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Jan Fidrmuc
J. D. Tena

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Abstract

We analyze the impact of the UK national minimum wage (NMW) on the employment of young workers. The previous literature found little evidence of an adverse impact of the NMW on the UK labor market. We focus on the age-related increases in the NMW at 18 and 22 years of age. Using regression discontinuity design, we fail to find any effect of turning 22. However, we find a significant and negative employment effect for male workers at 21, which we believe to be an anticipation effect. We also find a negative effect for both genders upon turning 18. The age-related NMW increases may have an adverse effect on employment of young workers, with this effect possibly occurring already well in advance of reaching the threshold age.

JEL-Code: J210, J310.

Keywords: minimum wage, employment, young workers, regression discontinuity design.

Jan Fidrmuc
Department of Economics and Finance
Brunel University
Uxbridge, UB8 3PH
United Kingdom
Jan.Fidrmuc@brunel.ac.uk
or jan@fidrmuc.net

J. D. Tena
Department of Statistics
University Carlos III
C/Madrid 126
28903 Getafe (Madrid)
Spain
jtena@est.econ.uc3m.es

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1 Introduction

The imposition of a mandatory minimum wage, whether at national, regional or industry level, is a common instrument of economic policy. Most OECD countries impose some form of a minimum wage (Dolton and Rosazza-Bondibene, 2011) and many less developed countries do likewise (even Hong Kong, traditionally a bastion of the laissez-faire approach, introduced a minimum wage in 2010). Nevertheless, the minimum wage is a contentious measure, one that is often blamed for raising workers' earnings at the expense of worsening employment prospects for those out of work. Indeed, standard neoclassical economic theory predicts that, under competitive markets, a wage floor should either have no effect on employment (if set at a sufficiently low rate) or should lower employment by preventing the least productive workers from finding work at market-clearing wages.[1]

To date, the empirical evidence on the employment effect of the minimum wage is equally inconclusive. In a review, Neumark and Wascher (2007) argue that the bulk of the evidence from the US as well as other countries points to a negative employment effect of introducing (or increasing) the minimum wage. Workers who are most likely to be affected by the minimum wage, such as young workers and the low-skilled, experience especially large disemployment effects (nevertheless, the negative effect is mitigated somewhat when young workers are subject to a lower minimum wage rate). The range of estimated elasticities, however, is very broad: from significantly negative to significantly positive. This resonates with the findings of Dolado et al. (1996) who consider the employment effect of minimum wage rules in France, the Netherlands, Spain and the UK. Their estimates range again from negative (especially for young workers) to positive. The meta studies by Card and Krueger (1995b) and Doucouliagos and Stanley (2009), conclude that there is little evidence that the minimum wage lowers employment. Rohlin (2011) considers US firms' location choices and finds that increasing the state minimum wage discourages new firms from locating in the state. Hence, if anything characterizes the current state of the discourse on the employment effect of the minimum wage, it is a lack of consensus.

[1] Once we relax the assumption of competitive markets, however, the theoretical predictions can change dramatically. Assuming monopsony in the labor market, in particular, can result in a positive employment effect of the minimum wage (Dolado et al., 1996): monopsony employer can push wages below the marginal product of labor, thereby maximizing profits while depressing employment. Imposing a wage floor, correspondingly, reduces the employer's profits and increases employment.

The UK introduced the current national minimum wage (NMW) framework relatively late, in April 1999.[2] Thereafter, the NMW has been subject to regular annual revisions, coming into effect every October from 2000 onwards. Since its introduction, the effect of the NMW on employment has been analyzed by a number of studies. Stewart (2004) and Dickens and Draca (2005) consider the effect of the NMW's introduction and the annual increases, respectively. Dolton, Rosazza-Bondibene and Wadsworth (2009) utilize the fact that, unlike the NMW rates, average earnings vary considerably across the regions of the UK. They use the resulting variation in the 'bite' of the NMW at the regional level to assess its impact on employment. Invariably, these studies (as well as others not cited here) find little evidence that the UK NMW has had an adverse effect on employment. The main (and probably only) exception is a recent study by Dickens, Riley and Wilkinson (2012) who present evidence that the introduction and annual NMW increases reduce the employment of part-time women, a segment of the labor market that is especially exposed to the minimum wage.

In this paper, we seek to contribute further to this discussion. We focus on a particular institutional feature of the UK minimum wage regulation: the existence of separate (lower) rates for young workers. At its introduction in 1999, the NMW was formulated with two distinct rates: the adult rate for workers aged 22 and over, and the so-called development rate for those between 18 and 21 years of age.[3] In 2004, an additional rate was introduced for those aged 16 and 17 who were not subject to the NMW until then. The ratio between the adult rate and the development rate has been approximately 1.2 while the ratio between the development rate and the 16/17 rate has been approximately 1.35. This means that young workers earning the NMW rate relevant for their age are subjected to a sharp wage increase upon turning 18 and then again at 22. While productivity is likely to increase with age, workers who are 22 or 18 are at best only slightly more productive than those one year younger. The fact that workers on either side of the cutoff ages are eligible for substantially different NMW rates creates a quasi-experimental setting that can be analyzed by means of regression discontinuity design (henceforth RDD; see Imbens and Lee, 2008; van der Klaauw, 2008; Lee and Lemieux, 2010). Arguably, the characteristics of workers on either side of the cutoff age are very similar and therefore the main difference between them is the applicable

[2] Until 1993, the Wages Councils had the power to set minimum wages for specific industries (although not all industries had a Wages Council). No minimum wage was in place in the period between 1993 and 1999.

[3] From October 2010, the upper limit for the development rate has been lowered to 20. The data used in our analysis, however, pertain to the period before this change.

NMW rate.[4] The forcing variable, age, can be influenced neither by the workers nor by their employers (or anyone else, for that matter). Therefore, when comparing workers who are just above the cutoff age and those just below, the difference between them is as good as random. The 'treatment' category then consists of young workers older than the cutoff age while the rest constitute the 'control' group. We apply therefore the RDD methodology to determine whether the age-related minimum-wage increases have any negative effect on the employment prospects of workers as they turn 22 or 18.

Our work extends earlier research by Dickens, Riley and Wilkinson (2010, henceforth DRW) who consider the effect of age-related increases in the NMW on the employment of low-skilled young workers in the UK using also the regression discontinuity design. They find, somewhat surprisingly, that low-skilled young workers are significantly more likely to be employed and significantly less likely to be either unemployed or out of the labor force as they turn 22. They attribute this to an increase in their labor supply: if the development rate is below the reservation wage of some workers, such workers postpone their labor market entry until they can be certain of earning at least the adult NMW rate. However, the result disappears when they consider all workers rather than only the low-skilled ones.[5] The latter result is especially peculiar as they find the NMW to have a positive effect for those workers who are most likely to be paid the minimum wage and should therefore be more adversely affected than young workers overall.

In this paper, we revisit and extend the result of DRW (2010). Our analysis differs from theirs in a number of important aspects. First, we consider all workers rather than only the low-skilled ones. Young workers are generally more likely to be subject to the minimum wage, more or less independently of their skill level. DRW indeed report that the shares of low and high skilled workers paid the minimum wage are only marginally different from one another: 10% of high skilled vs 11% of low skilled workers earn less than the adult rate at the age of 21.[6] Second, while we also follow the regression discontinuity approach, we argue that the discontinuity effect can take two forms: besides the usual level (jump) effect on the

[4] In most of our analysis, we focus on those subject to the 18-21 rate. The workers aged 16-17 differ from their older counterparts in several important ways: they are more likely to be in full-time education, their employability is lowered by restrictions such as not being allowed to sell alcoholic beverages and their eligibility to benefits is more limited. Therefore, it is difficult to discern whether any employment effects that may occur upon turning 18 are due to becoming eligible to the higher NMW rate or whether they are entirely attributable to the age effect.

[5] Low skilled workers are defined as those whose qualifications are no higher than the GCSE exams (equivalent to incomplete high school).

[6] Table 3 (p. 26), Dickens, Riley and Wilkinson (2010).

probability of employment, there can be also a slope (kink) effect (see Dong, 2012). Third, we recognize that while the case that we consider is of quasi-experimental nature, the treatment occurs due to a deterministic rather than random process (aging).[7] Therefore, young workers' labor market outcomes can be affected by the higher NMW rate at or before the cutoff age. To account for this, we estimate the discontinuity effect not only at the cutoff ages of 18 and 22 but for every month of age between 18 and 23. Finally, and rather trivially, our analysis is based on an extended data set relative to the one used by DRW.[8]

Our results are intriguing. In contrast to DRW (2010), we find that turning 22 has no effect on employment. Instead, we find that male workers are less likely to be employed when they are around 21 years old. This is consistent with employers anticipating the wage hike that would occur at 22 and dismissing or not hiring workers approaching that threshold. In addition, we find also a negative effect of turning 18; moreover, that negative effect is found both for males and females at this age.

The next Section presents the data used in our analysis. The results of the discontinuity analysis are in Section 3. Section 4 concludes the paper by summarizing the results and suggesting some tentative avenues for further work.

2 Data and Methodology

We investigate the issue at hand using the UK Labour Force Survey (LFS), a quarterly nationally-representative survey of households across the UK. Each quarter, it reports on approximately 60 thousand households and over 100 thousand individuals aged 16 and above. Each household is retained in the survey for five consecutive quarters, with one-fifth of households replaced in each wave. The survey contains detailed demographic and socio-economic information on the respondents, including their labor-market outcomes. As the NMW was introduced in April 1999, we use all quarterly datasets available from April-June 1999 to October-December 2009.

The LFS contains information on the precise date of birth of every respondent.[9] We use this information to compute the age of each individual in months. We also have the date the survey was carried out. By comparing these two dates, we can determine the precise age of

[7] See section 6.3.1 in Lee and Lemieux (2010).

[8] We extend the data by two quarters. This does not have a material effect, as we are able to replicate DRW's results in our extended data set when we follow their methodology.

[9] This information is not available in the publicly released LFS datasets. We are grateful to the Low Pay Commission and the Office for National Statistics for giving us access to the restricted release of the LFS.

each respondents in months on the day when the survey was carried out (even when their birthday falls within the month in which they were interviewed). We start by placing the cutoff point at the young workers' 22nd birthday. As is common in the regression-discontinuity literature, we redefine age so that it equals 0 in the month when the individual reaches the cutoff age. That is, instead of age we use *age–264*, where age is expressed in months. Although each LFS quarterly data set contains information on around 100 thousand individuals, only a relatively small fraction of them are close to the cutoff age. Therefore, we consider the widest possible observation window: workers whose ages are between 15 months below and 15 months above the cutoff age (recall that each worker appears in the LFS for five quarters, or 15 months). As a robustness checks, we replicate the analysis also for 12 and 6 month before-and-after windows.

The regression discontinuity design is concerned with determining how the outcome of interest (labor-market status in this case) changes when individuals pass the relevant cutoff point (18 or 22 years of age). The RDD method, however, assumes that the forcing variable, age, is continuous. If this assumption is met, then we can compare outcomes observed in an arbitrarily small neighborhood around the cutoff, with *age* approaching 0 (recall that the forcing variable, *age* is defined as age less the cutoff age). Age, however, is as a discrete rather than continuous variable. Lee and Card (2008) argue that this introduces uncertainty in the choice of functional forms in regression discontinuity designs. In this setting, it is no longer possible to estimate the impact of a covariate on the dependent variable by simply computing averages within arbitrarily small neighborhoods of the cutoff point, even with an infinite amount of data. Instead, it is necessary to choose a particular functional form for the model relating the outcomes of interest to the forcing variable. Of course, it has to be tested whether the specification error of the proposed functional form is not significantly different from a fully flexible functional form that allows for different impacts of the discrete values of the covariate for each different age.

In a standard RDD specification, we would estimate $E[Y_1 - Y_0|X_i = 0]$, where Y_0 and Y_1 are the pre-treatment and post-treatment outcomes of interest, respectively, evaluated at the cutoff of the forcing variable, $X_i = 0$. Note that Y_0 and Y_1 can be described by the following functions

$$E[Y_1|X_i = 0] = \theta + \alpha^* * X_i + \beta * d + \varepsilon_1 \qquad (1)$$

$$E[Y_0|X_i = 0] = \theta + \alpha * X_i + \varepsilon_0 \qquad (2)$$

where θ includes the constant and any other covariates and d is a dummy taking value 0 before and 1 after the cutoff. Note also that

$$Y = d * Y_1 + (1 - d) * Y_{0.}$$

The standard approach therefore is concerned with identifying the change in the mean outcome associated with a discrete change in the threshold variable, i.e. $E[Y_1 - Y_0 | X_i = 0]$. This can be estimated using the following functional form (see Lee and Card, 2008):

$$E[Y | X_i = 0] = \theta + \alpha * X_i * (1 - d) + \alpha^* * X_i * d + \beta * d + \varepsilon \qquad (3)$$

where Y is the variable of interest, X_i is the forcing variable less the cutoff, and $\varepsilon = d * \varepsilon_1 + (1 - d) * \varepsilon_0$. When evaluated at $X_i = 0$, the discontinuity effect is captured by the coefficient estimate of β.

Nevertheless, recent literature points out that the discontinuity effect may not be limited to β. In particular, the discontinuity may be associated with a slope change in addition to, or instead of, a jump in the intercept of the response function at the cutoff point. This possibility is discussed in detail in Dong (2011) who outlines the two possibilities formally and then presents evidence of kink effects with respect to the take-up of early retirement in the US. Other studies offer analogous findings. Jacob and Lefgren (2004) study the impact of remedial education programs on academic performance. They find, similarly, evidence of a slope change instead of a level effect at the cutoff. Card et al. (2008) show that the change in the probability of retirement at 65 (the age of Medicare eligibility) is again more consistent with a change in the slope than with a level effect. Card et al. (2009) label this approach ‘Regression Kink Design (RKD)’. Theorem 1 in Dong (2011) generalizes these arguments by showing that in cases where there is no jump (level effect), the treatment effect is equal to the ratio of the kink in the outcome function and the corresponding kink in the probability of the treatment. She points out that a slope effect is especially likely in situations when the treatment is assigned gradually, for example when there is discretion about taking up benefits or retiring.

We therefore consider both types of discontinuity effects: the level (jump) effect and the change in slope (kink effect). More specifically, the outcome of interest is the probability of being employed, unemployed or inactive at the cutoff age. We estimate the following equation:

$$E[y | age, d] = \mathrm{F}(\theta + \alpha_0 * age_i * (1 - d) + \alpha_1 * age_i^2 * (1 - d) + \alpha_0^* * age_i * d + \alpha_1^* * age_i^2 * d + \beta * d) = \mathrm{F(u)} \qquad (4)$$

where y_i is equal to one if the individual is employed (unemployed, inactive), F is a standard normal cumulative distribution function, age_i is the age in months less the cutoff, d is a dummy variable equal to one when the individual's is at the cutoff age or older and θ again includes any remaining terms such as the constant and the covariates (qualifications, ethnic origin, apprenticeship, region of usual residence and being full time student). We allow for the effect of age to be different before and after the young workers attain the threshold age. This is standard in the regression discontinuity approach, reflecting the fact that the effect of the forcing variables may change after the cutoff. If we did not allow different slope coefficients, the pre-cutoff and post-cutoff relationships would be estimated using information contained in the both parts of the sample: those pertaining to the pre-treatment sub-sample would be estimated using information affected by the treatment and vice versa (see Lee and Lemieux, 2010). Age takes the form of a quadratic polynomial which we test against an alternatives fully-flexible specification with each age in months captured by a separate dummy.

In expression (4), the jump in the probability of a particular employment status at the cutoff point (level effect) is measured as the marginal effect associated with the disctontinuity dummy, d. However, because F is a non-linear (probit) function, computing the change in the slope is more complicated than merely comparing the coefficients of the age polynomial before and after the cutoff (α_0 and α_1 vs α_0^* and α_1^*). Norton *et al.* (2004) show how to evaluate the marginal effect for probit models and we adapt this procedure to our particular case:

$$\frac{\Delta \frac{\Delta F(.)}{\Delta age}}{\Delta d} = F(\theta + \beta) - F(\theta - \alpha_0^* + \alpha_1^* + \beta) - F(\theta) + F(\theta - \alpha_0 + \alpha_1) \quad (5)$$

Note that we evaluate this expression by double-differentiating the functional form at age equal 0 and -1 and at d equal 1 and 0. For robustness we also treat age as a continuous variable and compute the slope change as the difference of the derivative of the response function at d equal 1 and 0 but it does not change our findings (these results are available under request).

An important issue to point out is that in our particular model this interaction effect could be nonzero even if $\alpha_0 = \alpha_0^*$ and $\alpha_1 = \alpha_1^*$. This is because of non-linearity which implies that the marginal effect of age depends also on the parameter β. Therefore, expression (5) provides a more complete picture of the discontinuity effect than that provided simply by β in the traditional level effect. The traditional approach, instead, focuses only on the jump effect and thus ignores the fact that the slope coefficient can change at the cutoff as well. The traditional approach, instead, focuses only on the jump effect and thus ignores the fact that the slope coefficient can change at the cutoff as well.

3 NMW and Young Workers

To assess the impact of age-related MNW increases, we start by looking at individuals on either side of 22 years of age (corresponding to 264 months). Table 1 reports regression results for the probability of being employed. We present estimates for males and females separately as well as for both genders together, and with and without additional covariates. Unlike DRW (2010), we consider all individuals, regardless of their skill level: as we argued above, both skilled and unskilled young workers have very similar propensities to be paid the NMW. Specification (4) is tested against a fully flexible functional form. For men we cannot reject that both specifications are significantly different at the conventional levels while for women the quadratic specification was rejected, in which case we also considered a cubic specification with no material change in the results. The row denoted *discontinuity* reports the slope (kink) marginal effect at the discontinuity, as given by equation (5). *Dummy*, in contrast, stands for the marginal effect of d. DRW only consider the sign and significance of this latter effect, which as we argue above ignores a potentially important part of the discontinuity effect. However, neither the slope effect nor the discontinuity dummy on its own are significant when workers turn 22. This is in line with the findings of DRW who also report an insignificant result when they include all individuals rather than only the low-skilled ones.

For the sake of comparability, we also replicate DRW's analysis of low-skilled workers: these are those who left school at the age of 16 after completing their GCSEs and those who report having no qualifications. DRW found a significant positive effect of turning 22 for low-skilled workers, suggesting that becoming eligible for the adult NMW rate increases rather than reduces their employment. Our results replicating their analysis are summarized in Table 2. They are broadly in line with those of DRW but somewhat weaker.[10] In particular, while the discontinuity dummy is always positive, it is never significant for females, and for males and for all workers it is significant only in the 5-10% range. More importantly, the combined level and slope effect is never even close to being significant. We are therefore unable to

[10] Note that while we attempt to replicate DRW's results, there are some potentially important differences between their analysis and ours. In particular, we consider a 15-month window before/after the individual's 22nd birthday while they only consider 12 months, we compute the age in months slightly differently as discussed above, our data include three additional quarters in 2009, and, finally, although we sought to include the same covariates as them, it is possible that some of the covariates may be coded or formatted differently.

confirm their finding of a positive employment effect of turning 22 and becoming eligible for the adult NMW rate.

Next, Tables 3 and 4 present the regression results for unemployment and inactivity, considering again all workers regardless of their skill level. As before, the slope effect of the discontinuity is never significant. Note however that the dummy alone is significant and negative in the regressions for unemployment with all individuals: this mirrors the similar finding of DRW. As we argue above, accepting this as the only effect of the discontinuity would be wrong as it ignores the fact that the effect of the age polynomial also changes upon surpassing the age threshold.

In summary, we find thus no evidence that the approximately 20% increase in the rate of the NMW at the age of 22 has any effect – whether positive or negative – on young workers' employment, unemployment or inactivity. This conclusion does not depend on whether we consider all young workers or only the unskilled ones.

To probe the NMW effect on young workers further, we undertake a number of extensions. In Table 5, we consider the effect of turning 22 on employment conditional on the individual's employment status (employed, unemployed or inactive) in the previous quarter. It may well be that the increase in the NMW rate that applies to workers as they reach their 22nd birthday affects employed and unemployed workers differently: while some of those who were employed at 21 may lose their jobs, others may only enter the labor market or intensify their job search attracted by the higher NMW rate. If this is the case, then the effects, presented in Table 1, could be insignificant because these two kinds of effects cancel out. The analysis is again presented separately for males and females (to save on space, we omit the results for both genders). In the first two columns of Table 5, we present the estimates for the probability of remaining employed, conditional on being previously employed. The estimated effect of turning 22 is negative, especially for men, but it is not even close to being significant at conventionally accepted levels. Hence, young workers who were employed at the age of 21 are not more or less likely to be employed after their 22nd birthday. The next two columns present the estimates of the probability of being employed at 22, conditional on being unemployed before. The last two columns, in turn, present the corresponding estimates for those who were inactive before the quarter in which they turned 22. Again, none of these coefficients are significant, suggesting that controlling for the labor market status of young workers just before they turn 22 makes little difference to our findings.

In Table 6, we consider only those young workers who earn less than the adult rate when they are 21. Such workers are bound to be affected by the age-mandated increase in the NMW upon turning 22. The previous analyses, in contrast, included all workers, regardless of whether their wages had to be raised or not. As before, we are unable to find any significant discontinuity effect (level or slope) on employment probability. One drawback of this analysis, however, is the rather small sample size, which may be responsible for the lack of significant results.

As the last robustness check, we repeat the discontinuity analysis for workers turning 21 and 23 years of age (Table 7). The finding of no significant effect at 22 years of age may be either attributed to the NMW having no impact on employment, or it may indicate that the employment effect does not coincide with the workers' 22nd birthdays. In particular, age is a deterministic process and employers can take action motivated by workers reaching a particular age before or after they actually attain that age. This is indeed what appears to happen: the slope effect suggests that male workers are significantly less likely to remain employed after turning 21. In contrast, reaching their 23rd birthday has no significant impact on employment of males or females. Note that this negative result only appears when we consider the slope effect; the level effect is not significant. This again highlights the importance of assessing both effects of the discontinuity rather than considering only the coefficient of the discontinuity dummy.[11]

The fall in employment probability at 21 for men may be an anticipation effect: employers are aware of the age-related NMW increase that young workers are entitled to after their 22nd birthday and dismiss them well in advance of the relevant date and/or they refrain from hiring workers between the ages of 21 and 22. We pursue this possibility further and repeat our analysis for every age in one-month increments between 18 and 23 years. Since we estimate dozens of coefficients, it is more instructive to depict the results graphically. Figure 1 presents the slope effect for males, Figures 2 and 3 summarize the findings for females (using quadratic and cubic age polynomial, respectively) and Figure 4 features those for both genders combined. The solid line captures the employment probability while the dotted lines correspond to the 95% confidence interval. An interesting pattern emerges. The employment

[11] We replicate the discontinuity analysis at 21st, 22nd and 23rd birthday with 6 and 12 month estimation windows instead of 15 months (see the Appendix). The results obtained with the 6 month window are never significant. This may be due to the lower number of observations when using the shorter estimation window. Moreover, the discontinuity effect may take time to become sufficiently pronounced. The regressions with the 12 month window generally paint the same picture as those discussed above. In particular, the discontinuity effect is negative both at the age of 21 and 22 for males: the former is significant at 10% while the latter is not significant.

probability goes up and down, occasionally being significant positive or negative. Most of these upsurges and dips are not very pronounced and tend to be observed only for a very short period. This is to be expected, given that we estimate a relatively large number of coefficients. We observe, nevertheless, a significantly negative employment probability for both males and females when they are 18 (we return to this below). Thereafter, the effect appears consistently positive for both males and females (the latter when age is accounted for with a quadratic polynomial) for several months when they are between 18 and 19 years old: this is likely attributable to the end of full-time secondary education. Then, the employment probability is negative for young males for some five months around their 21st birthday; no such effect is observed for females at this or any other age.[12]

We can only speculate what drives these results. The age-related NMW rates apply equally to men and women yet we only observe negative employment effect for the former. This may reflect the fact that the labor market positions of men and women are substantially different. As we argued above, the negative effect around men's 21st birthday may be due to anticipation effects whereby employers choose to dismiss workers in advance of the age at which the NMW is set to increase. As age is an entirely deterministic process that all young workers are subject to, the effect on employment can indeed occur at any time before the discontinuity.

An alternative explanation could be that the negative effect around the 21st birthday is due to an influx of university graduates into the job market which increases the competition for jobs. However, while it is true that university students graduate when they are 21 (assuming they went to university immediately after completing secondary education), the bulk of them enter the job market in the summer or autumn after graduation. They would therefore reach 21 years of age during their final year in university and only a small fraction of them would be turning 21 exactly at the time when they graduate.

Finally, we also consider the NMW threshold at 18 years of age. Recall that those turning 18 become eligible for the development rate which historically has been some 35% above the 16-17 rate. As before, we consider all workers, irrespective of skills (although the differences in skill levels at this age are not particularly large). Table 8 reports the results. Turning 18 is associated with a significantly negative slope effect for both genders (as is already apparent in the Figures): becoming eligible for the higher NMW rate is associated with lower

[12] Moreover, the quadratic age polynomial is rejected by the model, as is also the cubic alternative (the latter results, presented in Figure 3, also yield insignificant effects).

employment probability. Note that again this negative effect is observed only when we consider the slope effect: the dummy itself is not significantly different from zero (except for females). The insignificant coefficient for the discontinuity dummy is in line with the finding of DRW. The differences in the conclusions reached when considering the discontinuity dummy only and when looking also at the changed effects of the age polynomial again underscores the importance of assessing the full effect of the discontinuity.

As we argued before, turning 18 is associated with a host of other important changes besides becoming eligible for a higher NMW rate. For example, UK law requires anyone selling or serving alcohol to be 18 or older, which makes those under 18 ineligible to work in bars, restaurants and many shops. This makes the negative effect that we found all the more remarkable. An alternative explanation would link the effect that we observe to the end of full-time secondary education. In the UK, education is currently compulsory until the age of 16 but many students stay enrolled for another two years to complete their secondary education. Those who do so without enrolling in higher education upon graduating then generally enter the job market when aged 18. This may explain why the employment probability first dips around the 18th birthday and then rises, both for males and females.

Note that our analysis is based on estimating the functional form in expression (4). However, it is also relevant to study if the main conclusions in the paper are upheld when we adopt a specification similar to (4) but imposing the restrictions $\alpha_0 = \alpha_0^*$ and $\alpha_1 = \alpha_1^*$. Although we prefer specification (4) because it already encompassed this restricted case and also it allows for comparison with DRW, the constrained version of the model is interesting since it allows us to test the contribution of allowing slope parameters to change at the threshold age on the estimation of the jump effect. Under the restricted model for all workers, we also find strong evidence of a negative jump effect of NMW on the probability of employment at 18 (-0.02 with a p-value of 0.001). Moreover, the impact at 21 is negative but only marginally significant (-0.009 with a p-value of 0.097). Finally, we find no significant effect at 22 (0.002 with a p-value of 0.39) and a significant and positive impact at 23 (0.01 with p-value:0.009). These results suggest that the main reason for not finding a significant jump effect at 22 or an effect at 18 in the unrestricted model is the fact that slope parameters are allowed to change before and after the threshold.

5 Conclusions

The received wisdom in the UK concerning the national minimum wage is that it has had little adverse impact on employment. In this paper, we revisit this issue. We consider young workers and investigate whether their employment prospects are affected by the fact that different rates apply to different age groups: during the period covered by our analysis, the NMW featured different rates for those who are 16-17, 18-21 and more than 22 years old. Using the regression-discontinuity approach, we find that although the effect of turning 22 is negative, it is not statistically significant. This contrasts with an earlier finding by Dickens, Riley and Wilkinson (2010) who argued that becoming eligible for the higher adult rate from the age of 22 increases the employment of unskilled young workers. We believe their finding is potentially flawed because they do not take into account the fact that the effect of age on employment probability also changes at discontinuity. Specifically, their analysis (as ours) estimates a non-linear (probit) function featuring an age polynomial and the discontinuity dummy. Because of this, the marginal effect of the discontinuity, when evaluated correctly, features coefficients estimated both for the dummy and age. Furthermore, the discontinuity can affect the employment probability by causing either a level or slope effect. Dickens, Riley and Wilkinson only consider the former effect. When we account for the slope effect, we find that turning 22 has no effect on the employment of young workers, whether they are unskilled or skilled.

In contrast, we do find evidence of a negative employment effect for males at the age of 21. While in the period we have studied the NMW does not change at this age, we believe this result may be driven by the anticipation of the later minimum wage increase at 22. This reflects the specific nature of the case that we, and Dickens, Riley and Wilkinson, consider. While the regression discontinuity approach is usually used to study the effects of outcomes that are assigned (approximately) randomly, there is nothing random about the outcome in this case: all young workers eventually turn 22. The effect associated with the discontinuity (higher NMW rate applying to those aged 22 and above) therefore can occur anywhere in the neighborhood of the cutoff age, whether before or after. The fact that we find a negative effect approximately one year before it should occur intuitively makes sense. The cost of hiring a 21-year old is substantially lower only for employers seeking short-term staff; those wishing to retain this worker in the long term would enjoy only a temporary cost advantage.

Our findings thus suggest that the age specific minimum wage rates do affect employment. This is confirmed also by our finding that both genders experience a negative employment effect at the age of 18, when they become eligible for the 18-21 NMW rate (35% higher than the 16-17 rate).

The UK NMW rules concerning young workers were modified in October 2010 in that the threshold age for the adult rate has been lowered from 22 to 21. Future research will show how this has affected the employment prospects of young workers. Our findings would suggest that the age at which this effect occurs may shift further so that even workers younger than 21 may see their employment prospects diminished.

Finally, our work has two important methodological implications. First, it underscores that when applying the regression discontinuity approach to non-random deterministic processes through time, the effect need not coincide with the discontinuity. Instead, it can occur either before or after the discontinuity is reached. Second, it is important to correctly account for the effect of the regression discontinuity in cases when it can entail both level and slope effects. In particular, the negative employment effects that we find at 18 and 21 are only apparent when we consider both the slope effect.

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Table 1 Discontinuity Effect on Employment: All Young Workers. Marginal effects at mean values and standard deviations between brackets.

	All		Males		Females	
	with covariates	without covariates	with covariates	without covariates	with covariates	without covariates
Discontinuity[(1)]	.00122 (.00244)	.00227 (.00236)	-.00228 (.00331)	.00055 (.00328)	.00368 (.00353)	.00356 (.00336)
Dum[(2)]	.00482 (.00800)	.00480 (.00772)	.00567 (.01097)	.00502 (.0107)	.00589 (.01154)	.00348 (.01103)
No. observations	136,591	136,591	66,582	66,582	70,009	70,009
Chi-statistic for Whole regression	26345.97	638.70	15412.56	480.74	12942.46	218.54
Pr>Chi	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.1524	0.0037	0.1918	0.0060	0.1411	0.0024
Chi-statistic for quadratic	27.11	29.11	27.55	. 34.08	44.13	53.25
Pr>Chi	0.3503	0.2539	0.3292	0.1063	0.0105	0.0008

Notes: (1) estimated discontinuity effect taking into account the impact of age and the threshold dummy variable; (2) estimated impact of the threshold dummy variable. Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 2 Discontinuity Effect on Employment: Low Skilled Young Workers. Marginal effects at mean values and standard deviations between brackets.

	All		Males		Females	
	with covariates	without covariates	with covariates	without covariates	with covariates	without covariates
Discontinuity[(1)]	.00211 (.00418)	.00224 (.00415)	.00214 (.00555)	.00270 (.00561)	.00061 (.00595)	.00193 (.00589)
Dum[(2)]	.02940 (.01402)*	.02241 (01386)	.03380 (.01852)	.02807 (.01859)	.02486 (.02002)	.01822 (.01971)
No. observations	43809	43809	20457	20457	23352	23352
Chi-statistic for Whole regression	2686.26	3.24	1621.56	42.32	1174.80	14.47
Pr>Chi	0.0000	0.6633	0.0000	0.0000	0.0000	0.0129
R2	0.0478	0.0001	0.0705	0.0018	0.0370	0.0005
Chi-statistic for quadratic	45.31	43.99	24.89	30.52	61.38	58.20
Pr>Chi	0.0077	0.0109	0.4683	0.2054	0.0001	0.0002

Notes: (1) estimated discontinuity effect taking into account the impact of age and the threshold dummy variable; (2) estimated impact of the threshold dummy variable. Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 3 Discontinuity Effect on Unemployment. Marginal effects at mean values and standard deviations between brackets.

	All		Males		Females	
	with covariates	without covariates	with covariates	without covariates	with covariates	without covariates
Discontinuity[(1)]	.00118 (.00126)	.00107 (.00135)	.00190 (.00195)	.00175 (.00212)	.00037 (.00160)	.000200 (.00170)
Dum[(2)]	-.008830 (.00425)*	-.00919 (.00452)*	-.01013 (.00659)	-.01104 (.0071)	-.00844 (.00535)	-.00819 (.00565)
No. observations	136,591	136,591	66,582	66,582	70,009	70,009
Chi-statistic for Whole regression	3489.80	61.34	2721.18	44.54	1170.22	15.95
Pr>Chi	0.0000	0.0000	0.0000	0.0000	0.0000	0.0070
R2	0.0446	0.0008	0.0621	0.0010	0.0347	0.0005
Chi-statistic for quadratic	19.40	15.69	26.00	23.85	23.16	20.95
Pr>Chi	0.7776	0.9237	0.4078	0.5278	0.5682	0.6955

Notes: (1) estimated discontinuity effect taking into account the impact of age and the threshold dummy variable; (2) estimated impact of the threshold dummy variable. Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 4 Discontinuity Effect on Inactivity. Marginal effects at mean values and standard deviations between brackets.

	All		Males		Females	
	with covariates	without covariates	with covariates	without covariates	with covariates	without covariates
Discontinuity[1]	-.00151 (.00160)	-.00347 (.00220)	.00038 (.00249)	-.00252 (.00291)	-.00451 (.00334)	-.00389 (.00323)
Dum[2]	.00539 (.00698)	.00444 (.00705)	.00695 (.00819)	.00615 (.00919)	.00287 (.01072)	.00474 (.01047)
No. observations	136,591	136,591	66,582	66,582	70,009	70,009
Chi-statistic for Whole regression	29973.84	541.74	20380.64	446.08	13752.84	189.13
Pr>Chi	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.1971	0.0036	0.3135	0.0069	0.1614	0.0022
Chi-statistic for quadratic	21.83	25.18	27.69	24.00	30.59	46.73
Pr>Chi	0.6455	0.4521	0.3225	0.5194	0.2030	0.0053

Notes: (1) estimated discontinuity effect taking into account the impact of age and the threshold dummy variable; (2) estimated impact of the threshold dummy variable. Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 5 Probability of Employment Conditional on Employment Status in Previous Quarter. Marginal effects at mean values and standard deviations between brackets.

	All		Males		Females	
	with covariates	without covariates	with covariates	without covariates	with covariates	without covariates
Discontinuity(1)	-.00184 (.00158)	-.00004 (.00181)	-.01189 (.00936)	.01636 (.01102)	.00030 (.00663)	-.00500 (.00518)
Dum(2)	.00483 (.00822)	.00114 (.00843)	-.01864 (.04345)	.01636 (.05514)	.03364 (.02418)	.02886 (.01552)
No. observations	27921	26030	3956	2671	6795	11815
Chi-statistic for Whole regression	42.09	30.76	7.89	11.21	7.48	10.13
Pr>Chi	0.0000	0.0000	0.1625	0.0473	0.1876	0.0716
R2	0.0037	0.0029	0.0017	0.0033	0.0016	0.0014

Notes: (1) estimated discontinuity effect taking into account the impact of age and the threshold dummy variable; (2) estimated impact of the threshold dummy variable. Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 6 Probability of Employment for Workers Earning Less than Adult Rate. Marginal effects at mean values and standard deviations between brackets.

	Males	Females
Discontinuity[(1)]	.000242 (.01783)	-.00684 (.01279)
Dum[(2)]	.014173 (.04104)	.008331 (.03483)
No. observations	1365	1931
Chi-statistic for Whole regression	4.06	7.96
Pr>Chi	0.5404	0.1582
R2	0.0047	0.0066

Notes: None of the estimations include covariates. (1) estimated discontinuity effect taking into account the impact of age and the threshold dummy variable; (2) estimated impact of the threshold dummy variable.

Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey. The regressions do not contain additional control variables due to low number of observations.

Table 7 Falsification Tests: Discontinuity Effects at 21 and 23. Marginal effects at mean values and standard deviations between brackets.

	21 years		23 years	
	Males	Females	Males	Females
Discontinuity[(1)]	-.00994	-.001039	.00435	-.00179
	(.00326)**	(.00349)	(.00318)	(.00336)
Dum[(2)]	-.00764	-.00186	.01043	-.01325
	(.01150)	(.01184)	(.01023)	(.01138)
No. observations	68324	70647	65206	70622
Chi-statistic for Whole regression	17001.14	12155.02	13443.49	14310.83
Pr>Chi	0.0000	0.0000	0.0000	0.0000
R2	0.1947	0.11285	0.1879	0.1602

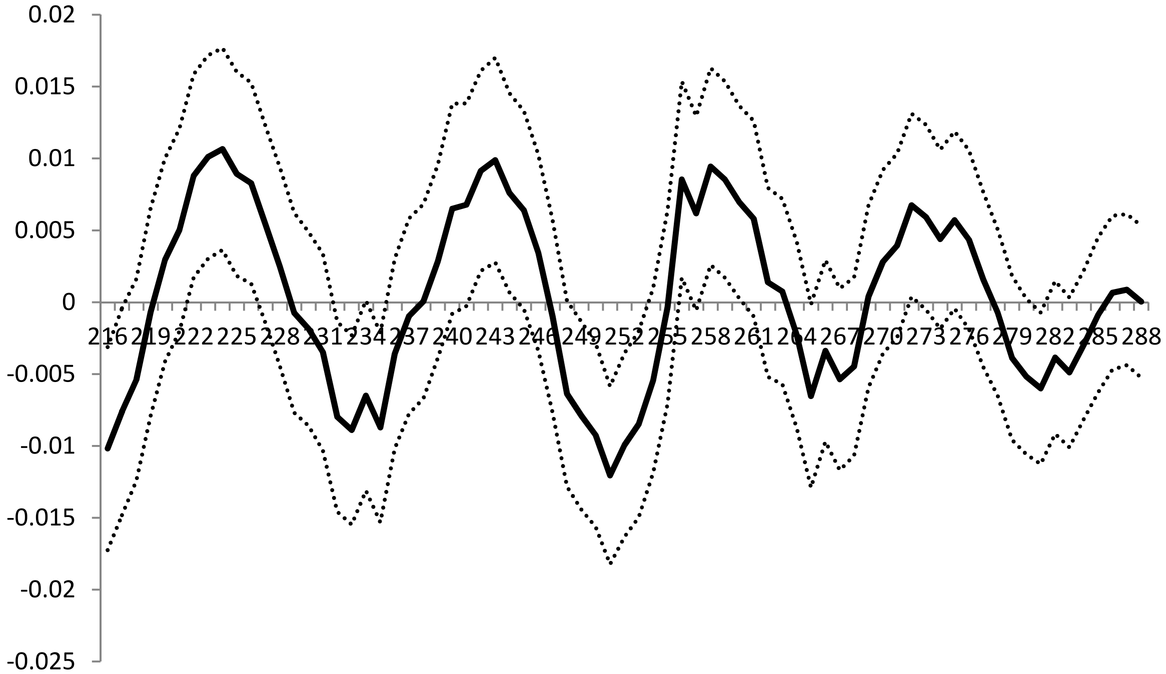
Notes: All the estimations include covariates. (1) estimated discontinuity effect taking into account the impact of age and the threshold dummy variable; (2) estimated impact of the threshold dummy variable. Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 8 Discontinuity Effects at 18. Marginal effects at mean values and standard deviations between brackets.

	Males	Females	All
Discontinuity[(1)]	-0.01018 (0.00361)**	-.01009 (.00362)**	-0.00984 (0.00255)**
Dum[(2)]	-0.00238 (0.01253)	-.0253495 (.01263)*	-.012706 (0.00888)
No. observations	67641	65023	132664
Chi-statistic for Whole regression	16587.27	9896.45	25665.83
Pr>Chi	0.0000	0.0000	0.000
R2	0.1788	0.1110	0.1410

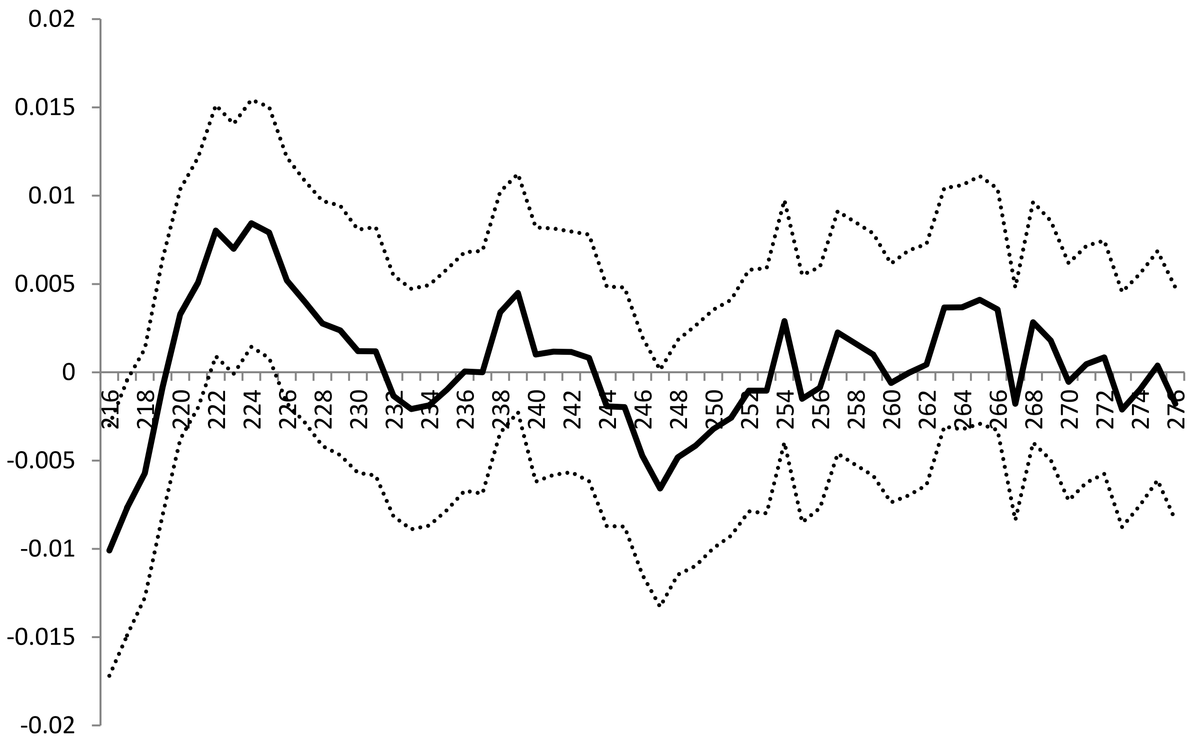
Notes: All the estimations include covariates. (1) estimated discontinuity effect taking into account the impact of age and the threshold dummy variable; (2) estimated impact of the threshold dummy variable. Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Figure 1 Discontinuity Effects by Month, Ages 18 to 24, Males



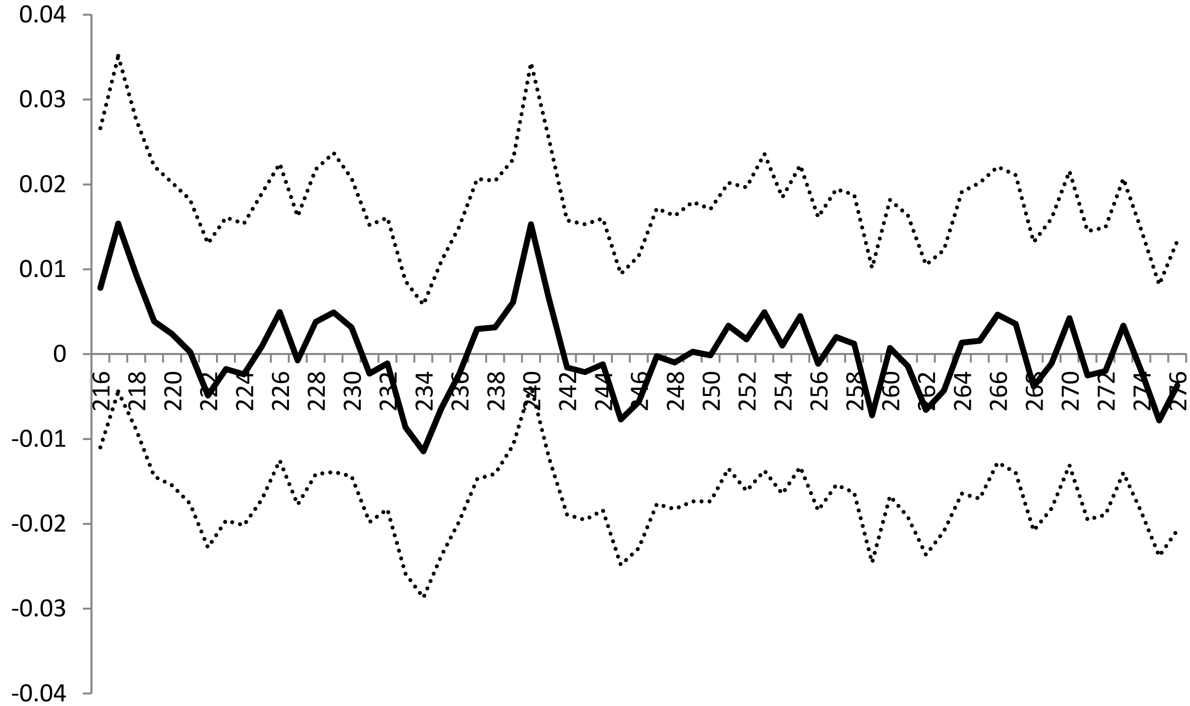
Notes: The points at which birthdays occur are: 18 years (216 months), 19 (228), 20 (240), 21 (252), 22 (264), 23 (276) and 24 years (288 months). Dotted lines represent the 95% confidence interval.

Figure 2 Discontinuity Effects by Month, Ages 18 to 24, Females (quadratic age polynomial)



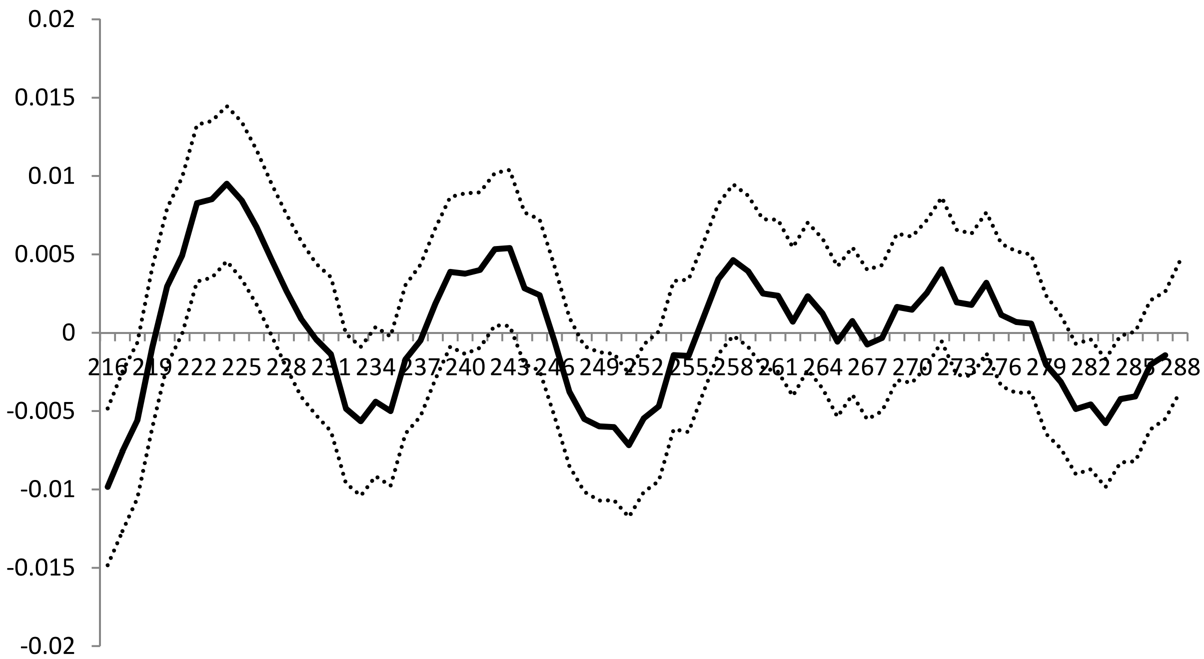
Notes: The points at which birthdays occur are: 18 years (216 months), 19 (228), 20 (240), 21 (252), 22 (264), 23 (276) and 24 years (288 months). Dotted lines represent the 95% confidence interval.

Figure 3 Discontinuity Effects by Month, Ages 18 to 24, Females (cubic age polynomial)



Notes: The points at which birthdays occur are: 18 years (216 months), 19 (228), 20 (240), 21 (252), 22 (264), 23 (276) and 24 years (288 months). Dotted lines represent the 95% confidence interval.

Figure 4 Discontinuity Effects by Month, Ages 18 to 24, Both Genders



Notes: The points at which birthdays occur are: 18 years (216 months), 19 (228), 20 (240), 21 (252), 22 (264), 23 (276) and 24 years (288 months). Dotted lines represent the 95% confidence interval.

Appendix

Regression-discontinuity analysis: Alternative time windows

Total workers. Discontinuity Effects at 21, 22 and 23

	21 years		22 years		23 years	
	6 months	12 months	6 months	12 months	6 months	12 months
Discontinuity[1]	.00092	-.00461	.00116	-.00045	-.00961	.00096
	(.00969)	(.00350)	(.00965)	(.00350)	(.00891)	(.00334)
Dum[2]	.01341	-.00430	.01026	.01483	-.01239	-.00188
	(.01425)	(.00945)	(.01395)	(.02617)	(.01323)	(.00876)
No. observations	57797	109453	57513	108102	56417	107005
Chi-statistic for Whole regression	11048.03	21478.97	11245.37	20836.73	10430.78	19855.19
Pr>Chi	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.1458	0.1496	0.1536	0.1520	0.1563	0.1562

Notes: All the estimations include covariates. (1) estimated discontinuity effect taking into account the impact of age and the threshold dummy variable; (2) estimated impact of the threshold dummy variable. Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Male workers. Discontinuity Effects at 21, 22 and 23

	21 years		22 years		23 years	
	6 months	12 months	6 months	12 months	6 months	12 months
Discontinuity[1]	.01042	-.00883	-.00024	-.00239	.01077	.00532
	(.01352)	(.00476)	(.00793)	(.00479)	(.01269)	(.00459)
Dum[2]	.02918	-.00307	.00052	-.00303	-.00365	.00668
	(.01976)	(.01316)	(.01919)	(.01260)	(.01750)	(.01159)
No. observations	28583	53899	27978	52724	27086	51396
Chi-statistic for Whole regression	6610.71	13098.40	6656.79	12248.60	5547.02	10567.76
Pr>Chi	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.1812	0.1900	0.1955	0.1919	0.1885	0.1888

Notes: All the estimations include covariates. (1) estimated discontinuity effect taking into account the impact of age and the threshold dummy variable; (2) estimated impact of the threshold dummy variable. Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Female workers. Discontinuity Effects at 21, 22 and 23

	21 years		22 years		23 years	
	6 months	12 months	6 months	12 months	6 months	12 months
Discontinuity[(1)]	-.00925	-.00136	-.00665	.01457	-.01932	-.00362
	(.01389)	(.00508)	(.01375)	(.01321)	(.01955)	(.00484)
Dum[(2)]	-.00170	-.00589	.02335	.00031	-.02845	-.01020
	(.02049)	(.01353)	(.02011)	(.00506)	(.01264807)	(.01295)
No. observations	29214	55554	29535	55378	29331	55609
Chi-statistic for Whole regression	5040.66	9529.44	5505.22	10287.81	5987.72	11228.77
Pr>Chi	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.1290	0.1282	0.1417	0.1417	0.1628	0.1602

Notes: All the estimations include covariates. (1) estimated discontinuity effect taking into account the impact of age and the threshold dummy variable; (2) estimated impact of the threshold dummy variable. Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.