $[c-h, c+h]$ (where h denotes the selected bandwidth) and by estimating a kernel-weighted linear regression based on the following equation[5]:

$$Y_i = \alpha_0 + \alpha_1 T_i + \gamma_0(X_i - c) + \gamma_1 T_i(X_i - c) + \epsilon_i, \quad X_i \in [c-h, c+h] \tag{1}$$

where the treatment variable T_i takes the value 1 for individuals facing the new minimum school leaving age and where the kernel weights are defined as $w_c = K_h(X_i - c)$ for a chosen bandwidth h. I used a triangular kernel $K_h(u) = (1 - \frac{|u|}{h})\mathbb{1}(|u| < h)$ which is boundary optimal (Cheng et al., 1997). As in the global polynomial regression setting, the estimated coefficient $\hat{\alpha}_1$ on the treatment variable in equation (1) measures the average causal effect of the minimum school leaving age increase on the outcome Y at the assignment threshold c. To select the bandwidth h, I implemented the cross-validation procedure developed by Ludwig and Miller (2007). Following Lee and Card (2008), robust standard errors are obtained by clustering at the school cohort level.

The results displayed in table B.3 are very close to those obtained with a global fourth order polynomial approximation. They show that while the 1967 Berthoin reform and the 1972 ROSLA both raised workers' average educational attainment by about .3 years, the French reform had no sizeable effect on female and male wages. On the contrary, the estimated returns to the raising of the minimum school leaving age to 16 in England & Wales are positive and significant for both women (5.7-6.5%) and men (5.2-5.9%). The coefficients reported in columns 6 and 7 show no significant impact of compulsory schooling laws changes on employment and self-employment rates in France and England & Wales.

2.3.2 Changing the bandwidth size

Tables B.4 and B.5 address the potential concern that the local linear regression estimates could be sensitive to the choice of the bandwidth. Table B.4 for France and table B.5 for England & Wales show the first stage, reduced form and 2SLS estimates obtained when changing the value of the bandwidth used in the local linear regressions (i.e. the number of school cohorts included on each side of the cutoff point) from 4 to 10. The coefficients are fairly stable across the different bandwidth sizes. Moreover, the returns to compulsory schooling are never significant for French female and male workers but are always positive and significant in the case of England & Wales. These results indicate that the estimates reported in the paper are robust to the choice of the bandwidth.

[5] This version of the local linear estimation is equivalent to finding:

$$\min_{\alpha_0,\alpha_1,\gamma_0,\gamma_1} \sum_{i=1}^{N} \left[Y_i - \alpha_0 - \alpha_1 T_i - \gamma_0(X_i - c) - \gamma_1 T_i(X_i - c)\right]^2 K_h(X_i - c)$$

where $K_h(u)$ is the kernel, and h is the bandwidth.

2.3.3 Estimating jumps at placebo discontinuities

Tables B.6 and B.7 perform placebo tests by estimating jumps at points where there should be none. Following the guidelines set by Imbens and Lemieux (2007), I test non-parametrically (LLR) for jumps in average educational attainment and hourly wages at the median of the two subsamples of pre- and post-reform school cohorts in France (table B.6) and England & Wales (table B.7). The placebo cutoff points are 1948 and 1958 in the French case and 1953 and 1963 in the case of England & Wales.

The reduced form estimates corresponding to each of these cutoff points are displayed separately for female (panel A) and male (panel B) workers. None of the reported coefficients is significantly different from zero, which indicates that there are no significant jumps at the non-discontinuity points. I am therefore confident that the paper's RDD estimates are picking up the causal impact of the compulsory schooling law changes on individual schooling and earnings in both countries.

3 Appendix to section 6 (An interpretation based on certification effects)

3.1 Impact of compulsory schooling laws on the distribution of credentials

Table B.8 is the analog of table 4 in the paper, except that the RDD estimations are performed non-parametrically using local linear regression rather than a parametric global polynomial approximation. The impact of the raising of the minimum school leaving age to 16 on the distribution of academic qualifications is estimated separately for women and men in France (columns 1 and 2) and in England & Wales (columns 3 and 4). The coefficients are fairly close to those reported in table 4 in the paper. In the French case, there is no sign of a clear discontinuity around the 1953 school cohort for both women and men. On the contrary, a strong discontinuity can be observed in the case of England & Wales around the 1958 school cohort: the fraction of respondents holding no qualification drops sharply (by 11.0 points for women and by 6.3 points for men) while the fraction of respondents holding a certificate of junior secondary schooling (O-Level or CSE) goes up by a similar percentage (12.0 points for women and 6.1 points for men).

3.2 Returns to junior high school certification in England & Wales

The estimates reported in tables 3 and 4 of the paper can be combined to estimate the wage returns of obtaining a junior high school certificate (as opposed to no qualifications at all). It should be noted, however, that the corresponding 2SLS estimates probably yield an upper bound to the returns of credentials, since one cannot exclude that the 1972 ROSLA had a positive impact on the human capital accumulation of pupils who, although they failed the GCE O-level/CSE examinations, might have learned skills relevant for the labour market while preparing the certification exams.

Table B.9 estimates the first stage and reduced form impact of the 1972 ROSLA on the probability of holding a junior high school certificate (GCE O-Level or CSE) on hourly wages, separately for male and female workers. The estimated returns to the GCE O-level/CSE are higher for men (34%) then for women (15-17%), although the former are less precisely estimated. These returns are of the same order of magnitude as existing estimates derived from OLS specifications (e.g. McIntosh (2006) who estimates wage returns to GCE O-Levels in the range of 12 to 31% for males and 10 to 30% for females using the Quarterly Labour Force Survey for individuals born in the late 1950s).

3.3 Impact of the 1972 ROSLA on literacy scores

Table B.10 is the analog of table 5 in the paper, except that the RDD estimations are performed non-parametrically using local linear regression rather than a parametric global polynomial approximation to evaluate the impact of the 1972 ROSLA on the literacy scores of male and female British participants to the 1994 International Adult Literacy Survey (IALS). The results are consistent with the parametric estimates reported in table 5. They suggest that the raising of the minimum school leaving age to 16 in England & Wales led to a positive and significant improvement in literacy skills of 7 to 10 points on all three scales for women, and a positive (but only marginally significant) impact on the quantitative literacy skills of men of 11 to 15 points.

4 Appendix to section 7 (Discussion of alternative explanations)

4.1 Differences in compliance rates across pre-determined characteristics?

Figures A.1 and A.2 show that compliance with the French Berthoin reform was essentially concentrated on 14 year-olds, who had to stay in school until the age of 16, whereas in England & Wales, it was concentrated on 15 year-olds who had to stay in school for an additional year. In France, "compliers" represent roughly 10% of a school cohort while in England & Wales, the corresponding fraction is about 22%. However, since the French reform extended compulsory schooling by two years while the British reform extended it by only one year, both reforms had a similar impact on the average school leaving age (i.e. roughly four additional months spent in school).

I performed a number of checks to examine whether there are other differences in pre-determined characteristics of the compliers in France and in England & Wales. The specific dimensions I considered are gender, country of birth and ethnicity (which can only be analyzed in England and Wales).

Compliance rates by school leaving age and gender. To compare the compliance rates by age and gender in France and in England & Wales, I computed the distribution of school leaving ages for individuals belonging to the last pre-reform cohort (i.e. 1952 in France and 1948 in England & Wales) and estimated the impact of the law change on this distribution using the usual RDD specification. The RDD estimations are performed on the full sample of individuals who belong to school cohorts 1944-1962 (France) and 1949-1967 (England & Wales).

The results are displayed in **table B.11**. The first two columns report the distributions of school leaving ages for women and men belonging to the last pre-reform cohort, while the third and fourth columns report the estimated impact of the compulsory schooling law change on the distribution of school leaving ages.

These results indicate that in both countries, the female distribution of school leaving ages was comparable to the male distribution. However, the compliance rates were slightly higher for women. In France, the 1967 reform reduced the fraction of individuals who left school at the age of 14 by 11.4 ppt for women vs. 8.3 ppt for men (it should be noted, however, that the reform's impact on the distribution of men's school leaving ages seems to have extended beyond the age of 16); in England & Wales, the 1972 reform reduced the fraction of individuals who left school at the age of 15 by 24.1 percentage points for women vs. 21.2 percentage points for men.

The fact that compliance rates are slightly higher for women than for men in both countries (with a possibly stronger gap in France) is, together with other differences in male and female labour market outcomes, an important justification for performing separate estimations by gender. This is why all the results in the paper are reported for women and men separately.

Compliance rates by school leaving age and country of birth. The second set of comparisons contrasts the compliance rates of native vs. foreign born individuals in both countries.

Table B.12 is constructed in the same manner as table B.11. The results indicate that the compulsory schooling reforms had little or no effects on the distribution of school leaving ages for individuals born abroad in France and in England & Wales, which is not surprising since a large fraction of these individuals would have completed their education outside of these respective countries.

There are, however, notables differences between both countries. In France, the average educational attainment of foreign-born individuals is lower than that of the native-born, whereas the opposite is true in England & Wales. This result is consistent with the findings of Algan et al. (2010) who show that on average, first-generation immigrants are more educated in the UK than in France. Another notable difference is that while the 1972 reform in England & Wales had a small but significant impact on the distribution of school leaving ages for individuals born outside of England & Wales, the school reform of 1967 in France had no impact at all. This result is probably a consequence of the fact that in France, family migration is predominant among the inflows of permanent-type immigrants whereas labour migration is more important in the UK (OECD, 2010).

In light of these differences, it is clear that using the full sample of respondents to the French and British Labour Force surveys would not necessarily yield comparable estimates. This is the reason why I chose to exclude foreign-born individuals from both samples. All the results discussed in the paper are estimated using the samples of native-born only.

Compliance rates by school leaving age and ethnicity (England & Wales only). Unfortunately, compliance rates by age and ethnicity cannot be estimated in France, since it is forbidden by law to collect statistics referring to "racial or ethnic origin".

In the British LFS sample, non-White individuals represent 6.5% of the full sample of English and Welsh respondents born between 1944 and 1967. However, once the sample is restricted to native born individuals, the fraction of non-white respondents falls to 1.7% only.

Table B.13 shows the distribution of school leaving ages and the corresponding impact of the 1972 reform in England & Wales for White vs. non-White individuals who were born in England & Wales. The results are similar for both groups, which suggests that at the time of the 1972 ROSLA, compliance rates were not strongly associated with ethnic background.

Although information on ethnicity is unavailable in the French data, I doubt that the results would look very different. Indeed, since the inflows of non-White immigrants to France were marginal until the late 1950s, most individuals born in the 1940s and 1950s with non-White ethnic background would have been born outside of France. Hence it seems unlikely that the sample of native-born individuals includes more than a negligible fraction of non-Whites.

The fact that non-White ethnic groups represent only a very small fraction of the native-born and that, at least in England & Wales, their compliance rates are similar to the rest of the population, rules out the possibility that ethnicity plays a significant role in explaining the discrepancy between the estimated returns to compulsory education in France vs. England & Wales.

Overall, the comparison of compliance rates across a number of pre-determined characteristics suggests that apart from a few differences that come mainly from the fact that the initial compulsory schooling age was lower in France (14) than in England & Wales

(15), "compliers" in both countries appear to share reasonably similar attributes. Individuals who were compelled to stay in school for one or two more years belong to the bottom third of the school leaving age distribution and are found both in the male and the female population (with a slightly higher compliance rate for women). Finally, the empirical evidence indicates that ethnicity does not offer a credible explanation for why the raising of the minimum school leaving age had a positive impact on wages in England & Wales, but not in France.

4.2 Differential wage rigidity?

A second candidate explanation for my findings pertains to the role of wage rigidity. The influence of trade unions on the setting of wages, the size of the public sector and the level of the minimum wage are commonly cited determinants of wage rigidity that might be considered as possible explanations for the lack of wage gains in the French case.

The influence of trade unions on the setting of wages? The role of unions in collective bargaining and wage setting is an important channel to consider when attempting to explain the lack of wage gains in the French case.

The comparison of unionization rates in France and Britain offers a starting point for examining this issue. OECD figures reveal that the fraction of unionized workers is higher in the UK than in France. According to the 2004 *Employment Outlook* (OECD, 2004, chap. 3, p. 145), the trade union density rate in 1970 was 45% in the UK but only 22% in France. Despite the steady decline of trade union membership that occurred in the UK during the 1980s, the fraction of workers who were members of a trade in 2000 remained higher in the UK (31%) than in France (10%).

Unionization rates are, however, only weakly related to the role of trade unions in collective wage bargaining and to the nature of pay-setting arrangements. Statistics on collective bargaining coverage suggest that French unions might in fact exert a stronger influence on the setting of wages that their British counterparts. While collective bargaining coverage rates were high in both countries in 1980 (80% in France vs. 70% in the UK), the fraction of workers who are covered by collective bargaining agreements in the UK has fallen to 30% in 2000, but has increased to 90% in France (OECD, 2004).

The fact that collective bargaining coverage is higher in France than in the UK could be interpreted as a plausible explanation for the lack of monetary returns to the raising of the compulsory schooling age in 1967, since the influence of unions on the setting of wages could have limited the possibility of wage gains. In practice, however, industry level bargaining agreements (which are the most important in terms of coverage) have only limited importance in determining individual wages in France, for several reasons:

1. The minimum wage (SMIC) is set directly by the State and supersedes any minimum pay rates that are negotiated below that level by industry agreements.

2. Most collective bargaining agreements are not focused on wage negotiations but rather on issues such that training, retirement, employment contracts, gender equality, hours of work and work organization. According to the French Ministry of Labour's annual report on collective bargaining (DARES, 2009, p. 18), out of a total of 1,108 industry level agreements signed in 2009, only 421 dealt with pay increases.

3. Finally, and most notably, statistics released by the French ministry of Labour indicate that for the majority of French employees working in the private sector, pay rises are negotiated on a individual basis. Using administrative records that cover all workers employed in private firms with more than 10 employees, a recent report (DARES, 2006, see in particular table 3) shows that only 34% of workers whose pay went up in 2004 (representing 86.4% of all workers) depended on a general increase (at the firm level), whereas 49% saw their pay increase through the combination of a general increase and an individual pay rise, and 17% depended entirely on an individual pay rise. According to the same study, the individualization of wages was even more prevalent in 2000, since less than 26% of workers whose pay went up depended on a general increase only.

In light of these statistics, I do not perceive the structure of collective bargaining in France as the most likely explanation for the lack of wage gains experienced by individuals who increased their educational attainment as a result of the compulsory schooling reform of 1967.

Wage rigidity in the public sector? Wage rigidity in the public sector is another factor that could be invoked to explain the lack of wages gains in France. However, several empirical observations seem to rule out the possibility that the French vs. British discrepancy in estimated returns to compulsory education is related to cross-country differences in the size of the public sector or to more rigid wage setting in the French public sector.

In the samples I constructed from the French *Enquête Emploi* and the British Labour Force Survey, the share of public sector employment is similar in both countries over the periods covered by the study (1990-2002 in France, 1993-2004 in England & Wales). In the French sample, 23% of workers are employed in the public sector whereas in the British sample, the fraction is 24%. The cross-country gap in the size of the public sector thus seems too small to account for the difference in estimated returns. However, wage setting in the public sector might be more flexible in the UK than in France. In order to further examine this issue, I performed two series of tests.

First, I estimated whether the raising of the minimum school leaving age in both countries had any impact on the probability of being employed in the public sector rather than in the private sector. The coefficients for France are reported in the first two columns of **table B.14** and the corresponding estimates for England & Wales are reported in the first two columns of **table B.15**, separately for female and male workers. The specification used in column 2 includes a quartic in age in the set of regressors. The results indicate that neither of the reforms had a significant impact on the probability of being employed in the public vs. the private sector. If anything, the point estimates suggest that in the French case, the raising of the minimum school leaving age was associated with a slight decline in the fraction of workers employed in the public sector, which, according to the wage rigidity assumption, should have favored a better alignment of wages with individual productivity.

The second test aims at comparing the returns to compulsory education in the private and the public sector, for both countries. Columns 3 to 6 of tables B.14 and B.15 report the 2SLS estimates of the returns to compulsory education in the public and the private sector in France and in England & Wales. In the French case, the coefficients are all insignificant and close to zero, which rules out the existence of positive returns to the raising of the minimum school leaving age both in the public and the private sector. In the British case, the coefficients are positive for both sectors but smaller and not significantly different from zero in the public sector. This result could be interpreted as evidence that wage rigidity in the public sector might mitigate the returns to compulsory education. However, the fact that the reduced form estimates are not significant when we consider public sector employees could also be due to the fact that the sample is relatively small compared to the sample of private sector employees.

Overall, these results suggest that the French vs. British discrepancy in estimated returns to compulsory education cannot directly related to the French public sector's larger size or stronger wage setting rigidity.

The minimum wage? A potential explanation for the zero returns to the French Berthoin is that the minimum wage might have prevented the higher productivity of early school dropouts from being reflected in their earnings, these individuals being overrepresented at the bottom end of the wage distribution where the minimum wage is known to have a large "bite". I explored this possibility by testing two implications of the minimum wage hypothesis.

First, if the minimum wage was the main explanation for the lack of monetary gains from increased compulsory education in France, one would expect the raising of the minimum school leaving age to have translated into higher employment rates for early school dropouts, by reducing the fraction of individuals whose productivity falls below the minimum wage threshold. The empirical evidence does not, however, support this hypothesis. **Figures A.3 and A.4** are the graphical counterparts of the regressions displayed in column 6 of table 3 in the paper. The evolution of employment rates across school cohorts 1944-1962 in France and school cohorts 1949-1967 in England & Wales exhibits no obvious discontinuities at the cutoff points (1953 in France and 1958 in England & Wales).

Since the employment effects of the raising of the minimum school leaving age are likely to have been be larger for young workers than for older ones, I estimated similar regressions using the French *Enquête Emploi* that were conducted between 1975 and 1989. Although these pre-1990 surveys do not include information on earnings, they do provide information on employment status. By construction, individuals in that sample are observed at a younger age (31) than those included in the 1990-2002 sample (43). The reduced form effects of the impact of the Berthoin reform on the employment rates of these younger women and men are reported in the top panel of **table B.16**. None of the coefficients are significantly different from zero, which rules out the existence of indirect labour market returns from the Berthoin reform, in the form of higher employment rates rather than higher wages.

I performed a similar exercise for England & Wales using the annual Labour Force Surveys that were conducted in the UK between 1979 and 1991 (again, wages are not available for that period). The results, which are displayed in the bottom panel of table B.16 indicate that the 1972 ROSLA had not significant effect on the probability of being employed for English/Welsh women and men observed in their younger years. Hence wages appear as the main labour market outcome along which a divergence can be observed between the French and British compulsory schooling reforms.

A second implication of the wage rigidity hypothesis is that the individuals whose educational attainment was positively affected by compulsory schooling laws would be more likely to become self-employed if their higher productivity was not rewarded in the wage and salary sector. Following Pischke and von Wachter (2008), I test this hypothesis by focusing on the evolution of the fraction of self-employed workers across school cohorts. **Figures A.5 and A.6** are the graphical counterparts of the regressions displayed in column 7 of table 3 in the paper. The evolution of self-employment rates across school cohorts 1944-1962 in France and school cohorts 1949-1967 in England & Wales shows no clear discontinuities at the cutoff points. These results provide no support to the wage rigidity argument, since self-employment rates were unaffected by the French and British schooling reforms.

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Figure A.1: *Distribution of school leaving ages in France across school cohorts 1944-1962. Source:* Enquête Emploi *(1990-2002).*

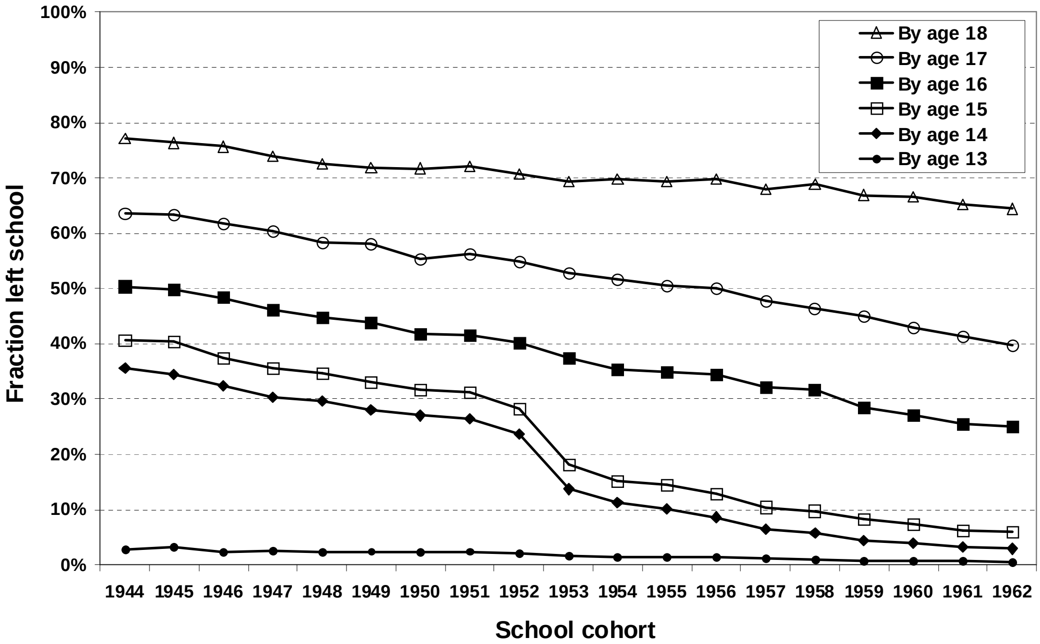


Figure A.2: *Distribution of school leaving ages in England & Wales across school cohorts 1949-1967. Source: Quarterly Labour Force Survey (1993-2006).*

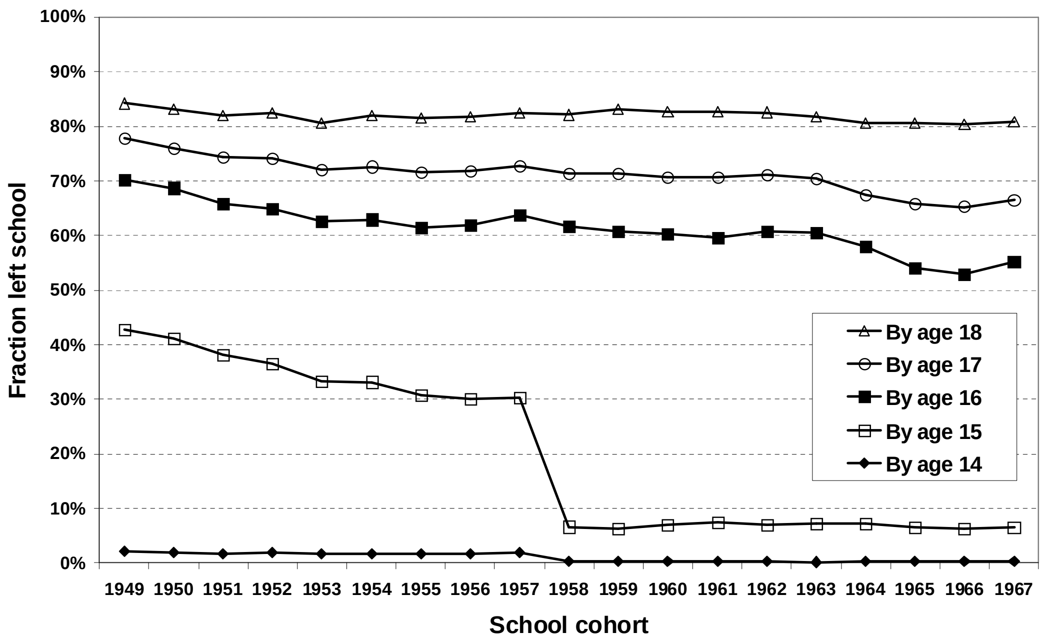
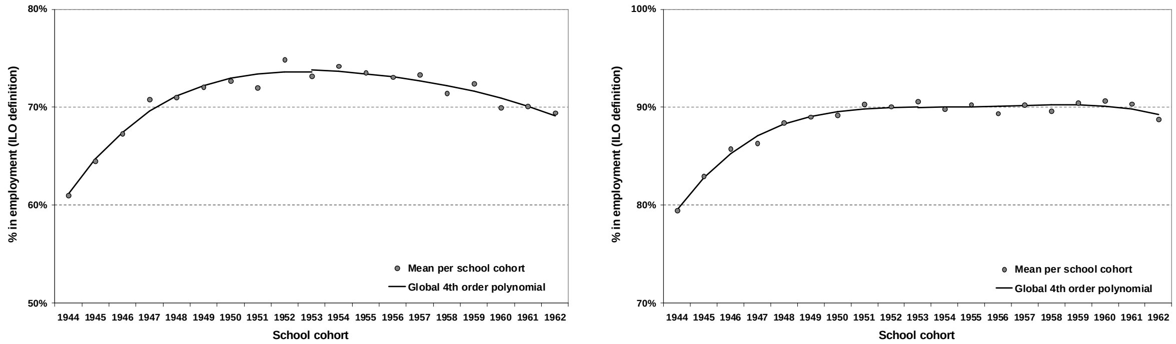


Figure A.3: *Impact of the 1967 Berthoin reform in France on the probability of being in employment, calculated for women and men separately. School cohorts 1944-1962. Source:* Enquête Emploi *(1990-2002).*

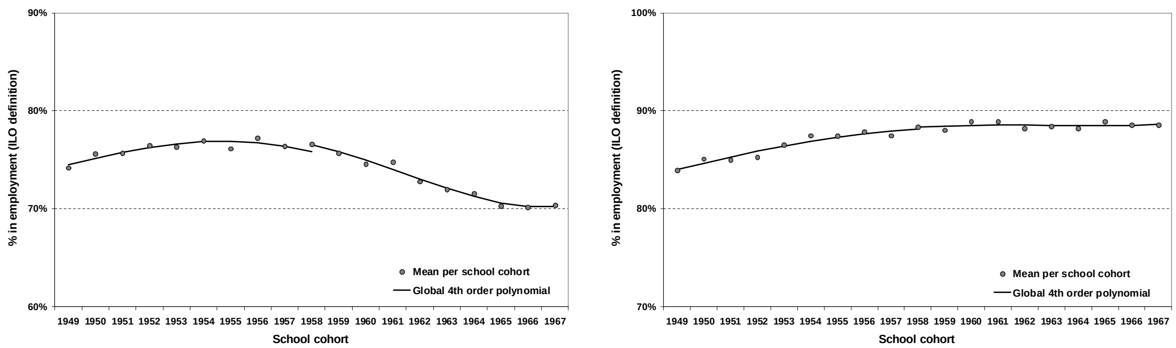
(a) Women (b) Men



Notes: The dots show the employment rates within each school cohort cell for the subsample of French women and men born between 1944 and 1962. The solid line represents the fitted values from a global fourth order polynomial regression allowing for an intercept shift at the 1953 school cohort.

Figure A.4: *Impact of the 1972 ROSLA in England & Wales on the probability of being in employment, calculated for women and men separately. School cohorts 1949-1967. Source: Quarterly Labour Force Survey (1993-2006).*

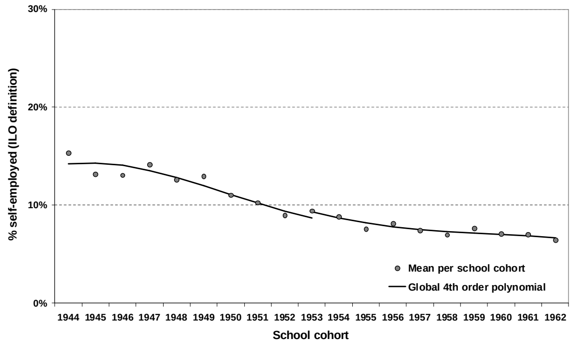
(a) Women (b) Men



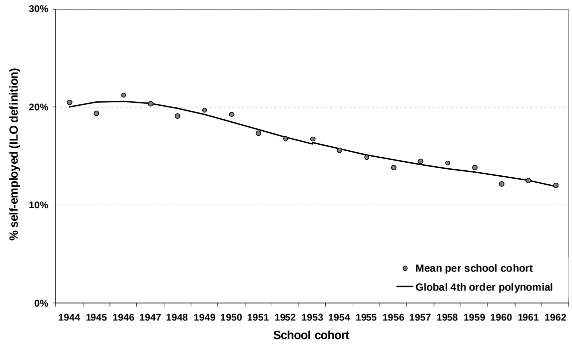
Notes: The dots show the employment rates within each school cohort cell for the subsample of English and Welsh women and men who belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967). The solid line represents the fitted values from a global fourth order polynomial regression allowing for an intercept shift at the 1958 school cohort.

Figure A.5: *Impact of the 1967 Berthoin reform in France on the probability of being self-employed, calculated for women and men separately. School cohorts 1944-1962. Source:* Enquête Emploi *(1990-2002).*

(a) Female workers



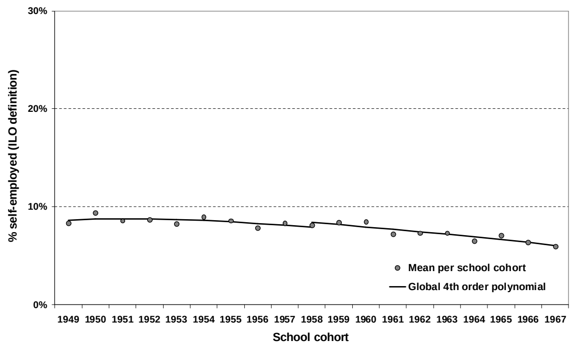
(b) Male workers



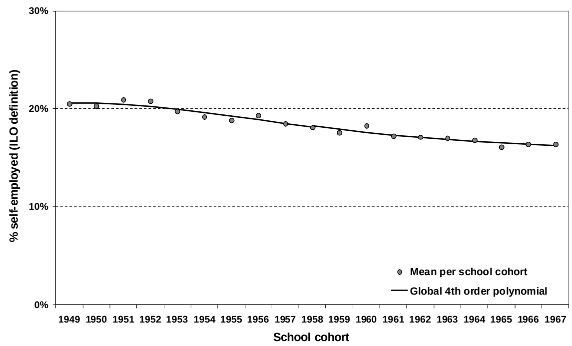
Notes: The dots show the self-employment rates within each school cohort cell for the subsample of French female and male workers born between 1944 and 1962. The solid line represents the fitted values from a global fourth order polynomial regression allowing for an intercept shift at the 1953 school cohort.

Figure A.6: *Impact of the 1972 ROSLA in England & Wales on the probability of being self-employed, calculated for women and men separately. School cohorts 1949-1967. Source: Quarterly Labour Force Survey (1993-2006).*

(a) Female workers



(b) Male workers



Notes: The dots show the self-employment rates within each school cohort cell for the subsample of English and Welsh female and male workers who belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967). The solid line represents the fitted values from a global fourth order polynomial regression allowing for an intercept shift at the 1958 school cohort.

Table B.1: *Estimated impact of the 1967 Berthoin reform and the 1972 ROSLA on the distribution of school leaving ages in France and in England & Wales. Sources:* Enquête Emploi *(1990-2002) and Quarterly Labour Force Survey (1993-2006).*

	France (1)	England & Wales (2)
Panel A: Women and men		
% left school by age 13	-.005***	-.000
	(.002)	(.000)
% left school by age 14	-.095***	-.015***
	(.006)	(.001)
% left school by age 15	-.094***	-.225***
	(.008)	(.005)
% left school by age 16	-.015***	-.012*
	(.005)	(.007)
% left school by age 17	-.007	-.005
	(.005)	(.006)
% left school by age 18	-.009	.004
	(.005)	(.005)
Number of observations	155,562	247,440
Panel B: Women		
% left school by age 13	-.005*	-.000
	(.003)	(.000)
% left school by age 14	-.110***	-.015***
	(.010)	(.001)
% left school by age 15	-.113***	-.238***
	(.013)	(.004)
% left school by age 16	-.008	-.006
	(.006)	(.011)
% left school by age 17	-.014	.008
	(.010)	(.008)
% left school by age 18	-.016	.019***
	(.012)	(.006)
Number of observations	78,586	129,813
Panel C: Men		
% left school by age 13	-.005***	-.000
	(.002)	(.002)
% left school by age 14	-.080***	-.016***
	(.007)	(.002)
% left school by age 15	-.075***	-.210***
	(.006)	(.011)
% left school by age 16	-.013**	-.018*
	(.007)	(.011)
% left school by age 17	.000	-.018*
	(.009)	(.011)
% left school by age 18	-.003	-.012*
	(.008)	(.006)
Number of observations	76,976	117,627

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. The dependent variables are the fraction of individuals who left school by a given age. Each coefficient comes from a separate fourth order global polynomial regression allowing for an intercept shift at the cutoff point (1953 for France and 1958 for England & Wales). Regressions for France (column 1) are performed on a sample constructed from the 1990 to 2002 French *Enquête Emploi* which includes individuals who were born in France, belong to school cohorts 1944 to 1962 and were aged 25-60 when surveyed. To avoid using the same individual several times, the sample is restricted to respondents who were interviewed in the first of the three survey waves. Regressions for England & Wales (column 2) are performed on a sample constructed from the 1993 to 2006 Quarterly Labour Force Surveys which includes individuals who were born in England & Wales, belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967) and were aged 25-60 when surveyed. To avoid using the same individual several times, the sample is restricted to respondents who were interviewed in the fifth of the five survey waves. Standard errors are clustered at the school cohort level.

Table B.2: *Impact of the 1967 French Berthoin reform on school leaving age and hourly wages. Source:* Enquête Emploi *(1990-1999).*

France – Enquête Emploi (1990-1999)

	First stage		Reduced form		2SLS	
Dependent variable:	School leaving age		Log of hourly wages		Log of hourly wages	
	(1)	(2)	(3)	(4)	(5)	(6)
Women:						
Coefficient	.273***	.273***	-.001	-.001	-.003	-.005
s.e.	(.027)	(.027)	(.005)	(.006)	(.020)	(.021)
Number of observations	37,057	37,057	37,057	37,057	37,057	37,057
Men:						
Coefficient	.265***	.263***	.003	.002	.011	.007
s.e.	(.022)	(.021)	(.010)	(.010)	(.039)	(.038)
Number of observations	40,225	40,225	40,225	40,225	40,225	40,225
Controls:						
Quartic in age	No	Yes	No	Yes	No	Yes

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. The dependent variables are age left full-time education (columns 1 and 2) and log of hourly wage (columns 3 to 6). Each coefficient comes from a separate fourth order global polynomial regression allowing for an intercept shift at the cutoff point (1953). The sample is constructed from the first wave of the 1990 to 1999 French *Enquête Emploi* and includes female and male wage earners who were born in France, belong to school cohorts 1944 to 1962, left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. Standard errors are clustered at the school cohort level.

Table B.3: *OLS, First stage, Reduced form and 2SLS effects of the 1967 Berthoin reform in France and the 1972 ROSLA in England & Wales on school leaving age, hourly wages, employment and self-employment using local linear regression with a triangular kernel. Sources:* Enquête Emploi *(1990-2002) and Quarterly Labour Force Survey (1993-2006).*

	OLS	First stage		Reduced form				2SLS	
Dependent variable:	Log of hourly wage	School leaving age		Log of hourly wage		Employed	Self-employed	Log of hourly wage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				PANEL A: FRANCE – ENQUÊTE EMPLOI (1990-2002)					
Women									
Coefficient	.087***	.288***	.287***	.000	-.002	-.007	.006	.002	-.008
(s.e.)	(.001)	(.035)	(.032)	(.002)	(.003)	(.011)	(.005)	(.007)	(.009)
Number of observations	47,670	47,670	47,670	47,670	47,670	78,586	55,874	47,670	47,670
Men									
Coefficient	.073***	.286***	.278***	-.002	-.005	-.001	.002	-.005	-.019
(s.e.)	(.001)	(.028)	(.026)	(.008)	(.006)	(.003)	(.004)	(.027)	(.023)
Number of observations	50,927	50,927	50,927	50,927	50,927	76,976	68,315	50,927	50,927
				PANEL B: ENGLAND & WALES – QUARTERLY LABOUR FORCE SURVEY (1993-2006)					
Women									
Coefficient	.119***	.327***	.328***	.019***	.021***	.003	.003	.057***	.065***
(s.e.)	(.001)	(.018)	(.020)	(.002)	(.002)	(.003)	(.004)	(.006)	(.007)
Number of observations	75,304	75,304	75,304	75,304	75,304	129,813	96,321	75,304	75,304
Men									
Coefficient	.095***	.281***	.279***	.015**	.016**	.005	-.003	.052**	.059**
(s.e.)	(.001)	(.030)	(.029)	(.006)	(.007)	(.004)	(.003)	(.022)	(.025)
Number of observations	68,535	68,535	68,535	68,535	68,535	117,627	102,920	68,535	68,535
Age controls	Quartic	None	Quartic	None	Quartic	Quartic	Quartic	None	Quartic

Notes : *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. Each coefficient comes from a separate regression. The dependent variables are log of hourly wage (columns 1, 4, 5, 8 and 9), age left full-time education (columns 2 and 3), an indicator variable for being employed (column 6) and an indicator variable for being self-employed (column 7). The coefficients reported in columns 2-8 are estimated using local linear regressions with a triangular kernel and an optimally selected bandwidth (using the cross validation criterion) on both sides of the cutoff point (school cohorts 1953 in France and 1958 in England & Wales). The French sample (panel A) is constructed from the first wave of the 1990 to 2002 French *Enquête Emploi* and includes individuals who were born in France, belong to school cohorts 1944 to 1962, left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. The English and Welsh sample (panel B) is constructed from the fifth wave of the 1993 to 2006 Quarterly Labour Force Survey and includes individuals who were born in England & Wales, belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967), left full-time education between the age of 6 and the age of 25 and were aged 25-60 when surveyed. The samples used in column 6 include all individuals. The subsamples used in column 7 are restricted to workers. The subsamples used in column 1-5 and 8-9 include wage earners only. Standard errors are clustered at the school cohort level.

Table B.4: *Sensitivity to the choice of bandwidth. Estimated impact of the 1967 Berthoin reform in France on school leaving age and hourly wages of female and male workers using local linear regression with different bandwidth values. Source:* Enquête Emploi *(1990-2002).*

France – Enquête Emploi (1990-2002)

	Local Linear Regression (triangular kernel)		
	First Stage (dependent var: age left full-time education)	Reduced form (dependent var: log of hourly earnings)	2SLS (dependent var: log of hourly earnings)
	(1)	(2)	(3)
<u>Panel A: Women</u>			
Bandwidth=4	.275***	.000	.001
	(.030)	(.002)	(.006)
Bandwidth=5	.278***	-.002	-.007
	(.026)	(.002)	(.008)
Bandwidth=6	.287***	-.002	-.008
	(.032)	(.003)	(.009)
Bandwidth=7	.278***	-.003	-.010
	(.030)	(.004)	(.012)
Bandwidth=8	.271***	-.003	-.010
	(.028)	(.004)	(.014)
Bandwidth=9	.264***	-.002	-.009
	(.027)	(.004)	(.015)
Bandwidth=10	.265***	-.001	-.006
	(.026)	(.004)	(.015)
Number of observations	47,670	47,670	47,670
<u>Panel B: Men</u>			
Bandwidth=4	.247***	-.008	-.032
	(.005)	(.008)	(.034)
Bandwidth=5	.253***	-.007	-.029
	(.015)	(.006)	(.024)
Bandwidth=6	.278***	-.005	-.019
	(.026)	(.006)	(.023)
Bandwidth=7	.282***	-.005	-.018
	(.025)	(.006)	(.022)
Bandwidth=8	.276***	-.005	-.019
	(.023)	(.006)	(.021)
Bandwidth=9	.271***	-.006	-.022
	(.021)	(.006)	(.020)
Bandwidth=10	.272***	-.007	-.024
	(.020)	(.006)	(.020)
Number of observations	50,927	50,927	50,927
Age controls	Quartic	Quartic	Quartic

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. The dependent variables are age left full-time education (column 1) and log of hourly wage (columns 2 and 3). Each coefficient comes from a separate triangular kernel weighted linear regression which is estimated for different values of the bandwidth. All regressions include a quartic in age and are performed on a sample constructed from the 1990 to 2002 French *Enquête Emploi* which includes wage earners who were born in France, belong to school cohorts 1944 to 1962, left school between the ages of 6 and 25 and were aged between 25 and 60 when surveyed. To avoid using the same individual several times, the sample is restricted to respondents who were interviewed in the first of the three survey waves. Standard errors are clustered at the school cohort level.

Table B.5: *Sensitivity to the choice of bandwidth: estimated impact of the 1972 ROSLA in England & Wales on schooling and hourly wages of female and male workers using local linear regression with different bandwidth values. Source: Quarterly Labour Force Survey (1993-2006).*

England & Wales – Quarterly Labour Force Survey (1993-2006)

	Local Linear Regression (triangular kernel)		
	First Stage (dependent var: age left full-time education)	Reduced form (dependent var: log of hourly earnings)	2SLS (dependent var: log of hourly earnings)
	(1)	(2)	(3)
Panel A: Women			
Bandwidth=4	.336***	.019***	.056***
	(.013)	(.002)	(.002)
Bandwidth=5	.332***	.021***	.064***
	(.019)	(.002)	(.007)
Bandwidth=6	.328***	.021***	.065***
	(.020)	(.002)	(.007)
Bandwidth=7	.318***	.021***	.066***
	(.021)	(.002)	(.006)
Bandwidth=8	.304***	.022***	.071***
	(.021)	(.002)	(.009)
Bandwidth=9	.287***	.022***	.077***
	(.023)	(.002)	(.011)
Bandwidth=10	.276***	.022***	.081***
	(.024)	(.002)	(.012)
Number of observations	75,304	75,304	75,304
Panel B: Men			
Bandwidth=4	.311***	.014*	.045*
	(.009)	(.008)	(.025)
Bandwidth=5	.295***	.016**	.053**
	(.020)	(.007)	(.024)
Bandwidth=6	.279***	.016**	.059**
	(.029)	(.007)	(.025)
Bandwidth=7	.260***	.016**	.062**
	(.033)	(.007)	(.025)
Bandwidth=8	.241***	.015**	.062**
	(.036)	(.007)	(.025)
Bandwidth=9	.224***	.015**	.066**
	(.036)	(.007)	(.027)
bandwidth=10	.208***	.014**	.067**
	(.038)	(.007)	(.027)
Number of observations	68,535	68,535	68,535
Age controls	Quartic	Quartic	Quartic

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. The dependent variables are age left full-time education (column 1) and log of hourly wage (columns 2 and 3). Each coefficient comes from a separate triangular kernel weighted linear regression which is estimated for different values of the bandwidth. All regressions include a quartic in age and are performed on a sample constructed from the 1993 to 2006 Quarterly Labour Force Surveys which includes wage earners who were born in England & Wales, belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967), left school between the ages of 6 and 25 and were aged 25-60 when surveyed. To avoid using the same individual several times, the sample is restricted to respondents who were interviewed in the fifth of the five survey waves. Standard errors are clustered at the school cohort level.

Table B.6: *Testing for jumps in school leaving age and hourly wages of French female and male workers at non-discontinuity points. Source:* Enquête Emploi *(1990-2002).*

France – Enquête Emploi (1990-2002)

	Local Linear Regression (triangular kernel)			
	First Stage (dependent var: age left full-time education)		Reduced form (dependent var: log of hourly earnings)	
	Non-discontinuity points		Non-discontinuity points	
	1948	1958	1948	1958
	(1)	(2)	(3)	(4)
Panel A: Women	.081	.032	.005	-.006
	(.049)	(.034)	(.003)	(.004)
Number of observations	21,474	26,196	21,474	26,196
Panel B: Men	.063	-.034	.002	-.003
	(.038)	(.027)	(.004)	(.004)
Number of observations	22,008	28,919	22,008	28,919
Quartic in age	Yes	Yes	Yes	Yes
School cohorts	1944-1952	1953-1962	1944-1952	1953-1962

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. Columns 1 and 2 are the reduced form estimates of the jumps in age left full-time education at particular non-discontinuity points (school cohorts 1948 and 1958), estimated using a local linear regression based on a triangular kernel and an optimally selected bandwidth. Columns 3 and 4 are the reduced form estimates of the corresponding jumps in the log of hourly wages. The regressions are performed separately on the subsample of French wage earners who faced the pre-reform minimum school leaving age of 14 (school cohorts 1944-1952) and on the subsample of wage earners who faced the post-reform minimum school leaving age of 16 (school cohorts 1953-1962). These subsamples are constructed from the sample of wage earners in the 1990 to 2002 French *Enquête Emploi* who were born in France, belong to school cohorts 1944 to 1962, left school between the ages of 6 and 25 and were aged 25-60 when surveyed. To avoid using the same individual several times, the sample is restricted to respondents who were interviewed in the first of the three survey waves. All regressions include a quartic in age. Standard errors are clustered at the school cohort level.

Table B.7: *Testing for jumps in schooling and hourly wages of English and Welsh female and male workers at non-discontinuity points. Source: Quarterly Labour Force Survey (1993-2006).*

England & Wales - Quarterly Labour Force Survey (1993-2006)

	Local Linear Regression (triangular kernel)			
	First Stage (dependent var: age left full-time education) Non-discontinuity points		Reduced form (dependent var: log of hourly earnings) Non-discontinuity points	
	1953	1963	1953	1963
	(1)	(2)	(3)	(4)
Panel A: Women	-.008	.039	.006	.001
	(.019)	(.030)	(.004)	(.003)
Number of observations	33,187	42,117	33,187	42,117
Panel B: Men	.008	.011	-.000	.005
	(.020)	(.019)	(.004)	(.003)
Number of observations	29,094	39,441	29,094	39,441
Quartic in age	Yes	Yes	Yes	Yes
School cohorts	1949-1957	1958-1967	1949-1957	1958-1967

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. Columns 1 and 2 are the reduced form estimates of the jumps in age left full-time education at particular non-discontinuity points (school cohorts 1953 and 1963), estimated using a local linear regression based on a triangular kernel and an optimally selected bandwidth. Columns 3 and 4 are the reduced form estimates of the corresponding jumps in the log of hourly wages. The regressions are performed separately on the subsample of English and Welsh wage earners who faced the pre-reform minimum school leaving age of 15 (school cohorts 1949-1957) and on the subsample of wage earners who faced the post-reform minimum school leaving age of 16 (school cohorts 1958-1967). These subsamples are constructed from the sample of wage earners in the 1993 to 2006 Quarterly Labour Force Surveys who were born in England & Wales, belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967), left school between the ages of 6 and 25 and were aged 25-60 when surveyed. To avoid using the same individual several times, we include only respondents who were interviewed in the fifth of the five survey waves. All regressions include a quartic in age. Standard errors are clustered at the school cohort level.

Table B.8: *Reduced form effects of the 1967 Berthoin reform in France and the 1972 ROSLA in England & Wales on the distribution of the highest academic or vocational credential held by women and men, estimated using local linear regression with a triangular kernel. Sources:* Enquête Emploi *(1990-2002) and Quarterly Labour Force Survey (1993-2006).*

	France		England & Wales	
	Women	Men	Women	Men
	(1)	(2)	(3)	(4)
Highest Held Qualification:				
% No qualification	-.008	-.008	-.110***	-.063***
	(.010)	(.006)	(.006)	(.004)
% Primary schooling certificate	-.001	-.000	n/a	n/a
	(.005)	(.004)	n/a	n/a
% Low vocational qualification	-.012**	-.017**	-.015***	-.003
	(.006)	(.007)	(.001)	(.003)
% Junior secondary schooling certificate	.004	.010***	.120***	.061***
	(.003)	(.002)	(.004)	(.007)
% Intermediate vocational qualification	.007*	.006	-.004	.002
	(.004)	(.004)	(.003)	(.010)
% Senior secondary schooling certificate or above	.005	.004	.004	.002
	(.007)	(.003)	(.004)	(.007)
Number of observations	78,586	76,976	129,752	117,543

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. Each coefficient comes from a separate regression. The dependent variables are the fraction of individuals who hold a particular academic or vocational credential as their highest qualification. The coefficients are estimated using local linear regressions with a triangular kernel and an optimally selected bandwidth (using the cross validation criterion) on both sides of the cutoff point (school cohorts 1953 in France and 1958 in England & Wales). The French sample is constructed from the 1990 to 2002 French *Enquête Emploi* and includes individuals who were born in France, belong to school cohorts 1944 to 1962, left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. The English and Welsh sample is constructed from the 1993 to 2006 Quarterly Labour Force Surveys and includes individuals who were born in England & Wales, belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967), left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. Standard errors are clustered at the school cohort level.

Table B.9: *First stage, reduced form and 2SLS estimated return of holding of holding a junior high school certificate (GCE O-Level or CSE) on hourly wages in England and Wales. Sources: Quarterly Labour Force Survey (1993-2006).*

England & Wales – Quarterly Labour Force Survey (1993-2006)

	First stage		Reduced form		2SLS	
Dependent variable:	GCE O-level/CSE dummy		Log of hourly wages		Log of hourly wages	
	(1)	(2)	(3)	(4)	(5)	(6)
Women:						
Coefficient	.124***	.123***	.019***	.021***	.151***	.169***
s.e.	(.008)	(.008)	(.003)	(.003)	(.057)	(.056)
Number of observations	75,304	75,304	75,304	75,304	75,304	75,304
Men:						
Coefficient	.054***	.054***	.019**	.019**	.339**	.339**
s.e.	(.001)	(.001)	(.008)	(.008)	(.141)	(.139)
Number of observations	68,535	68,535	68,535	68,535	68,535	68,535
Controls:						
Quartic in age	No	Yes	No	Yes	No	Yes

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. The dependent variables are an indicator that takes the value 1 if the individual's highest credential is a GCE O-Level or a CSE (columns 1 and 2) and the log of hourly wage (columns 3 to 6). Each coefficient comes from a separate regression. All regressions include a fourth order polynomial in school cohort and an intercept shift at the 1958 school cohort. The sample is constructed from the fifth wave of the 1993 to 2006 Quarterly Labour Force Survey and includes wage earners who were born in England & Wales, belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967), left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. Standard errors are clustered at the school cohort level.

Table B.10: *Reduced form effect of the 1972 ROSLA on the literacy scores of British women and men, estimated using Local Linear Regression with triangular kernel. Source: 1994 International Adult Literacy Survey (IALS).*

	Women (1)	Men (2)
Literacy Score:		
Prose literacy	7.34**	14.81*
	(3.31)	(6.59)
Document literacy	10.23**	11.59
	(4.64)	(9.27)
Quantitative literacy	6.84**	15.12*
	(2.75)	(9.20)
Number of observations	830	662

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. Each coefficient comes from a separate regression. The dependent variables are the scores obtained by British participants to the 1994 International Adult Literacy Survey on each of the three literacy scales (prose, document and quantitative literacy), separately for women and men who were born in the UK between 1949 and 1967 and left school between the ages of 6 and 25. The coefficients are estimated using local linear regressions with a triangular kernel and an optimally selected bandwidth (using the cross validation criterion) on both sides of the cutoff point. Scores are in the range 0-500. Individuals born in 1957 are excluded from the sample because some of them (born between January 1 and August 31) faced a minimum school leaving age of 15 while the others (born between September 1 and December 31) faced a minimum school leaving age of 16. Regressions include a fourth order polynomial in birth cohort allowing for an intercept shift at the cutoff point. Standard errors are clustered at the year of birth level.

Table B.11: *Compliance rate by school leaving age and gender for the 1967 Berthoin reform in France and the 1972 ROSLA in England & Wales. Sources:* Enquête Emploi *(1990-2002) and Quarterly Labour Force Survey (1993-2006).*

	Distribution of school leaving ages among the last pre-reform school cohort		Impact of law change	
	Women (1)	Men (2)	Women (3)	Men (4)
France (1967 reform):				
left school at age 13 or below	4.2%	4.3%	-.006**	-.010***
s.e.			(.003)	(.003)
left school at age 14	19.9%	22.6%	-.114***	-.083***
s.e.			(.007)	(.008)
left school at age 15	4.1%	4.9%	-.004	.004
s.e.			(.004)	(.003)
left school at age 16	11.7%	11.8%	.110***	.068***
s.e.			(.008)	(.008)
left school at age 17	13.6%	15.1%	.002	.015***
s.e.			(.008)	(.008)
left school at age 18 or above	46.4%	41.1%	.012	.005
s.e.			(.009)	(.005)
Number of observations	4,540	4,576	83,455	82,466
England & Wales (1972 reform):				
left school at age 13 or below	0.5%	0.3%	-.001	-.000
s.e.			(.001)	(.001)
left school at age 14	1.8%	2.2%	-.016***	-.019***
s.e.			(.002)	(.002)
left school at age 15	27.2%	25.9%	-.241***	-.212***
s.e.			(.006)	(.006)
left school at age 16	30.8%	33.8%	.238***	.216***
s.e.			(.007)	(.007)
left school at age 17	10.4%	8.0%	.008*	.002
s.e.			(.005)	(.005)
left school at age 18 or above	29.4%	29.8%	.012*	.013**
s.e.			(.006)	(.006)
Number of observations	7,089	6,488	141,910	126,821

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. Columns 1 and 2 report the distributions of school leaving ages for women and men belonging to the last pre-reform cohort (1952 in France and 1957 in England & Wales). Columns 3 and 4 report the estimated impact of the compulsory schooling law change on the distribution of school leaving ages. Each coefficient comes from a separate fourth order global polynomial regression allowing for an intercept shift at the cutoff point (1953 in France and 1958 in England & Wales). The French sample is constructed from the 1990 to 2002 French *Enquête Emploi* and includes native and foreign born individuals who belong to school cohorts 1944 to 1962, left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. The English and Welsh sample is constructed from the 1993 to 2006 Quarterly Labour Force Surveys and includes native and foreign born individuals who belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967), left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. Standard errors are clustered at the school cohort level.

Table B.12: *Compliance rate by school leaving age and country of birth for the 1967 Berthoin reform in France and the 1972 ROSLA in England & Wales. Sources:* Enquête Emploi *(1990-2002) and Quarterly Labour Force Survey (1993-2006).*

	Distribution of school leaving ages among the last pre-reform school cohort		Impact of law change	
	Native born (1)	Foreign born (2)	Native born (3)	Foreign born (4)
France (1967 reform):				
left school at age 13 or below	2.0%	37.6%	-.006***	-.002
s.e.			(.002)	(.027)
left school at age 14	21.7%	14.5%	-.105***	-.010
s.e.			(.005)	(.010)
left school at age 15	4.4%	6.0%	.001	-.010
s.e.			(.002)	(.011)
left school at age 16	12.0%	9.2%	.094***	.014
s.e.			(.008)	(.019)
left school at age 17	14.8%	7.9%	.009	-.008
s.e.			(.006)	(.012)
left school at age 18 or above	45.1%	24.8%	.007	.014
s.e.			(.006)	(.015)
Number of observations	8,514	502	155,562	10,359
England & Wales (1972 reform):				
left school at age 13 or below	0.1%	3.2%	-.000	-.006
s.e.			(.000)	(.007)
left school at age 14	1.8%	3.15%	-.018***	-.012*
s.e.			(.001)	(.007)
left school at age 15	28.4%	10.2%	-.243***	-.055***
s.e.			(.004)	(.012)
left school at age 16	33.4%	21.9%	.242***	.060***
s.e.			(.005)	(.017)
left school at age 17	9.0%	11.0%	.007*	-.008
s.e.			(.003)	(.013)
left school at age 18 or above	27.3%	50.6%	.012***	.021
s.e.			(.004)	(.018)
Number of observations	12,405	1,172	247,440	21,291

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. Columns 1 and 2 report the distributions of school leaving ages for native and foreign born individuals belonging to the last pre-reform cohort (1952 in France and 1957 in England & Wales). Columns 3 and 4 report the estimated impact of the compulsory schooling law change on the distribution of school leaving ages. Each coefficient comes from a separate fourth order global polynomial regression allowing for an intercept shift at the cutoff point (1953 in France and 1958 in England & Wales). The French sample is constructed from the 1990 to 2002 French *Enquête Emploi* and includes native and foreign born individuals who belong to school cohorts 1944 to 1962, left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. The English and Welsh sample is constructed from the 1993 to 2006 Quarterly Labour Force Surveys and includes native and foreign born individuals who belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967), left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. Standard errors are clustered at the school cohort level.

Table B.13: *Compliance rate by school leaving age and ethnicity for the 1972 ROSLA in England & Wales. Source: Quarterly Labour Force Survey (1993-2006).*

	Distribution of school leaving ages among the last pre-reform school cohort		Impact of law change	
	Whites (1)	Non-Whites (2)	Whites (3)	Non-Whites (4)
England & Wales (1972 reform):				
left school at age 13 or below	0.1%	1.6%	-.000	-.006
s.e.			(.000)	(.013)
left school at age 14	1.8%	0.8%	-.018***	-.005
s.e.			(.001)	(.014)
left school at age 15	28.4%	23.2%	-.244***	-.173***
s.e.			(.004)	(.047)
left school at age 16	33.5%	28.0%	.242***	.224***
s.e.			(.005)	(.056)
left school at age 17	9.0%	9.6%	.007*	.044
s.e.			(.004)	(.039)
left school at age 18 or above	27.2%	36.8%	.012***	-.083
s.e.			(.004)	(.051)
Number of observations	12,295	110	243,450	3,990

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. Columns 1 and 2 report the distributions of school leaving ages for White and non-White individuals belonging to the last pre-reform cohort in England & Wales (1957). Columns 3 and 4 report the estimated impact of the compulsory schooling law change on the distribution of school leaving ages. Each coefficient comes from a separate fourth order global polynomial regression allowing for an intercept shift at the cutoff point (1953 in France and 1958 in England & Wales). The sample is constructed from the 1993 to 2006 Quarterly Labour Force Surveys and includes individuals who were born in England & Wales, belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967), left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. Standard errors are clustered at the school cohort level.

Table B.14: *Impact of the 1967 French Berthoin reform on the probability of working in the public sector and on hourly wages in the public and private sector. Source:* Enquête Emploi *(1990-2002).*

France – Enquête Emploi (1990-2002)

Sample:	All workers		Public sector employees		Private sector employees	
Dependent variable:	Employed in the public sector		Log of hourly wages		Log of hourly wages	
	(1)	(2)	(3)	(4)	(5)	(6)
Women:						
Coefficient	-.013	-.013	.008	.006	-.001	-.002
s.e.	(.009)	(.009)	(.009)	(.009)	(.009)	(.009)
Number of observations	47,670	47,670	15,386	15,386	32,284	32,284
Men:						
Coefficient	-.009	-.009	-.001	-.003	.002	-.000
s.e.	(.009)	(.009)	(.027)	(.026)	(.008)	(.007)
Number of observations	50,927	50,927	10,655	10,655	40,272	40,272
Controls:						
Quartic in age	No	Yes	No	Yes	No	Yes

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. The dependent variables are an indicator that takes the value 1 if the worker is employed in the public sector (columns 1 and 2) and the log of hourly wage (columns 3 to 6). Each reduced form coefficient comes from a separate fourth order global polynomial regression allowing for an intercept shift at the 1953 cutoff point. The sample is constructed from the 1990 to 2002 French *Enquête Emploi* and includes wage earners who were born in France, belong to school cohorts 1944 to 1962, left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. The samples used in columns 1 and 2 include all wage earners. The subsamples used in columns 3 and 4 include public sector employees only while the subsamples used in columns 5 and 6 include private sector employees only. Standard errors are clustered at the school cohort level.

Table B.15: *Impact of the 1972 ROSLA in England & Wales on on the probability of working in the public sector and on hourly wages in the public and private sector. Source: Quarterly Labour Force Survey (1993-2006).*

England & Wales – Quarterly Labour Force Survey (1993-2006)

Sample:	All workers		Public sector employees		Private sector employees	
Dependent variable:	Employed in the public sector		Log of hourly wages		Log of hourly wages	
	(1)	(2)	(3)	(4)	(5)	(6)
Women:						
Coefficient	.008	.008	.011	.015	.021***	.022***
s.e.	(.008)	(.008)	(.015)	(.015)	(.005)	(.005)
Number of observations	75,304	75,304	27,755	27,755	47,549	47,549
Men:						
Coefficient	-.006	-.006	.014	.014	.021**	.021**
s.e.	(.004)	(.004)	(.018)	(.019)	(.010)	(.010)
Number of observations	68,535	68,535	13,956	13,956	54,579	54,579
Controls:						
Quartic in age	No	Yes	No	Yes	No	Yes

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. The dependent variables are an indicator that takes the value 1 if the worker is employed in the public sector (columns 1 and 2) and the log of hourly wage (columns 3 to 6). Each reduced form coefficient comes from a separate fourth order global polynomial regression allowing for an intercept shift at the 1958 cutoff point. The sample is constructed from the 1993 to 2006 Quarterly Labour Force Survey and includes wage earners who were born in England & Wales, belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967), left full-time education between the ages of 6 and 25 and were aged 25-60 when surveyed. The samples used in columns 1 and 2 include all wage earners. The subsamples used in columns 3 and 4 include public sector employees only while the subsamples used in columns 5 and 6 include private sector employees only. Standard errors are clustered at the school cohort level.

Table B.16: *Reduced form impact of the 1967 Berthoin reform in France and the 1972 ROSLA in England & Wales on the probability of being in employment, for males and females separately. Sources:* Enquête Emploi *(1975-1989) and Annual Labour Force Survey (1979-1991).*

Dependent variable: employed	Women		Men	
	(1)	(2)	(3)	(4)
France (1967 reform):				
Coefficient	.005	.000	-.011	-.000
s.e.	(.009)	(.010)	(.011)	(.007)
Number of observations	84,295	84,295	80,250	80,250
England Wales (1972 reform):				
Coefficient	.001	-.003	-.005	-.003
s.e.	(.005)	(.004)	(.003)	(.004)
Number of observations	185,333	185,333	175,582	175,582
Controls:				
Quartic in age	No	Yes	No	Yes

Notes: *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level. The dependent variable is an indicator for being employed. Each coefficient comes from a separate fourth order global polynomial regression allowing for an intercept shift at the cutoff point (1953 for France and 1958 for England & Wales). Regressions for France (top panel) are performed on a sample constructed from the 1975 to 1989 French *Enquête Emploi* which includes individuals who were born in France, belong to school cohorts 1944 to 1962 and were aged 25-60 when surveyed. Regressions for England & Wales (bottom panel) are performed on a sample constructed from the 1979 to 1991 Annual Labour Force Surveys which includes individuals who were born in England & Wales, belong to school cohorts 1949 to 1967 (i.e. born between September 1, 1948 and August 31, 1967) and were aged 25-60 when surveyed. Standard errors are clustered at the school cohort level.